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THE HISTORY
OF
THE CONCEPT OF GRAMMATICALISATION

by
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TABLE OF CONTENTS

Table of Contents	i
VOLUME I	V
Acknowledgements	vi
List of Abbreviations	viii
Part 1: Introduction.....	9
1. Introduction.....	10
1.0.1 Grammaticalisation Defined?	13
1.0.2 The History of the Concept of Grammaticalisation	14
1.1 <i>Aims and Objectives</i>	14
1.2 <i>Methodology</i>	18
1.2.1 A General Methodology of the Historiography of Linguistics.....	19
1.2.1.1 Metalanguage	22
1.2.1.2 Dating	24
1.2.1.3 Influence.....	25
1.2.1.4 Objectivity.....	26
1.2.2 More on the Specific Methodology of this Thesis.....	29
1.3 <i>Outline</i>	35
2. Background	38
2.1 <i>The History of Grammaticalisation Studies</i>	38
2.2 <i>The Term Grammaticalisation</i>	45
2.2.1 The Oxford English Dictionary Online.....	50
2.2.2 Merriam-Webster Online	51
2.2.3 Svenska Akademiens Ordbok (SAOB) Online.....	51
2.3 <i>Gradual and Unidirectional Change?</i>	55
2.4 <i>Grammaticalisation Theory?</i>	55
Part 2: The Concept of Grammaticalisation.....	59
3. The Unidirectionality Hypothesis.....	60
3.0 <i>Introduction</i>	60
3.0.1 Outline of Chapter Three	61
3.0.2 Introduction to Unidirectionality and Grammaticalisation	62
3.1 <i>The Unidirectionality Hypothesis</i>	63
3.1.1 What is Unidirectionality?	63
3.2 <i>The Strong Hypothesis and Reconstructions in Historical Linguistics</i>	73
3.2.1 Unidirectionality and Reconstructions.....	75
3.3 <i>Explaining Unidirectionality</i>	78
3.4 <i>Counterdirectional Processes and Counterexamples</i>	85
3.4.1 Lexicalisation.....	90
3.4.2 Degrammaticalisation	109
3.4.3 Regrammaticalisation, Functional Renewal, Exaptation	119
3.5 <i>An Informative, or Confusing, Look at Some Examples</i>	127
3.5.1 The Example Confusion	127
3.5.1.1 <i>Hiu tagu</i> > ... > <i>heute</i>	127
3.5.1.2 <i>Haidus</i> > <i>-heit</i>	129
3.5.1.3 <i>-ade</i> > <i>ade</i>	130
3.5.1.4 <i>mente</i> > Adverb formative	131

3.5.1.5 Latin <i>-esc-/-isc-</i> (inchoative) > Romance languages (‘meaningless’ part of the inflection of some verbs)	132
3.5.2. The Process Confusion	134
3.5.2.1 Senior(em) > Sp. Señor, It. Signore, Fr. Seigneur	134
3.5.2.2 Suffix, submorphemic unit or transmorphemic unit > derivations or inflections	135
3.5.2.3 Conversion from a lexical category to a grammatical category	136
3.6 <i>Uniformitarianism</i>	137
3.7 <i>Conclusions and Summary</i>	145
4. Reanalysis and Analogy in relation to Grammaticalisation	148
4.0 <i>Introduction</i>	148
4.1 <i>Reanalysis</i>	151
4.1.1 Defining Reanalysis	155
4.1.2 Ambiguity as a Prerequisite of Reanalysis	171
4.1.3 Reanalysis and Grammaticalisation - Synonyms?	173
4.1.4 The History of Reanalysis	178
4.2 <i>Analogy</i>	180
4.2.1 Analogy – Terminology and Concept	181
4.2.2 The History of Analogy	186
4.2.3 Simplification?	194
4.3 <i>Summary and Conclusions</i>	195
VOLUME II	197
Part 3: The History of Grammaticalisation	198
5. Early Treatments of Grammaticalisation (The Nineteenth Century)	199
5.0 <i>Introduction</i>	199
5.0.1 Early Treatments of Grammaticalisation during this Period.	200
5.0.2 Agglutination Theory, Historical Grammar and Grammaticalisation	202
5.1 <i>Germany</i>	203
5.1.1 Wilhelm von Humboldt (1767-1835)	203
5.1.2 Karl Wilhelm Friedrich von Schlegel (1772-1829)	205
5.1.3 August Wilhelm von Schlegel (1767-1845)	207
5.1.4 Franz Bopp (1791-1867)	210
5.1.5 Georg von der Gabelentz (1840-1893)	216
5.1.6 Berthold Delbrück (1842-1927)	218
5.1.7 Hermann Paul (1846 - 1921)	221
5.1.8 Karl Brugmann (1849-1919)	223
5.2 <i>France</i>	229
5.2.1 Jean Baptiste de la Curne de Sainte-Palaye (1697-1781) & Abbé François Seraphin Regnier Desmarais (1632-1713)	229
5.2.2 François-Juste-Marie Raynouard (1761-1836)	231
5.3 <i>Britain</i>	234
5.3.1 The Reverend Richard Garnett (1789-1850)	234
5.3.2 Francis Henry Trithen / Friedrich Heinrich Trithen (1820-1854)	235
5.3.3 Friedrich Max Müller (1823-1900)	236
5.3.4 John Earle (1824-1903)	238
5.3.5 Frederic William Farrar (1831-1903)	242
5.3.6 Archibald Henry Sayce (1845-1933)	243
5.3.7 Henry Sweet (1845-1912)	252

5.3.8 Lionel Graham Horton Horton-Smith (1871-1953).....	256
5.4 <i>The United States</i>	259
5.4.1 William Dwight Whitney (1827-1894).....	259
5.4.2 Maurice Bloomfield (1855-1928).....	260
5.4.3 Edwin Whitfield Fay (1865-1920).....	261
5.4.4 Journal of the American Oriental Society.....	263
5.4.5 American Journal of Philology.....	264
5.4.6 Summary.....	265
5.5 <i>Scandinavia</i>	267
5.5.1 Rasmus Kristian Rask (1787-1832).....	267
5.5.2 Johan Nikolai Madvig (1804-1886).....	273
5.5.3 Esaias (Henrik Wilhelm) Tegnér the younger (1843-1928).....	276
5.5.4 Adolf (Gotthard) Noreen (1854-1925).....	279
5.6 <i>Summary</i>	284
6. Coining, Forgetting and Reviving Grammaticalisation (1900-1970)	286
6.0 <i>Introduction</i>	286
6.1 <i>The Presumed Coiner of the Term Grammaticalisation – (Paul Jules) Antoine Meillet (1866-1936)</i>	290
6.1.1 Grammaticalisation 1912-1970.....	295
6.2 <i>Britain</i>	296
6.2.1 Arthur Waley (1889-1966).....	296
6.2.2 Thomas Burrow (1909-1986).....	297
6.2.3 John Lyons (1932-).....	298
6.2.4 Christopher John Elinger Ball (1935-).....	298
6.3 <i>The United States</i>	300
6.3.1 Edward Sapir (1884-1939).....	300
6.3.2 Leonard Bloomfield (1887-1949).....	301
6.3.3 Joseph Harold Greenberg (1915-2001).....	303
6.3.4 Dwight Le Merton Bolinger (1907-1992).....	304
6.4 <i>Scandinavia</i>	306
6.4.1 Otto Jespersen (1860-1943).....	306
6.4.2 Erik Wellander (1884-1977).....	315
6.4.3 Elias (Gustaf Adolf) Wessén (1889-1981).....	324
6.4.4 Alf Sommerfelt (1892-1965).....	332
6.4.5 (Karl) Birger Bjerre (1902-1993).....	333
6.5 <i>Scholars from Other Regions</i>	337
6.5.1 Jerzy Kuryłowicz (1895-1978).....	337
6.5.2 Viktor Maksimovich Žirmunskij (1891-1971).....	338
6.5.3 Émile Benveniste (1902-1976).....	339
6.6 <i>Summary and Conclusions</i>	341
7. The Revival? (1970 – 2002)	346
7.1 <i>The Corner Stones in Modern Grammaticalisation Theory</i>	346
7.2 <i>Lesser Known Linguists Working on Grammaticalisation</i>	381
7.2.1 Ways of Appreciating the Popularity of Grammaticalisation.....	382
7.2.2 Grammaticalisation Studies in Scandinavia – How has the Concept Fared in Smaller Research Communities?.....	384
7.3 <i>Summary and Conclusions</i>	406
Part 4: Summary and Conclusions	416
8. Conclusions and Summary	417
8.1 <i>What is then Grammaticalisation?</i>	417

8.2 <i>The Development of Grammaticalisation over Two Centuries</i>	418
8.3 <i>The Grammaticalisation Revival (?)</i>	421
8.4 <i>Grammaticalisation in One of the Smaller Corners of the Western World</i>	422
8.5 <i>Grammaticalisation Theory?</i>	423
8.6 <i>Unidirectionality</i>	424
8.7 <i>Final Comments</i>	425
Bibliography	426

Volume I

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LIST OF ABBREVIATIONS

ACC	accusative
GEN	genitive
Gmc	Germanic
IE	Indo-European
MHG	Middle High German
NRG	New Reflections on Grammaticalisation
OE	Old English
OHG	Old High German
PDE	Present Day English
PDG	Present Day German
PL	plural
PTC	participle
SG	singular
UD	unidirectionality

PART 1: INTRODUCTION

1. INTRODUCTION

This is the story of a concept which came to grow into such an important part of linguistics between 1970 and the present that it has entered into many linguistic conversations not even concerned with this concept at all. Some linguists started commenting of loan words “grammaticalising” to the grammar of the language that borrowed them, for instance, borrowed adjectives adapting to the system of agreement in the borrowing language. ‘Cool’ could thus be said to have grammaticalised in Swedish because one can now say *det koola huset, den koola bilen, han är koolare än jag* (= ‘the cool house, the cool car, he is cooler than I am’). Other linguists added it to the titles of their papers to draw attention but then just discussed language change in general, hardly, if at all, touching on grammaticalisation.

This thesis will show the reader how a concept can grow among linguists for many decades, even centuries, how it can change its form and be popular one minute only to be an (almost) outcast the next.

But, this is also a story of a concept which exists between two ‘worlds’, the lexicon and the grammar. It is a story which touches on how the lexicon and the grammar have been and are understood and how this can affect the entities which are in the no-man’s land between these two, part of both, part of neither.

Let me start by introducing the concept, and noting some of the problems surrounding it due to it existing in this region between the lexicon and the grammar. No lay-person can of course be expected to know what we mean by *grammaticalisation*, it is very much a technical term within linguistics and the language sciences. It actually seems as though the concept of grammaticalisation can be very hard to pin down and explain even for a linguist who thinks he/she has found

an example of this, or who wishes to explain what it is to someone else. Many linguists resort to using examples when they try to explain what grammaticalisation is, even when their audience consists of other linguists.

Grammaticalisation is when something becomes grammatical. But **when** is an item grammatical and when is it not grammatical? And if something is not grammatical is it then necessarily lexical? Or is it ungrammatical, i.e. incorrect, which is probably how the lay-person would understand it? This brings us straight into the heart of the fire in a sense, namely the difference between the lexicon and the grammar – where do we draw the line? This is an old problem, and it could certainly be treated at book-length, and it bears a lot on the topic of this thesis. Even when it is not stated explicitly, it may often be there implicitly, in that linguistic disagreements are (at least) sometimes grounded in differing views on what the lexicon and the grammar entail. Are, for instance, derivational morphemes part of the lexicon or of the grammar? Most linguists seem to agree that inflectional morphemes are part of the grammar, whereas derivational morphemes are a bit more problematic. But even if we put inflectional morphemes down to the grammar, does that mean that they are not somehow in the lexicon as well? ^{1, 2} However, does this really matter in the case of grammaticalisation? After all, it is often stressed that grammaticalisation emphasises the non-distinctness of categorisation in language (cf. Hopper & Traugott 1993: 1, 7).

Similarly, what about word order? It is quite clear that word order can be used with grammatical functions, so if it develops that kind of use, should the development then be seen as grammaticalisation? It certainly means that word order can become more grammatical, or does it not?

Is grammaticalisation, then, a process, or a syndrome, or a symptom, or an epiphenomenon which deserves no individual study? The various uses of the term do not give us any definite answers about what grammaticalisation is exactly. Furthermore, it is an area of rather fuzzy definitions, often only marginally revised as compared to other linguists' definitions. Part of the definitions often seem to consist of repeated examples, which only add to the confusion since researchers may have interpreted the examples differently: something which the fuzzy definitions that

¹ Thanks to Isabelle Buchstaller, University of Edinburgh, for discussing this with me in some detail. (May 2002)

² For instance, in the field of Lexical Phonology and Morphology both inflectional and derivational morphemes are part of the lexicon, or “attached in the lexicon”. (April McMahon, p.c. 2003).

occasionally accompany these examples do not make clear at all (cf. Lindström, forth.). Often, it can be hard to pin down exactly what makes the examples good examples of grammaticalisation.

Let us consider the development of *be going to* into a future marker. This exhibits several features that have been considered important parts of grammaticalisation, e.g.: phonological attrition (*I'm gonna*), semantic bleaching (literal movement > purpose > abstract movement in the sense of future), reanalysis ([going [to X]] > [going to [X]]), univerbation (*going to* > *gonna*). But it could also be seen as a form of lexicalisation (cf. Traugott, 1994) in that it conventionalises an implicature, makes it part of the meaning of the phrase *be going to*. Therefore, it might be seen as the creation of a new item in the lexicon (*lexicalisation*).

As Hopper and Traugott, for instance, have noted, there are at least two concepts of grammaticalisation: grammaticalisation as a framework and as a phenomenon. Below (section 1.1) I elaborate on how I shall define grammaticalisation for this thesis. I believe that both of the concepts of grammaticalisation are, in fact, still evolving, still changing. However, perhaps we can say that the fundamental idea behind the concepts has been around for a long time and is still more or less the same – as reflected in the definitions of the phenomenon, which all usually say that grammaticalisation involves something becoming (more) grammatical. This is an idea that even bears some resemblance to etymological studies, as can be seen in the work of John Horne Tooke (1736-1812), for instance. It also certainly has many connections to agglutination theory as noted by Lehmann (1982 [1995]) and Heine et al (1991a), and as will be explored in chapter 5 in particular.

Recently, there have been some suggestions that grammaticalisation is only an epiphenomenon (cf. section 2.4 and 7.3). This suggestion also makes it quite clear that it could be merely a linguistic tool (cf. Campbell and Janda, 2001). It could be a heuristic which has no real existence outside a linguist's toolbox for analysis and description. Still, some linguists definitely believe that grammaticalisation is something that exists in language, waiting for us to find it and start using it to explain what goes on in language. It is important that we try to bear in mind that this is not necessarily the case. We also need to come to an understanding of what we ourselves think grammaticalisation is, because this may bias our findings with regard to language change.

1.0.1 Grammaticalisation Defined?

There have certainly been attempts to provide definitions of the concept of grammaticalisation, and some linguists appear to think they do know exactly what grammaticalisation means. In fact, up until recently, most linguists who worked on grammaticalisation did not seem aware of there being any problems with its definition. However, the question is, do they actually know, or are they wrapping themselves up in nice, warm, cosy blankets, by repeating definitions they have learnt by heart which tell us very little? If one persists in using the same examples repeatedly, one also steers clear of the danger of exploring other examples that may make one realise that one does not know quite what the phenomenon is that one is ‘explaining’. Or one may miss the fact that it is simply an epiphenomenon and might not have anything to give to the research community.

I admit that since grammaticalisation is, in fact, currently used quite frequently in both typological and historical linguistic research, the concept is being tested and pulled and changed. This is likely to be one of the reasons why the uses today may sometimes seem somewhat too heterogeneous. But it is interesting that the definitions have not changed, nor have the paradigm examples. So linguists are still quite happy citing Jerzy Kuryłowicz’s (1895-1978) definition from 1965³:

Grammaticalization consists in the increase of the range of a morpheme advancing from a lexical to a grammatical or from a less grammatical to a more grammatical status, e.g. from a derivative formant to an inflectional one. (Kuryłowicz, 1965 [1975]: 52)

At first, this definition appears fairly straightforward. But when is a morpheme *lexical* and when is it *grammatical*?

³ In the last stages of finalising my thesis I have come across some comments which imply that **some** linguists are now becoming more sceptical of the old definitions in that they have started to consider grammaticalisation of constructions more (cf. e.g. Traugott, 2003b). I have, however, not had the opportunity to incorporate this into my final version and can only hope to return to this in the near future.

1.0.2 *The History of the Concept of Grammaticalisation*

There has not been much work done on the history of grammaticalisation, although there have been occasional paragraphs, articles and chapters on the topic. At the time of writing, probably because of the enormous interest in the phenomenon of grammaticalisation among linguists today, there has also been an increase in the mentions of its history. Many articles mention that the term *grammaticalisation* was (presumably) coined by (Paul Jules) Antoine Meillet (1866-1936) (1912), in fact this seems more or less generally acknowledged among grammaticalisationists. And some mention that it was revived in the 1970s by Givón (1971a) (cf. e.g. Bybee et al., 1994: 4).

The most thorough treatments (Heine et al., 1991a; Lehmann, 1982 [1995]) of the history of grammaticalisation are nonetheless far from complete. However, there is certainly much that can be learnt from them by the scholar with a critical mind, who realises that he/she may have to be sceptical of some of the data and conclusions. The reader of these histories must also take care to note when the author admits that he/she is **not** sure that they have got the history absolutely right.

1.1 Aims and Objectives

The main objective of this project has been to gain some (preliminary) insight into the history of grammaticalisation. By this I mean that I have aimed to study the concept of grammaticalisation and the term *grammaticalisation* (as well as other terms that have been used for similar concepts) and search linguistics, and more specifically grammar and historical linguistics, for its history. It has not been my intention to study the history of any particular languages and look at the grammaticalisations that have taken place in those languages. Although I have looked more closely at some phenomena in languages which may be classed as grammaticalisation, this has only been as a way of gaining a better and deeper understanding of what grammaticalisation can be said to be. My main focus has been on the theory, the concept and the term. I wished to be able to show how close to this concept linguists came in the nineteenth century and how it changed and developed into what it is today. I hoped that I would also be able to come to an understanding of

what influenced linguists to start to look upon certain forms of language change in this way.

In the following parts and chapters, I intend to clarify what grammaticalisation is today (see especially part 2 and chapter 7 in part 3). This means that I will present a picture of how the **term**, *grammaticalisation* (but also *grammaticisation*, *grammatisation*, and similar), has been used; which examples it has been used to label and how it has been defined more recently. I will also consider which processes or phenomena have been viewed as distinct from grammaticalisation and why, and which have been seen as part of grammaticalisation in some way. The main objective here is to contribute to the understanding of grammaticalisation by showing the research community exactly how grammaticalisation has been discussed, to alert it to some problems and thereby hopefully lead to some means of improving the understanding of one another's work and through this also increasing the possibility of making advances in the understanding of these kinds of changes.

I will also follow the history of the **concept** that we usually refer to as *grammaticalisation* back through two centuries (Part 3). I aim to present a commentary on this field of research, a field which at this point in time has acquired an important place in linguistic theory. A lot of work has been published in relation to grammaticalisation in the last 20-30 years; however, there is still a large amount of 'fuzziness' surrounding the topic. I hope that a historical overview can serve two purposes from this perspective. Firstly, that it can give us the opportunity to see things from a different angle. This can possibly enable us to better understand language change through being freed from the subjectivity that results from unconsciously seeing things from a modern perspective, based on the theories and ideas that are most prominent today. Secondly, I also hope that it will serve to give us some knowledge of what has already been done, to save us from reinventing the wheel and making the same mistakes again. An added bonus is that we can bring to light some of the linguists whom we may have nearly, or more completely, forgotten.

There have been claims that some linguists during the second half of the twentieth century had forgotten, or did not know, that grammaticalisation was a concept with a long history and some rather prominent linguists in its past (see e.g. Lehmann, 1982 [1995]: 6). It appears to be a field of research which has gone in and out of fashion in linguistics more than once. Still, both Meillet and Lehmann make it

clear that it never seems to have completely disappeared in the periods when they say it was less popular. Meillet said in 1912 that it had never gone away completely, but that it had, however, been much less popular in the last 40 years:

Sans avoir jamais été perdu de vue, l'autre procédé d'innovation, le passage de mots autonomes au rôle d'agents grammaticaux, a été beaucoup moins étudié durant les quarante dernières années.⁴ (Meillet, 1912; 1921: 133)

Similarly, Lehmann concluded that “[i]n the period of American and even of European structuralism, topics such as grammaticalization were not fashionable” (Lehmann, 1982 [1995]: 6). However, later he proceeded to say that “[o]utside structuralism, the Indo-Europeanist tradition of grammaticalization theory remained uninterrupted” (Lehmann, 1982 [1995]: 7). Still, the linguists and publications with an explicit interest in grammaticalisation after the 1920s and up until the late 1960s, early 1970s, appear to have been very few. Lehmann (1982 [1995]) lists hardly any who happened to mention something which resembles grammaticalisation between Meillet and Givón (1971a): certainly, there was hardly anyone who used the term, or who was actually interested in the theory. The only work where “agglutination theory” is said to figure “prominently” is in a book by Carl Meinhof (1857-1944) (1936). And in a later period of more frequent writings on grammaticalisation in historical and comparative linguistics Émile Benveniste (1902-1976) and Kuryłowicz were two of the linguists who mentioned something much like what we call *grammaticalisation* today, and Kuryłowicz even used the actual term *grammaticalisation* (cf. Benveniste, 1968; Kuryłowicz, 1965 [1975]).

This work is intended to throw some light on the issue of the extent to which some linguists in the 1970s disregarded the past of this topic, and the degree to which the topic has drifted in and out of popularity. I hope that I will be able to bring to the fore some of the scholars who have contributed to what linguists think and believe today, and more specifically – what ‘grammaticalisationists’ think and believe. Because grammaticalisation has been a phenomenon whose popularity seems to have fluctuated historically, some have not seemed to realise that it has a history, whereas

⁴ Translation: Without ever being lost from sight, the other process of innovation, the passing of autonomous words into the role of grammatical agents, has been much less studied in the last 40 years.

others have looked for some form of ‘pedigree’, perhaps accepting the possibility that they have found an ancestor a bit too easily.

With the position grammaticalisation holds in linguistics today, and more particularly in historical linguistics, functional grammar and typology, we must try to learn more about its history and development and we ought to aim to give credit to and learn from the linguists who have worked in this ‘framework’ and on this phenomenon in the past. It is important that we do this as thoroughly as possible and do not simply dive on the first instance of a possible predecessor within this area of linguistics, but look as widely as possible for instances of similar concepts. We should not just jump at the first possible ‘ancestor’ that appears, since this could lead to more confusion than usefulness. When we find a similar concept this must not only be thoroughly scrutinised, but we must also consider what the possible links are between the particular piece of writing in which it appears and the work on grammaticalisation at present. Another aim of this thesis is therefore to give an **objective** overview of the history of this concept, which can serve to show whether it is true that this concept has gone through cycles of varying popularity and, hopefully, clarify what may have caused this variation.

I also believe that we can easily get tangled up in webs of fuzzy terminology, which may cause us to change the meaning of the terms. One indication that linguists might not have fully understood the terminology could be that they use repeated examples rather than more precise definitions. Consequently, another aim of this thesis is therefore to study how well defined the concepts closely related to grammaticalisation have been, whether the meaning of various associated terms has changed and how much reliance has been put on definitive examples.⁵ The overarching objective here is that we should understand the importance, as well as the restrictions, of terminology, definitions and examples in science.

I will make an attempt at bringing greater clarity to some of today’s linguistic terminology, while depicting the history of a term and a concept. Since I believe that no one seems quite certain of what exactly grammaticalisation is, there will be no attempt at this early point to provide a definition, which aims to cover the exact concept. However, I will need to stipulate what it is I shall be looking for in the past, and for this I will make use of two rather broad definitions of grammaticalisation:

⁵ N.B. There may well have been a lack of terminology or the terms may have been changed.

- (1) The development of grammatical means of expression – such as inflectional morphemes, word order restrictions, function words, including or excluding derivational forms, etc. (incl. examples such as *foot-feet*).
- (2) The change through which a lexical element becomes a grammatical element (where a ‘grammatical element’ may include or exclude derivational forms).

I should add that I realise that since the term *grammaticalisation* is believed to have been used for nearly a century, we cannot change the fact that during this time it has been used to mean different things. What we can do is try to find what the uses have in common and try to suggest a standardisation based on this. But, primarily we can make linguists aware of the differences and the lack of clarity, and thereby argue for proper definitions whenever terms like *grammaticalisation*, *lexicalisation*, *degrammaticalisation*, *reanalysis*, etc., are used, to promote a better and more unified understanding of these phenomena. The objective is to realise the importance of definitions and the role of terminology in linguistics, as was mentioned above.

1.2 Methodology

In attempting to write a history of any concept or theory, there are plenty of factors one has to take into account. In this case I am tracing a concept, an idea – an idea which touches upon every level of linguistic description: morphology, phonology, syntax, vocabulary, discourse, semantics and pragmatics. This makes it even more important to keep an open mind about where the influence may come from, and what the concept is exactly. It is also essential that we consider linguists’ more general thoughts on language, language change and language evolution at various points throughout the history of linguistics, as well as the ‘social standing’ of particular languages in different countries, and in the international research community. This may influence the type of language that is seen as superior and more beautiful, or more logical, and thereby what is seen as a more highly evolved language. Furthermore, we need to bear in mind which were the mother tongue(s) of the scholars who have had an effect on the emergence of this concept, because as Harris and Taylor (1997: 57) note, “linguistic theorizing” is likely to be “based on extrapolation from the native language of the theorist” to some extent.

We must also bear in mind that we are ‘people of the past’. What I mean by this is that what we think, and the theories we develop, depend upon what other people have thought and hypothesised before us. However, just as we cannot (nor would we want to) freeze a particular language at one stage, nor can we freeze the science of language, and therefore nor can we freeze its terms. We are continuously learning more about the world in which we live, including the languages we speak and how the human mind works. This necessitates the development of our theories and terminology. As Robins puts this:

Linguistic science today, like other parts of human knowledge and learning, and like all aspects of human cultures, is the product of its past and the matrix of its future. [...] Scientists do not start from scratch in each generation, but work within and on the basis of the situation which their science, and science in general, has inherited in their culture and in their age. (Robins, 1997: 3)

1.2.1 A General Methodology of the Historiography of Linguistics

Methodology of history or historiography of linguistics may seem a rather underdeveloped area. Only quite recently have methods of the history of linguistics become an area of debate and linguistic historians have started to attempt to formulate some principles for work in this area. But still it may be questionable whether (the general) historiographer of linguistics pays much attention to this, as indicated by Andrew Linn:

Historians of linguistics have training in languages or psychology, for example, but very rarely in historical study, which many of us launch into without having any real idea about how trained historians approach the past. [...]

Most practitioners in humanities subjects simply study history or literature or society or whatever in blissful ignorance of the abstract arguments raging about how they should be doing it. (Linn, 2001: 208-209)

The area of the history of linguistics is not often taught as a separate course or module at universities, instead parts of the history of linguistics tend to be worked into different courses and students may not get much opportunity to specialise in this field. Many scholars in the field of the history of linguistics only have the chance to study this area of interest ‘on the side’, so to speak, which may lead to a feeling of

not having enough time to devote to the worries of methodology, wanting instead to produce studies with facts and hypotheses about linguistics and linguists. This can lead to the feeling that that is much more important and that it cannot go awry simply because they have not taken the time to ponder over their methodology.

But it is always important to consider which methods to employ in one's work, in order to obtain reliable results. However, as David Cram says that does not have to mean that "we need to take time out of our busy lives as practising chroniclers and historiographers to smoke our pipes, Gandalf-like, and reflect philosophically on our activities" (Cram, 2003: 16). But instead he claims that metahistoriography should be seen as a necessary **part** of our work on the history of linguistics (Cram, 2003: 16).

Konrad Koerner has spent quite a lot of time, in recent years at least, criticising scholars's findings from a methodological angle. With the following general remarks he has indicated how bad the situation may have seemed at one point at least (hopefully it has improved to some extent since then):

In contrast to linguistics proper the history of linguistic thought has been presented and treated generally without theoretical prerequisites, with the result that its authors engage in writing chronicle not history to use Collingwood's (1946: 202f) [(see Collingwood, 1946 [1970])] important distinction. Others [...] have indulged in 'Whig history' (Collingwood), i.e. presenting earlier periods in such a manner that one's present is strengthened. (Koerner, 1974: 121)

However, on a more positive note, we can see that Koerner has now admitted that:

Recent years have witnessed the appearance of major studies devoted to questions of methodology and epistemology in the writing of the history of linguistics ... (Koerner, 1987: 13)

Much of this has been of his own doing, but there are a number of other linguists who have also come to be rather active in this area, such as Peter Schmitter (e.g. 1999) and Pierre Swiggers (e.g. 1983; 1986), and other historiographers also touch on this issue occasionally (e.g. Cram, 2003; Linn, 2001). Cram (2003: 11-12) claims that Schmitter and to some extent also Klaus D. Dutz were among the first to concern themselves with the metahistoriography of linguistics. And he believes that it was in

the 1980s that this research area really started to become popular, although he says that at a point during the 1990s it started to lose some of its force again.

The increased interest in historiographical methodology is seen by Koerner as a “sign of linguistic historiography coming of age” (1987: 13). He recognises that work within this field of interest has a long history, but at the same time claims that history of linguistics only became a “*bona fide* subject of academic research” in the latter half of the twentieth century (1995b: 3). And even then it took years for it to become more scientific and objective, frequently having been seen more as a way of promoting a particular branch of linguistics, for instance (Koerner, 1995b). Swiggers (1983: 55), similarly, sees the 1970s as the period when the historiography of linguistics “s’est organisée professionnellement”⁶ and when more attention started to be paid to methodology.

Schmitter (1999) has noted that historiography can be approached in different ways. He distinguishes between what he calls *Pluralist Historians / Narrativists* and *Positivist Historians*. The difference between the two types of historian is essentially that the positivist wants to give us the ‘true history of linguistics’ and believes that there is only **one** true history. The pluralists / narrativists, conversely, recognise that history is a matter of interpretation and that there can therefore be several versions of history. This narrative view, Schmitter notes, has sometimes been seen as a story among historians themselves. He claims that others before him (e.g. Mackert, 1993) have put him in the group of *Pluralist Historians*, a categorisation he seems to agree with, whereas historiographers like Koerner he puts in the group called *Positivist Historians* (Schmitter, 1999).

History and historiography of other sciences can clearly be drawn on when trying to formulate methods for historical studies of linguistics. For instance, in Koerner’s (1987) article he refers to a number of papers on methods of historiography of psychology and the behavioural sciences, which have brought up the issue of the dating of references (Brozek, 1970; Vande Kemp, 1984). Similarly, Linn (2001) has also indicated that it would be desirable, or even necessary, that historiographers of linguistics should familiarise themselves much more with methods of history:

⁶Translation: organised itself professionally

The history of linguistics has two parent disciplines – history and linguistics – but it has inherited most of its genetic make-up from history, the direct investigation of the past, and only a little bit from linguistics, the direct investigation of language. (Linn, 2001: 208)

But even though Koerner himself makes use of work on historiography in general and the historiography of other disciplines, he stresses that we should not expect to be able to adopt methods of general history or history of other disciplines directly without first adapting them to better suit our own requirements (Koerner, 1995a).

Many of the problems of historiographical method that Koerner (1987; 1995a) raises certainly need to be considered also in my work. Several of these may seem fairly obvious. However, since I think it can be worthwhile to state things explicitly I will go through them one by one and try to relate them to this thesis (cf. Koerner, 1987; 1995a).

1.2.1.1 Metalanguage

One of the observations made by Koerner is that the use of current linguistic terms in historical work can utterly distort the facts. He describes the problem in this way:

... ‘the problem of metalanguage’ in linguistic historiography [...] the use of a language for the description of linguistic concepts, ideas or theories of earlier periods in the study of language which does not misrepresent the meaning or intention of a given author while at the same trying to make the reflections of past epochs in the discipline accessible to the present-day practitioner in the field. (Koerner, 1987: 13)

I agree that we must be careful how we use terminology and how we relate concepts to one another, so that we do not read our own knowledge and ideas into the knowledge and concepts of the past, which will only produce anachronisms and misunderstandings. Still, I think that we must recognise that we as historians of linguistics normally want to relate the past to the present. We want to be able to see where there has been a continuation of ideas and where there has been change, perhaps even revolutionary change (cf. Koerner, 1995a). Furthermore, we want to write our work in a language that makes it accessible to modern linguists, and this sometimes leads to the introduction of contemporary, recent terminology in discussions of the past (cf. Koerner, 1995a). This makes the theoretical issue rather

complicated, and it makes it especially important to be aware of the problems that it may involve.

In this thesis, the metalinguistic aspect will not only be a point of critical analysis of current work on grammaticalisation. Anachronisms in the terminological and conceptual studies of historical aspects of work on grammaticalisation will also be an issue. Historical aspects are quite often mentioned and criticised in the writings on grammaticalisation from the last few decades, but frequently in a rather anachronistic manner. Hopper and Traugott (1993), for instance, seem critical of Meillet (1912) when they conclude that grammaticalisation and reanalysis were more or less the same to him in his famous paper, which is presumed to have given us the term *grammaticalisation*.

Meillet appears to have identified reanalysis with grammaticalization. However, although many cases of reanalysis are cases of grammaticalization [...], not all are the result of reduction of a lexical item or phrase into one that is more grammatical, less lexically categorial, etc. (Hopper and Traugott, 1993: 48)

As we have seen, Meillet made a distinction between the development of new grammatical forms and arrangements on the one hand, and analogy on the other. The first, which he called grammaticalization, is the result of what we now call reanalysis. (Hopper and Traugott, 1993: 56)

But *reanalysis* was not available as a term at the time, and if the concept of grammaticalisation in Meillet's sense happened to be close(r) to what we mean by *reanalysis*, then that is something we simply have to accept. We cannot assume that because someone, for instance, Meillet, uses 'our term' *grammaticalisation*, he should necessarily mean the same as we do now.

Metalinguistic issues will also need to be constantly borne in mind in the discussion of earlier work that may have included a **concept** similar to what could today be called *grammaticalisation*, but which was not called so then. Perhaps it was not labelled by any term at all at that particular point in the past, or by that scholar. We should also remember that it is quite possible that we will find that older concepts bearing some resemblance to what we mean by *grammaticalisation* today, are broader or more narrow. One concept might, for instance, include grammaticalisation, lexicalisation and degrammaticalisation, or alternatively only reanalysis. Still, they are important to consider. In fact, they would be very interesting to us since they could suggest an alternative categorisation which may

show us that something may be interpreted differently. However, they should not be referred to as *grammaticalisation* since that would only cause confusion and misunderstandings.

I should mention that as a means of distinguishing between the term and the concept, I shall be using italics <*grammaticalisation*> for terms, and regular script for concepts <grammaticalisation>. If I want to stress something or introduce a new concept of importance this will be presented in bold type, e.g., <**grammaticalisation**>, <*grammaticalisation*> and the same will be used for regular emphasis, as in <**more** distinct>, except in quotations where I will naturally retain the original means of emphasis, unless the emphasis is added by me and this will then be clearly indicated. While on the subject of orthography, let me also mention that single quotation marks will be used for odd expressions, new terms, etc, while inline quotations will appear within double citation marks.

1.2.1.2 Dating

Koerner (1987) bases his comments regarding the problem of dating on Brozek (1970) and Vande Kemp (1984). He does not discuss this problem very much himself, and actually only mentions that it can be of some importance. Naturally, the degree of importance also varies depending on the specific study. I do not think this aspect should be too problematic in this thesis, however, apart from in the discussion of possible influences, which is also where Koerner (1987) notes that it is of particular importance.

It is also worthy of note that, as all scholars are well aware, the time from the first presentation of a paper to the publication of the same can be a matter of years and it may therefore be difficult to conclude who was first to use a term or to propose a new theory, or hypothesis. This is likely to produce some problems in my treatment of the ‘revival of grammaticalisation’ in the 1970s. However, I hope to be able to overcome this by taking note of earlier presentation dates when given, as well as details of the actual conferences when papers have been published in proceedings a couple of years after the conference took place.

In the treatment of the 1970s and 1980s I have also contacted some of the linguists who were working on fields related to grammaticalisation at that time. I judged it as particularly important to contact Joan Bybee, Talmy Givón, Bernd Heine, Paul John Hopper, Christian Lehmann and Elizabeth Closs Traugott, who

have all clearly made important marks on grammaticalisation as it stands today. These scholars have all responded and I have had somewhat deeper discussions with Givón and Traugott.

1.2.1.3 Influence

We often speak quite loosely of influence, and, as Koerner says, the term *influence* has not been very well defined in the history of linguistics (Koerner, 1987: 14; cf. 1995a: 14):

As a matter of fact, most writers do not define it at all but simply use it as if everyone was in agreement on the meaning of this concept. (1987: 14)

This, of course, not only applies to the term *influence* but to much terminology. Still, since some of the main hypotheses in the history of linguistics deal with the issue of influence, it is very important that we should stress this.

Possible influences will be discussed at various points throughout this thesis, in particular the role which Meillet's thoughts may have had on linguists working on grammaticalisation more recently. It may be useful to distinguish between two levels of influence – both (1) influence on the theory of grammaticalisation and (2) influence which has led scholars to consider grammaticalisation-like changes in the first place.

It has also been noted that there are different kinds of influence, although these are seldom distinguished in the literature. We can hypothesise influence due to, for instance, “shared experiences, education, and the *Zeitgeist* in general,” but also “direct influence that could be documented on the basis of explicit references, textual parallels, public acknowledgement, and the like” (Koerner, 1995a: 14).

I will attempt to provide some clarity on the matter of influence in my own work by distinguishing between four different types of influence:

- *direct influence*: not in the sense of Koerner, but in the sense of a (recognised or possible, see below) connection between two scholars.
- *indirect influence*: when a scholar was either influenced by another scholar's work through a third scholar or through a school, or when a scholar's work seems influenced by the *Zeitgeist*.

- *recognised influence*: when it is clear that there was definitely a connection between two (or more scholars) through explicit references or acknowledgements of debt.
- *possible influence*: when we can only hypothesise as to whether a scholar was influenced by someone or something, based on his/her library records, similarities of style, similar notions, etc.

I believe it is important in attempting to promote knowledge about the history of linguistics that we do not hold back on our hypotheses even if we cannot always prove them. Therefore, I shall be including references to possible influence, not yet proven, as well as those cases where it is more certain that a person has been influenced by someone or that the framework of grammaticalisation has been influenced and shaped by someone or something (e.g. through explicit references).

1.2.1.4 Objectivity

Another aspect that we should consider in our work, if we want historiography to be as scientific as possible, is our objectivity. As Schmitter (1999: 201-204) has remarked, there are different forms of objectivity that have to be considered: empirical objectivity (“*empirische Objektivität*”), interpretative objectivity (“*interpretative Objektivität*”), restrictive objectivity (“*restriktive Objektivität*”) and narrative objectivity (“*narrative Objektivität*”).

Empirical objectivity concerns the reconstruction of facts, where one naturally has first of all to secure the sources – e.g. how one dates the texts, how one finds out who the author is, and how one secures its authenticity. Since securing of the source involves working with an object that can be empirically studied, this is called *empirical objectivity*.

Historians of linguistics try to reconstruct theories, elements of theories etc; reconstructed facts that they usually attempt to build primarily on texts from the past. For this it is important to know a fair amount about the author and his/her connections to and thoughts about other authors. As Schmitter says, it is in this case very much a matter of interpretation and one’s interpretation of a text is likely to be influenced by one’s research questions among other factors. But Schmitter still sees it as objective, although more qualitative, since it can be checked and reviewed. He chooses to call this *interpretative objectivity* (Schmitter, 1999: 202-203).

This aspect of objectivity may perhaps be slightly more important to this specific thesis than the first one. Since I have mainly worked with well-known historical texts, there should not be much problem with dating and authorship nor with authenticity. However, one problem relating to this could be when texts are read in translation and there may have been a misunderstanding or misrepresentation on the part of the translator. I have therefore always read the most relevant passages in the original language, even though I have read at least some translations of German texts for two reasons: (1) My understanding is greater in English, which also means that I can read faster in English and (2) it is possible that English-speaking scholars will have primarily read the translations when these were available and it will therefore be important not to forget to look at these as well as to compare them to the original.

Naturally, I impose my own interpretation on the texts, but I have attempted to be as objective as possible. I am aware of the fact that I may also very easily read the current concept of grammaticalisation into earlier works. I have attempted to avoid this by trying to keep an open mind as to what grammaticalisation is – considering all kinds of uses, broad and narrow, synchronic and diachronic, in the present, and also by looking closely at related processes – such as reanalysis and bleaching. I hope that this will have helped me detect the aspects of grammaticalisation that were present in earlier works, without necessarily drawing the conclusion that the scholars had a concept of grammaticalisation – and being open to seeing other (possibly) related concepts. I have also read widely in the current literature before starting to look at the earlier works which I have studied. This was the only way that I could get a better understanding of what grammaticalisation is today, which naturally I needed, in order to know what kind of concept I wanted to look for in the past. Nonetheless I refrained from defining the concept for as long as possible, keeping it as a rather vague idea, to ensure my own openness to various other ideas that might have been similar.⁷

The next point on objectivity that Schmitter (1999) raises is the question of the selection of one's facts. Here the researcher needs to realise that he/she can easily bias the selection and must therefore work to make sure that this does not happen:

⁷ I finally specified two kinds of grammaticalisation that I would look for and compare previous concepts to, see 1.1.

Es sind dies zum einen die Kriterien des Objektsbereichs, der Wissenschaftlichkeit und der Methodizität, zu denen dann zum anderen noch bestimmte chronologische, geographische, produzentenbezogene und thematische Spezifikationskriterien sowie allgemeine Relevanzkriterien treten. Das bedeutet, daß die Faktenselektion ganz von den Entscheidungen, die der Historiograph trifft, abhängt. Daher ist die Faktenselektion noch stärker als die vorangehenden Stufen vom historiographischen Subjekt her geprägt. (Schmitter, 1999: 203)⁸

The main point that he makes about objectivity is that it has to have “intersubjektive Gültigkeit” (i.e. intersubjective validity). He calls this *objectivity of selection – restrictive objectivity* (Schmitter 1999: 203-204). This is, of course, important in my work, and I have tried to guarantee such objectivity by trying to understand what was important at various points in time, so that I do not misinterpret or emphasise the wrong things.

Another important aspect of historiography is, according to Schmitter, the ‘bundling’ of theories and the comparison of theories. As long as this can also be checked against the sources and general means of interpretation, it can be called *narrative objectivity* (Schmitter 1999: 204). This is important to bear in mind also in this thesis. Sources must be clear and it must be evident that the interpretations are based on what they say, so that they can be checked. I have tried to make the sources as transparent as possible and I have also included numerous quotations to make it simpler to cross-check my interpretation with the originals. Unfortunately, this has made this thesis rather long.

Like Schmitter, Swiggers (1983: 73-74) has discussed the objectivity of historiography and in fact he has questioned it:

C’est ici que nous préconisons une attitude instrumentaliste: en effet, l’historiographie ne peut dire que l’objet historique *est* tel (ou *a* une telle structure) qu’il l’a reconstruit. Les différents points de vue et leurs élaborations respectives sont autant de caractérisation de *x*, à condition que celles-ci lui conviennent.⁹

⁸ Translation: First of all, there are the criteria for the area of the object, of the scientificity, and of the method, secondly there are also certain chronological, geographical, producer-determined and thematic criteria of specification, as well as general criteria of relevance. This means that the selection of facts depends entirely on the decision reached by the historiographer. For this reason the selection of facts is more strongly affected by the historiographical subject than the preceding level.

⁹ Translation: It is here that we recognise an instrumentalist attitude: in fact, historiography can only say that the historical object *is* such (or *has* such a structure) as it has reconstructed it. The different points of view and their respective elaboration are so much of the characterisation of *x*, on the condition that these ones suit it.

Swiggers (1983) is more sceptical than Schmitter (1999), of the possibility of conducting objective historiography. However, he still considers it important and he also proposes various definitions and explanations of what it is that historiography attempts to do. His scepticism is probably simply another way of ensuring that people understand that there is not just one truth, that there is not just one history – in other words, it is very clear that Swiggers, like Schmitter and unlike Koerner, could be seen as a pluralist historian.

1.2.2 More on the Specific Methodology of this Thesis

A study of this kind, which primarily aims to present a picture of the history of a concept within linguistics will, of course, mainly be working from published material in the field, rather than from actual linguistic data. I will discuss as many uses of *grammaticalisation*, *grammaticisation* and near synonymous (and closely related) terms as possible in the last thirty years (1971-2002)¹⁰, in an attempt to enable the reader to form a clear understanding of what grammaticalisation is today. This has also been a necessary step for me in my research in order to know what to look for in earlier work where the term *grammaticalisation*, or similar terms, was not used. I have also thoroughly analysed the examples that are cited, and will discuss some of them as appropriate. A study of the examples not only provides a better understanding of how the researcher has come to his/her conclusions and what the differences are between the various uses of the term *grammaticalisation* (and its near-synonyms), but it has also been a way for me to form a clearer idea of what grammaticalisation is.

Grammaticalisation is currently a huge field of research, not least due to the amount of work that has been done in the field in more recent years. I will therefore have to restrict my work in some way. To cover the whole field of grammaticalisation studies from its beginning (whenever that may be) up until today would not be possible. I have therefore decided to take primarily an Anglo-American-Scandinavian approach, with some necessary glimpses of the French and the German situations, the latter of which will be treated more thoroughly of the two.

¹⁰ These dates have been selected because in 1971 Talmy Givón's article which has been considered to revive grammaticalisation was published (cf. Bybee et al., 1994: 4; Lehmann, 1982 [1995]) and 2002 marks the publication of the proceedings from the first of the *New Reflections on Grammaticalization* conferences, which was held in Potsdam, Germany (1999) (Wischer and Diewald, 2002).

In other words, I shall be researching the development of grammaticalisation in Britain, the USA, and Scandinavia (Denmark, Norway and Sweden). However, since Germany and France have at times been the leading centres of academic study in general and not least for linguistics, and since German scholars have shown a lively interest in grammaticalisation today and has been very influential in the area, these two countries will also be looked at. Occasionally, works by linguists in other countries will also be considered if they have published in international journals or internationally circulated books, such as proceedings and edited volumes on grammaticalisation.

My reasons for choosing these areas are as follows:

The USA has in the latest upsurge in grammaticalisation studies, i.e. since the 1970s, been an important centre for work in this area, with linguists like Joan L. Bybee, Talmy Givón, Paul John Hopper, and Elizabeth Closs Traugott, in particular, making important impacts.

Scandinavia will be looked at for several reasons. For one, Scandinavian linguistics deserves more mention than it sometimes receives internationally, something which is much due to the fact that Scandinavian scholars have a very good network amongst themselves and quite often appear mainly at Scandinavian conferences and in Scandinavian journals, where they often present / publish in one of the Scandinavian languages.¹¹ But also because Scandinavia has raised many interesting scholars in the past which at the time often had quite good connections with scholars in Germany, France and also in Britain and the USA. And, last but not least, because most work that appears on grammaticalisation still comes from the USA and Germany it would be interesting to see how much this has influenced one of the smaller corners of the western world (from the point of view of the size of the population and the academic community).

Britain does not appear to have had a very active tradition of grammaticalisation studies, however it was the home of one of the scholars who has been seen as a predecessor of the theory, namely Horne Tooke (see e.g. Jooker, 1999; Lehmann, 1982 [1995]) and it will be interesting to see how his views are reflected in the present framework / theory and how his work and ideas might have

¹¹ One scholar who has done some work on this recently is the Norwegian Jan Engh who has looked particularly at where Norwegian linguists have tended to publish their work in the last 100 years. (Jan Engh, p.c. 2003)

been carried on. In addition, Britain is of interest since it is one of the major English-speaking countries, a country with access both to reading and publishing in, international journals. Thereby British linguists also have the opportunity of being read extensively, but still Britain does not seem to have played a very prominent role in the more recent grammaticalisation debates. It will therefore form an interesting geographical area to compare to both the USA (one of the centres in the development of modern grammaticalisation studies) and Scandinavia (a small region where scholars quite often mainly publish in intraregional (i.e. Scandinavian) publications and appear at intraregional conferences rather than internationally, and are in that way more isolated from the international research community even though they certainly read international publications).¹²

As I mentioned above, France will also appear on occasion in the following pages. These will be rather sporadic appearances since I have not been able to conduct a full scale study of grammaticalisation studies in France, but will nevertheless prove very important since the French linguist Meillet is often seen as the coiner of the term *grammaticalisation*. In addition, Meillet's student, Benveniste, has also been mentioned in previous works on the history of grammaticalisation (cf. e.g., Hopper and Traugott, 1993; 2003; Lehmann, 1982 [1995]).

It seems that Germany and the USA have, in fact, been particularly active in the development of grammaticalisation since 1970. Germany has played an important role in shaping the concept both in the past before the term existed and more recently. Some of the German linguists that have had a role in grammaticalisation more recently are: Gabriele Diewald, Martin Haspelmath, Bernd Heine, Christian Lehmann; and Lehmann (1982 [1995]) mentions concepts similar to grammaticalisation being present in the works and ideas of, e.g., Wilhelm von Humboldt (1767-1835), August Wilhelm von Schlegel (1767-1845) and Georg von der Gabelentz (1840-1893).

I have tried to find earlier theories which bear some resemblance to modern grammaticalisation theories, which seem to revolve around the same concept, although they were not called by that name. Sometimes they have not really been given any particular label at all. The works that I set as my starting point were:

¹² It is quite possible that similar theories of language change have existed in other linguistic traditions (see for instance Heine et al (1991a) who suggest ancient Chinese correlations to the concept), but it will not be possible to go into this in this thesis.

Franz Bopp (1791-1867) (1816) *Über die Konjugationssystem der Sanskritsprache in Vergleichung mit jenem der griechischen, lateinischen, persischen und germanischen Sprache.*

August Wilhelm von Schlegel (1767-1845) (1818) *Observations sur la langue et la littérature provençales.*

Rasmus Rask (1787-1832) (1811) *Vejledning til det islandske eller gamle nordiske sprog.*

The first of the three works was chosen as a starting point partly because Bopp has been seen as one of those who introduced the concept of the root from the Indian grammarians into Western scholarship. Since grammaticalisation has a strong connection to flexions and affixes, the root-concept is likely to have been important in the history of grammaticalisation. Another reason for setting this work as a starting point is that Lehmann (1982 [1995]: 3) has recognised that it was the first to apply agglutination theory.

A. Schlegel has explicitly been mentioned as the one who introduced many of the stock examples of grammaticalisation (Lehmann, 1982 [1995]: 1) in his *Observations* from 1818, and therefore this forms another obvious starting point. Rask, finally, was undoubtedly the greatest linguist in Scandinavia at the beginning of the nineteenth century. He was very interested in comparative linguistics and he also had an interest in typology, which means that there is a strong chance he could have noted something similar to grammaticalisation. His first study of comparative linguistics was a prize essay presented to the Royal Danish Academy in 1814, but which was only published in 1818 (Rask, 1818b). However, even before that he had written a more descriptive historical study of Old Norse which, given his interest in historical comparative grammar, could prove of interest, and will therefore be one of the starting points (Rask, 1811) (cf. Hovdhaugen et al., 2000).

Even though I say that these are my starting points, I will occasionally go back even further. This will, however, only be the case if one of the linguists within the period I aim to study directly refers to a specific publication or linguist in the past, or if the scholar to whom this leads us makes direct reference to an even older scholar or publication. This is seen as a means of attempting to ascertain where and when the concept of grammaticalisation may have started to take shape, and as a means of facilitating future work on the history of grammaticalisation.

There are certain scholars who are already quite well known as predecessors to modern grammaticalisation theory, such as Humboldt, Gabelentz. And occasionally

(see e.g. Jooker, 1999; Lehmann, 1982 [1995]) even earlier scholars, like Étienne Bonnot de Condillac (1714-1780) and Horne Tooke, are also mentioned in connection with grammaticalisation. I have, however, not attempted to go back to Condillac and Horne Tooke in any depth. Since, as I said above, I have used the early nineteenth century as my cut-off point, and since these two scholars are already recognised as possible predecessors, I have decided not to take a closer look at their work due to time and space limitations, although they would certainly be worthy of a more detailed study of their ideas. I hope to broach this subject in the near future.

In my selection of scholars and publications I have first of all aimed to include the linguists that have been seen as the big names of historical-comparative linguistics in the nineteenth century, since these are likely to have been the works that were most widely read. Sometimes I have found clear signs in their work of a concept similar to grammaticalisation and sometimes there are also references which seem to indicate that someone has treated something similar before someone else. However, at times there has been a need to stray from the well trodden path a little to see how the ideas and concepts of the ‘big names’ were received by the lesser known scholars of the time.

To make my selection of nineteenth-century linguists from Britain and the USA, who have been less widely studied than the German linguists, I have relied on Hans Aarsleff (1983) *The Study of Language in England 1780-1860* for Britain, and Julie Tetel Andresen (1990) *Linguistics in America 1769-1924*, John Joseph (2002) *From Whitney to Chomsky: essays in the history of American linguistics* and Konrad Koerner (2002) *Toward a History of American Linguistics* for the USA. But I have also included some linguists who appeared to have treated something similar, or at least dealt with historical-comparative linguistics, when I examined various journals of the time. I have consulted the *Proceedings of the Philological Society* (1854 -) and *Transactions of the Philological Society* (up to 1900), *The Journal of Philology* (up to 1900), and *The Quarterly Review* (although quite briefly) from Britain and *Journal of the American Oriental Society* (1849-1899) and *The American Journal of Philology* (1880-1902), from the USA.

Similarly, for Scandinavia I have based my selection of scholars on Even Hovdhaugen, Fred Karlsson, Carol Henriksen and Bengt Sigurd's (2000) *The History of Linguistics in the Nordic Countries*.

For the twentieth century, I have looked at well known works on grammaticalisation between 1971-2002. I have also consulted edited books on grammaticalisation and searched the MLA bibliographical database for hits on *grammaticalisation*, *grammaticisation*. I have also looked at journals and followed up on references in books and articles on grammaticalisation to see if they also featured comments on grammaticalisation. In addition to this I have tried to search, by country, using the internet search engine *Google* to see what I could find on grammaticalisation using the search terms *grammaticalisation*, *grammaticalization*, *grammaticisation*, *grammaticicization*, *grammatisation*, *grammatization*, and for Scandinavia also *grammatikalisering*, *Grammatikalisierung* (I have done this a number of times, but the really thorough searches were carried out in 2003).

The earlier part of the twentieth century has been treated by first of all looking at some of the more well known works, such as Saussure's *Cours* (1972 [1985]; 1983; 1916 [1966]), Sapir (1921), Bloomfield's *Language* (1935 [1969]). But more scholars have been selected with the help of books on the history of linguistics, as for the nineteenth century. I have also included articles I have discovered that have seemed of interest. The books I have based my selection on are partly the same as the ones above, with the addition of Keith Brown and Vivien Law (eds) (2002) *Linguistics in Britain: Personal Histories* for Britain. Naturally I have also made use of library catalogues where I could find out more exactly what the linguists whom I had selected had actually written.

In addition, I have also relied on advice from other scholars, e.g. John Joseph (University of Edinburgh), Andrew Linn (University of Sheffield), April McMahon (University of Sheffield), Harry Perridon (University of Amsterdam, the Netherlands), Henrik Rosengren (Lund University, Sweden), Clemens Knobloch (University of Siegen, Germany), some of whom contacted me after postings on *Linguist List*, *NordLingNet*, *Forum for the History of Linguistics* (*Forum for the History of Linguistics (Linghist)*); *Linguist List*; *NordLingNet*), and some I have contacted directly via email.

The scholars whom I have selected to consult for Scandinavia have also been discussed with Even Hovdhaugen. The British and American scholars have been discussed somewhat with Andrew Linn and John Joseph, although having consulted three books of (parts of) the history of American linguistics, two on (parts of) the

history of British linguistics and some journals, I felt confident that I had looked at those who had shown most interest in historical linguistics.

I am aware that there are many more works and linguists that I could have consulted, but the time and space restrictions for this thesis did not make this possible. However, I hope that someone, hopefully I myself, will be able to write a more complete history of grammaticalisation one day soon.

1.3 Outline

This first chapter is intended to introduce the reader to the concept that will be studied in this thesis, and give a brief idea of some of the problems involved in its definition and use. I have tried to show briefly that the concept is also very closely connected to the concepts of lexicon and grammar, which are often taken for granted in linguistics. The thesis will mainly deal with the history of the concept of grammaticalisation, and a few introductory words have been said on this in this chapter.

Chapter 2 gives more information on the known history of grammaticalisation (2.1). The chapter also includes a section (2.2) discussing the term *grammaticalisation*, its history, meaning, and connotations and a brief outline of the meanings of *lexicon* and *grammar* in English and in Swedish. In section 2.3 I introduce the discussion of gradual and unidirectional change in connection to grammaticalisation, a discussion which will later be expanded on in chapter 3. And then, in section 2.4, I bring up the issue of whether grammaticalisation is a theory or not.

The second part will deal primarily with the modern concept of grammaticalisation, how it is defined, how it relates to other processes, and its directionality. It starts with a chapter on the unidirectionality hypothesis (chapter 3), where I treat the idea that grammaticalisation can only proceed in one direction. Here, I also discuss some possible counterexamples, and other changes which are supposed to include examples of the reverse of grammaticalisation, or (at least) a move in the opposite direction to grammaticalisation. I also call attention to some examples that have been used to illustrate **both** grammaticalisation and changes that are seen as including counterexamples to the unidirectionality hypothesis of

grammaticalisation: I raise the question of what effect this has on grammaticalisation. Furthermore, I also touch on the so-called uniformitarian hypothesis and what that means for grammaticalisation and the unidirectionality hypothesis.

In the next chapter (chapter 4) I discuss two processes which are sometimes seen as playing important roles in grammaticalisation: reanalysis (4.1) and analogy (4.2). I discuss some of their history, what they are, but also what their relation to grammaticalisation might be.

Next is a part which treats the history of grammaticalisation (Part 3). This begins with a chapter on the history of grammaticalisation in the nineteenth century (chapter 5), whether there was a concept of grammaticalisation then, and/or how this concept was taking form and developing. The chapter is split into different sections treating the different geographical regions that I have looked at: first of all I look at the two leading centres of scholarship Germany (5.1) and France (5.2) and then I move on to look at the regions that I have concentrated on, viz. Britain (5.3), the United States (5.4) and Scandinavia (5.5).

After this I move forward in time to the period between 1900-1970, to see what work (if any) was carried out on grammaticalisation during that time (chapter 6). This chapter is divided into section 6.1 which primarily discusses Meillet, section 6.2 which looks at Britain, 6.3 the United States, 6.4 Scandinavia. The next chapter (chapter 7) similarly treats the same regions once more but during the period from 1970 up to 2002, from the time of the ‘revival’ of grammaticalisation up until the publication of the proceedings of the first *New Reflections on Grammaticalization* conferences. In chapter 7 however I have decided against dividing the chapter into sections as much and instead I discuss the ‘corner stones’ (Givón, Traugott etc.) in 7.1, lesser known linguists who have worked on grammaticalisation (7.2) and then I try to pull all the information together in a discussion and summary at the end, section 7.3, where I also discuss the issue of whether grammaticalisation should be seen as an epiphenomenon only and what I believe grammaticalisation should be seen to be.

All the chapters in part 3 are concluded by a short summary of what has been discussed in the chapter (5.6, 6.4 and 7.3 respectively). The last part of the thesis (Part 4) includes my conclusions and sums up in one chapter (chapter 8) the main points that were presented in the previous chapters.

In sum, it can be said that the possibility of a constant evolution of the concept of grammaticalisation will form one part of this thesis, which will try to establish how the concept has developed and to show that terminology and concepts, like everything else in language, are difficult, and most likely impossible, to tie down and attach to a particular fixed meaning and usage. Still, that does not mean that we cannot learn something about our subject from earlier phases of its study, whichever form they may have taken. Nor does it mean that we should ignore the fuzziness and confusion, and simply allow it to continue.

This thesis will give an outline of the studies of grammaticalisation in Britain, the United States and Scandinavia. Furthermore, the phenomenon will be compared to other phenomena of change with which it has often been related, viz. reanalysis and analogy (chapter 4) and an overview will be given of the ideas involved in the so-called *unidirectionality hypothesis* (chapter 3).

2. BACKGROUND

2.1 The History of Grammaticalisation Studies

Hopper and Traugott (1993: 7) claim that grammaticalisation studies have evolved from two different interests:

1. “a recognition of the general fluidity of so-called categories”
2. “recognition that a given form typically moves from a point on the left of the cline to a point further on the right, in other words, that there is a strong tendency toward unidirectionality in the history of individual forms.”

It is easy to see a certain similarity between the latter point and the works of Humboldt, A. Schlegel and August Schleicher (1821-1868). Schleicher’s ideas of language typology and language decay stressed that languages were at their high-point or at least most perfect when they were as synthetic, or inflected, as possible. It is interesting to view grammaticalisation from this perspective, because if one has an evolutionary ‘bell-curve’, where a language improves until it reaches the top of the bell and then starts to slide down the other side, exactly where on this curve would grammaticalisation take place? According to Schleicher, languages only improve in the evolutionary, pre-historical stage, after which things only really get worse. So if grammaticalisation was something that happened during the evolutionary stages then it would be on the way up to the top of the bell curve. However, the fact that it still seems to happen, even though according to the traditional view of languages progressing from an isolating to an agglutinating to an inflectional stage Indo-European languages seem to be ‘decaying’, would mean that it is also a historical change.

Lehmann (1982 [1995]) deals with the development of a concept of grammaticalisation in a slightly different way to Hopper and Traugott (1993), focusing on who might have been important, and which linguistic traditions have been involved in the major steps forward in grammaticalisation studies, rather than on the kinds of ideas that have been the source of inspiration for such a concept. About the linguistic traditions, he says:

Summing up, we can say that the theory of grammaticalization has been developed by two largely independent linguistic traditions, that of Indo-European historical linguistics and that of language typology. (Lehmann, 1982 [1995]: 8)

In this thesis, I will try to bring the two together in some sense, by dealing both with the linguistic facts and theoretical ideas behind the concept of a phenomenon of grammaticalisation, and with the linguistic traditions which it is thought may have had an influence on the development of this framework in linguistics.

As mentioned in chapter 1, the most thorough treatments of the history of grammaticalisation consist of sections in Heine et al. (1991a) and Lehmann (1982 [1995]: ch. 1) and are rather incomplete. Basic introductions to the history of grammaticalisation are also given by Hopper and Traugott (1993: ch. 2), Harris and Campbell (1995: 2.2.1) and for earlier views on the history of grammaticalisation there is also Hodge (1970), who discussed its history before the revival of grammaticalisation and without making use of the term. Givón's role in the 'revival' of grammaticalisation must be recognised, however Lehmann (1982 [1995]: 8) says that Givón's famous statement "Today's morphology is yesterday's syntax" echoes Hodge, seemingly implying that Givón may have been influenced by Hodge even though Givón (1971a) does not acknowledge such a connection himself.

The history of grammaticalisation has been treated from an even earlier perspective by Lieve Jooen (1999) who, feeling the need for historical treatments of grammaticalisation, discusses the links between grammaticalisation as we know it and the eighteenth-century philosophers Condillac, Horne Tooke and Adam Smith (1723-1790). She claims that these scholars did not have the same sense of grammaticalisation as we do now, though something similar was part of their theories of language:

The concept of grammaticalization, as we conceive of it, did not occur in eighteenth-century grammars of English. Aspects of it did, however, form a pertinent part of contemporary general theory of the functions of speech and the original lexical and grammatical development of language. (Joonen, 1999: 294)

From these histories it seems that we can be rather certain that a concept **similar** to grammaticalisation had emerged quite some time before the actual term was coined. However, Meillet is the first person who is **known** to have used the term in a sense close to how we use it today (Lehmann, 1982 [1995]: 1). Lehmann says that notably Meillet always used the term in quotation marks (Lehmann, 1982 [1995]: 4), which he sees as an indication of how new the term was then. But we should then note that Givón also used the variant *grammaticization* in citation marks when he first started using a term similar to *grammaticalisation* in the mid-1970s (Givón, 1975: 49).¹³

Lehmann (1982 [1995]) talks of grammaticalisation theory as a descendant of agglutination theory, which he views as a continuous theory since Humboldt and Schlegel's typological studies, based mainly on Humboldt's evolution theory which is seen as the basic *Agglutination Theory*. However, Lehmann notes that it was Bopp who first applied the theory (1982 [1995]: 3), and he believes that there are links as far back as to Condillac and Horne Tooke (1982 [1995]: 1-8). So it appears that people have had thoughts about closely related topics at least since the mid-eighteenth century. Similarly, Heine et al (1991a: 7) note a link between grammaticalisation and agglutination theory, however they claim that the concept may be older still:

The question as to the origin and development of grammatical categories is almost as old as linguistics. This fact should not stop us, however, from viewing grammaticalization as a new paradigm. [...]

It would seem that the notion of grammaticalization was first recognized outside the world of Western scholarship. At the latest, since the tenth century, Chinese writers have been distinguishing between "full" and "empty" linguistic symbols, and Zhou Bo-qi (Yuan dynasty, A.D. 1271-1368) argued that all empty symbols were formerly full symbols (Harbsmeier, 1979: 159ff.). (Heine et al., 1991a: 5)

Introductions to grammaticalisation in general can occasionally be found at book length, but also in linguistic encyclopedias and textbooks of historical

¹³ *Grammaticalized* first appears in Givón's work in 1977 (Givón, 1977).

linguistics. There are two textbooks on grammaticalisation in general available to the student or lay-person who is interested in the subject. The one most frequently mentioned is the American textbook by Paul John Hopper and Elizabeth Closs Traugott, *Grammaticalization* (1993). But there is also a German book by Gabriele Diewald (1997), *Grammatikalisierung – Eine Einführung in Sein und Werden grammatischer Formen* which could count as a textbook. There are also some introductory sections in some linguistic encyclopedias – Paul J. Hopper (1992a) ‘Grammaticalization’, *International Encyclopedia of Linguistics* (OUP) and Elizabeth Closs Traugott (1994) ‘Grammaticalization and Lexicalization’, *The Encyclopedia of Language and Linguistics* (Pergamon Press). And of course, grammaticalisation nowadays usually gets a section in textbooks of historical linguistics such as Campbell (1998), Crowley (1997), Harris and Campbell (1995), Lehmann (1992), McMahon (1994) and Trask (1996) although the chapter under which it gets treated varies – it can come under syntactic change, morphological change, lexical change or semantic change.

The possibility that the concept had been around for many years, even decades and quite possibly even centuries when the term was used by Meillet in 1912, might not be quite as straightforward as it sounds. There are signs that the term *grammaticalisation* has been used differently by different people. There seems to have been a development in how people understand the term in the many years since it was coined, even though this may not always have resulted in any major changes in the definitions given, as can be seen if we compare Kuryłowicz’s and Hopper and Traugott’s examples and definitions (as cited above) to Meillet’s first explanation and examples of grammaticalisation. Even though Hopper and Traugott make it clear that they think that Meillet had a different view of grammaticalisation than they do (Hopper and Traugott, 1993: 48), their basic definition is the same.

We define grammaticalization as **the process** whereby lexical items and constructions come in certain linguistic contexts to serve grammatical functions, and once grammaticalized, continue to develop new grammatical functions. (Hopper and Traugott 1993:xv; emphasis mine)

Grammaticalization is the study of grammatical forms, however defined, viewed as entities undergoing processes rather than as static objects. (Hopper and Traugott 1993:18)

... le passage d'un mot autonome au rôle d'élément grammatical. (Meillet, 1912; 1921: 131)¹⁴

... la “**grammaticalisation**” de certains mots crée des formes neuves, introduit des catégories qui n'avaient pas d'expression linguistique, transforme l'ensemble du système. Ce type d'innovations résulte d'ailleurs, comme les innovations analogiques, de l'usage qui est fait de la langue, il en est une conséquence immédiate et naturelle. (Meillet, 1912; 1921: 133, emphasis mine)¹⁵

Nevertheless, there **are** certain differences between how Meillet ‘defines’ grammaticalisation, and how Hopper and Traugott and Kuryłowicz define it. In all three definitions, grammaticalisation is a phenomenon where lexical items become grammatical, at least that is (broadly) how we are likely to interpret all three. However, Meillet does not speak of lexical morphemes as Kuryłowicz does, but of independent words becoming grammatical elements. A rather more restrictive view of grammaticalisation than that of lexical morphemes becoming grammatical morphemes. Kuryłowicz’s view is one which could easily include a change from derivational to inflectional morphemes, if we class derivations as lexical, something which Meillet’s view could not necessarily do.

Hopper and Traugott widen the definition even more, while at the same time restricting it. They start off with “lexical items or constructions”, instead of e.g. autonomous words (Meillet) or lexical morphemes (Kuryłowicz); this means that they can possibly include morphemes, words and whole phrases at the lexical end of the scale, but not anything that does not become what they class as a grammatical morpheme. Hopper and Traugott also call attention to the fact that *grammaticalisation* can be a framework looking at these kinds of phenomena in language, something which neither Kuryłowicz nor Meillet mention.

“Grammaticalization” as a term has two meanings. As a term referring to a **framework** within which to account for language phenomena, it refers to that part of the study of language that focuses on **how grammatical forms and constructions arise, how they are used, and how they shape the language**. The framework of grammaticalization is concerned with the question of

¹⁴ Translation: ... the passing of an autonomous word into the role of a grammatical element (All translations will be my own unless otherwise indicated.)

¹⁵ Translation: ... the grammaticalisation of certain words creates new forms, introduces categories that had no linguistic expression, transforms the whole system. Apart from this, this type of innovation results, as the analogical innovations, from the use that is made of the language, it is an immediate and natural consequence thereof.

whether boundaries between categories are discrete, and with the interdependence of structure and use, of the fixed and the less fixed in language. [...] The term “grammaticalization” also refers to the actual **phenomenon** of language that the framework of grammaticalization seeks to address, most especially **the processes** whereby items become more grammatical through time. (Hopper and Traugott, 1993: 1-2, emphasis mine)

The only real difference between Kuryłowicz’s definition and Hopper and Traugott’s is the addition of context and the substitution of “items and constructions” for morphemes. Both definitions seem reasonably clear and easy to grasp at first. If the reader has read Hopper and Traugott’s (1993: xv) preface where their first definition of grammaticalisation is given, he/she should also be aware that the term in their eyes has “two functions” (1993: xv):

The term grammaticalization [...] refers not only to processes observable in language, but also to an approach to language study, one that highlights the interaction of use with structure, and the non-discreteness of many properties of language.

This is something that Kuryłowicz does not go into and which might not have been the case in the 1960s in the same way as now. Later, to bind the definition more visibly to the sense of ‘grammatical’, Hopper and Traugott say that:

Grammaticalization is in some sense **the process *par excellence*** whereby structural relationships and associations among them are given grammatical expression. (Hopper and Traugott, 1993: 72, italics original, emphasis (bold) mine)

According to Hopper and Traugott, grammaticalisation is a study which is said to deal with something as undefined as “entities undergoing processes” (Hopper and Traugott, 1993: 18). But what kind of processes is it that the entities are undergoing in grammaticalisation – is it any kind of process?

It is not only Hopper and Traugott (1993) who have seen fit to split grammaticalisation into different groups in some sense. Harris (1997) distinguished between two classes of definitions of grammaticalisation:

(1) “the process of becoming part of grammar, of being entered in a grammar, or of changing in grammatical function.”

(2) “the process by which a word becomes a clitic, a clitic an affix, and an affix a synchronically unanalyzable part of another morpheme; it includes the processes of phonological reduction and semantic bleaching”

Both of Harris’s two types of grammaticalisation in fact fall into the second kind of grammaticalisation defined by Hopper and Traugott, since they both deal with the phenomenon. What Harris clarifies is that there are different approaches one can take to the phenomenon, and that illustrates that the concept is even more diverse than Hopper and Traugott have shown.

Quite an important issue in grammaticalisation studies is the relation between reanalysis and grammaticalisation. Hopper and Traugott indicate that they believe that Meillet is equating grammaticalisation with reanalysis, which they define using Langacker’s famous definition:

... “change in the structure of an expression or class of expressions that does not involve any immediate or intrinsic modification of its surface manifestation” (1977: 58). (Hopper and Traugott, 1993: 40)

If he does equate grammaticalisation and reanalysis, he is not alone in doing so. Campbell (2001: 143) believes that Heine and Reh (1984), for instance, have done so too. However, Meillet’s explanation of what grammaticalisation is certainly does not sound very much like a definition of reanalysis, but more like grammaticalisation. Also, speaking of reanalysis and grammaticalisation, in the modern sense, in Meillet’s writings is very anachronistic. Still, it is true that reanalysis often seems to be part of changes which we classify as grammaticalisation and some of Meillet’s examples may possibly resemble reanalysis in our eyes more than we think they resemble grammaticalisation.

If we look back again at Hopper and Traugott’s two definitions we can note another important tendency in grammaticalisation studies, namely that both definitions speak of processes (“process whereby lexical items...” (1993: xv) and “undergoing processes” (1993: 18)). Is grammaticalisation one process or several (note that this varies in the two segments just cited)? What counts as a grammaticalisation process? Is it the grammaticalisation process(es) which are studied by the grammaticalisation framework or other processes as well? The problem with the diverse definitions of grammaticalisation as a process or processes,

has been discussed by Joseph (2001). He has noted how even one and the same person can refer to the phenomenon as **one** process one moment and **several** processes the next, **or** as the result of something (presumably another process). This shows some of the diversity and confusion currently existing in this field of research.

2.2 The Term *Grammaticalisation*

Many times in the history of linguistics linguists have noted that terminology can be problematic. Terms have been used in an odd way, or a new term has been used even though a perfectly acceptable older term already existed. But much more problematic is of course the issue of when terms are felt to have been used in different ways. Even more problematic are cases where a term is used in different ways without scholars having realised it – so they assume that everyone uses it to mean the same thing.

Grammaticalisation seems to be a fairly ‘young’ term, since most linguists refer it back to Meillet in the early 1910s, however it appears to have gone out of fashion after that, and only been used in the odd publication until we reach the middle of the 1970s after which it becomes more and more popular. It has also been noted that other terms have been used for similar or identical phenomena, such as *reanalysis*, *agglutination theory*, etc. (cf. e.g. Campbell, 2001; Heine et al., 1991a; Lehmann, 1982 [1995]) and there are more recent alternatives like *grammaticise*, *grammatise*, not to mention the fact that these terms can be spelt with either *-s-* or *-z-*.¹⁶ An added complication is the fact that the most common term, *grammaticalisation*, has been used differently by different people, an issue which has been raised in some of the more recent studies of the concept (cf. e.g. Giacalone Ramat, 1998; Lindström, forth.; Wischer, 2000).

Since terms are part of the lexicon of a language, like any other item in the lexicon, they cannot be frozen in one particular sense. They are bound to change and be adapted according to the needs of the fields in which they are used. However, terms in linguistics are also tools that we make use of in our work as linguists. So, for instance, when we have studied the history of a language for a while we may

¹⁶ I will here use the *s*-spelling unless I am directly citing someone who uses the *z*-spelling.

announce that we have found that certain parts of the language have undergone grammaticalisation and other linguists are expected to know what we mean by this. But we can only do this because we have a concept of grammaticalisation which is partly tied to our having a term *grammaticalisation*, since however the term is defined, the fact that we have a term gives us a focus for the concept. It also gives other researchers distinct terms to look for in one's work to see if something which they are interested in has been discussed: has *aspect*, *analogy*, *syntax* etc been treated? If we did not have these terms we would have to read through the **whole** (or at least large parts of) works of every single linguist before we would know whether they were talking about something that we would find interesting for our own work.

Unfortunately, once a term has entered into the general vocabulary of a field people feel that they can use it and assume that everyone will understand what they mean. But, considering the fact that some linguists (see above) have noted that *grammaticalisation*, for instance, has been used in different ways by different people, there can be no way of assuming that A will understand what B means when he/she uses the term since they might have a different understanding of the term. This **fact**, means that it is important that terms are defined when we use them. They do not have to mean the same on every occasion when they are used, but it should be clear what they mean in that particular instance and it would be recommendable that diverging meanings be kept to a minimum if possible. One would hope that the different uses of a term should generally have at least some connection.

In the last thirty years some alternative terms to *grammaticalisation* have been suggested for various reasons. One is that we are not sure exactly what the term *grammaticalisation* actually stands for, or what it meant to begin with? Some believe the term has some less desirable connotations, another reason to suggest alternative terms:

The derivational pattern which the word *grammaticalization* belongs to suggests that it means a process in which something becomes or is made grammatical (cf. *legalization*). In view of this, the term is unfortunate in several respects. Firstly, the term '**grammatical**' has various meanings. In the above explication of *grammaticalization*, *grammatical* signifies that which belongs to, is part of, the grammar, as opposed to, e.g., what belongs to the lexicon, stylistics or discourse. Apart from this *grammatical* has come to mean something completely unrelated to the notion of grammaticalization: *x is grammatical* is an abbreviation of *x is grammatically correct* and accordingly means that x conforms to (as opposed to: is incompatible with,

violates) the rules of grammar. What is particularly distressing about this ambiguity is the fact that while *grammatical* may have either meaning in attributive use, it can only have the second meaning in predicative use; and yet the first meaning is needed in the predicative use which is made of it in the above explication of grammaticalization.

[...] *grammaticalization* must mean a process in which something becomes or is made **m o r e** grammatical. [...] in a theory of grammaticalization, the term ‘grammaticality’ would be needed to mean the degree of grammaticalization which an element has reached. Again, however, this term (or its variant ‘grammaticalness’) is currently based on the other meaning of *grammatical* and therefore means the well-formedness of something according to the rules of grammar. (Lehmann, 1982 [1995]: 9, italics, bold and emphasis through spacing original.)

When discussing the original word formation that gave us the term grammaticalisation, there is however another issue that we need to bear in mind. When we discuss the term *grammaticalisation* we must remember that, presumably, this is a loan word. It is thought to have been borrowed into English from French at some point after 1912. And it is therefore also important to ask what connotations the terms *grammaticalisation*, *grammaticale*, *grammaticalité* had **in French** at that time. Scientific terminology is quite often borrowed between languages, and unless we are purists and believe that we should form new words only from language-internal sources, we will have to accept this. But this also means that we **must** be careful and very precise with our definitions, and make sure that people understand exactly what the terms mean, and do not interpret them according to language **internal** resources entirely.¹⁷ Nevertheless, it seems as though the French interpretation is approximately the same as the English one in this case. Let us look at how a few French dictionaries describe *grammatical* and *grammaire*:¹⁸

Dictionnaire Étymologique de la Langue Française (Bloch and von Wartburg, 1989: 302)

Grammaire – grammatical

From Latin *grammatica*, *grammaticalis*.

Dictionnaire du Français Contemporain (Dubois, 1966: 574)

Grammaire

The scientific study of morphological and syntactic structures. Books that treat this study.

¹⁷ A ‘native’ derived word in technological or scientific usage can of course also be interpreted much more narrowly than what the semantics related to the derivational pattern would suggest.

¹⁸ The meaning of *lexicon* and *grammar* will be discussed presently.

Grammatical

e.g. *exercices grammaticaux, l'analyse grammaticale*

Dictionnaire Générale de la Langue Française (Hatzfeld and Darmesteter, 1920: 1188)

Grammaire

The science that studies the rules of language; or a book of such rules.

Grammatical

Relating to grammar, esp. *analyse grammaticale*, parsing; conforming to grammar.

Notably, some of the more recent French dictionaries (Guilbert et al., 1973; Rey, 1988) also include entries for *grammaticalisation* and *grammaticaliser*. The etymological description usually says that *grammaticalisation* is derived from *grammaticaliser* and that it is a term from the **mid**-twentieth century and the description of both conform to what we would call *grammaticalisation*, *grammaticalise* today, see e.g.:

Phénomène selon lequel un élément lexical, doté d'un sens défini, devient un élément purement grammatical: ...¹⁹ (Guilbert et al., 1973: 2285)

The term *grammaticalisation* will be the one used in this thesis. This is not in any way because I side with a particular interpretation of the term or a particular view of the concept, or because I think that Lehmann is wrong in claiming that there are some problematic connotations with the term and that there are some difficulties with its derivation.²⁰ It is simply because this is the most commonly used term, it seems, and because changing the term can cause serious problems. I am not the only one who has kept the term even though it was not found to be perfect, cf. Hopper and Traugott, for instance:

In recent linguistics there is some variation between this word [grammaticalization] and the newer form “grammaticization.” In adhering to the older form of the word, we do not intend any theoretical point other than to maintain a continuity of terminology. We believe that a terminology can

¹⁹ Translation: Phenomenon through which a lexical element, attributed with a definite meaning, becomes a purely grammatical element...

²⁰ However, in relation to the problem I noted above, of the term presumably being borrowed from French, problems with the English derivation are not quite as relevant as they could be. They are still problematic from an interpretative point of view, however if the term *grammaticalisation* was indeed borrowed from French as one word then it should not be compared too closely with the English derivational pattern, unless we want to come to the conclusion that it was wrong to borrow it because it is too close to a word that could have been created from language internal resources with a different meaning!

and should survive quite radical changes in the ways the terms that comprise it are understood by successive generations of scholars. (Hopper and Traugott, 1993: xv-xvi)²¹

There are also signs in the literature that *grammaticalisation* and *grammaticisation* have come to diverge somewhat. *Grammaticisation* is now occasionally distinguished from *grammaticalisation*:

... it is said that “grammaticalization” stresses the historical perspective on grammatical forms, while “grammaticization” focuses on the implications of continually changing categories and meanings for a synchronic view of language ... (Hopper and Traugott, 1993: xvi)

Another issue with regards to the meaning of the term *grammaticalisation* has already been briefly mentioned in chapter 1, namely the issue of what grammar and lexicon are exactly? This is never easy to find out since linguists very rarely spell out what they mean by the two (the two being ‘paradigm’ examples of terms that we take for granted as linguists). However, it is important that we try to find out what they mean since their meaning will have enormous importance on how we understand *grammaticalisation* and *lexicalisation*, not to mention the common definition of grammaticalisation as the process of an item changing from being something lexical into something grammatical.

Basically the definitions of lexicon and grammar depend on the linguist using them, although we can try to get some idea of how the two parts of language (this seems to be how they are often seen) are described in dictionaries. I will give a short outline therefore of the views presented in three dictionaries, one from each of the main regions looked at in this thesis: Britain (the Oxford English Dictionary online), the United States (Merriam-Webster online), and for Scandinavia (Sweden: Svenska Akademiens Ordbok [=the Swedish Academy Dictionary] online).

²¹ As for the term *grammaticization* being a newer term we should however note that the OED lists uses of a term *grammaticize* from 1673 but in the meaning of either discussing grammatical points or making grammar, reducing something to grammatical rules (Oxford English Dictionary, 1989g).

2.2.1 *The Oxford English Dictionary Online*

The OED (Oxford English Dictionary, 1989b) splits up *grammar* under six main headings, plus one for its attributive sense, and some subsections. The meanings that it lists include grammar as a part of the study of language, which is listed under the same main heading as when it is used for different sciences of language (historical grammar, universal grammar, philosophical grammar, etc.). *Grammar* can in addition be a book (of grammar); how a particular person uses grammatical forms and rules; and the phenomena that a grammar would normally treat (syntax, inflections, etc.) which according to the OED is a very late sense, their oldest citation stemming from the mid-nineteenth century. According to the OED the term has also been used to refer to Latin and to scholarship or literature in the past; and it also lists uses where it means fundamental principles of a science or art, a book which presents such principles and last but not least there are the attributive uses, e.g. *grammar school* and senses derived from this. However, the most relevant sense to us here appears to be the sense of such a phenomenon that is treated in a grammar.

Grammatical (Oxford English Dictionary, 1989c) similarly has five main headings and some additional subheadings. Its meanings stretch from something “[o]f or pertaining to grammar”, where it can also be the opposite of logic; to grammatical meaning, in the sense that one gets at a certain kind of meaning after one has applied the rules of grammar to a text; to when the use of language conforms to the rules of grammar; and even when something is part of or correct according to the principles of an art.

The other side of language, the *lexicon*, is described by the OED (Oxford English Dictionary, 1989d) as a book of words, a vocabulary; a complete set of meaningful units in a language; (and a kind of game that became popular in the 1930s). Notably for the second sense, whereby a *lexicon* is seen as a complete set of meaningful items in a language, there are only three citations and they are all from the twentieth century and it seems that this meaning is the one that we are interested in in this case.

Similarly, the related adjective *lexical* is said to mean either “[p]ertaining or relating to the words or vocabulary of a language” or alternatively “[p]ertaining to, of the nature of, or connected, with a lexicon” in which it is not made clear what *lexicon*

should then mean, but the citation makes it seem as though one means a book of words (Oxford English Dictionary, 1989e).

2.2.2 Merriam-Webster Online

The Merriam-Webster online dictionary (Merriam-Webster, 2003a) defines *grammar* as “the study of the classes of words, their inflections, and their functions and relations in a sentence”, but also as a description of the preferred patterns of syntax and inflections in a language. In addition, it mentions that *grammar* can mean “the characteristic system of inflections and syntax of a language”; a system of rules that defines what is characteristic of a language; textbooks of the above; the manner of writing and speaking according to the grammatical rules and the principles of an art or science. It seems that of most interest in relation to *grammaticalisation* is the meaning of a system of syntax and inflections, which we should note excludes all other parts of language. *Grammatical* is only defined as “of or relating to grammar” or “conforming to the rules of grammar” (Merriam-Webster, 2003b), of which the first is most relevant for grammaticalisation.

Lexicon is briefly defined as a book of the words in a language including their definitions; or the vocabulary of a language, a group, a subject or a person; as well as having the possible meaning of a complete list of all the morphemes in a language and in addition Merriam-Webster online lists a use of it meaning “repertoire” or “inventory” (Merriam-Webster, 2003c). The most relevant sense for grammaticalisation studies appears to be the complete list of morphemes in a language.

Lexical, as expected, is only defined as “of or relating to words or the vocabulary of a language as distinguished from its grammar and construction”, or simply “of or relating to a lexicon” (Merriam-Webster, 2003d).

2.2.3 Svenska Akademiens Ordbok (SAOB) Online

Grammatik (‘grammar’) is by SAOB (Svenska Akademiens Ordbok, 2001a) said to be seen as the science of the structure of language and its development; sometimes in older usage a more general reference to linguistics or philology; and it can also be used for a book which discusses grammar. *Grammatisk* (‘grammatical’) is, as in the

British definition of grammatical, said to be something which pertains to grammar. It is noted that earlier it could also mean linguistic (Svenska Akademiens Ordbok, 2001d). Unfortunately, none of these definitions seem to go very well with grammaticalisation.

Lexikon ('lexicon'), according to the SAOB, means a book which lists the vocabulary of a particular group, or it can refer to the complete list of words in a language or a variety of a language; and it can also be used to refer to a particularly knowledgeable person (Svenska Akademiens Ordbok, 2001b). The second of these seems most relevant in this context. And quite naturally *lexikalisk* ('lexical') is said to mean something pertaining to the lexicon in the form of a book or the vocabulary of a language (Svenska Akademiens Ordbok, 2001c).

Unfortunately, the dictionaries do not make it absolutely clear what is seen as being part of the grammar and what is seen as part of the lexicon. This is probably all that we could expect seeing as *grammar* and *lexicon* are specialised linguistic terms. Still, since they are also terms that people use in their everyday language, that would also be what linguists base their more specialised concepts on. Words and similarly meaningful items clearly tend to be classed as part of the lexicon, which is thereby perhaps more restricted than the grammar, although it clearly includes **all** linguistic elements as long as they are meaningful. This means that it includes autonomous content words, function words, derivational morphemes **and** inflectional morphemes.²² The grammar is the structure of a language, or more generally the study of language, including e.g. syntax and inflectional endings. However, if we say that it is the study of language it would also have to include the lexicon and if we say that it is the structure of language then we have to try to specify what that means exactly, which bits are then included apart from syntax and inflectional morphemes – or are they the only ones? It is only the American online dictionary *Merriam-Webster online* which clearly restricts grammar to syntax and inflectional morphemes, the other dictionaries are much more general in their definitions of grammar.

Derivational morphemes could be seen as somewhere in between the lexicon and the grammar, because they change the lexical meaning as well as occasionally, at least, having a grammatical function in that they can change the category to which

²² How idioms and phrases are to be treated is not clear.

the word belongs. Kuryłowicz sees derivational morphemes as less grammatical than inflectional morphemes, presumably because they are usually seen as having some meaning of their own, whereas inflectional morphemes are merely functional. It has also been proven that derivational morphemes occur closer to the stem of the word than inflectional morphemes (see e.g. Bybee, 1985: 33, 96). Bybee (1985) has suggested that this may be because derivational morphemes are more relevant to the meaning of the root or stem of the word.

Interestingly, most examples of grammaticalisation are indeed of the evolution of inflectional morphemes, adpositions, and auxiliaries, not of derivational morphemes. But even more fascinating is the fact that there are some examples of the development of derivational morphemes that have been seen as rather basic paradigmatic examples of grammaticalisation, and that are therefore fairly frequent in introductions to the topic:

As preliminary examples, we might include the Old English nouns *hād* ‘state, quality’ and *līc* ‘body’, which have become the Modern English suffixes –*hood* and –*ly*; German *Viertel* ‘quarter’ and *Drittel* ‘third’, which contain reduced forms of the noun *Teil* ‘part’; [...] (McMahon, 1994: 160-161)

If we look at Hopper and Traugott’s book *Grammaticalization* (1993) they also confuse us slightly with their treatment of derivational morphemes, which they indeed treat as little as possible. They say that:

Grammaticalization is the study of grammatical forms, however defined, viewed as entities undergoing processes rather than as static objects. (Hopper and Traugott, 1993: 18)

Then they specify *grammatical forms* as “grammatical words”, “derivational forms”, “clitics” and “inflections”, i.e. including both inflectional and derivational morphemes, only to go on to specify two different *clines* – a cline of grammaticality and a cline of lexicality, the former ending in an inflectional morpheme and the latter in a derivational morpheme (Hopper and Traugott, 1993: 4-6). This means that, for the most part, they seem to be excluding one of their “grammatical forms” from grammaticalisation, if we assume that the cline of grammaticality is to be understood as the primary cline for grammaticalisation. However, their definition above builds on “grammatical forms”, without any further specifications or restrictions, being

involved in grammaticalisation (cf. critique by Cowie, 1995: 185-186) and should therefore also include derivational morphemes according to their definition of what a grammatical form is. Bybee (1985) conversely speaks of a continuum of non-discrete categories from lexical via derivational to inflectional morphemes, parts of which she also illustrates diachronically.

Finally, I would like to return to the issue of what is grammatical. Some linguists have suggested that grammaticalisation should also cover changes in word order. Meillet, for example, clearly sees this as a highly related process, and therefore claims that word order changes could possibly be a case of grammaticalisation (Meillet, 1912; 1921: 147-148). However, others disagree. Hopper and Traugott claim that the fact that word order change is not unidirectional means that it cannot be a case of grammaticalisation (Hopper and Traugott, 1993: 22-23). They do however admit that there could be a strong connection between grammaticalisation and word order changes. And it sounds as though they may be willing to consider them as part of a broader definition of grammaticalisation:

...word order changes may be the outcome of, as well as the enabling factors for, grammaticalization in the narrower, prototypical sense used in this book of the process by which lexical items used in certain contexts come to mark grammatical relations. These changes are not unidirectional. Therefore, they should not be identified with grammaticalization in the narrower sense. However, given the broader definition of grammaticalization as the organization of grammatical, especially morphosyntactic material, they cannot be excluded from consideration. (Hopper and Traugott, 1993: 50)

It is interesting that they have chosen to exclude word order change from grammaticalisation, but this is also natural in their eyes, since word order cannot be said to be a grammatical form and grammaticalisation according to them involves the development of grammatical forms (Hopper and Traugott, 1993: 18). But the fact remains that word order, grammatical words and morphological elements can play similar roles in language.

2.3 Gradual and Unidirectional Change?

Language change is gradual; it does not happen over night. Grammaticalisation is also gradual. Many grammaticalisationists have also claimed that grammaticalisation is unidirectional, i.e. that it can only proceed in one direction and sometimes it has also been claimed that it cannot be reversed. But there are serious questions which arise in connection with this hypothesis. First of all the hypothesis can seem rather circular, since unidirectionality often appears to be inherent in definitions of grammaticalisation (cf. Cowie, 1995: 188-189; Norde, 2001a). Secondly, Gabelentz and many after him have suggested that language change is spiralic, but if it is – can grammaticalisation then be unidirectional? This of course depends on what one means by *unidirectional*. Hopper and Traugott's definition is that "there is a relationship between two stages A and B, such that A occurs before B, but not vice versa" (1993: 95). But if grammaticalisation is spiralic, stage A could be preceded by stage B. In fact, it definitely would be on some occasions, but on a different level of the spiral. This is because a spiral predicts that we come back to a position parallel to the point where we started. However, unidirectionality could also be taken to mean that the **exact** opposite of grammaticalisation, a mirror image of a grammaticalisation process, will not happen. This would mean that an independent lexeme that has been reduced, that has become more functional and has possibly been affixed to another word, will not reappear as an independent lexeme in exactly the same form (cf. Norde, 2001a). Various types of counterexamples have been discussed in recent years, e.g. *degrammaticalisation*, *lexicalisation*, *regrammaticalisation*, and *exaptation*.

2.4 Grammaticalisation Theory?

When I started working on my thesis in 2000, one issue that was often raised when speaking to people about grammaticalisation was whether or not it could be called a *theory*. This question was also occasionally raised in writing, but nowhere near as

often as in conversation. It seems that now, three years later, the debate has settled down a bit, however it is still out there and raises its head every now and then. But, I believe it is clear that Heine (2003: 584) is correct when he says:

... this question is not, and has never been, an issue [for most students of grammaticalisation], since their concern is simply with describing grammatical change and the implication it has for a better understanding of language use; whether their work deserves or needs to be elevated to the status of a theory is not considered by them to be of major moment. (Heine, 2003: 584)

But Heine also claims that, in his eyes, there definitely is something that deserves to be called a *theory*:

... grammaticalization theory is a theory to the extent that it offers an explanatory account of how and why grammatical categories arise and develop. (Heine, 2003: 578)

Still, it must be admitted that some linguists, some grammaticalisationists, have forced the issue of it being a theory quite far. Bybee et al (1994: 9-22) have, as Newmeyer (1998: 235) remarks, spoken of grammaticalisation as a theory and listed eight hypotheses which **make up** that theory. Newmeyer complains that a list of hypotheses is not a theory (1998: 240) and questions whether a grammaticalisation **theory** really exists. For there to be a theory of grammaticalisation, Newmeyer believes that grammaticalisation would also have to be a distinct process, “a phenomenon *of a particular type*, namely, one driven by a distinct set of principles *governing the phenomenon alone*” (1998: 233-234, italics original). And needless to say, Newmeyer (1998: 226) believes that:

... *there is no such thing as grammaticalization*, at least in so far as it might be regarded as a distinct grammatical phenomenon requiring a distinct set of principles for its explanation. (Newmeyer, 1998: 226, italics original)

Newmeyer also says he believes the idea that there is a grammaticalisation **theory** is a contradiction of other claims by grammaticalisationists, references to grammaticalisation as the “result of *independent* historical developments, each of which falls out of some independent theory” (Newmeyer, 1998: 235). He therefore sees grammaticalisation as an epiphenomenon (1998; 2001).

According to Heine (2003: 584), Campbell (2001) should also have claimed that grammaticalisation is not a theory, and indeed, Campbell attacks grammaticalisation theory in a way quite similar to Newmeyer's:

“Grammaticalization theory” has no explanatory value because what it claims to explain is explained already by other well-understood mechanisms which lie behind it and, as is generally agreed, it cannot “explain” without appeal to these other mechanisms and kinds of change. (Campbell, 2001: 151)

Still Campbell's arguments are more for an epiphenomenal view of grammaticalisation, an argument which also Newmeyer (1998; 2001) has advanced and which may in some ways be linked to the question of whether grammaticalisation is a theory. But the two are not the same!

Campbell himself also speaks of “grammaticalization theory” more than once (“A final point to consider is the claim in **grammaticalization theory** that grammaticalization allows ...”, “...will not be explained by looking merely inside **grammaticalization theory** alone...” (Campbell, 2001: 152-153, emphasis mine)), whereas if he was against it being called a theory he should perhaps have said “grammaticalisation **studies**” (emphasis mine), or “among grammaticalisationists”. So although Campbell is critical of grammaticalisation as a distinct phenomenon and prefers to view grammaticalisation as a “heuristic” (2001: 158), he does not deny the possibility that some scholars work with a grammaticalisation **theory**.

To round off this section, let us remind ourselves of what a theory is. The Oxford English Dictionary defines it as:

A **scheme or system of ideas or statements** held as an **explanation or account of a group of facts or phenomena**; a hypothesis that has been confirmed or established by observation or experiment, and is propounded or accepted as accounting for the known facts; a statement of what are held to be the general laws, principles, or causes of something known or observed. (Oxford English Dictionary, 1989f: §4a, emphasis mine)

Newmeyer is clearly right that a theory is not a list of hypotheses, the hypotheses should already have been confirmed and now count as fact. But I think that it is true of grammaticalisation that grammaticalisationists have a list of hypotheses which they believe **have been** confirmed and which they **do** count as fact, and to that extent according to the OED's definition, hypotheses not only **can** be, but they **must** be, part of a theory.

I think it is also true that grammaticalisation is “an explanation or account of a group of [...] phenomena” – certain changes that tend to happen in language are most easily explained by appealing to grammaticalisation theory. A theory which says that this type of change has happened before, (perhaps) it usually happens in a certain direction and these are the ways in which grammatical elements have (often) arisen in the past. In other words, a theory of how grammatical forms develop in language, and the tendency for this to be a phenomenon of a change from a lexical item to a grammatical element.

I have proven here that grammaticalisation cannot be dismissed as a theory a priori. Hence I will keep a relatively open mind as to whether grammaticalisation should be seen as a theory or not, and I will return to this issue in my conclusions.

PART 2: THE CONCEPT OF GRAMMATICALISATION

3. THE UNIDIRECTIONALITY HYPOTHESIS

3.0 Introduction

Grammaticalization consists in the **increase** of the range of a morpheme advancing **from a lexical to a grammatical** or from a less grammatical to a more grammatical status, (Kurylowicz, 1965 [1975]: 52, emphasis mine.)

[Grammaticalisation has evolved partly out of a] recognition that a given form typically moves **from a point on the left of the cline to a point further on the right**, in other words, that there is a strong tendency toward unidirectionality in the history of individual forms. (Hopper and Traugott, 1993: 7, emphasis mine.)

Seen as a fundamental characteristic of grammaticalisation, by most grammaticalisationists, unidirectionality has been included in many definitions of the concept and phenomenon of grammaticalisation, at least implicitly. It has frequently come to be addressed as an inherent or intrinsic characteristic of grammaticalisation, and it has become increasingly difficult to question the hypothesis. However, recently, more and more people have started to realise that there are problems with this hypothesis, and even more problems with making the hypothesis into more than a hypothesis, an intrinsic part of grammaticalisation. Is grammaticalisation really unidirectional? What do we actually mean by that word?

This aspect of grammaticalisation has become one of its most often asserted, but also one of its most debated, characteristics, and a historical outline and description of the concept of grammaticalisation must therefore both consider **what** the unidirectionality hypothesis is and **how** it has come to be part of grammaticalisation or whether it has always been part of it.

3.0.1 Outline of Chapter Three

This chapter will present a study of *the unidirectionality hypothesis* or *principle*. After an introduction (section 3.0) where I outline the chapter and present the basic idea of the unidirectionality hypothesis, I will explain more exactly what the unidirectionality hypothesis is (3.1). This may seem like quite a straightforward and easy task. However, there are some diverging tendencies in linguists' views of what unidirectionality is, which may complicate an explanation or definition, as will the fact that linguists' grasp of what they themselves and others mean by grammaticalisation is rather poor. Since if they are not clear about what they mean by the process itself, how then can anyone say with any certainty that the process is unidirectional?²³ And even if the overall process of grammaticalisation seems unidirectional – maybe the various subprocesses do not have to be.

I will include a section where I briefly discuss the debate around the use of the unidirectionality hypothesis in reconstructions (3.2), after which I will look at some of the explanations that have been proposed for why grammaticalisation should be unidirectional (3.3).

The fourth section of this chapter (3.4) will deal with different counterdirectional and non-directional changes; lexicalisation (3.4.1), degrammaticalisation (3.4.2) and regrammaticalisation / functional renewal / exaptation (3.4.3). These different changes will be described and compared and in one of the following sections (3.5) we will look at some examples of all these different changes. The reason for leaving this until last, is that it is good to have had

²³ We may choose not to view grammaticalisation as a process, or a distinct phenomena. It may be viewed as several processes, it may be viewed as an epiphenomenon. By choosing to use the word *process* in this paragraph I do not intend to make a statement of what I consider grammaticalisation to be. I only use *process*, because change certainly is a process of some kind, but naturally it may involve several processes. However according to some all of the processes involved in grammaticalisation should be unidirectional, so the fact that I have chosen to speak of only one process here should not be a problem.

a look at the different theories first. It may then be easier to see the extent of the confusion which exists in the description of the examples, the same examples, occasionally, being labelled as *grammaticalisation* by one person and as *lexicalisation* by another (as discussed in Lindström, forth.).

In section 3.6 I will briefly look at the *Uniformitarian Principle*. This principle has become very important in historical linguistics, and certain connections to unidirectionality and grammaticalisation can be made. In this section I therefore wish to introduce the reader to this concept and to explore its relation to unidirectionality and grammaticalisation. At the end of the chapter I will then try to summarise the chapter and present some conclusions (3.7).

3.0.2 Introduction to Unidirectionality and Grammaticalisation

The early studies of grammaticalisation, e.g. Meillet (1912), do not state explicitly that unidirectionality is part of grammaticalisation (cf. Lindström, 2002). Nevertheless, they only speak of items moving in one particular direction, namely from lexical to more and more functional or grammatical.²⁴ So, how do we know if unidirectionality was a feature of grammaticalisation in earlier treatments of the subject? Many people have concluded from the fact that only one direction was mentioned and exemplified, that it was. But is this **non**-mention of examples of change proceeding in a different direction a valid basis for a hypothesis, or a statement of fact (seeing as unidirectionality is now often not simply a hypothesis so much as part of the definition of grammaticalisation)?

As long as we only define **grammaticalisation** in terms of a movement in a particular direction, there is nothing that says that there cannot be opposite movements. However, such changes would then not be grammaticalisation. It appears that this is how some linguists have defined grammaticalisation. Implicitly or explicitly, they have assumed that grammaticalisation is a movement from lexical to grammatical, and therefore any movement in a different direction will not count as grammaticalisation. But is that a valid categorisation to make? It may be true that changes that proceed in the opposite direction and lead to a less grammatical function can be seen as a separate type of change. But then what do we call processes where a

²⁴ Often unidirectionality is not explicit in the definitions now either, but the expositions of the phenomenon usually include unidirectionality as an important characteristic.

submorphemic element is given a grammatical function? For instance, we could say that the *-ee-* in *feet* has come to have function as a plural marker (cf. Cowie, 1995). What should that kind of change be called?

Another question is, do we want to distinguish the two directions, lexical > grammatical, grammatical > lexical, so rigidly? Are they not just two different directions of one process, or are they really different processes (if the direction grammatical > lexical can be said to exist at all)? And what about changes which seem to be able to move in any direction?

One possible solution might be that we could class all these changes as the same process, with more than one possible direction, but we might still want to give each direction a terminological label. However, we would also, presumably, find a term for the ‘superordinate’ process... We need to consider what makes it tenable to distinguish between different processes, and what should indicate that it would be better to recognise only one process. What is the opposite, or the reverse of grammaticalisation, if this exists at all? Or better put perhaps – what would the opposite of grammaticalisation entail? Unfortunately, it is unlikely that an answer to this can be given in this thesis, but hopefully we can at least take a step towards such an answer.

3.1 The Unidirectionality Hypothesis

We shall now go on to take a closer look at what unidirectionality is and what has been said on this matter.

3.1.1 What is Unidirectionality?

One of the most important issues to note in this chapter has already been referred to above, but I will repeat it due to its importance. If grammaticalisation and maybe also other changes are unidirectional, what exactly does that mean? Does it mean that **no** changes can occur in the opposite direction? Or that a change in the opposite direction is a different kind of change? Changes obviously do not always proceed in the same direction, there are counterexamples to the unidirectionality hypothesis, to the extent that there are examples of changes which move in the opposite direction

on any cline of grammaticality that one might want to define. So there should be some attempts at dealing with them, even though they may be rare.

As Lyle Campbell (2001: 124) says, linguists' "views concerning unidirectionality" have varied a lot. To some it has been hypothetical, to others definitive and axiomatic. Unidirectionality can also apply to all the different levels in language and how linguists have viewed it in this respect also differs, i.e. whether it has been seen to apply to all levels, or whether the linguist has concentrated on phonetic changes, or semantic changes, syntactic changes, or frequency, for instance (cf. Campbell, 2001: 132-133).

Cowie suggests that, unidirectionality in fact involves two things (1995: 188):

- (1) "a one-way street"
- (2) "a black hole from which lexical forms can never re-emerge"

She correctly stresses that we need to be sure of exactly what we mean by a phenomenon such as grammaticalisation (and a characteristic such as unidirectionality – my addition), if we are to be able to make any judgements as to the validity of the unidirectionality hypothesis (cf. Cowie, 1995: 188). Like others she also observes that the one-way street hypothesis is in fact predecided, in a sense, implicitly, by the very definition given by Hopper and Traugott (1993) (and others) of what grammaticalisation is, since unidirectionality is made into an intrinsic part of the definition:

... shifts in specific linguistic contexts from lexical item to grammatical item... (Hopper and Traugott, 1993: 126)

An intrinsic quality of the process of grammaticalization that is implicit in this [Kuryłowicz's (1965 [1975]: 52)] definition, is **unidirectionality**: less grammatical elements may grammaticalize into more grammatical elements but, generally, not vice versa. Although the reverse **does** occur, these counterexamples are usually ignored in grammaticalization textbooks, being "statistically insignificant" (Heine et al. 1991[a]:5; cf. also Traugott and Heine 1991[a]:4ff). (Norde, 1998: 211)

At a first glance, it certainly appears as though unidirectionality is intrinsic in most definitions of grammaticalisation. However, as far as it being intrinsic in

Kuryłowicz's definition is concerned, it must be said that this is probably not meant to be the case, at least not to the extent of **no** reversing processes being possible. Kuryłowicz did recognise that there was a reverse process, which he called *lexicalisation* (Kuryłowicz, 1965 [1975]:52). This means that unidirectionality should not be seen as inherent in **his definition**, but it might be intrinsic in **our reading and understanding** of his definition.

Nevertheless, since linguists have started to interpret older definitions in this way, some are now making unidirectionality an inherent characteristic in their own definitions of grammaticalisation. This has important effects on the unidirectionality hypothesis, and grammaticalisation overall. Campbell (2001: 126-127) notes that since unidirectionality is intrinsic in the definition of grammaticalisation, it cannot be used empirically. This is because, as Janda has noted, the unidirectionality hypothesis is "a tautology" if unidirectionality is part of the definition of grammaticalisation, "similar to the fact that, for instance, the process of walking due north is necessarily unidirectional" (Janda, 2001: 294).

Why is unidirectionality then being discussed as a separate principle or hypothesis? Possibly, because there are changes in the opposite direction and at some point it was felt that we needed to emphasise that those changes should not be viewed as grammaticalisation, but as something else. Consequently, the unidirectionality hypothesis in itself could be an indication that we are looking at a process which is not as irreversible as it is often made out to be. Although, it is sometimes used as an empirical measure of what is grammaticalisation and what is not, something which has been criticised by people such as Campbell (2001) and Tabor and Traugott (1998), because it is intrinsic to the definition and because we have not studied it enough yet, respectively.

Naturally, if grammaticalisation is the process of lexical items becoming grammatical, then there can be no counterexamples, and that means that Hopper and Traugott (1993) are contradicting themselves in admitting that there are **some** counterexamples, even though they say that they are rare (cf. Cowie, 1995: 188-189). And so have many others in that case. But we must be very careful. There can of course be counteracting forces and results of those forces, which represent movements in the converse direction to grammaticalisation. However, these cannot be classed as grammaticalisation, if that intrinsically means something unidirectional, moving from somewhere between a lexical pole on the left and a grammatical pole

on the right, so to speak. Hopper and Traugott (1993), for instance, also see the grammatical > lexical processes as distinct from grammaticalisation. Or do they? Cowie (1995: 187-189) notes that Hopper and Traugott (1993: 127) in fact seem to view the adoption of umlaut as a means of expressing function as an example of grammaticalisation and that clearly does not proceed from lexical to grammatical! As Cowie shows, they give examples of some of the rare counterexamples which clearly do not proceed from lexical to grammatical:

But perhaps the worst confusion of all attends the discussion of the genesis of grammatical forms from sources other than full lexical words, such as other grammatical forms, or from phonological material of some kind.

[...]

First, there is the morphologization of phonological *processes*. Hopper and Traugott give the development of umlaut plurals in Old English as an example: '*foot-feet* is the modern reflex of an earlier stage when the plural was *fot-i*; phonetically[,] the *o* was fronted before the *-i*[,] and when the *-i* [(|plural marker|)] was lost for phonological reasons, the fronted vowel remained as the marker of plurality' (p. 127). And as a marker of plurality, it has morphological status. (Cowie, 1995: 187-188)

They class this change as a form of *morphologisation*, but it seems they would also call it unprototypical *grammaticalisation*. However, as soon as there are some examples from non-lexical to grammatical it seems wrong to **define** grammaticalisation as a unidirectional movement from the lexicon and into the grammar. Then unidirectionality should at most be allowed to be seen as a hypothesis or a tendency. But of course **if** this is **only** morphologisation and not grammaticalisation, but with similar results as grammaticalisation, then the definition of grammaticalisation with unidirectionality as part of the definition is fine – although the hypothesis is then tautological.

This brings us to an important issue, namely, since there definitely seems to be counterdirectional and also non-directional processes, is grammaticalisation then to be considered closely related to those or not? Can they perhaps even be examples of grammaticalisation as may be the case in the previous example from Hopper and Traugott (1993)? If they are related, should there not then be some form of group classification? And some studies which look at the correlation between the different processes or directions? Hopper and Traugott (1993: 127) appear to have decided to

class at least some of them as unprototypical examples of grammaticalisation as mentioned above.

We must decide what we want *grammaticalisation* to apply to and what we mean by *unidirectionality*.²⁵ To some, unidirectionality is a distinguishing feature between grammaticalisation and reanalysis, since reanalysis is not unidirectional, but they believe that unidirectionality is an inherent quality of grammaticalisation (e.g. Norde, 1998: 213). This was Norde's early view and she has always believed that there are also counterdirectional changes. However, she thinks these should be kept separate, distinct, from grammaticalisation, which is something that she has emphasised more and more over the years (cf. Norde, 1998; 2001a; 2002). But Hopper and Traugott (1993: 127) should not be able to use unidirectionality as a distinctive mark, since they have said that there are some counterexamples which they also count as grammaticalisation. Still they claim that word order changes cannot count as grammaticalisation (narrowly defined, which is the definition they prefer) because they are not unidirectional:

More widely attested cases of reanalysis that call into question the identification of reanalysis with grammaticalisation include word order changes, which we discuss immediately below. These can have major effects on the morphosyntactic organization of a language, but do not exemplify the unidirectionality typical of grammaticalization. It is best, then, to regard grammaticalization as a subset of changes involved in reanalysis, rather than to identify the two (Heine and Reh 1984: 97; Heine, Claudi, and Hünemeyer 1991a: 215-20). (Hopper and Traugott, 1993: 50)

We must make sure that we deal with all examples we have, including those which show signs of a counterdirectional or non-directional process, in order to find out more about language change. We should not let the exceptions go unanswered, but must attempt to deal with them, as Cowie said:

Unidirectionality should not be regarded as a statistically significant universal, which happens to have some exceptions that must remain unaccounted for. (Cowie, 1995: 189)

²⁵ The different counterdirectional and non-directional processes of change will be dealt with more in a later section (section 3.4).

Cowie is certainly right in that we must account for all examples, all changes, whether they go in the direction we expect or not. And, as Lass (2000) has said, we have severe problems deciding when we have a representative sample of languages and varieties, and of changes in languages.

Say in the course of your work you have found 542 changes that confirm a direction, and none that don't. Question is, 542 out of *what*? Does a UD-believer's inability to find the counterexamples, and/or the observed frequency of the confirming instances, reflect a 'real' property of the domain or merely the accidental tendentiousness of the chosen data base? Note that *not* finding things is an *argumentum ex silentio*, which is not at the top of anybody's hierarchy of epistemic goodness. (Lass, 2000: 214)

Lass is quite right to advance this kind of criticism of statements of unidirectionality. Rarely are any numbers presented in grammaticalisation studies, and often the broader conclusions may seem almost intuitive. In addition, it is very difficult (or even impossible) to ever obtain as good a sample of the world's languages as would be needed to make general statements of all languages.

Language is a very complex matter to perform statistics and quantitative studies on. One way linguists try to do this is through corpora. Lass (2000: 213-215) complains about what he sees partly as a lack of corpus work on the subject of unidirectionality and grammaticalisation and therefore a lack of statistics. Although, at the same time he recognises that even if we were to do corpus studies, it would be very difficult to guarantee a representative sample. There are so many varieties of each language and how do we decide which varieties and how many languages and varieties should be included (2000: 215)? On top of this historical linguists of course face the problem of having no historical records to work on for most languages and varieties in the world (Lass, 2000: 215).

Until we are sure that we have a representative sample, until we know what our sample is, e.g., its size, and until we are also sure of its representativity, we cannot say whether a change is statistically significant or not. Nor can we actually say what is not possible, what is an exception and what is the rule. But we can still attempt to make tentative generalisations based on what we know. This is an important part of our job as linguists.

The different views of the reverse of grammaticalisation are connected with different terms, but also very different ideas as to what is necessary in order to prove

that there can be reversals. Do there have to be changes which exactly mirror the process of grammaticalisation? And does that mean that it has to be an actual example of grammaticalisation that is reversed? Or does it mean that an example of the opposite direction would only have to mirror a **typical** grammaticalisation process, in that it would have to involve phonological and semantic strengthening, the opposite forms of reanalysis, loss of bonding, breaking out of paradigms etc? But if the latter case is what it takes, then how can we ever specify what a reversal of grammaticalisation would be since we have not yet specified exactly which processes have to be involved in grammaticalisation?! And nor does it look as though we ever will. Similarly, if the former was what was needed - i.e. a mirror example of an actual instance of grammaticalisation then how can we ever prove that something is not such a case of a reversal of grammaticalisation seeing as we do not know everything about the history of all languages?!

According to Heine et al (1991a: 5) statistics confirm that the reverse movement, though possible, is **extremely rare** and therefore **insignificant**. But when are we correct to conclude that a possibility is insignificant? And how do we know that they **are** statistically insignificant? What is our sample? How big is it? How representative is it? Most of the languages of the world have no recorded histories. Heine et al (1991a) give us no answers to any of these questions. We only have their statement, with no supporting data.

Interestingly, there are linguists who have also suggested that counterexamples to the unidirectionality of grammaticalisation are more common than we tend to think (Ramat, 1992: 553):

It may be that degrammaticalization is statistically insignificant when compared with the large number of grammaticalization processes (Heine et al. 1991[a]: 4f.; see also Joseph and Janda 1988: 196), but its examples are by no means uninteresting, and not as scanty as one would prima facie incline to admit.

It seems that, essentially, Tabor and Traugott (1998) are right, in that we cannot decide how to view directionality until we have studied both directional examples and counterdirectional examples more closely and managed to come up with a theory that has been tested and proven right:

We take this as evidence that structural unidirectionality (in any formulation) is not an appropriate presupposition at this stage in the development of the field. We do not mean to imply that no formulation will ever become a reasonable background assumption, but we believe that it is crucial – indeed it is one of the most fascinating challenges in the field – to establish the plausibility of an explicit theory before making an assumption along these lines. (Tabor and Traugott, 1998: 265)

There are likely to be some advantages in keeping changes from the lexicon into the grammar distinct from their opposite. We, as humans and as scientists, rely a lot on categories. Categories help us notice parallels and theorise around similarities and differences we find within and between categories. And as McMahon has noted in a slightly different context, we have a “human predilection for seeing patterns” (McMahon, 1994: 330). But we always run the risk of categorising in such a way that we may miss something of importance, or perhaps even come to the wrong conclusions. There also appear to be cases where categories are applied without scholars being clear about what they mean exactly. Needless to say, this is a serious problem which can cause enormous confusion and does not seem to be at all rare within the framework of grammaticalisation.

We need to bear in mind that whatever we decide to do, the fact that we can see a pattern does not mean that there is a natural pattern. For instance, the fact that I think the formation of the clouds on a particular day looks like a man on a horse waving down at me does not mean that everyone else will see it, nor does it mean that it exists. It is a trick of my imagination when looking up at the clouds. What I am trying to stress by this is the fact that whatever we decide, whatever categories of change we decide to ‘see’ or use, we are working with artificial tools, meant to help us deal with reality. As Robins said:

‘The facts’ and ‘the truth’ are not laid down in advance, like the solution to a crossword puzzle, awaiting the completion of discovery. Scientists themselves do much to determine the range of facts, phenomena, and operations that fall within their purview, and they themselves set up and modify the conceptual framework within which they make what they regard as significant statements about them. (Robins, 1997: 3)

Many linguists have had to tackle the problem of counterexamples, and try to find a way out of this problem. As an example we can look at Cowie’s answer to this

problem, which is a non-directional approach to grammaticalisation and lexicalisation:

The grammaticalization process could be seen as like [sic] the movement of water into the pothole of a river bed. Water flows in from one predominant direction. But within the pothole there is a degree of turbulence, which causes water to move locally in the opposite direction. But it cannot turn round and flow out of the pothole in the opposite direction. Some water does spill out of the pothole and flow in the predominant direction, and this is the water that is lost from the pothole. There is very little change at the interface between form and meaning that could not be catered for by such a model, which does not allow for separate consideration of ‘effect on the lexicon’ and ‘effect on the grammar’. (Cowie, 1995: 190)

Other linguists have tried hard to save the unidirectionality hypothesis, using any means possible, from ignoring the counterexamples to using biological metaphors to reason their way out of their dilemma. Two examples of this are Dahl and Newmeyer:

I think grammaticalization is unidirectional in about the same sense as biological processes such as growth, maturation, and ageing are. [...] the biological processes that take place during our lives sometimes give rise to contradictory results but there can be no doubt that they are basically irreversible. (Dahl, 1996)

But suppose that grammaticalization were, in fact, a distinct process. If so, then unidirectionality would be the most unremarkable fact imaginable. [Janda, personal communication, according to footnote] The reason is that unidirectionality is a property of natural processes *in general*. (Newmeyer, 2001: 204)

The biological metaphor and comparison is often used for matters of language change and language evolution. But language is not an organism. Language is not even an autonomous thing. How can it possibly have a ‘biological’ life? Why should it even have to resemble biological life? Still as McMahon (1994: 314-340) has said, biological metaphors (as well as other metaphors) may be useful and helpful, as long as we make sure we understand what they mean and use them in the right way, something which has not always been the case:

... the unfortunate history of biological metaphor in linguistics need not discourage present-day linguists from seeking parallels with other disciplines; their task is rather to examine potential analogies carefully, to make sure they understand both sides of the equation, and not to overstate their case: 'we should be neither misled by metaphors nor afraid of them' (Wells 1987: 42). There is a good deal of terminological intermarriage among the sciences, and if the terms are understood, there seems no reason why linguistics should not also participate. (McMahon, 1994: 314-315)

But quite often, even grammaticalisationists admit that the change which they like to see as unidirectional, is not quite that. However, they like to do their best to save the unidirectional characteristic. Another attempt at accomplishing this has been made by assigning the counterexamples to other distinct changes, e.g. lexicalisation, regrammaticalisation, etc (see 3.4). Croft (1996) appears to claim that grammaticalisation can sometimes be bidirectional. Like many others he also stresses that grammaticalisation is most definitely a "different process from the word formation processes", examples of which are sometimes presented as counterexamples to the unidirectionality hypothesis. Haspelmath (1999: 1043) claims that grammaticalisation is "overwhelmingly irreversible". He also specifies his statement somewhat more and claims that some changes never happen whereas others might happen, but they are "extremely rare":

Prototypical functional categories never become prototypical lexical categories, and less radical changes against the general directionality of grammaticalization are extremely rare. (Haspelmath, 1999: 1043)

Dahl, Croft and Haspelmath all admit, in a way, that grammaticalisation is not unidirectional or irreversible in that they use expressions like "overwhelmingly irreversible", "extremely rare", "basically irreversible", and Croft (1996) even seems to say that it is sometimes "bidirectional". They do not simply say 'irreversible' and that other directions are 'non-existent'. Still they claim that it is unidirectional – the counter-examples are just the exceptions that prove the rule, it seems. Or as Hopper and Traugott put it:

Their [counterexamples of unidirectionality's] existence, and their relative infrequency, in fact help define our notion of what prototypical grammaticalization is. (Hopper and Traugott, 1993: 126)

It is true that we can define things, concepts and ideas by their opposite, through so called *negative definitions*. By stating what is not entailed or included in the concept, it can become clear what is, just like it might sometimes be easier to say what we do not want to do, than what we would be willing to consider doing during a two-week holiday. However, missing in this case is an explanation as to **why** these examples should not be included in grammaticalisation and why they should not disprove the unidirectionality hypothesis.

3.2 The Strong Hypothesis and Reconstructions in Historical Linguistics

It seems that the unidirectionality hypothesis comes in both strong and weak versions. Naturally it is the strong version that is most problematic, partly due to the fact that it is sometimes used in reconstructions.

The **strong hypothesis of unidirectionality** claims that all grammaticalization involves shifts in specific linguistic contexts from lexical item to grammatical item, or from less to more grammatical item, and that grammaticalization clines are irreversible. Change proceeds from higher to lower, never from lower to higher on the cline. (Hopper and Traugott, 1993: 126, my emphasis)

Hopper and Traugott (1993) admit that there are counterexamples. However, as can be seen from the second extract (below) they are not too concerned by them:

Extensive though the evidence of unidirectionality is, it cannot be regarded as an absolute principle. Some counterexamples do exist. Their existence, and their relative infrequency, in fact help define our notion of what prototypical grammaticalization is. [...]

Such counterexamples should caution us against making uncritical inferences about directions of grammaticalization where historical data is not available. Usually such inferences are justified, however, and the rare counterexamples should not be allowed to deprive us of a useful descriptive method and an important source of data. But the possibility of an anomalous development can never be absolutely excluded. (Hopper and Traugott, 1993: 126-128)

But perhaps there are even stronger hypotheses of unidirectionality. Some linguists even argue that all changes to the grammar of a language are unidirectional, not just grammaticalisation, for instance, Heine (1997a: 4); Heine, Claudi and Hünnemeyer (Heine et al., 1991a: 221); Heine (1994); Lehmann (1982 [1995]: 19) and Lass (1997: 267f.) are mentioned by Norde (2001a:233-234; 2002: 48). However, even though Norde believes that the linguists just cited distinguish between ‘grammatical change’ and ‘grammaticalisation’, it could be that Hopper and Traugott also refer to all grammatical changes in the quotation above, only they refer to them all as *grammaticalisation*. It could also be that the authors above, viz. Heine, Lehmann etc. were in fact referring to grammaticalisation. See for instance the following citation from Heine (1997a: 4), taken from Norde (2002: 48), which bears a strong resemblance to many definitions of grammaticalisation:

Grammatical change is unidirectional, leading from lexical to grammatical, and from grammatical to more grammatical, forms and structures.

A couple of pages later he says (as also cited by Norde (2001a: 233)):

The development of grammatical forms proceeds from less grammatical to more grammatical; from open-class to closed-class categories, and from concrete, or less abstract, to less concrete and more abstract meanings [...]. A number of exceptions to the unidirectionality principle have been claimed, but they have either been refuted or are said to involve processes other than grammaticalization. (Heine, 1997a: 6)

This seems to make it clear that Heine (1997a) does not mean to distinguish between ‘grammatical change’ and ‘grammaticalisation’ as Norde (2001a; 2002) interpreted him.

Norde (1998; 2001a; 2002) does appear rather sceptical about this assumption that grammatical changes are unidirectional. However, she in fact retains unidirectionality as a distinctive feature between grammaticalisation and reanalysis (Norde, 1998: 213), and she also retains a distinction between grammaticalisation and degrammaticalisation which is based on directionality. She thereby joins the weak unidirectionalist camp, where one may recognise that there are counterdirectional movements. However, they belong to a different type of change.

3.2.1 Unidirectionality and Reconstructions

Quite correctly, Norde (2001a: 234) also believes that the strong hypothesis is clearly, though only implicitly, part of the application of unidirectionality in reconstructions (see also, according to Norde, Heine et al (1991a: 221); Heine (1994); Haspelmath (1999)).

... no cogent examples of degrammaticalization have been found. This result is important because it allows us to recognize grammaticalization at the synchronic level. Given two variants which are related by the parameters of grammaticalization [...] *we can always tell which way the grammaticalization goes, or must have gone. The significance of this for the purposes of internal reconstruction is obvious [...]* (Lehmann, 1982 [1995]: 19), emphasis Norde (2001a: 234)

However, depending on the linguist's belief in the hypothesis, the presentation of the reconstructions is likely to vary. Some may use the unidirectionality hypothesis in reconstructions, but also make it clear that this is only a hypothesis and in that case we cannot claim that they necessarily subscribe to the strong hypothesis. It seems to me that in fact most reconstructers would recognise that the reconstructions are hypothetical.

Newmeyer (2001) also calls attention to some problems which strong forms of the unidirectionality hypothesis may lead to in reconstructions. He believes that it is "standard practice among grammaticalization theorists" to rely on unidirectionality in "internal reconstructions." A method which he says must be seen as "illicit" (2001: 206. cf. 215). However, he (2001: 216-217) sees an even more serious problem in relation to this, namely the use of reconstructions as evidence of the progression of grammaticalisation. This must admittedly be seen as severely circular, since grammaticalisation being unidirectional is used to create the reconstruction and reconstructions are used to prove how grammaticalisation usually proceeds. Newmeyer gives examples of this from Heine's (1994) and Bybee et al's (1991) work (Newmeyer, 2001:217-220). How far this is true needs to be looked into more carefully, but it certainly appears to be true in his examples.

Others have also commented on the practical use of the unidirectionality hypothesis in reconstructions. With some reservation Haspelmath says:

... reconstructed changes cannot be taken as evidence for a theory of language change, so we must be very careful. However, the directionality of change is very often evident even if the change is reconstructed; and, most importantly, certain changes recur again and again in languages, so the sheer mass of cases counts in favor of certain reconstructions. (Haspelmath, 1996)

Haspelmath shows some agreement with Newmeyer on the use of reconstructions as evidence, however, he sees no need to worry too much, because it is often so evident that change is directional that we can be quite confident about most reconstructions that are made.

Based on what we know about the world's languages and language change, it is quite accepted that we can have changes in two directions in phonology – namely assimilations and dissimilations. However, assimilations are clearly more common and will therefore be the ones that we work with first when we are trying to do reconstructions.²⁶ It therefore seems quite possible that there are also two directions involved in this kind of change, one of which is more common and therefore can be worked on as a first basis in reconstructions. But as Newmeyer says:

It would be unthinkable in mainstream historical linguistics, for example, to take some *reconstructed* vowel – nasal consonant sequence and add it to a data base which included *attested* changes, say for the purpose of arguing for a particular theory of language change. (Newmeyer, 2001: 217)

It should be obvious that the same applies to reconstructions based on the unidirectionality hypothesis of grammaticalisation.

Janda has remarked that we might be sure that the change we are looking at is unidirectional. However, he believes that does not mean that there will not have been other changes which have given the same result. It is also possible that the change was more complex since there could have been a chain of changes which at some point may have led back to the original starting point.

A crucial point concerning such *counterability* – as distinct from mirror-image reversibility – which appears to have been overlooked in previous discussions concerns the fact that grammaticalizationists often stress the utility (or even indispensability) of their “unidirectional” “diachronic path(way)s” for the purpose of historical-linguistic reconstruction (cf., for

²⁶ Thanks to April McMahon for bringing this up in a discussion I had with her about my work (Feb. 2001)

example, Bybee et al 1994, pp. 17-19; Heine 1997b [(1997b)], *passim*; Lemaréchal, 1997). Still even if (schematically) there existed two changes $X > Y$ and $Y > Z$ which were both irreversible in a mirror-image sense, the existence of a countering-process whereby $Z > Y$ would make it impossible to reconstruct X as the immediate etymon of every textually attested Y , since the actual historical sequence might have been $X > Y > Z > Y$. In this case, our awareness of a countering-process should reduce or shake our confidence in the chronological accuracy of any specific reconstruction-scenario. (Janda, 2001:296-297)

Croft dismisses the problems with reconstructions without much concern. He states that there is no need to worry since (according to him) historical linguists do not rely very heavily upon the unidirectionality hypothesis at all. They use evidence such as “[i]ndependently established sound changes, facts of syntactic distribution, sociolinguistic differences among variants, etc” (Croft, 1996). Croft may be right, in that perhaps historical linguists do not consciously rely on the unidirectionality hypothesis. But it is quite possible that all historical linguists do assume a certain unidirectionality in their reconstructions, but that they may do so subconsciously.²⁷

In a way they are all right. Yes, unidirectionality cannot be assumed until we have proven that it applies to the change we are studying, which we cannot do without historical evidence. And we certainly cannot base our hypotheses and theories on reconstructions, which are based on the very ideas which form the basis of the hypothesis or theory that we are trying to prove. Reconstructions themselves however must be allowed to be uncertain. They are after all hypothetical, but sometimes we can be fairly sure that we have got them right. Janda in fact has suggested that we should indicate whose reconstructions we use and their certainty.

... Janda (1990, 1994) suggested – in all seriousness – that the asterisk as an indicator of reconstructed forms be replaced by a complex symbol of the form “n%(RN)”, where the variable “n” stands for a number showing the reconstructor’s (or the later writer’s) percentually-expressed level of confidence in a particular reconstruction, while the parenthesized “(RN)” stands for the initials of the reconstructor’s name (or the later writer’s name). (Janda, 2001: 298, fn. 10)

Such a convention would also make pathways which include reconstructions more useful and certainly much less confusing.

²⁷ This paragraph relies partly on a discussion with April McMahon (Feb. 2001)

3.3 Explaining Unidirectionality

There have been some attempts at explaining unidirectionality in recent years, but not many. Campbell (2001) looks at some of the explanations, but dismisses them one by one. One of the possible explanations he mentions is metaphor, which he says has been suggested by Heine et al (1991a) and it is also part of the explanation offered by Haspelmath (1999). He only mentions one other article which has attempted an explanation, namely Tabor and Traugott (1998), but I hesitate to recognise any attempt to explain unidirectionality in that article. It certainly explores the hypothesis in an attempt to find empirical justification for it, but they appear to admit that as yet they have not been able to “establish the plausibility” of a theory of unidirectionality which has previously been assumed (Tabor and Traugott, 1998: 265).

Haspelmath (1999) recognises that an explanation of unidirectionality has hardly ever been attempted, however he says that people have started to pay attention to this around the time of writing, i.e. the late 1990s. Using Keller’s invisible-hand theory Haspelmath tries to show that grammaticalisation is a “side effect”, which happens when people try to get noticed and therefore change their language slightly. This actually resembles Lehmann’s much earlier (1985) comments on the motivations behind grammaticalisation. He claimed that “[t]he received grammatical devices are notoriously insufficient to adequately express what he [i.e. every speaker] wants to say.” Which is why less grammaticalised expressions are used to be “more prominent” (1985: 315). Lehmann, in fact, also proceeds from this to propose an explanation to why degrammaticalisation should be so rare, he claims that it would “presuppose a constant desire for understatement, a general predilection for litotes” (Lehmann, 1985: 315). This must be seen as one of the most absurd explanations of why the reverse of grammaticalisation should not happen. And as Norde (2002) says, why does he make it sound as though if we use litotes **sometimes** (as we certainly do, it is in fact a popular rhetorical device), that would equal constantly? And as Campbell (2001: 135) also notes, in comment to Haspelmath’s (1999) aversion to accept that people would use abstract expressions to denote something more concrete, litotes is in fact quite common.

This also bears some similarity to Haspelmath's own explanation to why grammaticalisation should be unidirectional.

... degrammaticalization is by and large impossible because there is no counteracting maxim of "anti-extravagance," and because speakers have no conscious access to grammaticalized expressions and thus cannot use them in place of less grammaticalized ones. (Haspelmath, 1999: 1043)

However, there is one common feature in language, which could be an indication that there indeed is such a thing as a maxim of anti-extravagance, namely euphemism.²⁸ Interestingly, this is a counteracting force which has been noted by Heine (1997a) even though he himself is quite a firm believer in unidirectionality.

The exact status of such exceptions remains to be investigated; for the time being, I will assume that certain specific forces can be held responsible for exceptions. Such forces will have to do, in particular, with the pragmatics of linguistic communication (cf. Forscheimer 1953:37ff. [(Forscheimer)]) and relate to psychological and sociological factors such as taboo strategies and euphemism, politeness, humbleness, paternalism, and the like (see Allen & Burridge 1991 [(Allen and Burridge, 1991)]).

[...]

Yet, although it can be violated in the presence of alternative cognitive principles, the unidirectionality principle turns out to be statistically significant and can serve as a basis for generalizations on both linguistic evolution and language structure. (Heine, 1997a: 152-153)

Heine actually treats euphemism and some other counterdirectional forces, much like the neogrammarians treated analogy – a safe way of classifying the exceptions without actually explaining them at all. Just one of those funny things people do, or that happens in language... That he is an adamant believer in unidirectionality is also clear: just as he and his colleagues previously claimed that regrammaticalisation and degrammaticalisation are "statistically insignificant" (Heine et al., 1991a: 4-5), he now claims that unidirectionality is "statistically significant" (1997a: 153). But once more he fails to produce any statistics to support such a statement.

So, there are forces which can cause metaphor to be used to produce counterdirectional changes. What about Haspelmath's other claim, the claim that we have no conscious access to functional items? Campbell (2001) has criticised this, as

²⁸ Thanks to April McMahon for bringing up euphemism in one of our discussions (Feb. 2001).

well as other parts of Haspelmath's argument. The main problem with this he says is the fact that it "rests on sheer assertion" (Campbell, 2001: 139). This is true. Haspelmath provides no evidence neither in the form of his own data nor through references to other studies which would prove him right. And Campbell says counterexamples of unidirectionality also disprove this (2001: 139). The fact that there are what we think are counterexamples need however not disprove this argument, since language change is seldom conscious.²⁹

Haspelmath (1999) notes that generative linguists have generally seen no need for language change to be irreversible, since language change from their point of view consists of constructing or "choos[ing]" a different grammar in acquisition than the one that was used by the previous generation (1999: 1049).

... [l]anguage change is essentially a random 'walk' through the space of possible parameter settings (Battye and Roberts, 1995: 11, cited in Haspelmath, 1999: 1049)

[Lightfoot] suggests that languages, like biological populations, are continually in flux. Linguistically constructions may increase in frequency, or forms may be borrowed, producing novel structures; however, these are necessarily random and unpredictable, giving language the character of a chaotic system. Now and again, however, such random changes, along perhaps with gradual morphological change, may cumulatively alter the input data to the extent that children acquiring the language will set some parameter differently from the previous generation. (McMahon, 1994: 136)

Janda (2001: 299), for instance, also notes that just because we are speakers of a language, we cannot be expected to know the etymology of words and the history of syntactical constructions. A new generation cannot know which systems have existed beforehand. And this would seem to make directional changes even more complicated, indeed it would make them seem impossible. Naturally such explanations of language change cannot accommodate tendencies to unidirectionality very easily. But it is a fact that there are more changes in the direction from lexical to grammatical than vice versa. Why is that, if language change is random? How can reversals then be stopped?

²⁹ There are of course cases where someone consciously seems to have set off a change by producing a new formation or construction, but the decision to take this change on board rarely seems conscious.

This may have something to do with extralinguistic forces, such as Keller's maxims (see Haspelmath, 1999; Keller, 1990; 1994). The fact that one direction appears to be much more common for change is in fact a bit of a puzzle and a problem that the generative tradition and Lightfoot cleverly avoid by not talking much about directionality (cf. Haspelmath, 1999: 1049).

Maybe it is a sign of how difficult it is to explain language, that so few people have attempted to explain why grammaticalisation should be unidirectional. As Haspelmath (1999:1049) observes "the most striking fact about the previous explanations of unidirectionality is that there are so few of them". We would expect that there should at least have been attempts to explain this tendency. How can a hypothesis which is not even provided with well-argued explanations be assumed and accepted so broadly?

According to Haspelmath, Givón was the first to explicitly deal with unidirectionality (see further discussions in ch. 6 and 7). And Haspelmath also turns to Givón's writings in the search for an explanation to why grammaticalisation should naturally be unidirectional (Haspelmath, 1999: 1050), where we find points similar to those made by Janda (2001).

There are a number of reasons why such a process [i.e. the reverse of grammaticalization] should be extremely rare. To begin with, when a verb loses much of its semantic content and becomes a case marker, in due time it also loses much of its phonological material, becomes a bound affix and eventually gets completely *eroded* into zero. It is thus unlikely that a more crucial portion of the information content of the utterance... will be entrusted to such a reduced morpheme

[...]

Further, while the process of change through depletion is a *predictable* change in language, its opposite – enrichment or addition – is not. The argument here is rather parallel to the uni-directionality of transformations of deletion in syntax (Givón, 1975: 96; cited in Haspelmath, 1999: 1050)

Givón does not appear to be demanding mirror-image reversals of grammaticalisation for there to be such a thing as a reversal. Instead he seems to be relying on a theory that semantic content demands a form that is not too small (phonetically), i.e., as Haspelmath (1999: 1050) concludes from this, "there is an iconic relationship between form and meaning in grammaticalization". Givón then goes on to argue against a reverse process primarily on the basis of 'predictability'.

What is not predictable to happen, as “enrichment or addition” is not likely to, cannot, should not, happen. But Haspelmath actually hesitates at this. He says “[t]he problem with this argument is that the accuracy of predictability is generally quite low” (1999: 1050).

Lehmann (1982 [1995]) also used this section from Givón in his reflections on degrammaticalisation, with some clear reservations as to the predictability of the results of grammaticalisation, and we should probably be sceptical towards any claims that we can predict what changes are going to take place (cf. also the comments on reconstructions above, section 3.2):

If grammaticalization is really a unidirectional process, one must ask why this should be so. I [...] mention only the explanation that Givón (1975:96) has given. He says that grammaticalization essentially involves a deletion of both semantic and phonological substance. Degrammaticalization would have to be an enrichment in semantic and phonological substance. Now while the result of a deletion process may be predictable, its source is generally not predictable from the result; so the product of an enrichment process, or of degrammaticalization, would also not be predictable. This appears to be a step in the right direction. However, it remains to be seen, first to what extent the results of grammaticalization processes are really predictable, and secondly, if rules for these processes can be found, why natural languages cannot apply them, at least to non-zero elements, in reverse direction. (Lehmann, 1982 [1995]: 19)

As Haspelmath notes, many linguists have tried to explain language change through an interaction between two forces – clarity and economy (Haspelmath, 1999: 1050-1051).³⁰ These two forces lead to a cyclicity (or spirality) of change, because expressions will be economised until they can hardly be recognised and stop to fill their function, they must at some point be reinforced for clarity, and then the new expression will start its path towards ‘a better economy’... The question is, what does this mean for the unidirectionality hypothesis? As far as categories go this means that we will have a movement from A > B > A > B ... although the same form will not fill the A position every time. For instance, we may move from adpositions to case affixes to new adpositions and then on to new case affixes. Is this change still unidirectional?

However, if unidirectionality is meant to apply only to the same form (with some adjustments), the history of that form will probably usually not be spiralic, but

³⁰ cf. e.g. Georg von der Gabelentz, Antoine Meillet.

rather linear. It does after all seem most common that we move from a full lexical item to a more and more semantically and phonologically attritioned functional element, and it is true that it seems rare to have movements in the opposite direction.

Another person who has tried to shed some light on why grammaticalisation is so much more common than degrammaticalisation is Newmeyer (1998; 2001), but he is quite clear that, in his eyes, grammaticalisation is **not** unidirectional. His explanation is the “least-effort effect” where the explanation of the difference in frequency focuses on economy.

Functional categories require less coding material – and hence less production effort – than lexical categories. As a result, the change from the latter to the former is far more common than from the former to the latter. All other things being equal, a child confronted with the option of reanalyzing a verb as an auxiliary or reanalyzing an auxiliary as a verb will choose the former (Newmeyer, 2001: 214) (cf. Newmeyer, 1998: 276, cited in Haspelmath 1999: 1053)

Newmeyer, however, goes on to note that “all things are not always equal.” And that is why, according to him, grammaticalisation is not unidirectional:

There are other, conflicting, pressures that might lead the child to do the precise opposite. Analogical pressure might result in the upgrading of an affix to a clitic or the creation of a freestanding verb from an affix. (Newmeyer, 2001: 214)

Norde (2002: 58-60) is critical of the fact that both Newmeyer and Haspelmath only deal with changes from “functional to lexical categories”, which Norde does not consider cases of degrammaticalisation, i.e. a movement in the opposite direction along a grammaticalisation cline. A movement from a functional to a lexical category would normally classify as lexicalisation, in the meaning of something, anything, entering the lexicon, in Norde’s sense (see further in section 3.4.1).³¹ And even though I believe we must admit that also this kind of change, which Norde calls *lexicalisation*, is an important (sometimes) counterdirectional change, dealing solely with that kind of counterdirectional change will neither prove nor disprove unidirectionality.

³¹ Norde prefers Newmeyer’s ‘least-effort’. She means to say that Haspelmath’s *ifs, buts* – used metalinguistically are contradictions of his statement that we have no conscious access to functional items.

I also do not think that Norde is quite right about Newmeyer (1998; 2001). He does deal with several different kinds of upgrading (not only changes from functional to lexical). For instance, he has also treated the case Norde has worked on most, namely the Scandinavian genitive which in her eyes is a case of degrammaticalisation (1997; 1998; 2001a; 2001b; 2002).

What could cause a counterdirectional change, in other words, possible counterexamples to the unidirectionality of grammaticalisation? As mentioned briefly above, Heine (1997a: 152f.) suggests an external factor intruding and changing the cause of nature. Plank (1995), whom Norde (2001a; 2002) relies on in her explanations of the development of the Scandinavian *s*-genitive, on the other hand, argued that counterdirectional changes, or degrammaticalisation, should not be seen as “natural” since they are caused by what he calls *Systemstörung*, which is a form of **internal** change.

... affixal degrammaticalization is admittedly rare, but in case of favourable circumstances, such as some kind of internal *Systemstörung* [...] and a possibility of morphosyntactic reanalysis, it is by no means impossible. (Norde, 2002: 61)

Since it is clear that there is no directional restriction on change over all, I believe we need to look closer at any tendencies of directional preferences that would warrant a unidirectionality hypothesis for grammaticalisation. A decision to retain unidirectionality as part of grammaticalisation must be well and thoroughly argued for, with explanations as to why this is the best categorisation of these kinds of changes and also with explanations and arguments for how the counterexamples should be dealt with, **what** they are and **why** they are **not** grammaticalisation.

It is a fact that the reasons why one direction appears much more common do not have to imply unidirectionality, but could partly be due to poor sampling. Nevertheless, it is also likely to be due to natural reasons – some changes are more common than others, for instance compare assimilation and dissimilation (cf. Campbell, 2001: 136). It is quite possible that this asymmetry between the directions is due to both internal and external factors in language, perception and production. And it could also be affected by ‘unnatural’ language internal factors such as *Systemstörung*.

Often grammaticalisationists mainly claim that a mirror-image reversal of grammaticalisation cannot happen, and this I think most people seem to agree on (see Norde (2002) and Janda (2001: 295)). Many grammaticalisationists also seem to believe that a mirror-image reversal would be the only way the unidirectionality hypothesis could be proven wrong, and it does not happen.

What we need to understand, as Norde (2002) and Janda (2001) have observed, is that perhaps non-unidirectionality does not have to be a mirror of grammaticalisation, but can just involve a countermovement. As Janda has shown, the fact that there are not likely to be mirror-image reversal is not down to grammaticalisation, but rather due to other factors such as arbitrariness and “the non-existence of certain phonological strengthening processes which are imaginable but would be extremely bizarre” (Janda, 2001: 295; cf. also Newmeyer, 2001: 205). (However, some of those processes do happen – see Norde (1998: 212) and Campbell (2001: 136)).

It is also not enough to observe that one type of change is more common than another, we also have to try to explain this, and for unidirectionality these explanations have only just started to be suggested.

3.4 Counterdirectional Processes and Counterexamples

Even though many people have recognised that counterdirectional changes may be “rampant” (Newmeyer, 2001: 205) and that there may be a whole “myriad” (Janda, 2001: 299) of counterexamples to the unidirectionality hypothesis, it is still common that they are ignored or dismissed without much thought. As noted above, Heine, Claudi and Hünemeyer (Heine et al., 1991a: 5) call them “statistically insignificant”, without even presenting any statistical evidence for this,³² and Traugott and Heine (1991: 7) simply put them into the group of changes which occur through reanalysis.

Norde claims that Hopper and Traugott (1993: 126) are more unusual in that, as she sees it, they do in fact pay more attention to the counterexamples to unidirectionality and they even claim that they can be useful in defining grammaticalisation. Still, even though it is true that they say that the

³² As was also noted by Newmeyer (2001: 205)

counterexamples can “in fact help define our notion of what prototypical grammaticalization is” and that unidirectionality is an unresolved issue, I certainly would not say that they are very open-minded about unidirectionality. But it is true that they do at least pay some attention to the counterexamples, which at the time was still very unusual.

One way of dealing with the counterexamples and possibly improving the grammaticalisation framework is Norde’s (2001a) suggestion that “grammaticalization, non-directional and counterdirectional changes” should be carefully distinguished. She does not suggest that we should totally forget about unidirectionality, but she wants the strong hypothesis to be “rejected”. However, making a distinction between these three kinds of changes, would of course mean that the unidirectionality hypothesis is tautological (cf. Janda, 2001):

This implies that the unidirectionality of grammaticalization is indeed tautological, and that counterdirectional changes are not counterexamples to grammaticalization, but simply another (albeit less common) type of change. (Norde, 2001a: 238)

Giacalone Ramat (1998: 116) believes that one of the most problematic changes for the unidirectionality hypothesis may be cases which can be shown to be discontinuous, or that show a “*reversal of historical trends*” (emphasis original). The cases she has in mind are cases which have variously been called *exaptation* and *functional renewal*, for instance, where a morpheme refuses to disappear at the end of the grammaticalisation scale and instead finds a new function to fulfil. This means that an item definitely can move back and forth on a cline, and could therefore be seen as very harmful for the unidirectionality hypothesis. However, she also mentions such cases that simply do not continue towards the right of the cline, and towards zero, but instead stop at some point, something which is quite generally recognised as unproblematic and in fact quite common. Grammaticalisationists normally do not think that items **have to** progress to the end of the cline.

An interesting point and an example of misunderstandings that can quite easily arise, is Giacalone Ramat’s comment that an increase in morphological complexity, i.e. in inflections, for example, would run counter to ordinary grammaticalisation (Giacalone Ramat, 1998: 116). This does not seem to agree with most work done on grammaticalisation. Giacalone Ramat says that she has taken the

idea that grammaticalisation is accompanied by “morphological degeneration” from Lehmann (1982 [1995]: 132; 1985: 307). But it is part of grammaticalisation that a word may move closer to another word and indeed come to fill the function of an inflection.

The mistake Giacalone Ramat (1998) makes here is that she seems to take morphological degeneration as a characteristic of **all words** affected by the grammaticalisation process, cf. her example of verb inflections:

This may happen when personal pronouns grammaticalize to verb inflections involving a richer display of morphological distinctions than the original verbal paradigms. (Giacalone Ramat, 1998: 116)

Contrary to what she says, it is natural in the process of grammaticalisation that the word which is ‘approached’ by the grammaticalised item may gain increased morphological complexity, e.g. in the form of an inflection. The actual grammaticalised form, conversely, is usually reduced. It loses its ability to inflect, and may also be phonologically and semantically reduced. (One example which Lehmann (1982 [1995]: 132) uses is that of nouns turning into adpositions, which we can later sometimes see progressing into case markers.) The problem here appears to lie in the lack of clarity in some descriptions of grammaticalisation, but also in the move from a very word focused study of grammaticalisation to a phrase / construction focused approach.

Even though Giacalone Ramat (1998) recognises the problem of reversals along grammaticalisation clines, and even discontinuous examples that simply stop at some point on the cline, like many others, she seems to think that the only **true** counterexamples to the unidirectionality hypothesis would be ones which reversed the process completely, in other words, causing the construction to “return to the original state” (1998: 118). However, she believes that we need to study all the possible counterexamples carefully and she divides them into three groups:

(1) Non-continuous examples, which remain within the grammar, e.g. neuter gender is lost and gains the function of a collective plural marker in Italian. (1998: 112-114)

(2) Grammatical elements which become lexical items, e.g. *ade*, *anta*, *teens* (1998: 115)

(3) “[B]idirectional clitic/affix developments”, e.g. Irish *-mid*, *-muid* (1998: 115)

If we go on to look at a few more thoughts on degrammaticalisation and counterexamples to the unidirectionality hypothesis, we can start by returning to Heine et al (1991a). Heine et al (1991a: 5) have reported that Lehmann (1982 [1995]: 16-20) has found that many of the “cases of alleged degrammaticalization” presented are not actually counterexamples at all, but have just been poorly analysed. Lehmann criticises the reconstructions and etymologies which some people have suggested and he also appears to be saying that anything which is not a precise mirror image of the grammaticalisation process cannot be a degrammaticalisation process. He says, for instance, that:

None of these examples [from Kuryłowicz (1965 [1975]: 52f)] stands up to closer scrutiny. All of them suffer from the defect that the newly evolved derivational category does not possess a minimum of productivity, whereas those Proto-Indo-European derivational categories which they ultimately go back to [...] must clearly have been highly productive, for otherwise they could not have yielded inflectional categories.

An interesting recent source of thoughts on degrammaticalisation is Janda (2001), where Janda discusses the multitude of counterexamples to the unidirectionality hypothesis, as well as the scientific use of such a hypothesis. It is unfortunate, as well as notable, that Janda only presents very few counterexamples in his paper (2001: 300). For instance, he only gives five examples of the movement from affix to clitic or free word and three of those are presented extremely briefly – e.g. “freeing of bound person-/number-suffixes in Turkic dialects Tuvan/Tofalar”. However, he pays more attention to the last two examples, namely of the English genitive- ‘s and the Regional Spanish clitic *nos* (Janda, 2001: 301).

Considering the number of works he lists which have included some counterexamples, he should have looked at quite a few, and presumably reflected on them. So why could he not have presented a list of the counterexamples that people have used? A list of works which include counterexamples, does not tell us anything about how many counterexamples have actually been cited in the literature, since they could in fact all list the same one or two examples. So such a list would have been very interesting, and also valuable, much more so than a plain list of who cited

counterexamples. It would also have been interesting for the reader to see the examples and it could have helped him/her form their own opinions of what should count as a counterexample of unidirectionality and what should not.³³

As evidence of the falsity of the unidirectionality hypothesis, Janda claims that it is implausible that there should be any constraints to force changes to move only in one direction. He believes that there seems to be “a well-worn diachronic “path(way)” (= set of frequently co-occurring individual changes) of degrammaticalization” via euphemism, which should mean that this kind of counterdirectional change is quite common, even if it is not a case of “mirror-image reversal” (Janda, 2001: 300). Still he admits that it is possible that the grammaticalisation ‘process’ is “tautologically unidirectional”, i.e. that the fact that grammaticalisation is unidirectional is part of what grammaticalisation is (Janda, 2001: 299-300).³⁴

It is clear that most people are quite confused by counterdirectional and non-directional changes. Most of them had probably already formed an opinion of what grammaticalisation was before they came across these examples, and because unidirectionality often seems present in the definitions, they may well have assumed that and worked that into their concept of grammaticalisation. Giacalone Ramat (1998), who was mentioned above, is a clear example of someone who does not appear quite sure of what to say about the directionality of grammaticalisation:

For conceptual clarity they [i.e. changes of functional renewal] should be kept apart from cases of grammaticalization as a process proceeding from lexical forms to grammatical forms along a unidirectional cline. These considerations lead us to conclude that grammaticalization is a specific form of language change. Another possibility would be to reject unidirectionality as a necessary condition to decide what changes a grammaticalization theory should explain. I feel, however, that in this case what we call “grammaticalization theory” would be left with a too vague definition of its field, including almost every instance of change. The unidirectionality of changes from lexical categories to grammatical (functional) categories constitutes a significant constraint on possible language changes. (Giacalone Ramat, 1998: 123)

³³ I have started putting together such a list myself, although as yet I have not been able to complete it since it takes a long time to go through all the articles, papers and books which may be of importance. However, I hope I will be able to make this available soon.

³⁴ **If** it is a process, at all, which he is actually trying to prove that it is not. He wants it classed as an epiphenomenon.

She actually makes it quite clear that unidirectional and non-directional views of grammaticalisation both have pro's and con's. She decides to recognise that there are changes which do move in the opposite direction of ordinary grammaticalisation changes, or changes which like functional renewals or exaptations are non-directional. However, "for conceptual clarity" these should, in her eyes, be classified as an autonomous group of changes, because otherwise we would have to reject the unidirectionality hypothesis and constraint and that would allow too many changes to be classified as grammaticalisation. And grammaticalisation might then no longer be a workable linguistic tool...

Tabor and Traugott (1998: 231-235) have also discussed what we can do now that we know that there definitely are some possible counter-examples, or at least counter-directional and non-directional changes of some kind. They recognise three possible approaches that can be taken:

(1) We can "reject structural unidirectionality as criterial for grammaticalization", as they think that Nichols and Timberlake (1991); Hagège (1993) and Traugott (1995 [1997]) have done.

(2) Depending on the theory we are working with we can choose to treat it as irrelevant if our theory does not predict unidirectionality. This position is quite common among "linguists working in parametric theory", e.g. Lightfoot (1991; 1979); Roberts (1993) and Warner (1990).

(3) Tabor and Traugott themselves choose to empirically test if there is "structural unidirectionality", before they decide what the next step should be. They assert that "the jury is still out on the status of structural unidirectionality as a criterion for deciding which change episodes come within the purview of "grammaticalization studies" (1998: 231). And because structural unidirectionality is still but a hypothesis without exact formalisation and which has yet to undergo testing, it cannot be used as "a definition of the subject matter [viz. grammaticalisation]" (Tabor and Traugott, 1998: 235).

We shall now have a look at some of the processes which have been suggested to involve counterexamples to the unidirectionality hypothesis, but which have also sometimes been classed as distinct changes from grammaticalisation.

3.4.1 *Lexicalisation*

One of the possible terms which has been used for some potential counterexamples of the unidirectionality hypothesis is *lexicalisation*. But there appears to be some

confusion and/or disagreement concerning what this term refers to. Should the term *lexicalisation* be used to cover all lexical innovation processes, i.e. word formation of any kind – zero derivation; grammatical and other morphemes (bound/free) with or without the affixation of derivational morphemes becoming semanticised and autonomous; acronyms which become used as lexemes, etc? Or should it only be used for the process which moves linguistic items from a grammatical to a lexical status; or perhaps even from a grammaticalised to a (re)lexicalised status? Or should *lexicalisation* also be applied to something as different to the above as words which become part of another word? Then possibly differentiating two types of lexicalisation – *contractive lexicalisation* and *expansive lexicalisation* (see Greenberg, 1991: 301). There are more alternatives still for what *lexicalisation* could mean, some of which I shall come back to shortly, others of which are specific to certain subfields of linguistics, for instance computational linguistics, and involve a rather different kind of process.

A second important question is whether lexicalisation should be viewed as a process distinct from grammaticalisation, and therefore with no potential counterability as far as the (weak) unidirectionality hypothesis of grammaticalisation is concerned? Or should it be viewed as the opposite of grammaticalisation and therefore simply the process in reverse – i.e. proof that the process of grammaticalisation is not irreversible and unidirectional?

Let us have a look at how the term has been used, and what problems other linguists have noted. We can start by noticing that others have also remarked on the fuzziness surrounding the term and concept of *lexicalisation*:

Lexicalization, on the other hand, is a process less carefully and less systematically studied than grammaticalization. Thus, there is no general agreement about the use of the term itself. (Wischer, 2000: 358)

It actually appears as though *lexicalisation* and *lexicalise* have sometimes been used with what might seem like hardly any terminological consideration (see e.g. Hopper, 1992a).³⁵ Nevertheless, these terms can also be used in a more technical sense as the different uses below should indicate. And even the uses which seem

³⁵ Hopper (1992a: 81): “Grammaticalization of auxiliaries as verb inflections, with the suppletive **lexicalization** of categories in verb stems, therefore favors aspect over tense, and these over person/number.” (emphasis mine)

rather haphazard, can be sorted into the four categories of lexicalisation which Traugott has recognised.

Traugott (1994: 1485) mentions that *lexicalisation* is a term, which has been used to mean many different things. Nevertheless, she misses out some of the meanings which lexicalisation may have, as we shall see below.

(1) "... the expression as a linguistic form of a semantic property. [...] English *have* and *be* can be said to 'lexicalize' ownership, location, possession, existence (Bickerton, 1981)."

(2) "... the process whereby an originally inferential (pragmatic) meaning comes to be part of the semantics of a form, that is, has to be learned. For example, in speaking of the fact that the inference of prospective (*be going to* construction became part of the meaning of *be going to* as an auxiliary, it can be said that the inference of prospectivity is lexicalized. Lexicalization in the sense described is as much **part of** semantic change in general as of **grammaticalization**." (emphasise mine)

(3) "[T]he process whereby independent, usually monomorphemic, words are formed from more complex constructions (Bybee, 1985); this process is often called 'univerbation', [sic] One example is the development of words like *tomorrow*, which originated in a prepositional phrase; the boundary between preposition and root was lost, and a monomorphemic word developed."

(4) "[P]honological changes that result in morphological loss and the development of idiosyncratic lexical items, such as the English pairs *lie – lay*, *sit – set*, *stink – stench*, all of which have their origins in *i*-umlaut."

Of course, the fact that Traugott lists all these definitions does not mean that she is willing to use the term to mean all these different things herself. She simply recognises and alerts others to the diversity surrounding the term, which is very important especially considering that this is taken from an encyclopedia. Similarly, Laurel J. Brinton (2002: 70-74) has listed nine different uses of the term *lexicalisation*:

- (1) "Adoption into the lexicon"
- (2) "Falling outside the productive rules of grammar"
- (3) "Ordinary processes of word formation"
- (4) "Grammatical word (category) > lexical word (category)"
- (5) "Syntactic construction > lexeme"
- (6) "Bound morpheme > lexeme"
- (7) "Independent morphemes > monomorphemic form"

(8) “Idiomaticization”

(9) “Semanticization”

Lehmann (1989), conversely, makes it clear that not only does he recognise at least three different senses for the term *lexicalisation*, but he uses the term to mean all three of those things:

... daß einschlägige Phänomene den Übergang sprachlicher Einheiten ins Lexikon illustrieren sollten. Diese Bedingung wird von Prozessen sehr verschiedener Art erfüllt. [1.] Zum ersten können syntaktische Konstruktionen derart reduziert werden, daß ein ehemaliges Lexem zum Derivationsaffix wird [... 2.] Des weiteren können gebundene Morpheme submorphemisch und schließlich in die Wurzel integriert werden. [... 3.] Syntaktische Konstruktionen können noch einem anderen Sinne lexikalisiert, nämlich versteinert und inventarisiert werden [...]³⁶ (Lehmann, 1989: 12-13)

One of Lehmann’s senses of lexicalisation has been questioned by Ramat (1992), who believes that at least one of Lehmann’s examples of a type of lexicalisation should be seen as a clear case of grammaticalisation. Namely, sense one above, the change from a former lexeme to a derivational affix. The example that Ramat picks up on is OHG *haidus* > MHG *-heit* (Lehmann, 1989: 12; Ramat, 1992: 558).³⁷ Lehmann can be said to provide an alternative to this view in his recent (2002) paper on grammaticalisation and lexicalisation. In that paper he makes it clear that he believes these two phenomena to be closely related. In fact, he thinks that lexicalisation sometimes appears to ‘prepare the ground’ for grammaticalisation. And it is quite possible that already in 1989 Lehmann did not see grammaticalisation and lexicalisation as mutually exclusive.

Even so, I do not think that Ramat and Lehmann would be able to agree on a definition of lexicalisation. In fact, neither Traugott (1994) nor Lehmann (1989) includes the meaning of lexicalisation which Ramat uses and which appears to be immensely popular today, namely the one that can be illustrated by words like *ism*,

³⁶ Translation: ... that relevant phenomena shall illustrate the transfer of linguistic units into the lexicon. This condition is fulfilled by very different kinds of processes. [1.] Firstly, syntactic constructions can be reduced so that a former lexeme becomes a derivational affix [...2.] Furthermore bound morphemes may become submorphemic and finally intergrated in to the root. [...3.] Syntactic constructions can lexicalise in yet another sense, namely by becoming frozen and ‘inventarised’ [i.e. they become idiomatic]...

³⁷ This will be discussed further in section 3.5.

new lexemes which have developed from old affixes or parts of words (cf. Wischer's rather broad definition below).

Let us look next at a few of the other definitions of lexicalisation that have been given:

A reverse process [of grammaticalisation] is the *lexicalization* of a morpheme. (Kuryłowicz, 1965: 69; 1965 [1975]: 52)

Whenever a linguistic form falls outside the productive rules of grammar it becomes lexicalized. (Anttila, 1972: 151)

... we can say that it is lexicalized in the sense that synchronically it is a part of the host morpheme. (Greenberg, 1991: 301) ("contractive lexicalization" 1991: 301)

... the process that turns linguistic material into lexical items, i.e., into lexemes, and renders them still more lexical.³⁸ (Wischer, 2000: 359)

Only two of these four definitions above can be included under one of Traugott's four types of lexicalisation. Greenberg's and Anttila's definitions can both be included under type three, however, Anttila's is much wider than Traugott's type three above. Wischer on the other hand seems to have a concept of lexicalisation which is quite close to Lehmann's (1989). Whereas Kuryłowicz's definition does not necessarily fit into any of the types listed so far, except possibly some of the types Brinton mentions, e.g type (1) and (4) (see above).

It is of utmost importance to a hypothesis of unidirectionality that also Kuryłowicz speaks of lexicalisation, and even more strikingly that he sees it as the "reverse" of grammaticalisation (Kuryłowicz, 1965 [1975]: 52-53). Often Kuryłowicz's definition of grammaticalisation is quoted as a typical definition of grammaticalisation, and typical also in the sense that unidirectionality is seen as intrinsic to it. But how can that be if Kuryłowicz himself did not see grammaticalisation as irreversible?

Giocalone Ramat (1998) makes an important discovery in her discussion of the limits of grammaticalisation, something which had then already been alluded to

³⁸ Clearly a near-paraphrase of Kuryłowicz's (1965 [1975]) definition of grammaticalisation.

in Ramat's (1992) criticism of Lehmann's usage of the term *lexicalisation*. She claims that linguists do not seem able to agree on where the line should be drawn between grammaticalisation and lexicalisation (1998: 120). This may not be very clear from the actual definitions of grammaticalisation and lexicalisation, but this is partly because they are usually rather fuzzy, at least on a closer look. The examples which are used for the two processes, however, make this extremely clear, because as Giacalone Ramat says "the same phenomena are sometimes cited as exemplary cases of either linguistic process" (Giacalone Ramat, 1998: 120; cf. also Lindström, forth.).

Giacalone Ramat's own view of lexicalisation seems perhaps quite close to Lehmann's (1989; 2002):

... I argue that at the end of the cline the limits between grammatical elements and lexemes may be blurred, and propose to rethink the traditional view according to which grammaticalization and lexicalization are quite distinct, even opposite processes. Rather, they seem to be complementary or overlapping and processes of change such as loss of autonomy or univerbation are similar both in grammar and in the lexicon. (Giacalone Ramat, 1998: 121)

It seems that lexicalisation to Giacalone Ramat, for instance, is a process whereby words move together or one word becomes part of another, in other words, univerbation (cf Traugott (1994) sense 3 above). Naturally, this would not be expected to counter grammaticalisation, being in fact a highly common process in grammaticalisation. And this is also where she sees the overlap between grammaticalisation and lexicalisation. She illustrates her notion of lexicalisation "in the final stages of grammaticalization" with the following examples:

- 1) gradual development across time of affixes or prefixes may lead to lexemes in which the original affix is no more recognizable: this is the case of the Latin comparative suffix *-ior-* in Italian *signore*, French *seigneur*, with semantic shift from 'older' to 'noble, respectable man'. [...]
- 2) compound words tend to become opaque, as time passes: "only specialists of etymological research know that English *lord* comes from Anglo-Saxon *hlaf+weard* 'bread-guard'" (Hagège, 1993: 183).
- 3) gradual evolution of constructions across time may lead to new lexemes: well known cases are OHG *hiu tagu* (old Instrumental case) > German *heute*, Latin *ad ipsum* (Accusative) > Italian *adesso* 'now'. [...]
- 4) finally, cases of periphrastic constructions undergoing a process of 'idiomatization' might be considered here. A case in point would be the

Italian aspectual periphrasis *venire* + gerund, a grammaticalized construction expressing continuous and iterative aspect which has undergone obsolescence and reduction in use in Modern Italian (Giacalone Ramat, 1995). This is a reversal of the tendency to expand contexts for grammaticalized forms (Heine et al., 1991[a]) and its final outcome is the creation of almost idiomatic expressions to be located in the lexicon rather than in the grammar. (Giacalone Ramat, 1998: 121-122)

These examples are all of lexicalisations from “morphological or syntactic constructions” as Giacalone Ramat puts it, however her understanding of lexicalisation as a whole appears to be that it is the process by which new lexical items are created (1998: 121-122), similarly to Lehmann (1989). But if we compare her view with those of Lehmann (2002) there are also important differences. Lehmann sees lexicalisation more as a preparatory mechanism that may even be a prerequisite for grammaticalisation. To Giacalone Ramat, on the other hand, lexicalisation happens in the latter stages of grammaticalisation.

Like Giacalone Ramat, Wischer also believes that lexicalisation and grammaticalisation are more closely related than has been recognised, and “not at all generally contradictory” (Wischer, 2000: 355). The main, and maybe the only, difference that she sees between the two is that they “operate on different levels of the language” (2000: 355). And conversely to Giacalone Ramat (1998) she claims that some say grammaticalisation can follow on from lexicalisation:

Others point out that grammaticalization can be a further development of lexicalization, in the sense of a stricter codification of the lexicalized item (cf. Keller, 1995: 227). (Wischer, 2000: 359)

This appears highly similar to Lehmann’s views in his paper presented in 1999 (published 2002), at a conference which Wischer organised and later took part in editing the proceedings of. However, Wischer does not refer to Lehmann’s recent work. As already mentioned above, Lehmann’s latest thoughts on grammaticalisation and lexicalisation (2002), also mention the similarities and relations between grammaticalisation and lexicalisation. He believes that the two can be seen as working in parallel with one another. He (Lehmann 2002) claims that lexicalisation may be the initial step of the grammaticalisation of a construction, which unfortunately seems to have been slightly misinterpreted by Fischer and Rosenbach (2000: 5), who believe that he sees lexicalisation as “an aspect of

grammaticalization”. What Lehmann actually meant comes across better in their next sentence (2000: 5), where they claim that he refers to lexicalisation and grammaticalisation as “reduction processes” working only on different planes (cf. Wischer’s comments on levels which were discussed above). The fact that Lehmann says that grammaticalisation and lexicalisation may alternate, can be interpreted to the effect that lexicalisation could also take place after grammaticalisation, even though Lehmann appears to be mainly thinking about lexicalisation preceding grammaticalisation. One of his examples of how grammaticalisation follows lexicalisation, also makes it quite clear that grammaticalisation is not meant to **include** lexicalisation (as one could interpret Fischer and Rosenbach, see above):

For instance, before *auf Grund (von)* ‘on the basis of’ can ever get grammaticalized to a grammatical preposition, it must first be lexicalized to the lexical preposition *aufgrund (von)*. In this sense, grammaticalization presupposes lexicalization. (Lehmann, 2002: 1)

Greenberg (1991) also speaks of lexicalisation. Lexicalisation to him, however, is something which can occur during (but more towards the later stages of) grammaticalisation. In the final stages of grammaticalisation, phonological, semantic and distributional changes usually take place, according to Greenberg (1991). The semantic changes that occur are usually a form of loss of meaning, or *desemanticisation* as Greenberg calls it, but this works together with the distributional changes and can lead to a form of contraction or expansion. He alternates between calling this contractive and expansive *desemanticisation* and contractive and expansive *lexicalisation*, which makes it a bit confusing.

Contractive lexicalisation / desemanticisation to Greenberg is something similar to univerbation, or fossilisation. One difference between *lexicalisation* and *desemanticisation* that we can see, is the fact that Greenberg decides to **exclude** changes which involve a derivation becoming part of an inflectional morpheme, instead of a lexical item, from lexicalisation (1991: 311). What happens in *contraction* is that a morpheme becomes fossilised in certain lexical forms. This involves lexicalisation in the sense of becoming “synchronically [...] part of the host morpheme” (1991: 301), which we could also compare to *demorphologisation* in Hopper and Traugott (1993), to which we shall return shortly. It also involves

desemanticisation in the sense that it “can no longer be assigned a meaning” (1991: 301).

But there is also *expansive lexicalisation / desemanticisation*, which is when a linguistic item becomes more and more abstract as it expands its meaning, until it finally has “zero intension”, after which reinterpretation and *regrammaticalisation* may take place (see further section 3.4.3) (Greenberg, 1991).

Moreno Cabrera (1998) has likewise concluded that these two phenomena of lexicalisation and grammaticalisation are complementary. He also believes that it is possible that “grammaticalization feeds lexicalization” occasionally (1998: 211). Moreno Cabrera’s definition of lexicalisation is very much like one of Lehmann’s (1989), in that he defines it as “the process creating lexical items out of syntactic units” (1998: 214), and he also says that it “proceeds from syntax towards the lexicon. The source units for lexicalization are not lexical items but syntactically-determined words or phrases” (1998: 217). He also notes that lexicalisation processes have been studied as degrammaticalisation by, for instance, Ramat (1992), however he does not state explicitly what he thinks of that form of treatment of lexicalisation. It seems however as though he views his idea of lexicalisation as a form of degrammaticalisation also (cf. Moreno Cabrera, 1998: 224). Interestingly, Moreno Cabrera notes that since the lexicalisation of e.g. idioms involves a “lexicotelic process,” “lexicalization goes in exactly the opposite direction in regard to grammaticalization” (1998: 214). A statement which he then questions and explores further.

Now, we must of course ask how grammaticalisation feeds lexicalisation and in what sense these two processes are complementary. According to Moreno Cabrera (1998: 218) the derivation of an adjective from a verb of process can be seen as a process of grammaticalisation (e.g. Latin *scribere* (V) > *scribens* (PART.) > *scribens* (ADJ.)). I must admit I am sceptical towards this since even though the sense of the word changes it does so when a derivational affix is attached and it does not seem desirable that we should view all derivational processes as grammaticalisation if they happen to proceed in the same direction as grammaticalisation usually does. But in fact we could possibly say that the semantic change occurs in the zero conversion from participle to adjective, and that would be easier to accept as grammaticalisation. But still it would be pushing the limits of grammaticalisation, since participles often are very similar to adjectives.

Moreno Cabrera's next step is to claim that in Latin it was possible "to use present participles [such as the one above] as nouns denoting the agents of the actions formerly viewed as qualities", which he interprets as a move from quality to person and therefore in the opposite direction of the metaphorical abstraction hierarchy specified for grammaticalisation by Heine et al (1991a):

PERSON > OBJECT > PROCESS > SPACE > TIME > QUALITY

And that he believes is then an example of lexicalisation. It seems to me that his view of unidirectionality and the issue of degrammaticalisation is quite different to that of most other grammaticalisationists. Correctly, he states that "[i]t is claimed that when a lexical item grammaticalizes as a morpheme it is not in general possible for this morpheme to de-grammaticalize into a lexical item" (1998: 224). And then he says that "it would be much better to characterize this process as irreversible" (1998: 224), presumably rather than calling it unidirectional. But what his examples show is that (1) words can undergo derivational processes and become new words, (2) these new words may gain new meanings, which may in some sense be more abstract, (3) these new meanings may then gain new uses and become classed as a different part of speech. This in my eyes all shows more relation to word formation than to the typical discussions of grammaticalisation.

A rather different view of "*(re)lexicalisation*" (italics added), is presented by Janda (2001). He claims that it is in fact quite common in (informal) English that particles, prepositions and adverbs become more lexical, which is what he means by "*(re)lexicalization*", i.e. something similar to zero conversion. He also lists one example from German and one from French of this process, a process which he argues often takes place via "clipping-plus-metonymy" or "euphemism" (Janda, 2001: 299). And thereby he proves his point that it is "implausible [...] to expect there to exist any general constraints which would prevent the countering of grammaticalization" (Janda, 2001: 299-300).

It is interesting to note, in relation to Janda's comments about metonymy and clipping sometimes leading to "*(re)lexicalisation*", that Hopper and Traugott believe metonymy is the primary semantic and pragmatic process involved in the early stages of grammaticalisation (1993: 80-81, 87). Moreno Carbrera (1998: 211, 224-226), however, (similarly to Janda) has suggested that metonymy is more typical of

lexicalisation and that metaphor is the primary semantic mechanism of grammaticalisation. But he also notes that metaphor and metonymy are closely interrelated, in fact a metonymical mechanism may sometimes, according to him, result in a metaphor (1998: 224).

That metaphor and metonymy are closely related has been noticed also by others. For instance, Wischer (2000), who has been referred to above, has also explicitly dealt with the link between the phenomena in view of these two semantic mechanisms. She recognises that (part of) the reason why lexicalisation and grammaticalisation are so closely related could be because they involve “similar mechanisms” (2000: 359).

Some linguists have claimed that lexicalisation and grammaticalisation should be kept distinctly apart. Norde (1998: 212), and Hopper and Traugott (1993), for instance, believe this. Norde’s reasons are, first of all, that lexicalisation does not take just functional elements as its input, but anything which could possibly enrich the lexicon, anagrams, words which may be given new meanings, etc. Secondly, lexicalisation is not as gradual as grammaticalisation, and can therefore be neither grammaticalisation nor degrammaticalisation, according to Norde:

From the examples of the lexicalization of affixes it becomes evident that lexicalization is not simply ‘grammaticalization reversed’. **Instead of gradually shifting from right to left, passing through intermediate stages, they ‘jump’ directly to the level of lexicality.**

It should be emphasized however that lexicalization may be counterdirectional when grammatical items are involved, but it is essentially non-directional. (Norde, 2001a: 236, emphasis mine)

Other linguists believe lexicalisation can form part of degrammaticalisation, and that it quite often does so (see e.g., Ramat, 1992). But, even though Ramat, for instance, first says that “LEXICALIZATION IS THUS AN ASPECT OF DEGRAMMATICALIZATION”, he then clarifies this slightly by softening the tone a bit and stating that “degrammaticalization processes may lead to new lexemes” (Ramat, 1992: 550), which means that degrammaticalisation is not necessarily the only process which may involve lexicalisation. Norde (2001a: 237) disagrees with the connection made between lexicalisation and degrammaticalisation, she thinks that it is wrong to treat it “as synonymous with or a subset of degrammaticalization.”

Instead she insists that lexicalisation is a process distinct from grammaticalisation. However, still she spends quite some time on it, probably primarily because it shows that unidirectionality is not “a valid tool in linguistic reconstructions” (2001a: 238). It is therefore important to discuss the process, whether one sees it as a form of degrammaticalisation or not, since, even if it is not the reverse of grammaticalisation, it certainly proves that we cannot take unidirectionality for granted. If we have two words synchronically, which are related historically, one a verb and one a preposition, one cannot assume that the verb will definitely have preceded the preposition, nor vice versa. In that sense lexicalisation does have a certain amount of counterability.

Also Hopper and Traugott (1993: 127) go to some length to make sure that lexicalisation and grammaticalisation are kept apart, and thereby, since these two processes are clearly distinct, one cannot be the reverse of the other (cf. Cowie, 1995: 189). At first it seems that they do not appear to realise, as Norde does, that this still gives these examples some counterability. However, they do recognise that there are counterexamples to the unidirectionality hypothesis, and maybe examples of lexicalisation are included in those.

Hopper and Traugott’s use of lexicalisation is like Janda’s (and many others’). It primarily has a sense of conversion and zero-derivation. Sometimes the lexicalised words take a derivational affix, sometimes they do not.

Probably the most often cited putative counterexamples are those involving the lexicalization of grammatical items, as in *to up the ante, that was a downer, his uppers need dental work*. Similar examples can be found in other languages. For example, in German and French the second person singular familiar pronouns *du* and *tu* are lexicalized as the verbs *duzen* and *tutoyer* respectively, both meaning ‘to use the familiar address form’. **Since lexicalization is a process distinct from grammaticalization, is not unidirectional, and can recruit material of all kinds** (including acronyms, e.g., *scuba* [...]), data of this kind can and, we believe, should be considered examples of the recruitment of linguistic material to enrich the lexicon. In other words, they are not part of grammaticalization as a process, but rather of lexicalization. (Hopper and Traugott, 1993: 127, emphasis (bold) mine)

Cowie (1995: 189) concludes, in her review of Hopper and Traugott (1993), that unidirectionality forms part of the definition of grammaticalisation. It is certainly

quite evident through this that Cowie herself does not believe that grammaticalisation is unidirectional and this is explicitly confirmed by her statement that:

... lexicalization and other counterexamples could be subsumed under the heading of 'grammaticalization' in an unproblematic way. It is misleading to imply that lexicalization, in the sense in which *duzen* or *tutoyer* are examples of lexicalization, is a process independent of, but equipollent with, grammaticalization. [...] The nature of the examples of lexicalization, compared with the wealth of material that falls under 'grammaticalization', is a clear indication that Hopper and Traugott's second position (counterexamples to unidirectionality should be included under 'grammaticalization', but regarded as less typical [cf. Hopper and Traugott 1993: 126 on unprototypical grammaticalisation]) is the more correct one. (Cowie, 1995: 189)

But Hopper and Traugott (1993: 164) also mention another process which comes close to what some would call *lexicalisation*, viz. *demorphologisation*. This was mentioned above in the discussion of Greenberg. What it means is that the morphological function of a linguistic form is lost and that the form is also lost or becomes part of another morpheme. An example which they give is Present-Day English *seldom* which includes an old dative ending *-um*. The term *demorphologisation* is one that they borrowed from Joseph and Janda (1988), and they also employ the alternative and more positive term *phonogenesis* which means that " 'dead' morphemes become sedimented as phonological segments and over long periods actually create and repair the phonological bulk of words", and which is a term and concept taken from Hopper (1990; 1992b; which was in the end published in 1994). Other examples that they give include (Hopper and Traugott, 1993: 165):

-nd in English *friend*, *fiend* (from the Germanic present participle *-ende*)
Irish *imigh* 'go, leave' < Early Old Irish *imb-theg* 'about-go'
Irish *friotaigh* 'resist' < Early Old Irish *frith-to-theg* 'against-to-go'
Irish *tag-* 'come' < *to-theig* 'to-go'
Irish *fog-* 'leave' < *fo-ad-gab* 'under-toward-take'

Echoing the voice of many other linguists, e.g. Hopper and Traugott, Haspelmath (1996) also argues that zero-derivation or conversion is not a case of grammaticalisation or the reverse of grammaticalisation. His argument is based on

his view that it is not a matter of, for example, an adverb **becoming** a verb. Instead it is an addition to the lexicon based on a word that was already in existence and which may remain. It is simple word formation, and has **nothing** to do with grammaticalisation.

However, if we compare processes of zero-derivation or conversion to processes of grammaticalisation, we find that some grammaticalised items have split off from a continued lexical meaning when they grammaticalised, which causes what Heine and Reh (1984: 57-59) call a *split* and what Hopper (1991: 22, 24-25) and Hopper and Traugott (1993: 116-120) call *divergence*. This means that we may have one version of a lexical item which has undergone grammaticalisation, while another version, i.e. the word in a slightly different context, may remain in the language in a more lexical and original sense. One example of this is the English numeral *one*, which was *an* in Old English and meant something like ‘one, a certain’ and which developed into the numeral *one* as well as the indefinite article *a, an* (Hopper and Traugott, 1993: 117). Another example is French *pas* ‘step’ which still exists as a noun, but which also functions as a negative particle (Hopper, 1991: 24). Why could not the same be possible in a reverse process? Why should that be a sign that it is a distinct process?

Lexicalisation, as a term or as a concept, is clearly not as well known and studied as grammaticalisation. It appears that it is only more recently that it has become one of the stock items in textbooks on historical linguistics. Nevertheless, it does have a much longer history than that. It was used in Anttila’s textbook of historical and comparative linguistics in the early 1970s (see Anttila, 1972: 151, also quoted above). It was also used by Roman Jakobson (1896-1982) (1971 [1959]), in an article about one of Franz Boas’s (1858-1942) works (1938). Jakobson writes:

Besides those concepts which are grammaticalized and consequently obligatory in some languages but lexicalized and merely optional in others, Boas described certain relational categories [sic] compulsory all over the world: “the methods by means of which these *** relations are expressed vary very much but they are necessary elements of grammar.” (Jakobson, 1971 [1959]: 492)

Lehmann believes that this was when the “opposition between grammaticalization and lexicalization” was first “formulated” (Lehmann, 1982

[1995]: 6). But there are no signs that Jakobson was the first to use the term *lexicalisation*. There are no quotation marks and no definition. And grammaticalisation (even as a term) we know had existed for years, so the distinction and opposition could be much older. In addition, we should note that the distinction made here is **not** the same as that made in recent work on grammaticalisation. What Boas discussed, and Jakobson related, was the opposition between two **types** of expressions in language — lexical and grammatical expressions. He did not discuss the **development** of grammatical elements or lexical elements that developed from grammatical elements or univerbation or anything of that sort. It was a purely synchronic description of type.

Viktor Maksimovich Žirmunskij (1891-1971) (1961; 1966 [1961]) also used the term *lexicalisation* (Russian *leksikalisaziya*), however Lehmann correctly claims that this usage is different to Jakobson's and in fact more like Kuryłowicz's (1965 [1975]) use of the term (Lehmann, 1982 [1995]: 16-17). That meaning is also similar to Lehmann's third sense of lexicalisation, as discussed above (1989).

Word combination, in narrow sense of being more or less “bound up”, arises as a result of a closer grammatical or lexical unification of the group of words as the new meaning of the whole develops (grammatically or lexically) and becomes distinct from the aggregate meaning of its parts. Two trends are then possible:

(1) towards the grammaticalization [Russian *grammatisaziya*] (morphologization [Russian *morfologisaziya*]) of the word combination; that is to say, the group of words is transformed into a specific new analytical form of the word;

(2) towards the lexicalization [Russian *leksikalisaziya*] of the word combination; that is to say, the group of words is transformed into a more or less solid phraseological entity constituting a phraseological equivalent of the word in the semantic sense.

In either case, the end result of the process may possibly, though not necessarily, prove to be a unification of the word combination into a single (compound) word. (Žirmunskij, 1966 [1961]: 83)

Kuryłowicz's meaning is, however, wider than Žirmunskij's, it seems, in that it also includes changes from inflectional to derivational endings, e.g the development of a Latin inflectional neuter plural ending into a derivational ending to mark collectiveness (Kuryłowicz, 1965 [1975]: 52). Žirmunskij, as we can see in the

quote above, only used *lexicalisation* in the sense of a group of words that become a kind of set phrase, in other words a form of fossilisation or freezing.

Lehmann (2002: 2) has introduced the idea that we can have “[a]nalytic and holistic approaches” to words, and that therefore some expressions may have both lexicalised and grammaticalised senses:

Given an object of cognition of some complexity, the human mind has two ways of accessing it. The analytic approach consists in considering each part of the object and the contribution that it makes to the assemblage by its nature and function, and thus to arrive at a mental representation of the whole by applying rules of composition to its parts. The holistic approach is to directly grasp the whole without consideration of the parts. [...]

1. The relatively unfamiliar collocation *choose ... approach* is construed analytically, while the familiar *take ... approach* is construed holistically.
 2. The collocation *choose ... approach* could instead be accessed holistically, whereby the specific contribution of *choose* would essentially be foregone, and the whole would be largely synonymous with *take ... approach*; [...]
- (Lehmann, 2002: 2-3)

This seems somewhat related to Žirmunskij’s idea of lexicalisation, in that a change from analytic to holistic approaches to words may be similar to what he means by lexicalisation.

If our ‘tools’, i.e. in this case our terminology, are to be easily manageable and implemented, it would be practical, if we could at least say that all these uses of the term *lexicalisation* had a common denominator. But they do not. They can involve new lexical entries or they can involve the disappearance of one lexical item, merger of two lexical items, or creation of new lexical items from formerly grammatical items.

I wish that we could bring them together under one definition, but it appears that we cannot. Lehmann (2002) similarly has also recognised the difficulties due to the diversity of lexicalisation:

The purpose of this contribution is to clarify the concepts of ‘grammaticalization’ and ‘lexicalization’ in their mutual relationship (cf. Moreno Cabrera 1998). Such an explication cannot possibly justify all previous uses of these concepts, in particular not all those reported or endorsed in Lehmann 1989. (Lehmann, 2002: 2)

Still, we can hope that these kinds of treatments alert people to the disparate usage and the need to define the terms that we use, even when we have come across them in other people's work. This may in time lead to a standardisation and eventually a simplification of our work.

A final note in this section should be that even if someone claims that lexicalisation **can** be involved in degrammaticalisation, or grammaticalisation, that **does not** make it part of either. It makes it a possible parallel change. It may even be something which triggers one of the phenomena. This all depends on how one defines lexicalisation. This does not mean that lexicalisation is perfectly distinct from grammaticalisation, sometimes it may be hard to say which is taking place because the change in language might include both (under some interpretations of the term.) We also need to consider what it is that is lexicalised and/or grammaticalised. In a wider interpretation of grammaticalisation than in his previous work, Lehmann says (2002: 7), following the recent trend (though he refers to Meillet's (1915-1916 [1921]) comment regarding context) – that constructions, not elements, can be lexicalised or grammaticalised:

... one cannot properly say that a given element as such is either grammaticalized or lexicalized. Instead, it is the construction of which the element is a constituent which may embark on either course.

Linguist	Term and definition	Example
Roman Jakobson (1959)	<i>lexicalisation</i>	American Indian language: no grammatical device for number – hence also not obligatorily specified.
Viktor Žirmunskij (1966)	<i>lexicalisation</i> A group of words > set phrase. (<i>grammaticalisation</i>)	
Jerzy Kuryłowicz (1965)	<i>lexicalisation</i> The reversed process as compared to grammaticalisation	Latin inflectional neuter plural > derivational collective marker
Raimo Anttila (1972)	<i>lexicalisation</i> When a form no longer conforms to a productive rule of grammar.	<i>drive-in, bus, bil</i> ‘car’, <i>lord, hussy, nutmeat, sweatmeat, Holy Ghost, widow’s weeds, ism, ology, onomy, ocrasy, ade, itis.</i>
Christian Lehmann (1982 [1995])	<i>lexicalisation</i>	
Christian Lehmann (1989)	<i>lexicalisation</i> A process leading to new items in the lexicon, e.g. (1) from syntactic constructions reduced so that a lexeme becomes a derivation; (2) bound morphemes > submorphemic items > part of the root; or (3) fossilisation, freezing.	(1) <i>haidus</i> > <i>-heit</i> (derivational morpheme) (2) <i>Fluch-t</i> > <i>Flucht</i> (3) <i>koste es, was es wolle</i>
Christian Lehmann (2002)	<i>lexicalisation</i> Grammaticalisation and lexicalisation work in parallel. Lexicalisation can be the first step towards grammaticalisation.	<i>auf Grund von</i> > <i>aufgrund von</i>
Brian Joseph & Richard Janda (1988)	<i>demorphologisation</i>	OE <i>seld-um</i> > PDE <i>seldom</i> ³⁹
Joseph Greenberg (1991)	<i>lexicalisation</i> When something becomes part of the host morpheme (synchronic view), or when a morpheme is extended to more and more contexts. Can occur in the final stages of grammaticalisation.	numeral classifier > fossilised as part of a word, e.g. Kagaba <i>mai-gwa</i> ‘three’ < * <i>-kwa</i> ‘round object’ < e.g. ‘egg’
Paul Hopper (1990, 1994)	<i>phonogenesis</i> The creation of new “syntagmatic phonological segments” from old morphemes.	Eng. <i>seld</i> + dative plural (<i>-um</i>) > <i>seldom</i> Eng. <i>handiwork</i> < OE <i>handgeweorc</i> (incl. the Gmc prefix <i>ge-> i</i>) Germ. <i>ge-</i> + * <i>nug</i> > <i>genügen</i> ‘suffice’ > <i>vergnügen</i> ‘amuse’ ⁴⁰

³⁹ Example taken from Hopper and Traugott (1993: 164) since I did not have access to Joseph and Janda (1988) at the time of writing.

⁴⁰ “normally a stem may not take two unstressed prefixes” (Hopper, 1994: 33)

Paolo Ramat (1992)	<i>lexicalisation</i> The creation of new lexemes from old affixes for instance. Can be the result of degrammaticalisation.	-ism > ism
Paul Hopper & Elizabeth Traugott (1993)	<i>lexicalisation</i> cf. conversion and derivation <i>demorphologisation</i> When a linguistic form loses both form and function. <i>phonogenesis</i> When a linguistic form loses both form and function, and adds to the form of another morpheme / word.	<i>lexicalisation</i> : 'to up the ante', 'that was a downer', 'his uppers need dental work', German 'du' > 'duzen', 'scuba' (< acronym). <i>demorphologisation/phonogenesis</i> : 'seldom' < seld-um (dative suffix).
Elizabeth Traugott (1994)	<i>lexicalisation</i> 1. linguistic expression of a semantic property 2. inferential --> standard meaning 3. univerbation 4. phonological change --> lexical items (N.B. These are meanings that she recognises have been applied to the term.)	1. <i>have, be</i> POSSESSION, LOCATION, EXISTENCE, etc. 2. <i>be going to</i> inferred FUTURE > FUTURE auxiliary. 3. <i>tomorrow</i> prepositional phrase > monomorphemic word. 4. <i>sit - set, stink - stench</i> originating through i-mutation.
Juan Moreno Cabrera (1998)	<i>lexicalisation</i> The creation of lexemes from syntactic constructions. Opposite direction to grammaticalisation. Can follow grammaticalisation sometimes. Degrammaticalisation?	Latin <i>scribens</i> (participle used as adjective with a quality meaning) > <i>scribens</i> (noun with a person / agent meaning).
Anna Giacalone Ramat (1998)	<i>lexicalisation</i> The creation of new lexical items, e.g. univerbation. Loss of distinction between the stem and the affix. Can occur in the final stages of grammaticalisation.	It. <i>signore</i> : the Latin comparative -ior- > part of the root. Eng. <i>hlaf-weard</i> > <i>lord</i> : compounds > simple words. OHG <i>hiu tagu</i> > Modern German <i>heute</i> Idiomatic, frozen expressions.
Muriel Norde (1998, 2001, 2002)	<i>lexicalisation</i> Word formation. Not gradual! May involve grammatical items becoming lexical.	adverb > noun (<i>ups and downs</i>) adverb > verb (<i>up the price</i>) conjunction > noun (<i>ifs and buts</i>) pronoun > verb (Fr. <i>tu, toi</i> > <i>tutoyer</i> , Sw. <i>du</i> > <i>dua</i> , Germ. <i>du</i> > <i>duzen</i>)
Ilse Wischer (2000)	<i>lexicalisation</i> New lexical items or more lexical items (as compared to less lexical and more grammatical). Operates on a different level to grammaticalisation. Grammaticalisation can follow on from lexicalisation.	Germ. * <i>hiu tagu</i> > <i>heute</i>

Richard Janda (2001)	<i>(re)lexicalisation</i> Conversion into a more lexical category.	Eng. preposition > verb <i>off</i> Germ. preposition / particle and adverb > adjective <i>zu</i> (<i>eine zue Tür</i>)
Lauren Brinton (2002)	<i>lexicalisation</i> 1. new entry in the lexicon 2. no longer a productive grammatical rule 3. word formation 4. grammatical > lexical 5. “[s]yntactic construction > lexeme” (univerbation) 6. “bound morpheme > lexeme” 7. “Independent morphemes > monomorphemic form” 8. “Idiomatization” 9. Semanticization” (N.B. These are meanings that she recognises have been applied to the term.)	1. - 2. <i>house / hus-band</i> (split), <i>pickpocket</i> 3. derivation, compounding, conversion, back formation 4. particle > verb Eng. <i>up</i> , pronoun > verb Germ. <i>du</i> > <i>duzen</i> 5. Eng. <i>to + dæge</i> > <i>today</i> 6. suffix > independent word <i>ism, ade, ology, onomy, it is</i> 7. <i>lie / lay, foot / feet, *drank-jan</i> > <i>drench</i> 8. <i>blackmail, butterfly, unquiet</i> 9. -

Table 3.4.1: Linguists’ use of *lexicalisation*, *demorphologisation*, etc. with definitions.

3.4.2 Degrammaticalisation

Degrammaticalisation is another term which has been used as a label for counterexamples to the unidirectionality hypothesis of grammaticalisation. But as with *lexicalisation*, the term has been used and defined differently by different people, as may have become evident in the last section where degrammaticalisation was mentioned briefly because of its relation to lexicalisation. Once again we must try to find out what everyone means by the term, and what its relation to grammaticalisation (and lexicalisation) might be. It is hoped that this will enable us to clarify the concept. But as Norde says in her thesis: “it is hard to define what prototypical degrammaticalization is” (Norde, 1997: 229).

It has been suggested by Norde (2002) that part of the reason why many people have chosen to reject the possibility of degrammaticalisation, lies in its definition. This seems to be true but I think another part of the problem may lie in the definition of unidirectionality. Norde (2001a; 2002) has suggested that there are at least three common definitions of degrammaticalisation:

(Definition 1)

A “mirror image” of grammaticalisation (e.g. Bybee et al., 1994: 13; Lehmann, 1982 [1995]: 16-17)

(Definition 2)

“loss of grammatical meaning or function” (e.g. Heine et al., 1991[a]: 26; Koch, 1996: 241; Ramat, 1992: 551 ff)⁴¹

(Definition 3)

Some have also seen degrammaticalisation as (nearly) the same as “lexicalisation of grammatical items” (e.g., Ramat, 1992)⁴²

Norde herself is not happy with any of the three definitions she has found in other people’s work, since they do not keep lexicalisation, grammaticalisation and degrammaticalisation clearly apart. Instead she opts for a definition based on Hopper and Traugott’s “cline of grammaticality” (1993), even though this (in my eyes at least) may be criticised for the fact that it does not include derivations, which are instead part of a “cline of lexicality”. (Changes from inflection to derivation have been seen as counterexamples to the unidirectionality of grammaticalisation (cf. Kurylowicz, 1965 [1975]).) Norde chooses to use Hopper and Traugott’s cline because of how it treats clitics and affixes (p.c. 2001), noting however that other clines may be used and therefore, she says, there are other possibilities of what a degrammaticalisation process may look like (Norde, 2002: 47-48). It is surprising that she does not take the chance to formulate a new cline.

(Definition 4)

In general, degrammaticalization may be defined as the type of grammatical change which results in a shift from right to left on the cline of grammaticality [...] It should be noted however that different clines have been suggested in other works (e.g. one in which derivational suffixes are included as well), and hence there may be more kinds of degrammaticalization (Norde, 2002: 47-48, cf. 2001a)

As an example of how different the clines can be we should note that Harris and Campbell (1995) have a rather different cline to what we usually find in the grammaticalisation literature. On their cline, derivations occur further to the right

⁴¹ This is similar to what Hopper and Traugott call ‘demorphologisation’ (1993: 164-166): “loss of both the morphological function that a form once served and loss of the form itself (or absorption into the stem as a meaningless component)” (1993:164). Degrammaticalisation might be seen as a factor in demorphologisation.

⁴² Moreno Cabrera (1998) also means that lexicalisation has by some “been studied as degrammaticalization processes”, and he refers to Ramat 1992. This is discussed more in section 3.4.1 above.

than inflections, and they produce a counterexample of this movement which shows that derivational stem-forms may develop **into** inflectional affixes.

In most Germanic languages the neuter nouns that had ended in derivational –*os/-es* in Proto-Indo-European fell together with the neuter *a*-stems; this is an example of paradigm leveling. In Old High German this also occurred, and these nouns were declined in the singular according to the *a*- (strong) declension, though in the plural they retained their inherited stem-forming suffix, *-ir*. Occurring during the Old High German period only in the plural, the formant *-ir* was reanalyzed as a marker of the plural, an inflectional morpheme. (Harris and Campbell, 1995: 337-338)

This example could clearly be viewed as regrammaticalisation or exaptation (see section 3.4.3) and seems to me more naturally viewed as an example of that than of degrammaticalisation. Harris and Campbell clearly believe that grammaticalisation is not unidirectional, however they admit that there is a “general direction” and that reversals are “relatively few” (Harris and Campbell, 1995: 337-338).

Returning to the issue of there being different concepts of degrammaticalisation, as we can see above, Norde believes that people have not always restricted themselves to one of the kinds of definitions that she specifies. Sometimes they define the concept of degrammaticalisation with a combination of the different kinds of definitions cited above, e.g., Ramat (1992) in Norde’s eyes. But I think it would also be fair to clarify, even though it may seem obvious, that degrammaticalisation **usually** means a movement in the opposite direction to grammaticalisation, or something becoming less grammatical. Bybee et al (1994: 12-14), for instance, do not use the term *degrammaticalisation* at all, but Norde has interpreted them to speak of degrammaticalisation because they do discuss a “reverse direction”. They suppose that such a reverse direction is unknown in most cases of **semantic change** in connection with grammaticalisation and clearly much less common also in **phonological and grammatical changes**. This is interesting in that they believe that the semantics of a linguistic item are least likely to be reversed. But eventually, they also make it clear that they think that “grams do not **ordinarily** detach themselves” either, in other words it certainly is not only the semantic level that is unidirectional (Bybee et al., 1994: 13, emphasis mine). They admit that there are some examples which indicate that the reverse process to grammaticalisation is

possible, but most of them are reconstructions according to them, or have happened under “very special circumstances” (Bybee et al., 1994: 13-14).

Lehmann (1982 [1995]: 16-19) has a separate section for degrammaticalisation, which is a concept that he introduces in relation to a scale he has previously used for grammaticalisation (1982 [1995]: 13):

(isolating) > analytic > synthetic-agglutinating > synthetic-flexional > (zero)

He believes that this scale cannot run in the opposite direction, i.e. degrammaticalisation should never happen. Although, he recognises that people have indeed produced counterexamples, but he explains away most of them, e.g., by claiming that they are not exact mirror image reversals. Still, in some ways he seems open to the theoretical possibility that there might be reversals, even though he believes that they never occur in practice:

... it remains to be seen, [...] if rules for these processes [i.e. processes of grammaticalisation] can be found, why natural languages cannot apply them, at least to non-zero elements, in reverse direction. (Lehmann, 1982 [1995]: 19)

Ramat (1992) appears to have a slightly less linear view of change, since he emphasises the spiralic nature of grammaticalisation and degrammaticalisation. Both processes fit on the same spiral, a spiral caused by the “least-effort principle” and the “transparency principle”, aspects which may basically be seen as the principle of economy – economy for the speaker and the hearer respectively. Ramat then does not appear to view degrammaticalisation quite like a reversal or opposite to grammaticalisation, but more like a continuation or something that may alternate with grammaticalisation. He recognises that degrammaticalisation is indeed not as frequent as grammaticalisation. Nevertheless it certainly exists and he suggests that it is also “caused by a very basic principle of linguistic functions” (Ramat, 1992: 549). Looking at one of Lehmann’s definitions of grammaticalisation he tries to see what the reverse would entail, just like Lehmann previously did with Givón’s definition (cf. Lehmann, 1982 [1995]: 19). Is it a possible process and is it ever realised? He claims that with words like *bus*, *ism* and *ade* we do have a realisation of the opposite process, where formatives which he calls “grammatical” but “devoid of any

grammatical function” have acquired lexical status with “their own autonomous lexical meaning” (Ramat, 1992: 550). This leads him to conclude that “LEXICALIZATION IS THUS AN ASPECT OF DEGRAMMATICALIZATION” (1992: 550, emphasis original), by which he means that new lexical items may appear in the process of degrammaticalisation.

But Ramat’s examples of grammaticalisation might not be classed as such by everyone, because they involve the tricky category of derivational affixes, e.g. OE *dom* > *-dom*, OE *lic* > *-ly* (1992: 551). I presume that Hopper and Traugott (cf. 1993), for instance, might disagree. It is very difficult to know where derivations fit into people’s thoughts on grammaticalisation.

Furthermore, now that Ramat has concluded that (according to his definition) degrammaticalisation does exist, another question remains.

It may be that degrammaticalization is statistically insignificant when compared with the large number of grammaticalization processes (Heine et al. 1991[a]:4f, see also Joseph and Janda 1988:196), but its examples are by no means uninteresting, and not as scanty as one would prima facie incline to admit. The question we have to deal with is therefore, **why is it that grammaticalization and degrammaticalization coexist in natural languages?** (Ramat, 1992: 553, emphasis mine)

But perhaps before we ask that question, we should ask what makes us believe that degrammaticalisation is statistically insignificant? It is true indeed that we have not noted many degrammaticalisations compared to the number of grammaticalisations that we know of, but as Lass (2000) points out – we must consider what our sample is and how representative it is. It is quite unlikely that we ever obtain a good representative sample.

Haspelmath (1999, 2002) suggests that degrammaticalisation should be impossible, although he recognises that this is in fact not the case – there are some cases of, what he calls, degrammaticalisation, but these are “extremely restricted” (Haspelmath, 1999: 1043, 1046). He also tries to draw up some clearer guidelines for what is degrammaticalisation and what is not. For example, “metalinguistic uses” (*ifs*, *ands*, *buts*) are not to be seen as examples of degrammaticalisation (1999: 1046, fn). Notably, he also tries to explain why it is that these words cannot be counted as counterexamples, namely:

... because in grammaticalization the identity of the construction and the element's place within it are always preserved. (Haspelmath, 1999: 1046, fn 1.)

Norde agrees with Haspelmath (1999:1064), grammaticalisation does not affect the actual construction, everything remains in the same position. And therefore she assumes that degrammaticalisation should not affect “the identity of the construction” either (Norde, 2002). However, she criticises his *ifs, buts* example since quite reasonably she thinks that it contradicts his claim that we have no conscious access to items belonging to functional categories. But we should also consider whether it is actually true that grammaticalisation always preserves the element's place and the identity of the construction?⁴³ Even if this is true, should we not accept that the reverse process would not have to be quite like that? For one thing, whether the item keeps its place or not will depend on the word order restrictions at the time of the change.

Let us now have a brief look at some of the examples that have been suggested of degrammaticalisation. In an attempt to come to terms with grammaticalisation and its directionality, as well as with what degrammaticalisation would be if it existed, Norde (1997: 3; 1998: 212; 2001a: 235-236) notes a Dutch example. The Dutch suffix *-tig* has come to be used as an adverb, which not only involves a change from an affix to a full lexeme, but it also involves a difference in the pronunciation. The schwa in the affix has in the new lexeme changed from a schwa to a fuller vowel sound – as would be expected from a process which reversed grammaticalisation (1998: 212). So, it seems that even though Givón (1975) claims, quite reasonably, that phonological strengthening to a full vowel is something which cannot be predicted, it **can** happen.

A second example of a degrammaticalisation could be the Swedish *s*-genitive (affix > clitic), which has been thoroughly studied by Norde. She claims that:

... it is obvious that the *s*-genitive **etymologically** derives from a morphological genitive in the older Germanic languages, namely (in the case of Swedish) from the genitive singular masculine and neuter (*j*)*a*-stems. In this respect, the *s*-genitive demonstrates exactly the **reverse** of what is

⁴³ Lehmann (1982 [1995]: 159) speaks of a possibility that the word order may change, i.e. the position must change. He says that “the order in which the grammaticalized item is fixed in its construction differs from that order which was most natural when it was still a lexeme.”

predicted by the unidirectionality hypothesis, for it shows a transition from a more grammatical element – a case suffix – to a less grammatical one – a clitic. Consequently, at a first glance, *s*-genitives seem to be examples of **degrammaticalization**. (Norde, 1998: 217, emphasis original)

The English *s*-genitive has also often been discussed in relation to grammaticalisation and degrammaticalisation. Fischer and Rosenbach (2000: 29), for instance, speak of it as either the beginning of a new cycle, or as degrammaticalisation. Lehmann (1982 [1995]: 18-19) however, dismisses the possibility that the English *s*-genitive could be an example of degrammaticalisation, because he believes that since dialects had the alternative genitive construction “NP *his* NP”, which he says became homophonous with the case inflection, “the genitive suffix” could be reanalysed “as a clitic possessive pronoun” (Lehmann, 1982 [1995]: 19). Norde (2001a: 254-256) also discusses the possibility that the English *s*-genitive developed differently to the Swedish, having gone through an intermediary stage when it was common to use *his*, and by analogy also *her*, instead of the inflectional genitive, before it became a clitic. Campbell (2001: 129) says that Allen (1997) finally proved that the English genitive ending was not reanalysed so that it could occur separately, but rather that *his* was simply an orthographic variant. According to Norde (2001a: 255) Swedish is unlikely to have gone through a phase as in English with a possessive pronoun, even though it has also been suggested for Scandinavian languages by Janda (2001).

Part of Norde’s argument is built on trying to prove a change in grammaticality. As a measure of grammaticality Norde (1998: 217) uses first of all the “degree of fusion with the stem” which she says indicates that the *s*-genitive is less grammatical than the “morphological genitive”.

Inflectional suffixes (e.g. plural endings) may change the quality and/or quantity of the root to which they are attached [...] Using the degree of fusion as a criterion it can be made clear why the Modern Swedish *s*-genitive is not the same morpheme as the old case ending *-s*, which lives on in a few idiomatic expressions and in genitival compounds. (Norde, 1998: 217-218)

This appears to indicate that morphologically, and possibly phonologically also, degrammaticalisation would involve a reversal of the bonding process which Lehmann (1982 [1995]: 147-157) has suggested is involved in grammaticalisation.

When linguistic signs grammaticalise he says they go through “coalescence”, which brings them closer together. The degree of “bondedness” may vary from simple juxtaposition to merger. This seems to indicate a suggestion that degrammaticalisation involves a mirror image reversal. However, naturally a reversal does not have to be identical to the movements of the original process, just as if we were to decide to backtrack down a street we would not have to take exactly the same size steps, nor would we have to walk at the same pace, or place our feet at exactly the same spots.

Not all the processes and mechanisms indicate that the *s*-genitive has degrammaticalised. Although, we must remember that, we are not sure what has to take place for something to be grammaticalisation, we have not settled on any necessary or sufficient conditions, even though people have been looking for them (see e.g. Brinton and Stein, 1995). This makes it difficult to specify what degrammaticalisation should involve if it had to reverse every possible process that can be involved in grammaticalisation:

However, a serious impediment to the analysis of *s*-genitives as degrammaticalized forms is the fact that in some respects, *s*-genitives are **more** grammaticalized than morphological genitives. Firstly, the *s*-genitive exhibits the same kind of generalization as the RPP [= Resumptive Possessive Pronoun construction] *se* in Afrikaans, that is, in Modern English and Swedish *-s* is no longer confined to masculine and neuter stems singular. Secondly, its syntactic position has become fixed. (Norde, 1998: 218)

This generalisation could possibly be seen as continuing what Lehmann (1982 [1995]) calls “obligatorification” or what Hopper (1991) calls “specialization”. The choices disappear. Of course in this case we have always been quite restricted it may seem, since the choice was always made in relation to the gender and number of the head noun. But Lehmann (1982 [1995]: 138) still means that we can see it as though we have fewer choices if we lose some of the forms in the paradigm and that that would mean further grammaticalisation. And the fixation may be compared to what Lehmann (e.g. 1982 [1995]) has called “fixation” (of the “syntagmatic variability”). Another trademark, as Norde (1998) indicates, of grammaticalisation.

It is true that it might seem strange that something (which presumably is a form of degrammaticalisation) carries on as though it might have done during grammaticalisation, but that does not necessarily justify denying that this is

degrammaticalisation, because, as I said, we are not sure which is necessary and /or inherent in grammaticalisation.

Some have denied that the *s*-genitive is a genuine example of degrammaticalisation. Lass's (2000: 207) paper regarding the directionality of grammaticalisation finds degrammaticalisation quite rare and he claims that even the clear cases of degrammaticalisation are not counterexamples to unidirectional grammaticalisation. One example which he gives is the *s*-genitive. Unlike Campbell (2001) and Norde (1998; 2001a; 2002), Lass still claims that the *s*-genitive is problematic, since there is still not enough to go on in the presented evidence:

E.g. it is impossible, from the literature, to determine the status of well-known changes like the 'upgrading' of the Old English masculine/neuter *a*-stem genitive singular affix *-(e)s*, which derives from an Indo-European affix, into a clitic in modern English [...] (Lass, 2000: 224)

Haspelmath (2002, forth) similarly also still dismisses the Swedish example, as do Börjars, Eythórsson and Vincent (2002, 2003).

It is clear that the definitions of degrammaticalisation vary, as does the view of its relation to lexicalisation and grammaticalisation. But most people appear to agree that the term *degrammaticalisation* should be used for a movement in the opposite direction to grammaticalisation, even though they may define it differently and may not accept other's examples of degrammaticalisations as counterdirectional. All agree that if proper examples of degrammaticalisation are found, they would be counterexamples of the *Unidirectionality Hypothesis*. Unfortunately, we can see that now *degrammaticalisation* and *regrammaticalisation* have also started to get mixed up (for an example of this, see Fischer and Rosenbach, 2000: 6).

Linguist	Term and definition	Example
Christian Lehmann (1982 [1995])	<i>degrammaticalization</i>	Found no examples. (Dismisses several that have been suggested by others.)
Paolo Ramat (1992)	<i>degrammaticalization</i> This can either be a continuation of grammaticalisation or something that alternates with grammaticalisation.	<i>bus, ism, ade</i>
Joan Bybee et al (1994)	“reverse direction” semantic, phonological or grammatical changes in the opposite direction to grammaticalisation	Ir. 1 st person plural suffix – <i>mid/-muid</i> > 1 st person plural pronoun <i>muid</i>
Alice Harris & Lyle Campbell (1995)	reversal of grammaticalisation <i>degrammaticalized</i> <i>reanalysis</i>	Estonian – <i>pä</i> > <i>ep</i> (emphatic marker), – <i>s</i> > <i>es</i> (question particle) English <i>s</i> -genitive German derivational stemforming – <i>ir</i> > plural – <i>ir</i>
Muriel Norde (1997, 1998, 2001a, 2002)	<i>degrammaticalization</i> (1) mirror image of grammaticalisation (2) loss of grammatical meaning or function (3) approx. lexicalisation (4) grammatical change in the opposite direction on a cline of grammaticality	– <i>tig</i> : suffix > full lexeme (adverb) Swedish <i>s</i> -genitive: inflectional affix > clitic
Martin Haspelmath (1999, 2002, forth)	<i>degrammaticalization</i> The opposite of grammaticalisation.	not the Swedish <i>s</i> -genitive not metalinguistic uses (<i>ifs, ands, buts</i>)
Olga Fischer & Anette Rosenbach (2000)	<i>degrammaticalization</i>	Maybe English <i>s</i> -genitive
Roger Lass (2000)	<i>degrammaticalization</i>	not English <i>s</i> -genitive
Kersti Börjars, Thórhallur Eythórsson & Nigel Vincent (2002, 2003)	<i>degrammaticalization</i>	not Swedish <i>s</i> -genitive

Table 3.4.2: Linguists’ use of *degrammaticalisation*, with definitions.

3.4.3 *Regrammaticalisation, Functional Renewal, Exaptation ...*

As has been seen above, it is not always the case that elements that have lost their function and/or meaning disappear from the language (cf. e.g. demorphologisation and phonogenesis). Nor do the elements have to reverse themselves along a path leading back to the lexicon. *Regrammaticalisation* was used by Greenberg (1991) for the change whereby bleached linguistic elements are given a new function in the language, and usually this is a grammatical meaning, in fact in all known cases, according to Greenberg:

Another alternative is for it to expand its distribution, initially by semantically motivated extensions, but in an increasingly arbitrary way so that its meaning becomes highly disjunctive and even a prototypical definition cannot be readily formulated. [...] increasing extension leads to zero intension, so that the item has become desemanticized. There is, however, with expansive lexicalization a further possibility, namely its reinterpretation in a new function. In all cases known to me the new meaning is grammatical. We may call this process regrammaticalization. (Greenberg, 1991: 301)

The word itself had been used before Greenberg used it. Anttila uses it in his *Introduction to Historical and Comparative Linguistics* (1972), the earliest use of the term which I have come across:

In Indo-European languages noun stem-forming suffixes rarely have any clear meaning. But when inflectional endings are apocopated, their meaning sometimes is reassigned to the (originally meaningless) stem formant that remains. [...] These cases are just **regrammaticalizations**. (Anttila, 1972: 150, emphasis mine)⁴⁴

Obviously what Anttila has in mind is a similar, quite possibly the same, kind of change as that referred to by Greenberg (1991).

Greenberg also makes it clear that he wants to group together all the changes that result in grammatical elements:

⁴⁴ cf. section 3.4.2 above, where such a case is discussed by Harris and Campbell as *degrammaticalisation*

As with grammatical items, there is the possibility of interpretation of phonological items as having a grammatical significance, e.g. umlauting in German. This might be called grammaticalization from below. It is, compared to grammaticalization of lexical items, relatively infrequent and often a subsidiary method which accompanies others of the more common type. [...] ⁴⁵

In general I think it will be fruitful to consider within the same basic frame of grammaticalization, processes of development of grammatical elements from all sources. These will include the origin of grammatical elements from morphemes of more concrete meaning by semantic change, the grammaticalization of variants of phonological origin and the third major source, reanalysis with morpheme boundary shift. (Greenberg, 1991: 303)

Clearly, Greenberg is including in one category **all** of those changes which grammaticalisationists have had difficulty classifying according to the general definition of ‘change from lexical to grammatical, or to something more grammatical’. However, he does not suggest a term to use for all forms of development of grammatical elements, unless we should interpret it as though the superordinate term should be *grammaticalisation*, which seems quite possible from what Greenberg says in the quotation above.

Many other terms have been used as labels of changes which give linguistic elements new functions, e.g. *functional renewal*, *exaptation*, etc. Giacalone Ramat (1998: 109-111) mentions that research into this area of language change has been quite popular in recent years. She goes on to discuss some of the terms and examples, starting with Lass’s *exaptation*, but also mentioning *regrammaticalisation* and *functional renewal*. (The last seems to be the one she prefers).

Lass explains exaptation in the following manner:

Exaptation then is the opportunistic co-optation of a feature whose origin is unrelated or only marginally related to its later use. In other words (loosely) a ‘conceptual novelty’ or ‘invention’. (Lass, 1990: 80)

Say a language has a grammatical distinction of some sort, coded by means of morphology. Then say this distinction is jettisoned, PRIOR TO the loss of the morphological material that codes it. This morphology is now, functionally speaking, junk; [...] it can be kept, but instead of being relegated [...] it can be used for something else, perhaps just as systematic. (Lass, 1990: 81-82)

⁴⁵ cf. Cowie (1995) and Gaeta (1998) who call this *grammaticalisation*

He borrowed the term *exaptation* from evolutionary biology and Giacalone Ramat (1998) notes that it has been criticised by other linguists who have disapproved of the view that morphemes can be regarded as “linguistic junk” as Lass calls them.

Usually a morpheme is not suddenly left without a function, but goes through a period of more or less casual variation. Moreover, the process of functional emptying in most cases is not total, but only partial, [...] (Giacalone Ramat, 1998: 109)

This, however, is not what Lass means by ‘junk’. McMahon (1994: 338-340) tries to give us a better understanding of what Lass actually means, because Lass bases his ideas on a biological evolution theory which most linguists have little understanding of. These kinds of criticisms can also illustrate a tendency among linguists to not try quite hard enough to fully grasp the concept that has been borrowed.

Instead of linguistic junk, biologists and genealogists sometimes talk about “junk DNA”, and that is DNA which we **do not know** the function of, but it may still have a function. So there is no reason to criticise Lass on this point, he knew what he was talking about, but some others who have adopted his theory have taken him literally without learning more about the biological case scenario which he is basing his theory on. They appear to view it as though the element has no function, not that it is just without an obvious / known function.

Another linguist who has discussed this type of change is Vincent (1995). He uses Lass’s term *exaptation*, albeit, with some reservations. He is not happy with some parts of Lass’s theory. For example, he (like Giacalone Ramat (1998)) criticises Lass’s reference to non-functional, or empty morphemes, ‘junk’ as Lass calls them. He says that, as he himself showed in a study in 1992, “the morph(eme) in question is only partially empty, in the sense that some feature values are suspended while others are held constant” (Vincent, 1995: 436). However, apart from this being a misinterpretation of Lass, as noted above, it would be wrong only to criticise Lass and *exaptation* in this respect, because it is quite clear that Brinton and Stein (1995) view this as a prerequisite to *functional renewal*:

A prerequisite for functional renewal is that an older form be freed of its former meaning, becoming available for the acquisition of new meaning; this

may be achieved through grammaticalization or fixing of word order, for example. (Brinton and Stein, 1995: 34)

Also Heine, et al. (1991a: 4) who do not believe that there is a significant number of regrammaticalisations or degrammaticalisations, give as a definition of regrammaticalisation: “when forms **without any function** acquire a grammatical function” (emphasis mine), and thereby they also clearly recognise the theoretical possibility of such changes at least.

Quite like other fields which are related to grammaticalisation, there has been some disagreement over the examples of regrammaticalisation, exaptation and functional renewal. Giacalone Ramat (1998: 109-110), for instance, criticises one of Lass’s examples of exaptation, but we can also see that there are cases which might be considered regrammaticalisation by some, but which might be seen as grammaticalisation by others, for discussion see section 3.5 below.

Brinton and Stein (1995: 33) look at functions from a diachronic perspective and claim that functions change over time, even though, they say, most work on syntactic change has viewed it as a “formal renewal” without any effect on the semantics of the structure. They show in their paper that in the history of English there have been constructions which have fallen out of use and then been reintroduced but with new functions, to some extent at least. This is what they mean by *functional renewal*, it “refers to the retention or revival of an existing syntactic form with **a new or renewed function**” (Brinton and Stein, 1995: 34, emphasis mine).

Giacalone Ramat (1998: 112) criticises Brinton and Stein (1995) for having mistakenly suggested that *exaptation* and *functional renewal* might refer to exactly the same phenomenon (cf. Brinton and Stein, 1995: 44f). However, that is to twist what they say quite considerably. Brinton and Stein do suggest that this could be the case, but are certainly not claiming that it is definitely so, in fact they are requesting further work in the area.

Further empirical studies, and new analyses of classic cases are also necessary to sharpen the theoretical notions of both functional renewal and exaptation, and to determine whether there is an exact logical relationship between the two or whether it is simply a case of two labels for the same phenomenon, as it seems at this stage of discussion – after all, both are about form coming before function. (Brinton and Stein, 1995: 44-45)

The reason that Giacalone Ramat claims that exaptation and functional renewal are quite different is primarily that functional renewal refers to cases where an old function is **revived** after a time of having been forgotten, whereas Lass (1990: 82) looks at “genuine ‘novelt[ies]’” (Giacalone Ramat, 1998: 112), **new** meanings attached to **old** forms. However, Brinton and Stein also include novelties in their conceptual group of changes, which, they say, “refers to the retention or revival of an existing syntactic form with **a new or renewed function**” (Brinton and Stein, 1995: 34, emphasis mine). It is however correct that Lass does not include renewals of old functions in exaptation.

Nevertheless, Brinton and Stein seem to be uncertain of what exaptation means. Considering that they are themselves working on similar changes, possibly the same kind of changes, they should have studied the theory in enough depth to have good reasons for the coining of a different term than that already in use, before coining *functional renewal*. They must have some ideas which they find reasonably different from what they think Lass means. One reason could be that mentioned by Giacalone Ramat (1998: 112), namely that Lass’s exaptation seems to involve only “genuine ‘novelty’”, whereas Brinton and Stein’s category also includes renewals of older functions. They also say quite early that they will be looking at a **particular kind of** exaptation, which might confuse the reader even further.

Functional renewal, as discussed in this paper, then, is the exaptation of (surface) syntactic forms and processes. (Brinton and Stein, 1995: 34)

This seems to indicate that the difference, in their eyes, does not lie in the novelty or ‘non-novelty’ of the ‘new’ function. It seems it is more a matter of wanting to distinguish between changes on the syntactic level and Lass’s type of exaptation which can include everything from changes mainly in the morphology to changes in the syntax, and including also the ‘recycling’ of phonological changes in morphological functions.

Apart from discussing whether they include innovative functions or not, we could also ask what the actual difference is between genuinely new functions and renewed functions? Formalists would probably say nothing, because, if we go one step further, we must admit that there is a problem in explaining how today’s

children could possibly know what was common language practice several, or even just two, generations ago. If they cannot know that, then it would indicate that even what diachronically appears to be a renewal may be a novelty, an innovation. However, there is also of course the possibility that something which has disappeared from some/most dialects, or at least from the standard, may have remained in other dialects. It could then perhaps be ‘borrowed’ from there back into a dialect which had lost it for a while.

It is of great interest to the research community that Brinton and Stein also spend some time on possible prerequisites to functional renewals, what may cause it, and they also suggest that we should look at the possibility that there may be “necessary (structural?, internal?) and sufficient (external?, varietal?) conditions triggering this type of change” (Brinton and Stein, 1995: 45), which I fully agree with. Unfortunately, they have not yet found any definite triggers, so hopefully others as well as themselves will persevere.

Vincent (1995) also reflects on criticisms of Lass’s exaptation from another point of view, than on the matter of elements without any function which was mentioned earlier, namely that of language acquisition. But this is not a terribly productive criticism of Lass (1990). Even if we one day find out for certain that it is wrong to view language change as something which can happen rather continuously over time, and not just at acquisition, we could still use exaptation as a heuristic, as Vincent (1995: 437) perhaps also recognises (see below).

... exaptation cannot be a valid process of change since it labels a correspondence between two stages of a language’s development and is not therefore a phenomenon that could be encompassed in the mind of a single speaker, which is the only proper locus of linguistic change (Andersen, 1973). (Vincent, 1995: 436)

Apparently, Vincent does not see this as quite as problematic as Andersen (1973), instead he stresses the gradualness of change:

In practice, the transition between generations will not be instantaneous, and there will be a complex relation between acquisitionally induced change and its survival and diffusion through a speech community. Elucidating the role of exaptation – if only as a contributor of broken parts to the child’s and society’s constructive rebuilding of a system – seems in these circumstances,

to the present writer at least, to be a valuable epistemological contribution and one whose role should be explored further. (Vincent, 1995: 437)

Notably, Vincent chooses to coin a ‘new’ term, namely “*re-grammaticalization*” (italics mine) which he defines as: “the assignment of new morphosyntactic functions to elements which are already centrally part of the grammar, and typically part of the paradigmatic core of the morphological system” (1995: 438). But, as we have seen, the term had already been used by Greenberg in 1991, and by Anttila as early as 1972. Although it seems Vincent (1995) has a more narrow definition of the concept than Greenberg. Furthermore, after coining the new term Vincent never uses it again (in that article, at least), but instead carries on using the term *exaptation*.

Oddly enough, talking about the coining of the ‘new’ term *regrammaticalisation*, there are other people who have written for the same volume as the one in which we find Vincent’s paper (Vincent, 1995) who also use the term *regrammaticalisation*. Allen (1995: 1), in his paper about the *Regrammaticalization and Degrammaticalization of the Inchoative Suffix*, states as one of his aims that he wants to:

... show that grammaticalization is not unidirectional. In language, what is done can be undone and redone, so that a language can undergo not only grammaticalization but also regrammaticalization, or change in grammatical function, and degrammaticalization, or loss of grammatical role.

So, in the case of Allen (1995), regrammaticalisation does not necessarily entail former loss of function. However, it always includes an element acquiring a ‘new’ function (either one it has had, lost and regained or a completely new one).

Unlike Norde (2001a; 2002) and Plank (1995), Allen (1995: 7) sees a naturalness in regrammaticalisation and degrammaticalisation, because he sees no major differences between these kinds of changes and grammaticalisation. Still, it has to be admitted that regrammaticalisation, exaptation or functional renewal, is / are relatively rare, as far as we know.

Linguist	Term and definition	Example
Raimo Anttila (1972)	<i>regrammaticalization</i>	stem forming suffix > plural
Roger Lass (1990)	<i>exaptation</i> New use of a functional element	IE Ablaut: aspect (Perfect / Aorist) marker > Gmc number marker (> loss of number coding in many languages) Dutch adjectives $-\emptyset / -e$ (gender, determiner) (> early Afrikaans random usage) > Afrikaans $-e$ (attributive polymorphemic adjectives, unless comparative & adjectives with stem allomorphs). ⁴⁶ Germanic umlaut > plural marker
Joseph Greenberg (1991)	<i>regrammaticalization</i>	German neuter plural $-er$ < stem formative Definite article > nominative marker > Nominaliser (derivational) Romance $-sc-$ Derivation > Inflection
Bernd Heine et al (1991a)	<i>regrammaticalization</i> When forms without any function are given a function.	-
Andrew Allen (1995)	<i>regrammaticalization</i> A change in grammatical function.	inchoative meaning > durative / frequentative (Fr. <i>commencier</i> + INF.)
Laurel Brinton & Dieter Stein (1995)	<i>functional renewal</i> When a linguistic item without function is given a new or renewed function.	Perfect / resultative > resultative with subjective focus (Eng. <i>have</i> + OBJ. + PP) Eng. <i>do</i> Epistemic > Discourse functions (e.g. introducing topics, giving illustrative examples)
Nigel Vincent (1995)	<i>exaptation / re-grammaticalization</i> When a grammatical item is given a new morphosyntactic function.	Latin accusative plural suffix > Spanish plural suffix (no case) Latin nominative plural suffix > Italian plural suffix (no case)
Anna Giacalone Ramat (1998)	Discusses and compares: <i>exaptation</i> <i>regrammaticalization</i> <i>functional renewal</i>	not Lass's example of ablaut (aspect > number) Latin inchoative (derivative) suffix $-\bar{e}sc-/isc-$ > Rom. present (inflectional) person marker

Table 3.4.3: Linguists' use of *regrammaticalisation*, *exaptation* and *functional renewal* and their definitions of these concepts.

⁴⁶ Lass (1990: 95) sees this as a change from syntactically triggered inflection to lexically, internally triggered inflection.

3.5 An Informative, or Confusing, Look at Some Examples

The clearest sign that a term represents a fuzzy concept, must be if examples can be said to be representative of that phenomenon by some people, while others claim that they are representative of a phenomenon which (at least sometimes) is defined as the opposite of the first (cf. Lindström, forth.).

This can involve two kinds of examples. It can be a matter of exactly the same examples, which in the case of grammaticalisation would mean precisely the same morpheme developing into the same function and form (*the Example Confusion*). It can also mean that the examples, although not exactly the same, clearly represent the same process, but, that that process is called different things by different writers (*the Process Confusion*). I shall also include some examples where different terminology, but terms which **all** may be used to mean the same, have been employed.

3.5.1 *The Example Confusion*

3.5.1.1 Hiu tagu > ... > heute

One of the examples Meillet includes in his article, which presumably introduced the term *grammaticalisation* into linguistics (Meillet 1912, 1921), is the change whereby OHG *hiu tagu* (=this day) becomes MHG *hiutu* (=today) and later PDG *heute* (=today). Hopper and Traugott's introduction to grammaticalisation (1993) includes a criticism of this particular example. They think this is an example of a new lexeme, which one can easily take as implying that they mean that it is the result of lexicalisation.

It is, first of all, a little startling to find a change of this kind discussed under the rubric of grammaticalization, since it seems more appropriately thought of as illustrating the emergence of a new lexical item rather than of a grammatical formative. (Hopper and Traugott, 1993:23, cf. 2003: 24)

However, this particular example does not agree with their examples of lexicalisation either, which is exemplified by conversions from preposition to verb. So it seems they are refusing to categorise it at all.

Lehmann (2002), however sheds some light on how we can deal with the possibility that this may be grammaticalisation while at the same time being an example of something which produces a new lexical item. In similar cases, he believes that lexicalisation may occur first and function almost as a prerequisite for grammaticalisation. This, however, seems directly opposed to what Giacalone Ramat says. She (1998: 121-122) proposes that we should see this as a process of lexicalisation, but lexicalisation in **“the final stages of grammaticalization”** (1998: 121, emphasis mine). Clearly then lexicalisation cannot be the reverse of grammaticalisation, according to Lehmann and Giacalone Ramat. Instead, in this case lexicalisation means a form of univerbation (Giacalone Ramat, 1998: 122; Lehmann, 2002: 1). However, Lehmann (1982 [1995]: 151-152) distinguishes between different kinds of univerbation. When univerbation is a sign of *bonding* (which is something that is also involved in grammaticalisation according to him), it must involve at least one grammatical formative. What does that tell us about this example? Does the fact that one of the words in the original construction was a demonstrative pronoun make it grammatical univerbation?

Wischer (2000: 359) also discusses this example and uses it as evidence that Meillet was confused about the two processes, grammaticalisation and lexicalisation. This does not seem quite fair, since lexicalisation does not seem to have been a distinct concept at the time, or at least not to Meillet who does not discuss anything similar in any of his papers on grammaticalisation at least (1912; 1915-1916 [1921]; 1926).

Wischer recognises three forms of lexicalisation – phonological, morphological and semantic in this particular example, which she believes “have led to a complete demotivation and thus turned the former free syntactic unit into a lexical item” (Wischer, 2000: 359). But later she concludes that (some of) the processes involved in grammaticalisation and lexicalisation are very similar and that this particular element is difficult to place into one of the categories.

... it must be admitted that the new ‘lexical’ unit belongs to a rather closed class of adverbial, which opens up the question whether it has really become

an element of the lexicon or indeed a grammatical item. (Wischer, 2000: 359-360)

3.5.1.2 Haidus > -heit

One of the examples that Lehmann (1989: 12f.) uses to illustrate one kind of lexicalisation has been criticised by Ramat (1992: 558), viz. *haidus* > *-heit*. Ramat says that he cannot understand why Lehmann says this is lexicalisation when he himself finds it such a “fine example of grammaticalization”.

In Lehmann’s eyes this is however quite clearly a case of lexicalisation, because it results in an item which enters the lexicon:

... daß einschlägige Phänomene den Übergang sprachlicher Einheiten ins Lexikon illustrieren sollten. [...] Zum ersten können syntaktische Konstruktionen derart reduziert werden, daß ein ehemaliges Lexem zum Derivationsaffix wird [...]

B6. ahd. *haidus* > mhd. *-heit*
AHD Gestalt ABSTR.

[...]

B6 erinnert an die Entstehung von Derivationsuffixen wie dem in *Dummheit* aus ehemaligen Substantiven. (Lehmann 1989:12)⁴⁷

At a first glance it may seem as though Hopper and Traugott (1993) might have agreed with Lehmann on this example, since after all it is a change which follows their cline of lexicality and not their cline of grammaticality.⁴⁸ Although, on the only occasion they deal more thoroughly with an example of this kind (*mente* > *-ment(e)* (Romance adverbial suffix) discussed below), they say that it is a “straightforward instance of grammaticalization” (Hopper and Traugott, 1993: 131), and throughout most of their book they refrain from dealing with derivational affixes.

⁴⁷ Translation: ...that relevant phenomena shall illustrate the transfer of linguistic units into the lexicon. [...] Firstly, syntactic constructions can be reduced so that a former lexeme becomes a derivational affix [...]

B6. OHG *haidus* > MHG *-heit*
OHG Gestalt ABSTRACT.

[...]

B6. reflects the development of derivational suffixes as that in *Dummheit* from former nouns.

⁴⁸ *Cline of grammaticality* = “content item > grammatical word > clitic > inflectional affix”

Cline of lexicality = “a basket *full* (of eggs...) > a cup*ful* (of water) > hope*ful*”
(Hopper and Traugott 1993:7)

However, *-mente* does of course form an adverb rather than a noun (as *-heit* does) and adverbs are in some ways ‘more’ grammatical than nouns.

According to Ramat this is definitely grammaticalisation, because it is a noun acquiring affix value (cf. Ramat, 1992: 550), and can be compared to other cases such as *-dom* in *kingdom* and *-ly* in *certainly*. And rather paradoxically he backs this up with a quote from Lehmann:

Grammaticalization is a process leading from lexemes to grammatical formatives [...] A sign is grammaticalized to the extent that it is devoid of concrete lexical meaning and takes part in obligatory grammatical rules (Lehmann, 1982: vi, cited in Ramat 1992: 550)

A question which may partly lie behind the problem here is the underlying issue of what is grammatical and what is lexical?

3.5.1.3 -ade > ade

Ramat (1992) in a confusing way classes the change from *-ade* in ‘lemonade’ to *ade* as in *gatorade*, as three types of change; substantivisation, lexicalisation and degrammaticalisation, without making it clear how they are related.

... *bus*, *ism*, *ade* are grammatical formatives that were devoid of any grammatical function, that is, separated from their grammatical rules [...], and acquired concrete lexeme status with their own autonomous lexical meaning. LEXICALIZATION IS THUS AN ASPECT OF DEGRAMMATICALIZATION – or more exactly: degrammaticalization processes may lead to new lexemes. In fact lexicalization has to be seen as a process whereby linguistic signs formed by rules of grammar are no longer perceived (parsed) in this way but simply as lexical entries. Such lexicalized formants represent the exact converse of the already quoted cases of *-dom* [...] or *-ly* (Ramat, 1992: 550-551)

Giocalone Ramat (1998: 115) only tells us that Ramat calls this *degrammaticalisation*, which the reader of course assumes is his only term for it then. She also notes that Hopper and Traugott (1993) call it *lexicalisation*, though as noted above their only examples of lexicalisation are conversions, e.g., from preposition to verbs (cf. Hopper and Traugott, 1993: 127). But Giocalone Ramat is probably right in that they may then also call this kind of change lexicalisation, although the processes are quite different, so we cannot know for sure.

It is interesting to note that even though Giacalone Ramat (1998) discusses how others would classify these kinds of changes, she does not seem to want to commit herself to one interpretation. She only says that it is a change “from grammar to the lexicon” (Giacalone Ramat, 1998: 115).

In fact, all of Ramat’s three examples had already been included in Anttila (1972), as examples of lexicalisation. Thereby they clearly illustrate how the same examples have been recycled by different people over and over again. And it is possible that it is through influence from Anttila that Ramat chooses to call these *lexicalisation* even though he also wants to use the term *degrammaticalisation*.

One form of lexicalization is particularly clear in English, where many common suffixes or end parts of Greek-based compounds have become independent words (as in *bus*). Although this is not general, one can note the following cases, some of them rather literary or technical: *ism*, *ology*, *onomy*, *ocrasy*, *ade* (lemonade), *itis* (bronchitis), and also, from native materials, *teen* (teen-ager). Other languages also show this phenomenon, but we shall leave the subject with these examples. (Anttila, 1972: 151)

To sum up, we conclude that this may be an example of lexicalisation and/or degrammaticalisation, or possibly something else – either way it is still definitely a change “from grammar to the lexicon”, as Giacalone Ramat (1998) says.

3.5.1.4 *mente* > Adverb formative

Giacalone Ramat (1998: 120) has another slightly confusing section of citations related to lexicalisations where she discusses the adverb formative *-ment* in Romance languages. She says that Anttila (1989: 151) has noted that the change from an inflected noun to an adverb is a form of lexicalisation, while Hopper and Traugott (1993: 131f.) are correctly said to consider the Romance adverbial formation “a straightforward instance of grammaticalization: a new grammatical formative has come into existence out of a formerly autonomous word”.

Perhaps part of the confusion in this case can be explained by something else that Hopper and Traugott mention (1993: 135), namely that *-ment(e)* can be seen as either a derivation or an inflection. As I have mentioned before, people working on grammaticalisation have had problems deciding what to do with derivations, and in a

case such as this when they are not sure whether to class the affix as an inflection or a derivation, the problems can only get bigger.

It is true that Anttila (1972; 1989) says that noun > adverb is a form of lexicalisation (a process which he says is “quite parallel to the grammaticalization of the emphatic attributes of the French negative” (Anttila, 1972: 150)), and Hopper and Traugott say that noun > adverb formative is a case of grammaticalisation. But why put these two statements together as though they were opposed to each other, one calling it *lexicalisation* and the other calling it *grammaticalisation*? They are not opposites. Hopper and Traugott (1993) might agree with Anttila (1972; 1989) that noun > adverb would be a case of lexicalisation, but probably they would not since an adverb is usually seen as a more grammatical category than noun. It would probably be grammaticalisation, but it would also be a form of conversion. And they have seen conversions as lexicalisation on other occasions (see Hopper and Traugott, 1993: 127). In addition, it is worthy of note that *-mente* is not an adverb in itself but an adverb formative, as Hopper and Traugott say. It is therefore not clear whether Anttila’s statement should be applied to *-mente*.

Giocalone Ramat (1998) herself chooses to look at the same example in the same way as Hopper and Traugott (1993). And, as a possible indication that she may have misinterpreted Anttila (1972; 1989), she states that “[I]t is certainly not appropriate to take *-mente* as a lexical device” (1998: 120), although it is not clear what she means by this.

3.5.1.5 Latin *-esc-/-isc-* (inchoative) > Romance languages (‘meaningless’ part of the inflection of some verbs)

This is yet another of those examples which keeps reappearing, almost like the example of the French negative. But, unlike the example of the French negation, this of course is an example of a reverse process, that is if we look at the change whereby the inchoative derivation *-esc-/-isc-* was incorporated into the inflectional paradigm, as part of some present tense inflections. (And not at other parts of the history of this morpheme which have, according to Allen (1995), gone through grammaticalisation, regrammaticalisation and degrammaticalisation.)

The change has been classed as degrammaticalisation, lexicalisation and also exaptation. Ramat (1992: 551-552) calls it lexicalisation – but also

degrammaticalisation. Lexicalisation because: “Whenever a linguistic form drifts outside the productive rules of grammar, it becomes lexicalized (see Anttila, 1989: 151)” (Ramat, 1992: 552).

Allen (1995) refers to Ramat (1992), at the beginning of his article, as a good source of inspiration. He also appears to adopt Ramat’s treatment of this as being **both** degrammaticalisation and lexicalisation.

If a **morpheme entirely loses its grammatical function**, then degrammaticalization occurs. [...] The suffix which once signified the inchoative is now a marker of the singular or third plural forms in the present indicative active. “The derivational suffix has thus been incorporated ... into the inflectional system” and forms a bridge between earlier syntax and a later development in the lexicon (Ramat, 1992: 552). That is, **degrammaticalization is one way that morphemes become lexicalized.** (Allen, 1995: 5-6. emphasis mine)

Giacalone Ramat (1998) also cites Ramat (1992), among others. She claims that he treats this as lexicalisation, not mentioning the fact that it is also degrammaticalisation according to him. This is interesting to note in comparison to what she says about *ade* (see above, section 3.5.1.3), which she claims that Ramat viewed as degrammaticalisation (with no mention of him also seeing it as lexicalisation). What possible reason could she have to treat the cases differently? It could be that she herself wants to keep lexicalisation and degrammaticalisation distinct. But we must note that there does not seem to be any relation to her own classification, or analysis of this change. Maybe it was simply a slip of the finger?

The way Giacalone Ramat sees it, this morpheme “was desemantized and re-employed in a new function as a person marker” (Giacalone Ramat, 1998: 110-111). Putting so much stress on the fact that the morpheme still has some form of function, leads her to conclude that this change could be called exaptation (1998: 111):

The affix seems to have developed some marginal (phonological) function different from its original inchoative value and to have not yet reached the stage of phonogenesis (Giacalone Ramat, 1998: 111)

Others do not appear to agree that the old inchoative morpheme actually fills a function as a “person marker”. Instead they focus on the stress fixing function,

which Giacalone Ramat also alludes to in the inclusion of “phonological” in the quotation above, but also in stating that it “allows to fix the stress for the whole paradigm in a position after the verb stem” (1998: 111). However, perhaps that too can be viewed as a form of exaptation? One person who clearly thinks that this form now mainly functions as a ‘stress fixer’ is Rudes (1980: 343), who has studied its development into a new function, though he does not label this change.

From having concluded that this is a case of exaptation, Giacalone Ramat goes on to mention that Greenberg would call it *regrammaticalisation* (Giacalone Ramat, 1998: 111) if a grammatical element was given a new grammatical function. She also notes that he mentions the case of the Latin inchoative in his article. However, he does not appear to mean it as a case of regrammaticalisation. All that Greenberg actually says is that the change whereby a derivation becomes part of an inflectional system is similar, and parallel, to lexicalisation, which could make it also parallel to regrammaticalisation since that is a possible continuation from expansive lexicalisation (see above section 3.4.3) (Greenberg, 1991: 311).

3.5.2. *The Process Confusion*

3.5.2.1 Senior(em) > Sp. Señor, It. Signore, Fr. Seigneur

Ramat presents a few more examples of what he would call lexicalisation, e.g. a Latin comparative which in the development of the Romance languages lost its “grammatical status”.

Lat. *senior(em)* > Fr. *seigneur*, It. *signore* (1992:551)

However, the process in this case seems remarkably similar to Hopper and Traugott’s demorphologisation process, which can be part of grammaticalisation (Hopper and Traugott, 1993: 164-166). Giacalone Ramat (1998: 121) mentions that Lazzeroni (forth) has suggested that this could be a case of phonogenesis. However, she does not seem quite sure whether she agrees with Lazzeroni, because after all it is not just a matter of a “meaningless phonological sequence” but it is a whole “new lexeme”. She also uses this as an example of one type of lexicalisation in the final stages of grammaticalisation (1998: 121).

3.5.2.2 Suffix, submorphemic unit or transmorphemic unit > derivations or inflections

Many examples of lexicalisation have dealt with suffixes which have become new autonomous lexemes, or even submorphemic or transmorphemic units that have become productive derivations. Norde (2001a), Anttila (1972) and Ramat (1992) deal with these kinds of examples as lexicalisation, but Cowie (1995: 188) suggests in her review of Hopper and Traugott (1993) that at least some of these changes could be grammaticalisation. Her examples are *-a(o)holic*, *-burger*, “transmorphemic” units, which have found new meanings and become productive. I cannot see why these should be different to Ramat’s (1992: 550) examples of *-gate*, *(-)ade*, which he classes as lexicalisation. But nor can I see any reason why they should need to be distinguished from cases such as *-dom*, *-ly*, which Ramat (1992) calls *grammaticalisation*, even though the direction of the change is different.

Cowie (1995) considers the possibility of including some of Hopper and Traugott’s counterexamples of the unidirectionality of grammaticalisation in the category of grammaticalisation.

... there is the morphologization of phonological *processes*. Hopper and Traugott give the development of umlaut plurals in Old English as an example: “*foot-feet*’ is the modern reflex of an earlier stage when the plural was *fo*t*-i*; phonetically the *o* was fronted before the *-i* and when the *-i* plural marker was lost for phonological reasons, the fronted vowel remained as the marker of plurality.” ([Hopper and Traugott, 1993] p. 127) (Cowie, 1995: 188)

Simply, Gaeta (1998) recognises that umlaut and other phonological changes, even in wider syntactic contexts than on the word level, can be grammaticalised. Like Cowie, one of his examples is of how changes based on i-umlaut have been reanalysed and grammaticalised. Lass (1990: 98-99) also mentions this type of change, which he calls a *morphologisation*. He seems to see similarities to exaptation, but he does not quite want to call it that.

With phonologization came the possibility of morphologization: umlaut could be interpreted as an (opaque) mode of plural formation, with no phonological conditioning. [...]

Umlaut plurals are a slightly peripheral example, since they are not functionless or totally idle; ... (Lass, 1990: 98-99)

We could possibly also compare this to what Brinton and Stein (1995) say about there being two forms of functional renewal – one that involves a completely new function, and one that involves the return of an older function. This probably counting as a ‘new function’ even though it could be said to have been part of the meaning before albeit only by historical accident and inference.

This kind of change has been referred to by others as “*grammaticalization from below*” (e.g. Greenberg, 1991 italics mine), or classed as some form of regrammaticalisation. Lass, as we could see above, only sees it as a peripheral case of exaptation since he believes it could still be seen to have some function and since exaptation usually involves items that we **think** have no function. However, he also speaks of a similar example which he sees as a more prototypical kind of exaptation.

... PRES in classes I-III reconstructs with nuclear */e/, PRET₁ with nuclear */o/, and PRET₂ with zero; i.e. they reflect the old present/perfect/aorist alternation with some precision. The problem is that the PRET₁ ~ PRET₂ alternation does not correlate with tense (the reflex of IE aspect), but with number. Yet the PRET₂ zero-grade is on the face of it unlikely to reflect anything but an aorist. (Lass, 1990: 85)

But Giacalone Ramat (1998: 109-110) believes that Lass has got his facts wrong and prefers to treat this as an extension of an already existing pattern, something which in Lass’s own words cannot be exaptation:

There is no genuine ‘novelty’, only extension of use, within the same semantic domain. (Lass, 1990: 82)

3.5.2.3 Conversion from a lexical category to a grammatical category

One final type of example could be seen as a simple case of conversion or zero derivation, but it would be seen as lexicalisation by some linguists and as grammaticalisation by others. In this case it depends on how one views the actual change. In order to judge which is right or wrong we need to understand a lot about the history of the particular language where this has occurred, and understand the

language at different periods. But, naturally, we also need to have a clear understanding of what we mean by the terms.

The kind of change I am talking about is when, for instance, a pronoun becomes a conjunction. Anttila (1972: 151) called this *lexicalisation*, whereas Giacalone Ramat (1998: 120) calls it *grammaticalisation*. However, Giacalone Ramat (1998) does not think that the different interpretations are due to different analyses, but simply thinks Anttila's definition of grammaticalisation is too narrow.

In my view, such development lends itself to treatment as increased grammaticalization of already grammatical items which serve to express relations between clauses (Hopper and Traugott, 1993). The problem with Anttila's suggestion is that he seems to reject the possibility that the canonical cline:

lexical item > clitic > affix

is not completed, but only parts of it undergo evolution [...] (Giacalone Ramat, 1998: 120)

This is of course one of the times when it is quite clear that the controversy here is partly imagined. It is partly a matter of terminology and definitions.

3.6 Uniformitarianism

Uniformitarianism is related to any diachronic treatment of language. A brief explanation of what it is (or may be) and what it may be used for in linguistics is presented by McMahon:

Findings may then be generalised from changes in progress to completed changes, provided that we accept the Uniformitarian Principle, '... the claim that the same mechanisms which operated to produce the large-scale changes of the past may be observed in the current changes taking place around us' (Labov, 1972: 161; [Labov, 1972 [1978]]) (McMahon, 1994: 233)

There is more than one reason why it is important to consider uniformitarianism in connection with grammaticalisation, and unidirectionality in particular. First of all it has bearings on how we understand a unidirectionality hypothesis and what this is seen to entail. For instance, it affects our time perspective – how long we expect unidirectionality to have been applicable: is it something

which we can only observe in languages at the present or is it something which we can assume was the case all through the history (and evolution) of language(s)? And therefore it also affects the uses that can be made of the unidirectionality hypothesis and the concept of grammaticalisation.⁴⁹

Secondly, we can see parallels between the studies of grammaticalisation and ideas of unidirectionality, and the history of uniformitarianism. Deutscher (1998) has shown in his thesis that uniformitarianism does not seem to mean quite the same to most linguists today, as it did when it was first used in the nineteenth century. This is very important to note and consider for any person working in historical linguistics or within the history of linguistics, because the use of one term can affect the development and view of other related areas. Certainly this topic is one which reflects on every branch of historical linguistics, since many people would probably agree with Hopper and Traugott (1993: 38) that uniformitarianism is “an essential ingredient of most work in historical linguistics.” If it is, then we definitely need to know what it is exactly and if its meaning has changed since it was first used – what does that mean exactly?

This change in meaning, which has arisen in the last century, according to Deutscher (1998, 1999), does not only appear to have happened to this particular term and concept (cf. the changes in the meaning of grammaticalisation and the vagueness and confusion surrounding it now (see Lindström, *forth.*)). It seems it is quite common for terminology to develop and change just like any other part of the vocabulary and language in general. However, there is a problem in the fact that

⁴⁹We can also ask ourselves if we should see two periods in the development of human languages – a stage of evolution followed by a stage of history after evolution has seen to it that all the necessary structures and characteristics have evolved and language(s) have come into being. (This line of thinking has been developed in discussion with April McMahon, and should be developed further but due to time and space restrictions I cannot do so here.)

If it is true that language can be seen to have developed in two major stages, the evolutionary and the historical, then there is a chance that the changes may have been different in the ‘evolutional’ stage to what they are like now in the ‘historical’ stage. This would be because when the languages were no longer evolving they would change at least partly for other reasons and only by remodelling the material they have.

Grammaticalisation might have been an evolutionary process which survived into the historical period of language. But maybe one difference could be that it was unidirectional in the evolutionary stage but not any more? Or perhaps the reason that it is primarily unidirectional today is because it was an evolutionary tool at first and has only in the ‘historical period’ started to change into a different tool – triggered by other things? But if we go on to make assumptions like this, we are in fact saying that grammaticalisation is something which we are assuming exists in language itself and it is not just a linguistic tool and most certainly not an epiphenomenon! This we are not actually sure is true. In fact many linguists have claimed it is an epiphenomenon, e.g. Campbell (2001), Joseph (2001) and Janda (2001).

sometimes the changes involve misunderstandings. Often the terms are not defined in their new usage, since they have an ‘established’ (however varied) usage and people do not always realise that they are using them differently to how others have used them.

Sometimes when we are reading something and not paying too much attention to exactly what we are reading, we jump to conclusions and misunderstand what is being said based on our background knowledge and assumptions. It is quite sad that Deutscher should have to bring to people’s attention the fact that “laws” or “forces”; “processes”; and “synchronic states” are “not equivalent”. But, he does point this out and it seems that there really is a need to do so, because it is precisely in these terms that the confusion and change in uniformitarianism has arisen.

The synchronic interpretation of uniformitarianism neither follows from, nor is equivalent to, the original interpretation! [...] The original formulation of uniformitarianism asserted that the *forces* operating on language were the same in the past as they are in the present. The current synchronic interpretation of uniformitarianism asserts something entirely different, namely, that the *synchronic states* of past languages were the same in the past as they are in the present. The two interpretations are not equivalent, because similar *forces* can induce very different *processes*, when the initial conditions of the system are different. Moreover, similar *processes* can induce different *synchronic states* when the initial conditions are different. (Deutscher, 1998: 109)

However, it is not necessarily the case that the meaning has changed due to a misunderstanding of the hypothesis. It may well be that it has changed due to research which has noted that it **may** be more widely applicable, that it is possible to generalise. But we do **not** know, and we probably never will know if we really are correct, because we are dealing with hypotheses of the past. And, whether it was correct to generalise is also hard to say because we are only hypothesising about what has happened in the evolution and development of language(s). But, if after a thorough examination of language development, a linguist today comes to the conclusion that the uniformitarian principle should be revised and made more general, with the proper evidence to argue for this change, their ideas could be accepted by other linguists.

As it happens the two forms of uniformitarianism, as defined by Deutscher (1998), come into conflict in a strong reading of the unidirectionality hypothesis.

Fischer and Rosenbach (2000: 21) claim that strong unidirectionality can be taken to mean that “all grammatical elements are lexical in origin.” But then, they say this would predict that “there should have been a time when all languages were isolating”. However, I would like to add that this is only the case if both **diachronic uniformitarianism** (see quote from Deutscher above) and unidirectionality are true. Today, not all languages are isolating, there are other types of languages. So, if all languages were once isolating that would contradict **synchronic uniformitarianism**, according to which the language types today should be the same as the language types of the past.

Fischer and Rosenbach argue that Lass also holds this to be “counter-uniformitarian” (2000: 21). Lass says that, since we have not found any languages of the past that were totally isolating, nor any proof that all morphology should come from lexical sources, “positing a period when there was a ‘law’ that says ‘all languages are isolating, and all their material is lexical’ [...] is counter-uniformitarian”. This is because we have no record of such a language ever existing, because we believe that natural laws do not change, because if this was true we have no explanation for how we could have got the different types we now have of languages, because there are morphemes which we can see no clear lexical background and origin for (Lass, 2000: 216). He later makes it clear that he apparently believes “the principles underlying language change (such as unidirectionality)” (Fischer and Rosenbach, 2000: 21) could only be trusted to be the same in the past if the language types were the same – which means that if we think the unidirectionality hypothesis indicates that all languages were once isolating and all linguistic items once lexical, then changes such as grammaticalisation need not have been unidirectional in the past even if it could be proven to be so at present.

Lass’s claim in fact says that for diachronic uniformitarianism to be true, synchronic uniformitarianism must be true, and since certain laws of language can change the structure of a language, e.g. unidirectionality, one of them must be false. (Or the principle itself is false.) Lass uses this to prove that the unidirectionality hypothesis is wrong and says “Who is to say that in a linguistic world so different from ours that it has no morphology but only lexis the {lexical > grammatical} or {free > bound} or any other pathways didn’t run in the other direction? (Induction fails in universes non-isomorphic to the one in which the induction is made.)” (Lass, 2000: 217).

Fischer and Rosenbach call attention to Deutscher's (1999) discussion of synchronic and diachronic uniformitarianism, and help us to see how easily one may get tangled up in the web of confusion surrounding uniformitarianism. After indicating that Lass may have used the term to mean both, they go on to say that "[i]n other words, the fact that we do not have fully isolated languages now, cannot be used to dismiss the 'principle' of unidirectionality" (Fischer and Rosenbach, 2000: 22). This is because, according to the diachronic version of uniformitarianism, the types will not affect the laws of language.

If we instead reason around this statement using either or both of the versions of uniformitarianism we get utterly confused:

(1) According to synchronic uniformitarianism, the fact that there are no fully isolating languages today would be problematic for uniformitarianism if there had been such languages before (as according to the strong unidirectionality hypothesis there ought to have been). However this is not problematic for unidirectionality, which would predict that languages would change from isolating to more agglutinative.

(2) Diachronic uniformitarianism would mean that the fact that there are no fully isolating languages today could really be expected – if unidirectionality is a principle of language. It is not necessary for them to disappear nor to remain. If all language were fully isolating today – that would be a much more serious problem according to the diachronic view of uniformitarianism.

(3) But for Lass, assuming that unidirectionality has always been a process in language change is problematic, if the types of language have changed, i.e we cannot assume diachronic uniformitarianism if we cannot prove synchronic uniformitarianism, and as I have said before unidirectionality and diachronic uniformitarianism would mean a change of type eventually. In other words synchronic uniformitarianism could not be true - or alternatively unidirectionality or diachronic uniformitarianism would have to be false.

Deutscher draws attention to a monograph by Christy (1983), where Christy has studied how the term, and partly the concept of, *uniformitarianism* was adopted by linguists from geology. This gives quite a clear picture of what uniformitarianism has meant for linguistics and Christy shows how there are links between the adoption

of uniformitarian theory and/or method and linguistics becoming more scientific. He talks of how inductive reasoning becomes important and how organism is left behind. Since then however deductivism has risen in popularity in connection with the stress on description in linguistics since the early twentieth century.

The main tenet of the change to uniformitarianism was perhaps the change from speculation to induction from the known.

... the uniformitarian principle [...] resulted in a chronological revolution which made it possible to explain the origin and development of the earth and mankind by known and observable laws of causation. (Christy, 1983: 109)

Christy believes that we can see a change in the view of language evolution in connection with the introduction of uniformitarianism into linguistics. Before then change had always been seen as decay, but with the introduction of uniformitarianism things changed. It was still believed that there were destructive forces in language, such as sound change, however there were also forces which helped to “regenerate lost structure” (e.g., analogy), and thereby language(s) did not decay.

The limited chronology of Scriptures was replaced by a virtually unlimited amount of time, such that progressionist-type theories came to replace the doctrine of the divine origin of language. In other words, a new view of languages as being capable of progress replaced the idea that all historical language development was but post-lapsarian decay. (Christy, 1983: 109)⁵⁰

Still to this day there are ideas that do not appear to match up with the modern uniformitarian ideas. This is evident in grammaticalisation where we

⁵⁰ Languages were considered capable of progressive changes, however it seems that some changes were still viewed as bad, problematic and making the language inferior to its past forms. Around the time when things started to change it was very common to view certain changes as progression and others as decay, typically in Schleicher’s (*Die Darwinische Theorie*, 1863) typological movement up from primitive types of language to a more and more advanced and superior language – which, however, started to degenerate once it had reached the top, which in the Indo-European languages had already been reached. A theory of the language organism which grew, matured and then declined and died. Schleicher’s theory of Darwinian linguistic evolution built partly on Humboldt’s typology, which had strong links to Humboldt’s ideas of agglutination as a means close to perfection of expressing one’s thoughts. This theory has clear links to grammaticalisation, and it seems that even today many grammaticalisationists would think that grammaticalisation is good, whereas any possible (or impossible) degrammaticalisation would be bad and degenerative. This is of course a rather organistic view of language, which perhaps would have been better left behind, or rather left to deal with the origins of language. (cf. Harris and Taylor 1997: 175, 181-184, 186; Robins 1997: 201-206)

often see clear links to the old ideas of Horne Tooke and Condillac who believed that prepositions could be derived from nouns and that pronouns could become personal endings on verbs, respectively. Things in language do clearly change, they do most certainly not remain the same. However, this does not mean that the underlying processes, and certainly not the forces, need to change (i.e. it may agree with what Deutscher (1998) called “diachronic uniformitarianism”).

Whitney was the linguist who finally made uniformitarianism a definite part of linguistics and he concluded that:

The nature and uses of speech, and the forces which act upon it and produce its changes, cannot but have been essentially the same during all the periods of its history, (Whitney, 1867 [1973], cited by Christy 1983: 84)

Deutscher primarily blames the structuralists for the confusion and change of meaning, for step by step moving uniformitarianism away from diachrony and towards synchrony. This, he believes, had started in the early twentieth century (Deutscher, 1998: 107), but went on up until the 1990s when Deutscher claims that it appears to finally have settled in what he calls the synchronic sense (1998: 107). An extreme move had then been made from forces and processes all the way to language and language states.

... languages of the past (at least, those that we can reconstruct or find records of) are not different in nature from languages of the present. (Croft, 1990: 204)

The uniformitarian hypothesis, like other hypothesis of diachronic typology, is a general assumption about the nature of language and language change that can be considered a defining characteristic of diachronic-typological theory, in the same way that the innateness hypothesis of generative grammar (that most linguistic competence is biologically innate) is a general assumption that can be only quite indirectly verified or falsified. (Croft, 1990: 204)

What is it then that Hopper and Traugott (1993: 38) claim is an “essential ingredient” to historical linguistics? Is it the original (diachronic) uniformitarianism or is it the modern (synchronic) or is it both? Hopper and Traugott’s explanation of what uniformitarianism means sounds like a mixture of the two, but the latter half

makes it quite clear that they must have a more modern interpretation. Deutscher (1998: 108) also says that it appears that their “operational conclusion” coincides with the general modern usage, cf. Hopper and Traugott (1993: 38):

According to this principle, the linguistic forces that are evidenced today are in principle the same as those that operated in the past. Operationally, this means that no earlier grammar or rule may be reconstructed for a dead language that is not attested in a living one.

Apart from this being a bit vague, like dealings with the “synchronic interpretation” usually are, Deutscher says that the quotation above also has to be false, because “[c]learly, older languages had some rules and phenomena that are not attested at present” (1999). And of course, the fact that something is not attested in our sample does not necessarily mean that it does not exist! We can see influences of both versions of uniformitarianism in current linguistics and in discussions surrounding grammaticalisation and unidirectionality.

The Uniformitarian Principle naturally affects grammaticalisation and plays a role, for instance, in reconstructions based on the unidirectionality hypothesis. We need to understand this principle if we are to understand some of the theories surrounding language change, just like unidirectionality has also come to be one of the necessary, basic principles of work on language change. But there is a problem, because, as was briefly shown above, unidirectionality relates differently to the two versions of uniformitarianism. According to the original version of uniformitarianism, if we say that unidirectionality is almost like a force, it should be part of the past as well as the present, if it is part of the present. This would mean that we can use unidirectionality in reconstructions, because we can trust that it was the case in the past if it is true today.

However, unidirectionality can actually be seen as opposed to the synchronic uniformitarianism.

In order to say that the past languages themselves were ‘the same’ as present ones, we need to make many more assumptions about the nature of these forces and their direction. For example, we have to assume that there are no global directional changes, that all change is cyclic or random, [...] (Deutscher, 1999)

But what do we actually mean by unidirectionality? It applies to processes. But what is affected – is it the type or the category? I ask this because unidirectionality could be taken to imply that cyclicity or spirality of change is impossible, if we only look at the linguistic item that is changing. However, generally, unidirectionality is a characteristic only of one stage in the development, and deals only with one construction. It does not usually appear to be meant to bear on the long-term spirality of the change, which we can see in categories and constructions rather than in linguistic types. In that case, unidirectionality would not have to be opposed to the synchronic version of uniformitarianism.

More problematic however, for unidirectionality as well as for uniformitarianism, are changes that show a reversal. That means that unidirectionality is proven wrong as is diachronic uniformitarianism, because it means that (1) a process is hindered and (2) language is no longer proceeding unidirectionally, but instead moves in the opposite direction. However, what looks like a reversal does not necessarily have to be a reversal of a change. It could be that there is no real reversal. Lexical diffusion theory has shown that, for instance, in phonological change there have been signs of what have seemed like a reversal, although at a closer look it has turned out that it may have been the result of two (or more) changes. One change may start to change a sound in a particular context, but then another change begins which interferes by affecting the context, or by introducing a spelling pronunciation, which may reintroduce a sound that had been deleted (McMahon, 1994: 54-55).

Admittedly problematic for the synchronic kind of uniformitarianism, that Deutscher (1998) claims have developed with structuralism, are cases which show that a completely new construction can enter a language. This proves that all language types or language rules do not have to have been existent at all times.

3.7 Conclusions and Summary

It is clear from the discussions in this chapter that *grammaticalisation* and *lexicalisation* do not mean the same to all linguists, compare Lehmann and Ramat for instance. It also seems as though Hopper and Traugott (1993) and other grammaticalisationists today seem to have a different notion of grammaticalisation

than Meillet (1912; 1915-1916 [1921]) did. This clearly makes it valid to ask – whether unidirectionality was involved in grammaticalisation in the early treatments of the subject, and in Meillet (1912)? And if it should be seen as a characteristic of grammaticalisation today?

This chapter has tried to give a view of what the unidirectionality hypothesis is, and to raise the question, as others have done before, about whether it is true that grammaticalisation is unidirectional. An answer to this question has also been attempted at various points in the history of grammaticalisation, as has been illustrated at various points in the preceding pages. However, it is not easy to give a good answer.

Whether unidirectionality is true, depends on how we define grammaticalisation and what we mean by unidirectionality, which in turn is an issue that tends to be forgotten about. As has been seen above there are several names for processes which are supposed to move in the opposite direction, or that can move in either direction. But their relation to grammaticalisation and unidirectionality depends on what we think grammaticalisation is as well as on how we define the grammar and the lexicon.

It is true of course that it can be of interest to classify groups as narrowly as possible and if there are changes which only move in one direction, that is certainly interesting. It may also be interesting to look at processes in the two different directions along a cline and compare their frequency, plus the actual processes and phenomena that occur. Whether the processes should be said to move **only** in one direction or not, depends a lot upon how we define a reversal of the process, as well as how we define the process itself.

The truth of unidirectionality also depends on whether we wish it to be universal. If we do then the question is – how do we prove that it is? Discussions in relation to Lass (2000) have served to prove that this is not at all an easy thing to do. For one thing, how can we be sure that we have a representative sample, or even what a representative sample of all the languages (and dialects) of the world would be? But we must also consider what negative evidence (cf. Lindström, 2002) can actually tell us. If something does not occur in our sample, does that definitely mean that it does not exist? It does seem as though one direction is much more common, and this we need to investigate and try to explain in order to gain a deeper understanding of these kinds of changes.

There is certainly a close relation between grammaticalisation, degrammaticalisation, regrammaticalisation, exaptation and lexicalisation, which still needs to be defined. Sometimes it can be very hard to distinguish them all from each other. It is therefore possible that we need to define these terms every time we use them, in order to promote a standardisation and through that a deeper understanding of the superordinate concept – if there is such a thing. One problem with these terms has been that they have all quite often been used as labels without much in the way of an explanation or definition of what they actually involve and people have interpreted them differently.

I have also dealt briefly with Deutscher's work on uniformitarianism, where he has shown how a term can change its meaning just as any other item of the vocabulary. I believe that this has also happened with grammaticalisation and it seems that unidirectionality may be a characteristic which has attached itself to the concept of grammaticalisation at quite a late stage, even though it is also clear that more general ideas of (strong) unidirectionality were also common in the nineteenth century (as will be discussed in chapters 5 and 6 below). However, there is a problem with changes of meaning in terminology. Sometimes the changes involve misunderstandings and often the terms are not defined in their new usage, since they have an 'established' (however varied and possibly changed) usage and people do not always realise that they are using it differently. I also discussed how unidirectionality has been disproved through uniformitarianism, but I showed that it need not be in conflict with either version of uniformitarianism.

4. REANALYSIS AND ANALOGY IN RELATION TO GRAMMATICALISATION

4.0 Introduction

Reanalysis and analogy are both linguistic concepts that often tend to be mentioned in relation to grammaticalisation. Sometimes they are seen as contributing to grammaticalisation. At other times they are seen as superordinate to grammaticalisation, or grammaticalisation may even be seen as an unnecessary concept, a pure epiphenomenon – all changes being explicable through analogy and reanalysis, and maybe a few other known mechanisms (cf. Harris and Campbell, 1995).

Before *reanalysis* was introduced into linguistics,⁵¹ Meillet (1912) said that there are two processes of change that give us grammatical forms, viz. analogy and grammaticalisation:

Les procédés par lesquels se constituent les formes grammaticales sont au nombre de deux; tous les deux sont connus, même des personnes qui n'ont jamais étudié linguistique, et chacun a eu occasion, sinon d'y arrêter son esprit, du moins de les observer en passant.

L'un de ces procédés est **l'analogie**; il consiste à faire une forme sur le modèle d'une autre; [...]

⁵¹ That is, before it was introduced under that term. It seems possible that it had been recognised before, or rather that a similar concept existed earlier.

L'autre procédé consiste dans **le passage d'un mot autonome au rôle d'élément grammatical**. [i.e. grammaticalisation. TL]⁵² (Meillet, 1912; 1921: 130-131, emphasis mine)

Hopper and Traugott (1993) have suggested in their introduction to grammaticalisation that in fact it was analogy and reanalysis, rather than analogy and grammaticalisation that Meillet treated in his important paper from 1912 (1993: 48, 56). This may be correct in **their** usage of the terms, however it naturally seems wrong to accuse Meillet of meaning reanalysis by *grammaticalisation*, when he is presumed to have coined the term *grammaticalisation*, whereas *reanalysis* appears to be a later term in linguistics. Haspelmath (1998: 322), for instance, claims that reanalysis has only been used since the mid-1970s. If Meillet in fact meant reanalysis, then either (1) *grammaticalisation* (originally) meant the same as what we now mean by reanalysis and then we have adopted another meaning of the term, or (2) *grammaticalisation*, à la Meillet (1912), included both what Hopper and Traugott (1993) mean by *grammaticalisation* and what they mean by *reanalysis*, and the distinction is a later creation.

A comparison of Meillet's article (1912) and Hopper and Traugott's (1993) book also shows that such a conclusion on their part does not make much sense. Meillet uses similar examples to what they use, e.g. the Romance future and negation, and he also discusses the involvement of features such as semantic bleaching, phonological attrition and, yes, possibly also something that resembles reanalysis (see also section 6.1).

Reanalysis and analogy are in the eyes of Hopper and Traugott **mechanisms**, which play roles in the process(es) / phenomenon of grammaticalisation, along with the **cognitive processes** of metonymy and metaphor (1993). They appear to want to put analogy and reanalysis on a different level of diachronic linguistic analysis and description, than grammaticalisation, a different level in linguistic change (see table 4.1 below):

⁵² Translation: There are two processes through which the grammatical forms are made, both of them are known, even to people who have never studied linguistics, and everyone has had opportunity, if not to think about them carefully, at least to observe them in passing. One of these processes is analogy; it consists of creating a form modelled on that of another; [...] The other process consists of the passing of an autonomous word into the role of a grammatical element.

Grammaticalisation	
Reanalysis	Analogy
Metonymy	Metaphor

Table 4.1 An attempt at an illustration of Hopper and Traugott's (1993) view of the relation between grammaticalisation, reanalysis and analogy.

But others have in fact seen grammaticalisation and reanalysis as more or less the same thing, (see below section 4.1 and especially 4.1.3). And at other times it seems as though analogy and grammaticalisation are put on the same level, whereas reanalysis is on a different one.⁵³ But if we do that, should reanalysis be seen as hierarchically below or above grammaticalisation? And what would its relation be to analogy? And if we are going to start talking about levels we also need to consider whether this is a matter of relations between superordinate and subordinate processes or one of causation and prerequisites.⁵⁴ Which also leads on to the question of 'what is grammaticalisation'? Is it the whole process of change or only part of it? Furthermore, we can sometimes see similarities between all three processes, and it does not seem possible to say that one can, and usually does, cause the other, but not vice versa (cf. Newmeyer, 1998). Instead it seems that causation can work both from analogy to reanalysis, reanalysis to analogy, grammaticalisation to reanalysis, reanalysis to grammaticalisation, grammaticalisation to analogy and analogy to grammaticalisation. And on top of it all, of course, one cannot be sure whether a change will occur at all.

As most people are aware, analogy is a concept that is very old. It was used in the classical works on language. It has been seen as part of the second linguistic controversy in classical Greece, a controversy as to whether language was analogous or anomalous (cf. Robins, 1997: 25). Although recently this controversy has been questioned as we can see in Taylor (1986) and Seuren (1998: 23-27), for instance:

The analogy-anomaly quarrel is the most conspicuous of these dichotomies, and the most important, for it occupies the center of attention in the received accounts. (Taylor, 1986: 177)

⁵³ This could possibly be one reading of Meillet, although he does not distinguish a concept of reanalysis, or rather a concept that could be what we call *reanalysis*.

⁵⁴ Based on a discussion with April McMahon December 2001.

Blank (1982: 4), for example, throws the entire analogy / anomaly “controversy out of court on the grounds that it never took place”. and he is but the most recent voice to be heard on the matter. (Taylor, 1986: 181)

On the one hand, there is the tradition that developed along Heraclitean, Platonic and Stoic lines. On the other, there is the tradition of Aristotle and the Alexandrine philologists. It is generally believed (on the basis of evidence from Varro and Sextus Empiricus) that for a few centuries, from roughly 300 till 50 BC, these two traditions were at odds with each other, though there is a conspicuous scarcity of direct documentary evidence on this. (Seuren, 1998: 23)

We cannot automatically assume that this old concept of analogy is identical to what we mean by analogy today however. We need to compare the concept that is now called *analogy*, that is used in relation to grammaticalisation, to the old concept, in order to see if they really are the same (see further section 4.2). Unfortunately, however, there will not be enough space and time in this thesis for a suitably extensive exposition of the subject, even though it would be well worthy of further study and also relevant to the topic at hand. But, what is primarily of interest to us here is the connection between analogy, as we know it and as it developed during the last two centuries, and to grammaticalisation.

This chapter is an attempt to clarify what reanalysis (4.1) and analogy (4.2) are and what their relation is to grammaticalisation. It will also be an attempt to compare some stages in the history of the study of grammaticalisation to the history of the study of analogy, since there appear to be interesting parallels in the history of these two concepts that are important to consider. In connection with this I could also have tackled the connection between reanalysis, analogy, grammaticalisation, and metaphor and metonymy. However, this wide subject will have to be left for later study. Some treatment of this can however be found in, for example, Heine et al (1991a), Hopper and Traugott (1993), Moreno Cabrera (1998) and Wischer (2000).

4.1 Reanalysis

The relation between reanalysis and grammaticalisation has also been discussed by others (e.g., Campbell, 2001: 143-151; Haspelmath, 1998; Newmeyer, 1998: 241-251). However, I am hoping this will be a more objective treatment than some of the

former discussions of this have been. Both Campbell and Newmeyer are on a crusade, hoping to prove that grammaticalisation is dependent upon reanalysis and that in fact grammaticalisation is only an epiphenomenon. Whereas Haspelmath could be said to be on the opposite crusade, he wants to prove that grammaticalisation is **not** an epiphenomenon and that in fact it does not “need” reanalysis!

Since all three authors have a distinct aim with their treatises of the relation between these two processes, none of them is objective. But even if Campbell’s (2001), Newmeyer’s (1998) and Haspelmath’s (1998) treatments had been excellently and perfectly objective, I believe I would still have needed to discuss this here, because of the topic of this thesis and because I want to try to clear up some confusion in the linguistic concepts and terminology. Therefore, I believe it is very important to deal with reanalysis in relation to my own work and experience.

Langacker’s (1977) extensive and celebrated article on **syntactic** reanalysis mentions that reanalysis at that time was often considered as a mechanism in phonology:

The mechanism of reanalysis is by now quite familiar from phonology. Here the surface level is the phonetic level, which derives via phonological rules from abstract, phonological representations. Reanalysis in phonology is thus phonological change that does not involve immediate or intrinsic phonetic change. (Langacker, 1977: 58)⁵⁵

It is quite possible that this is why Langacker says his article deals with **syntactic** reanalysis, even though the article also includes a famous definition of reanalysis in general. Although, this general definition is also used for syntactic and morphosyntactic reanalysis. It is often the only definition that is given of reanalysis in works on the relation between reanalysis and grammaticalisation.⁵⁶ However, Langacker’s definition implies that it is structural reanalysis and not a broader superordinate concept that he has in mind, but note that structure according to Langacker also includes semantic values (see footnote below).

⁵⁵ Please note that he believes it to be common enough for him not to have to provide any references.

⁵⁶ This is also what Campbell (2001: 149) appears to see as the “widely accepted core meaning” of reanalysis. (Although Campbell’s reference is to page 64 in Langacker 1977. But Langacker gives no definition on that page, there he mainly discusses different types of reanalysis.)

... change in the structure⁵⁷ of an expression or class of expressions that does not involve any immediate or intrinsic modifications of its surface⁵⁸ manifestation. (Langacker, 1977: 58)

Sometimes it seems as though Langacker (1977) almost means grammaticalisation in the modern sense rather than reanalysis. He specifies that this change is “largely unidirectional” (Langacker, 1977: 104), and one of the differences which have quite often been noted (or suggested) between reanalysis and grammaticalisation is that the latter, but **not** the former, is unidirectional (cf. below, section 4.1.1, cf. also above in chapter 3.)

We should also note that Langacker specifies different types of reanalysis, and he actually admits that one of these types (“boundary creation”) is quite rare and implies that it moves in the opposite direction to the general movement of reanalysis (1977: 104). Therefore, we should probably interpret his statement about unidirectionality to mean that some (or perhaps most) types of reanalysis are unidirectional, and then possibly grammaticalisation is one type of unidirectional reanalysis.⁵⁹ This is in fact similar to a stand that also Hopper and Traugott could be understood to have taken on at least one occasion:

It is best, then, to regard grammaticalization as a subset of changes involved in reanalysis, rather than to identify the two (Hopper and Traugott, 1993: 50, cf. Norde 1998: 213, Campbell 2001: 143)^{60, 61}

⁵⁷ “The underlying level (or levels) relevant for syntactic reanalysis may consist of any aspect of morphological, syntactic, or semantic structure, i.e. anything more abstract than the surface level as defined above. These structural features include the location of morpheme and clitic boundaries, surface constituent structure, underlying constituent structure, the semantic value or syntactic function of morphemes, and so on.” (Langacker 1977: 62)

⁵⁸ “There are different ways of defining the surface level for purposes of discussing syntactic reanalysis, and they naturally define different classes of phenomena as instances of this category. For our purposes the surface level can be viewed as the phonemic level of representation, together with indications of word boundaries, but with no indication of constituent structure or boundaries smaller than word boundaries (such as morpheme or clitic boundaries).” (Langacker 1977: 61)

⁵⁹ Or it could be that it is the kind of reanalysis that one normally gets in grammaticalisation that is unidirectional. It is certainly quite clear from the rest of his paper that he is not concerned with grammaticalisation, for instance he never mentions anything like a movement from lexical to grammatical, even when he speaks of unidirectionality it is structural unidirectionality and structure is what he is concerned with throughout his paper.

⁶⁰ Hopper and Traugott 1993: 50 refer to Heine and Reh 1984: 97, Heine, Claudi and Hünnemeyer 1991a: 215-20)

⁶¹ But note that Hopper and Traugott appear to make grammaticalisation a kind of change that can be **involved** in reanalysis and **not** a kind of reanalysis!

But this is different in that grammaticalisation is not a form of reanalysis, but rather included in all reanalysis, according to this. Although Hopper and Traugott elsewhere see reanalysis as a mechanism that is often at work in grammaticalisation (cf. Hopper and Traugott, 1993: ch. 3):

... the mechanisms by which grammaticalization takes place: reanalysis primarily, and analogy secondarily. (Hopper and Traugott, 1993: 32)

Another interesting point here, is that Hopper and Traugott (1993: 50) refer to Heine and Reh (1984: 97) and Heine et al (1991a: 215-220), which could easily be interpreted to mean that they also view grammaticalisation and reanalysis in this way. And, they do in fact appear to see some links between the two, although they treat them as separate, partly on the basis of unidirectionality. There is nothing to indicate that they view them as though grammaticalisation is a subset of reanalysis, or “a subset of changes involved in reanalysis”.

Traugott (p.c.) clarifies her and Hopper’s position in their 1993 book, explaining that the two extracts above certainly were not meant to be opposed to one another and should not be read in that way. What they meant was that reanalysis can be a mechanism at work in grammaticalisation, and that grammaticalisation often involves reanalysis and many cases of reanalysis are also cases of grammaticalisation. And, that one type of change that can be “brought about” through reanalysis is grammaticalisation. Nevertheless, it is important to question how people have understood this and adopted it in their own work.

Most people who write on grammaticalisation seem to agree that there is a close connection between it and reanalysis and sometimes reanalysis (together with certain other mechanisms of change) is seen as making a grammaticalisation ‘mechanism’ or ‘process’ redundant. Lately, it has become popular to express views about grammaticalisation being an epiphenomenon, “not a mechanism of change in its own right” as Campbell says (2001: 141). Similar views have also been expressed by Newmeyer (1998; 2001), Janda (2001) and Joseph (2001), for instance. Campbell claims that in fact it is primarily *reanalysis*, but also *extension* that gives us change,

and thirdly (according to Harris and Campbell (1995)) *borrowing*.⁶² (We shall return to this discussion in 4.1.3.)

4.1.1 Defining Reanalysis

The ‘default’ definition of reanalysis appears to have become that of Langacker (1977), which is quoted in most texts dealing with reanalysis and grammaticalisation (see above). However, Hopper and Traugott find it important, unlike most others, to include their own brief definition of reanalysis, which they give even though they also admit to adopting Langacker’s. They claim that reanalysis brings about rule change, whereas analogy is only considered to affect the spread of a rule.

Reanalysis modifies underlying representations, whether semantic, syntactic, or morphological, and brings about rule change. Analogy, strictly speaking, modifies surface manifestation and in itself does not effect rule change, although it does effect rule spread either within the linguistic system itself or within the community. (Hopper and Traugott, 1993: 32)

One interesting difference in Hopper and Traugott’s definition as compared to Langacker’s is that Langacker (1977) does not mention rules, at least not explicitly, instead he speaks of structure.⁶³ The next question we need to ask therefore is of course whether they mean the same thing by reanalysis?

Hopper and Traugott’s definition of reanalysis appears similar to that which Meillet (1912) said of grammaticalisation and analogy – and may therefore be one of the reasons why Hopper and Traugott claim that Meillet is talking of reanalysis and not grammaticalisation:

Tandis que l’analogie peut renouveler le détail des formes, mais laisse le plus souvent intact le plan d’ensemble du système existant, la « grammaticalisation » de certains mots crée des formes neuves, introduit des

⁶² Borrowing in the meaning of “a mechanism of change in which a replication of the syntactic pattern is incorporated into the borrowing language through the influence of a host pattern found in a contact language” (Harris and Campbell 1995: 51). Note that they distinguish between this and *language contact* which to them is a situation, which naturally may lead to a change, but which does not necessarily lead to borrowing.

⁶³ Hopper and Traugott (1993) also speak of “underlying structure”.

catégories qui n’avaient pas d’expression linguistique, transforme l’ensemble du système.⁶⁴ (Meillet, 1912; 1921: 133)

Campbell gives a definition of reanalysis, which more strongly harks back to its most well known antecedent, viz. Langacker (1977):

Reanalysis changes the **underlying structure**⁶⁵ of a **grammatical**⁶⁶ **construction**, but does not modify **surface manifestation**.⁶⁷ (Campbell, 2001: 141, emphasis mine)

Shortly afterwards Campbell also compares Harris and Campbell’s (1995) definition to Langacker’s (1977), and claims that they incorporate at least part of Langacker’s notion of reanalysis (namely that it is a “change in the structure of an expression or class of expressions that does not involve any immediate or intrinsic modification of its surface manifestation” (Campbell, 2001: 141). He does not mention what their definition actually is, but the reader probably suspects it to be similar to his own. This proves to be true when we look up their definition. However, there is one difference in that Campbell (2001) speaks of “grammatical construction” whereas Harris and Campbell choose to use the term “syntactic pattern”, an interesting difference seeing as Campbell has also varied between grammatical and syntactic construction in his own independent writings (Campbell, 1998: 227; 2001: 141).

Reanalysis⁶⁸ is a mechanism which changes the underlying structure⁶⁹ of a **syntactic pattern** and which does not involve any modification of its **surface manifestation**⁷⁰. (Harris and Campbell, 1995: 50, cf. also 60, emphasis mine)

⁶⁴ Translation: While analogy can renew the details of the forms, but usually leaves the plan of the existing system as a whole intact, the “grammaticalisation” of certain words creates new forms, introduces categories that have no linguistic expression, transforms the whole system.

⁶⁵ constituency, hierarchial structure, grammatical categories, grammatical relations, cohesion (Campbell 1998: 227, 2001: 141)

⁶⁶ cf. Campbell 1998: 227 where he replaces ‘grammatical’ by ‘syntactic’.

⁶⁷ morphological marking, word order (Campbell 1998: 227, 2001: 141)

⁶⁸ For them reanalysis is one of three mechanisms of change – the others being extension and borrowing.

⁶⁹ “include at least (i) constituency, (ii) hierarchial structure, (iii) category labels, and (iv) grammatical relations.” (Harris and Campbell 1995: 50) cf. “include information regarding at least (i) constituency, (ii) hierarchial structure, (iii) category labels, (iv) grammatical relations, and (v) cohesion” (Harris and Campbell 1995: 61). Note that unlike Langacker they do not include semantic representation.

⁷⁰ “(i) morphological marking, such as morphological case, agreement, and gender-class, and (ii) word order.”

Harris and Campbell (1995: 50, fn 2) mention that they have based their definition on Langacker (1977: 58) and they also recognise that they have been influenced by Timberlake (1977).

Harris and Campbell (1995: 61) note that reanalysis **can** involve (or rather lead to) surface modification. However, only in combination with other changes, and not as a direct consequence of reanalysis. This has also been noted by Langacker (1977: 58) who said that such changes were “the natural and expected result of functionally prior modifications in rules and underlying representation”.

Interestingly, Harris and Campbell’s other source of inspiration is Timberlake (1977: 141). He gives only a rather short definition of reanalysis, without any direct mention of the surface manifestation. This may remind us of Hopper and Traugott’s definition above: “the formulation of a novel set of underlying relationships and rules” in that Timberlake too brings in the issue of rules. Timberlake also clearly distinguishes reanalysis from, what he calls, the “actualization”: “the gradual mapping out of the consequences of the reanalysis [...]”.⁷¹ The latter clearly resembles analogy in some ways, and this is emphasised by the fact that one of the examples that Timberlake uses is used by Campbell (1998: 228) to illustrate what he calls *reanalysis* followed by *extension*, but normally actualisation and extension or analogy should be kept apart (cf. Andersen, 2001). (I will return to that issue in section 4.2.)

Stage One: Reanalysis

näen miehe-m tule-va-m (Old Finnish)

I.see man-ACC.SG come-PTC-ACC.SG

“I see the man who is coming”

näen miehe-n tule-van (Modern Finnish⁷²)

I.see man-ACC.SG come-PTC

OR

I.see man-GEN.SG come-PTC

“I see the man who is coming”

⁷¹ One example he uses is that of a case ending (case 1) which through phonological change gains the same form as another (case 2) in some contexts. Certain structures therefore become ambiguous and reanalysis occurs which leads to case 2 taking over in this construction and spreading through actualisation to cover forms that are not homophonous for the two cases (Timberlake 1977: 146-148).

⁷² A sound change has changed final *-m* to *-n*.

Stage Two: Extension (or analogy)

näin venee-t purjehti-va-t (Old Finnish)

I.saw boat-ACC.PL sail-PTC.ACC.PL

näin vene-i-den purjehti-van (Modern Finnish)

I.saw boat-PL-GEN sail-PTC

(Adapted from Campbell 1998: 228-229)

It seems correct to make this kind of distinction and in some ways Hopper and Traugott (1993: 61) make a similar distinction when they claim that analogy makes reanalysis visible to us. Timberlake also makes it clear that we do not know of reanalyses until they are actualised, similarly Wurff (1990), in his model of diffusion, believes that we do not know they have happened until they have spread to a different variety (see below). This idea can also be seen in Harris and Campbell (1995) who suggest that actualisation takes place based on a multiple number of analyses which have arisen through reanalysis. This is probably the main sign of Timberlake's (1977) influence on them.

The existence of two (or more) reflexes from a single input, as in these examples, shows that after reanalysis (that is, in stage B), more than one analysis is available and must continue to be if more than one reflex is to survive. In instances of this type, after the additional changes of actualization one analysis come to be associated uniquely with one set of surface characteristics, while another analysis is associated with another set. [...]

The study of a number of attested changes suggests that in all reanalyses there are multiple analyses available during at least a part of the actualization. (Harris and Campbell, 1995: 83)

Wurff (1990: 25, 200) suggests that reanalysis must be based on “analogical principles”.⁷³ He also argues that reanalysis is possible, and indeed common, through diffusion. By this he means approximately that something may receive an alternative in one variant of the language which when it is met by speakers of another variant leads to reanalysis (Wurff, 1990: e.g. 31-32). Even though his notion of reanalysis

⁷³ It seems that according to Wurff analogy means that reanalysis has to fit the “overall pattern of a language”, as he quotes Koefoed and Van Marle (1987: 146) to have said (Wurff, 1990: 24-25). This appears to be somewhat different from the often discussed extension of reanalysis.

might seem a bit different to most other grammaticalisationists, he claims to mean roughly the same as one of the classical historical linguistics textbooks, namely Hock (1986: 327):

... it is possible that speakers engage in reanalysis and account for the same surface phenomenon by means of different rules. What is important is that such reanalyses may be extended and thus give rise to novel structures. (Hock, 1986: 327; also quoted in Wurff, 1990: 19)

However, there is a difference as for when Hock and Wurff believe that reanalysis can take place. The quote from Hock is concerned with the formulation of grammar during language acquisition, whereas Wurff sees reanalysis as having a “social locus”, and not occurring only at the acquisition stage (cf. Hock, 1986: 327; Wurff, 1990: 30).

Wurff also attempts to pin down some conditions that “are commonly [...] assumed to regulate its [i.e. reanalysis’] operation” (Wurff, 1990: 199):

... there must be prior surface change; there must not be counter-examples; there must not be a change in meaning; the new analysis must be based on analogical principles. (Wurff, 1990: 199-200)

The fact that Wurff suggests that there has to be surface change prior to reanalysis is rather interesting, considering that we normally associate reanalysis with the phrase ‘the surface manifestation is maintained’. However, of course the two statements look at different phases of the change and the ‘default’ definition of reanalysis does not at all seem to make such a change impossible. Although, if he is correct in assuming that surface change has to have occurred before reanalysis takes place, it is interesting that this has not been noted also by others. In fact it is something which does not become apparent at all in examples of reanalysis. But, there are also indications that Wurff might mean the same as others by *reanalysis*. He too mentions that there should be no “surface modification” **due to** reanalysis, but that reanalysis applies only to existing structures (Wurff, 1990: 19):

It should be noted that the reanalysis applies to an existing structure which does not undergo any surface modification as a result of the reanalysis. It will in fact be difficult to find evidence that reanalysis has taken place until the appearance of new structures, which could not be generated under the earlier

analysis, and which are due to an extension of the new analysis. Note also that the concept of reanalysis only makes sense if a restrictive model of grammar is used. (Wurff, 1990: 19)

Lehmann (1991: 494), like most linguists, says that grammaticalisation can involve reanalysis. He also claims, similarly to what Lightfoot and possibly Wurff can be seen to have said, that this would in that case mean that there had to be a model beforehand which could serve as a pattern. This model could be seen as an “analogical model”, which leads us to ask, why then would this not be analogy but reanalysis, if analogy can be viewed as a mechanism in itself? Is it not analogy because it does not involve a surface change, but is only triggered by an analogical pattern? This would prove that *analogy* has at least two meanings – one meaning a relation of likeness and one meaning a process based on a likeness, which involves surface changes.

Heine et al (1991a: 210) suggest that reanalysis, or *metaphorical transfer* as they also call it, can lead to metaphorical extension. In other words they see there being a movement from reanalysis to analogy instead of from analogy to reanalysis as Wurff (1990) suggests, so now reanalysis can lead to analogy, similar to that which Hopper and Traugott (1993) claim. Hopper and Traugott (1993: 61) say that analogy can give us a sign that reanalysis has occurred.⁷⁴

Givón (1991) has also noted that reanalysis is something which first tends to happen in the function and usually only later is succeeded by certain adjustments in the structure.

In diachronic change, as has been widely suggested, structural adjustment tends to lag behind creative-elaborative functional reanalysis (Givón, 1991: 123) (Followed by references to Givón, 1971a; 1975; 1979a; Heine and Claudi, 1986; Lord, 1973b)

But we must be careful not to confuse extension, actualisation, reanalysis and analogy. According to Andersen we should **not** see actualisation as the same as extension, rather it is distinct from reanalysis, extension (or analogy) and borrowing, being in short the “observable part of language change”:

⁷⁴ cf. also the discussion of Timberlake (1977) and others above on the issue of actualisation.

They [Harris and Campbell] also mention ‘actualization’ (77-88). They first describe this in the spirit of Timberlake (1977: 141) as the gradual mapping out of the consequences of ... reanalysis” (80) and then as a process or “period” of adjustment attendant on reanalysis (81). But then they apparently change their minds, finding “that each example of change under actualization [is] itself either an extension or an additional reanalysis” (80). And although the word *actualization* is used frequently in their subsequent exposition, it varies in meaning, sometimes subsuming “extension”, sometimes apparently being interchangeable with *extension* [...]

[...] their failure to come to grips with the fundamental difference between actualization and the three “mechanisms of change” they recognize, they miss the opportunity to integrate actualization – the observable part of language change – into their theory. (Andersen, 2001: 227)

Another way of seeing reanalysis as in a sense divided into more than one change, has been proposed by Heine and Reh (1984). In a way similar to Timberlake (1977) in particular, Heine and Reh (1984: 97-98) also recognise two kinds of “strategies”, or perhaps we could call them subprocesses, in syntactic reanalysis. However, according to them “syntactic transfer” applies an existing syntactic structure to a new context, which reminds us of analogy or extension in fact, and “syntactic adjustment” then amends the “conflict” which may arise between the new function and the syntactic structure. Transfer can trigger adjustment, but they recognise that it is possible that this does not always have to be the case, and that it could also be possible “that grammaticalization may lead straight to adjustment” (Heine and Reh, 1984: 99).

Syntactic Reanalysis	
Syntactic Transfer	Syntactic Adjustment

Grammaticalisation →
(Syntactic Reanalysis: Syntactic Transfer →)
Syntactic Reanalysis: Syntactic Adjustment

Table 4.2 Heine and Reh’s (1984) view of the different “strategies” of reanalysis and the relation between grammaticalisation and reanalysis.

This division into stages or subprocesses of reanalysis (one of which appears to resemble analogy) can also be compared to Lightfoot’s work on language change. Lightfoot (e.g. 1979: 101-115) has suggested that small changes appear before reanalysis, and that they can then build up to a big change, a reanalysis – in his view occurring during child language acquisition by producing a different setting of the

parameters than in the previous generation, which in turn produces surface consequences. Harris and Campbell (1995) disagree with this view of small changes leading up to the actual reanalysis:

Some scholars have taken the position that what they call reanalysis (defined differently) applies relatively late, with many small changes leading up to it (especially Lightfoot 1979 [...]) We have argued in section 4.4 (in part following Timberlake 1977; see also chapter 2) that such an approach leaves the small changes unexplained and that locating reanalysis earlier in the process provides an explanation for those changes. (Harris and Campbell, 1995: 93)

There is a possibility that analogy and reanalysis can work in close interaction, since reanalysis may be a way of resolving structural pressure, which could indicate that analogy can cause reanalysis or that reanalysis and analogy are parallel changes. And it indicates that they can at least sometimes be caused by the same thing, namely a form of structural pressure. Langacker (1977: 74) has also claimed explicitly that analogical pressure can lead to reanalysis.

... a reanalysis occurs in response to a particular set of factors present in a particular class of expressions; it resolves certain structural pressures or exploits the structural potential of those expressions. (Langacker, 1977: 96)

But why would the change then not be analogy? Unless analogy is just a relation and not a mechanism of change. This also reminds us of Heine and Reh's 'subprocess' of reanalysis, which they called *structural adjustment*, which they said takes place after syntactic transfer or as a direct consequence of grammaticalisation (Heine and Reh, 1984: 98-99). So, in that sense, it would be a form of reanalysis according to them.

Lightfoot appears to view reanalysis as being based on analogical relations, but as generativists have tended to do, he does not recognise analogy as a principle of change:

The fact that many re-analyses can be interpreted as analogical extensions does not make analogy a principle of change or anything more than a pretheoretical concept. On the other hand, the fact that the *form* of re-analyses cannot be predicted beyond imposing very general bounds on possible surface structure extensions, does not belittle the roles of analogy in governing language acquisition and therefore historical change. (Lightfoot, 1979: 373)

All of this indicates both the strong relationship between analogy and reanalysis and some of the dislike of analogy that has been apparent among generativists. It also makes it clear that the distinction between these two (reanalysis and analogy) is strongly theory bound.

Newmeyer (1998: 248-251) has noted, in relation to grammaticalisation, that it is not clear in which order the different processes should appear. A fact which he uses to try to argue for grammaticalisation being seen as an epiphenomenon. If it is true that reanalysis can be triggered by analogy and that analogy can be triggered by reanalysis we have a confusing mess on our hands. If we add to this that reanalysis may be a sign that grammaticalisation has occurred, or rather perhaps analogised reanalysis, i.e. extended reanalysis, can be a sign of grammaticalisation, we get even more tangled up in our own web of explanations. Furthermore, grammaticalisation does not have to involve reanalysis!

A major question that comes out of these suggestions of stages, subprocesses and small changes that might precede or be part of reanalysis, etc, is what constitutes 'the change' or indeed, 'a change'.⁷⁵ Lightfoot (1979) frequently asks this question. Is it the surface change or the change in the grammar, that is **the** change? In some ways grammatical change is so abstract that it is impossible to say when it has occurred and it is therefore more tangible to speak of and discuss the surface. However, the (original) idea behind reanalysis is that something happens underneath the surface which may later spread and cause changes in other places, which would not otherwise have occurred. But, when can we say that reanalysis starts and when does it finish? Since we cannot see the structure underneath, we cannot see how people analyse sentences subconsciously. This also means that we cannot tell that this has changed until there are repercussions on the language utterances that they produce. No answer to the question of what change is can be given here. But it is one that we should always bear in mind when working with change.

Reanalysis cannot only be divided into different stages, subprocesses that tend to occur one after another. As mentioned earlier, in his well known article, Langacker (1977) also talks of different types of reanalysis. He defines two main independent types of reanalysis "resegmentation" and "(syntactic/semantic)

⁷⁵ Thanks to April McMahon for bringing this up during one of our discussions (2001).

reformulation”, which can be further subdivided into various subtypes, and often both of these two independent types of reanalysis may be involved in one and the same change.

Resegmentation:⁷⁶	Reformulation:⁷⁷
Boundary Loss	Loss of semantic elements
Boundary Creation	Addition of semantic elements
Boundary Shift	Shift in the value of semantic elements

Table 4.3: Langacker’s (1977) different types of reanalysis.

The first type, *resegmentation*, affects the superficial syntactic structure. The latter, *reformulation*, affects the “more abstract aspects of semantic and syntactic structure.” This at first sounds like a contradiction when read out of context, since Langacker has already said that reanalysis should not affect the surface manifestation of the expression. But, he in fact talks of three levels: the surface level, the superficial (which is what he refers to here) and the more abstract level (Langacker, 1977: 63).⁷⁸

According to Campbell (2001: 149-151), Heine (1993a: 117-119) has recently come to suggest that reanalysis should be abolished from linguistics because of what basically comes down to its inconsistency and vagueness. It is not the first time Heine has pointed out this inconsistency, he also noted it in 1984 (Heine and Reh, 1984: 95) and 1991 (Heine et al., 1991a: 215, 1991b), and he came back to the issue again in 2003 (Heine, 2003). But as Campbell (2001) notes, if inconsistent usage was to decide which terms we use we would have to throw out many more and *grammaticalisation* would go long before *reanalysis*, which he believes has some kind of “accepted core meaning” (Campbell, 2001: 149; cf. Lindström, forth. and chapter three above).⁷⁹ But Heine (Heine and Reh, 1984: 96) has also earlier asserted that he believes that the concept of reanalysis is harder to define than grammaticalisation. So, he is not likely to accept Campbell’s claim that there is a “core meaning” which most people accept nor that grammaticalisation would be among the first to have to go, although I would most definitely agree with the latter

⁷⁶ Langacker (1977: 64)

⁷⁷ Langacker (1977: 79, 82) more difficult to subcategorise, this is only one way of doing so.

⁷⁸ cf. Heine et al (1991a) who complain about Langacker’s reference to surface.

⁷⁹ Here he is actually also arguing against himself, since Harris and he (1995) decide against using the term *analogy* precisely because it has been used in too many different ways.

at least. However, according to Campbell, Heine goes on using the term *reanalysis* in the publication from 1993, even after suggesting that it should be abolished (Campbell, 2001: 150; cf. Vincent, 1995 on regrammaticalisation vs exaptation). If this is true, it is truly unfortunate. However, even though I can see that Heine (1993a) criticises the varied usage of the term – he lists several different ways in which it has been used – I cannot find anywhere where he says that it should be abolished. Although in one of his latest writings he does his best to abolish it and steers clear of using it, except in a discussion of its problems (Heine, 2003).

Heine is not the only one who has objected to the variety in the treatments of reanalysis. Newmeyer (1998: 241-251) has shown some of the variation in the literature as to the order in which reanalysis, semantic change and phonological attrition occur in grammaticalisation. And he has pointed out that some linguists have been quite happy to suggest new definitions of *reanalysis*, which he himself does not agree with (Newmeyer, 1998). And, Newmeyer has also observed that the problem of deciding whether something is reanalysis or not can sometimes reside in which theoretical framework of description one chooses to work in (Newmeyer, 1998: 244-245).

I have said quite a lot about what Campbell sees in other people's work and it is now time to look at his own work on reanalysis again. It appears quite odd that Campbell (2001) should give a case of word order change as an example of reanalysis, without grammaticalisation, even though he has previously said that word order is part of the surface manifestation and should therefore, presumably, **not** be affected by reanalysis!⁸⁰ Campbell's inconsistency at times in this article (2001: 144-145) also shows in the fact that he lists the counterexamples to unidirectionality as reanalysis which does not involve grammaticalisation, even though he has just said that there are counterexamples to the unidirectionality of grammaticalisation which he seems to count as grammaticalisation. Presumably, the reason that he can say this, apparently without thinking he has contradicted himself, is that grammaticalisation to him is primarily reanalysis... Still, he cannot give something as an example of reanalysis that is **not** connected with grammaticalisation, if it has previously been

⁸⁰“... many reanalyses do not involve grammaticalization, e.g. many word order changes, changes from one syntactic structure or morphological category to another of equal or less grammatical strength, and all the instances of counterexamples to unidirectionality mentioned above [...]” (Campbell 2001: 144-145). Compare this to: “Reanalysis changes the underlying structure of a grammatical construction, but does not modify surface manifestation. [...] *Surface manifestation* includes [...] (2) word order” (Campbell 2001: 141).

said to **be** grammaticalisation! However, it could also be that he is in fact only reporting from Hopper and Traugott (1993), whom he refers to in this case, and their claim about counterexamples. But, they do not claim that counterexamples are only examples of reanalysis and not of grammaticalisation, they only claim that they are less prototypical examples of grammaticalisation.

Treatments of word order are extremely interesting in connection to grammaticalisation. Others (e.g., Hopper and Traugott, 1993: 50) have also suggested that word order changes could be seen as reanalysis, whereas according to Meillet (1912; 1921) it could be possible to view them as grammaticalisation. Carlson (1991: 202) mentions that Heine (1976; 1980) and Heine and Reh (1984) both treated the change from SVO to S Aux OV in Mande as reanalysis, via a nominalised clause object:

The erstwhile main verb became the new auxiliary, and the nominalized verb was reanalyzed as the main verb. The placement of the direct object before the nominalized verb is explained as due to the normal order of genitives in Mande languages. It is assumed that the object was syntactically the genitive possessor of the nominalized verb, and as such preceded its head noun. Reanalysis of the nominalized verb as the main verb (concomitant with reanalysis of the main verb as auxiliary) led to the reanalysis of the genitive as the direct object. All constituents remained where they were in the original structure. (Carlson, 1991: 202)

The word order changed at the level of categories and from the point of view of phrase structure even though in the original context for the change, no constituents moved. So, since nothing moves it does not immediately affect the surface and can therefore be seen as reanalysis. In other words, there is a difference between surface word order and abstract word order in the form of phrase structure.

We are back to the question of what constitutes change. I would also like to return to what Harris and Campbell said about reanalysis and surface change (1995: 61):

Reanalysis [...] does not involve any immediate or intrinsic modification of its surface manifestation. This definition [...] is not intended as a claim that changes involving reanalysis cannot additionally involve some modification of surface manifestation (though such modifications necessarily involve mechanisms other than reanalysis).

If word order changes are not reanalysis, then naturally they cannot be used as examples of cases of reanalysis which do not involve grammaticalisation, even if word order changes may **result** in reanalysis or vice versa. This must be clarified, even though it may prove difficult to do so. Perhaps we should say that word order changes are not reanalysis, however they can be triggered by reanalysis. Perhaps they can even be seen as a form of actualisation of reanalysis?⁸¹

That the Mande case of word order change (see above) probably should not be classified as grammaticalisation could be seen as being due to it not clearly expressing any function that was not already expressed. But then should grammaticalisation be viewed as something which aims to express something new grammatically or expressing something more efficiently? It does seem as though the question of whether word order change should be seen as reanalysis only or reanalysis and grammaticalisation, depends on the function of the change.⁸² Always bearing in mind that it can also be questioned whether word order change can be seen as reanalysis, since word order may be seen as part of the surface. As McMahon has mentioned (p.c.) there is also a possibility that we could see word order change as representing a reanalysis of the rules that result in the word orders that we get in a language. However, word order change can occur at two levels. It can be a reinterpretation or reanalysis of the surface constructions or items, giving something a new category label and this need not affect the surface representation. It can therefore be seen as reanalysis. It could also mean that the word order actually changes, that words are moved around, but as a result of the actualisation of the reanalysis. Once again, I ask: what is the change?

In relation to this we should also mention that according to Haspelmath (1998: 327-330) if a word changes its category label to a more grammatical category, this would be a case of grammaticalisation and **not** reanalysis. Therefore in his eyes the Mande example is definitely grammaticalisation, without reanalysis.

⁸¹ But compare above, actualisation of reanalysis has sometimes been seen as more or less the same as analogy or extension. So does that mean that this word order change equals analogy (and reanalysis) or that analogy and word order changes are different possible actualisations of reanalysis?

⁸² Or put differently, on the motivation of the change – cf. Lehmann (search for novelty) and the Neogrammarians (ease of articulation etc). (This section is based partly on a discussion with April McMahon (2001).

Since word boundaries are apparent in the surface manifestation of language, according to Langacker (1977: 62),⁸³ a change which leads to a loss of a word boundary should not count as reanalysis. However, at times it could be argued that Langacker in fact contradicts himself on this point, since he talks of resegmentation or boundary reduction (as types of reanalysis, cf. table 4.3) where words merge into one form (e.g., Langacker, 1977: 69, 103-104).

The new postposition resulting from resegmentation was then attached to nouns or postpositional object pronouns, no longer standing alone as an independent word. (Langacker, 1977: 69)

Other syntactic aspects of signal simplicity pertain to reduction in the status of units from relative independence to relative dependence. Boundary reduction is one type. I place under this rubric a continuum of processes that include the incorporation of independent words as clitics or affixes (i.e. reduction of word boundaries to clitic or morpheme boundaries), the reduction of clitic boundaries to morpheme boundaries, and the loss of morpheme boundaries. Another type is the reduction of a clause to a phrase and a phrase to a word. (Langacker, 1977: 103-104)

Usually changes like ‘a nadder’ > ‘an adder’ are viewed as good basic examples of reanalysis.⁸⁴ But this confusion is perhaps explained through the following statement which Langacker makes:

By boundary coincidence, I simply mean the tendency for different kinds of boundaries to occur in the same position in a string rather than in conflicting positions. One such case is the tendency for morpheme boundaries to occur at syllable boundaries rather than in the middle of a syllable; word boundaries are virtually always morpheme boundaries as well; constituent boundaries are usually (but not always) located at word boundaries. (Langacker, 1977: 111)

This means that a boundary change at a more abstract level may possibly cause a word boundary to disappear or change also, therefore resulting in a surface change even though it was not the surface that was reanalysed.

⁸³ This has been a point of debate in linguistics, and is an issue which has been discussed recently by Sven Öhman (Öhman, online reference).

⁸⁴ cf. Saussure who sees this as analogy (Saussure, 1916 [1966]: 232-234), and Anttila (1972: 93) who sees this as an example of reanalysis and analogy. Cf. also Langacker (1977: 65) who sees this as an example of boundary shift. Campbell (1998: 242-243) sees it as analogy or analogical reinterpretation! Bloomfield (1935 [1969]: 419) sees it as analogy. Cf. also Trask (2000: 210) who calls this *metanalysis* (which he says is morphological reanalysis) or *junction displacement* or *recutting*.

Reanalysis is of course quite difficult to discover since it happens underneath the surface. However, changes that occur afterwards may give us rather clear clues... For instance, it is apparently quite common that elements that disappear, by becoming part of another morpheme, may be reintroduced (sometimes in a different form) after reanalysis has occurred, so that in a manner of speaking an element is repeated. This is exemplified by Langacker, who notes that this “is the clearest evidence one could have that the reanalysis in question has indeed taken place” (Langacker, 1977: 94). One of his examples of this is the following:

When the reciprocal prefix *na- was reanalyzed as part of the postposition *noo in Proto Numic [...], the whole resulting sequence *noo was reattached to *na- to yield the locative reflexive *na-noo; since *na-P remained as an active construction and *-noo was a new postposition, nothing prevented its attachment to the *na- which spawned it. (Langacker, 1977: 93)

Another sign that reanalysis has occurred is when a previously optional item becomes obligatory (Langacker, 1977: 94), although here we come close to, or may even overlap with grammaticalisation. Obligatoriness is one of the distinguishing features that Jakobson sees between grammaticalisation and lexicalisation in his reading of Boas (1971 [1959]).

... those concepts which are grammaticalized and consequently obligatory in some languages but lexicalized and merely optional in others, [...] (Jakobson, 1971 [1959]: 492)

It could of course be the case that Boas saw grammaticalisation as involving obligatorification, **because** it did also involve reanalysis – i.e. it may not be grammaticalisation, but reanalysis that is the cause of the obligatorification. This also fits well with the fact that grammaticalisation in Jakobson (1971 [1959]) is more like a state than a process, it is a description of how things can be codified in language. Jakobson’s grammaticalisation is not the same as what we mean by grammaticalisation, as we will come back to in ch. 6. Rather he is referring to whether something is grammatically expressed or lexically expressed in a specific language.

However, more recent writers have also seen obligatorification as a sign of grammaticalisation (as a process), e.g. Lehmann (1982 [1995]: 139-140).

There may then be a certain freedom in either specifying the category by using one of its subcategories, or leaving the whole category unspecified. To the extent that the latter option becomes constrained and finally impossible, the category becomes obligatory. We shall therefore use the term '**obligatoriness**' as a – more handy – converse equivalent of 'transparadigmatic variability'. Correspondingly, the reduction of transparadigmatic variability will occasionally be called by the neologism '**obligatorification**'. (Lehmann, 1982 [1995]: 139)

It may be worth having a look at the examples that Langacker and Lehmann use, which show that they do mean more or less the same by obligatorification, however Langacker's example at least is of obligatorification at the affix stage whereas Lehmann's occurred while the item was still independent.

... the plural *=mi occurred optionally with the pronoun 'we' in Hopi and Proto Takic whenever this pronoun happened to be initial, but after it was incorporated as part of the pronoun its occurrence was of course obligatory. (Langacker, 1977: 94)

One of Lehmann's examples also involves a pronoun. But, as Lehmann shows, obligatorification may occur earlier than at agglutination:

In Standard Italian the free personal pronouns are used in subject position for emphasis only; there is no tendency to insert them when the sentence otherwise has no subject. In Portuguese there is some sociolectal variation in this respect and precisely this tendency makes itself felt in the substandard sociolects. In French, finally, the subject position must not be left open but must be occupied by a personal pronoun if no NP is there. (Lehmann, 1982 [1995]: 140)

The difference between the two is that in Langacker's example a numeral marker becomes obligatory as part of a pronoun, whereas in Lehmann's example the pronoun becomes obligatory in the construction and what has been called *prodrop* is no longer developed.

Other signs of reanalysis, according to Langacker, could be distributional changes (Langacker, 1977: 94-95). Reanalysis, as other syntactic changes, may also **trigger** new changes, which may serve as indicators of the previous change (Langacker, 1977: 95). Conversely, if we look back at what we said of Lightfoot's

analysis above, according to him reanalysis can be **caused** by many smaller conditioning factors (1979).

Exactly what reanalysis is and what effects it has, not to mention what may cause it, does not seem to be agreed upon, so to that extent Heine (1993a) has every right to be sceptical and critical of its uses. Even though I believe that it is true that we could perhaps talk of a “core meaning” in the sense that Campbell (2001) does, since Langacker’s (1977) definition and article is frequently (read ‘close to always’) referred to, it is also clear that the term can mean many different things, as illustrated by Heine (1993a), and that this is also related to one’s overall framework as argued by Newmeyer (1998).

It always seems as though one of the effects of reanalysis could perhaps be taken to be grammaticalisation. But, as Langacker (1977: 92) says:

Little of a truly systematic character can be said about the effects of reanalysis. The nature of these effects depends on the particulars of each individual case.

4.1.2 Ambiguity as a Prerequisite of Reanalysis

In some ways similar to the belief that analogy sometimes precedes reanalysis is the suggestion that ambiguity or opacity should be a prerequisite. A natural commonsense conclusion we may be able to draw from this is that reanalysis is based on associations of some kind. Timberlake (1977: e.g. 148) suggests that ambiguity is a necessary precondition before reanalysis can occur. Langacker (1977: 110-111) similarly talks of transparency and the aim at an optimal one-to-one relation between form and function as one of the factors at work in language change, such as reanalysis. Lightfoot has likewise claimed that opacity has an important role in language change such as reanalysis.

Typically, changes in various places in the grammar may occur and happen to have the effect of making existing initial structure analyses more opaque to the language learner. There seems to be a tolerance level for such exceptional behaviour or ‘opacity’, and when this is reached a radical restructuring takes place and renders the initial structures more transparent, easier to figure out

and ‘closer’ to their respective surface structures. (Lightfoot, 1979: 129; quoted in Wurff, 1990: 23)^{85, 86}

McMahon (1994) explains Lightfoot’s (1979) views on reanalysis and language change quite clearly. She also makes it clear that reanalysis or radical restructuring is to Lightfoot something sudden and ‘catastrophic’, even though the build up to it may be gradual. With the help of her reading of Lightfoot it also seems as though we can say that his sense of opacity does not equal ambiguity, but rather ambiguity is a form of opacity. In other words, there is a wider group of possible causes of reanalyses in Lightfoot’s eyes than in the eyes of those who claim that there has to be ambiguity before there is reanalysis.

Lightfoot proposes that complexity, opacity or exceptionality may build up in grammar across time, perhaps through such factors as foreign influence or speakers’ attempts to be expressive. [...] Eventually, exceptionality increases to the point where it violates the TP [Transparency Principle] by passing the permitted level of complexity, and at this stage the TP requires a catastrophic change or radical reanalysis in the grammar, making underlying forms conform to surface structures, [...] (McMahon, 1994: 120)⁸⁷

Even though Lightfoot’s views of reanalysis have clearly changed since 1979, the idea of opacity as one of the causes or motivations behind reanalysis appears to remain.

Others have seen ambiguity or opacity only as a possible cause, not as a necessary one. Harris and Campbell (1995: 30, 71-72), for instance, have said that “opacity” is not a prerequisite, but that it can, in the form of ambiguity, lead to grammatical change. But, they think it is also clear that ambiguity is not necessary, because there are examples of reanalysis occurring, even though unambiguous examples have been available.⁸⁸ However, a few years later, Campbell (2001: 141) calls ambiguity an “axiom” of reanalysis, i.e. more than one analysis **must** be possible. Another linguist who believes that reanalysis **can** be caused by opacity is

⁸⁵ Lightfoot (1979) uses the terms *reanalysis* and *radical restructuring/reanalysis* without making any apparent distinction between them.

⁸⁶ Wurff (1990) claims that his own sense of reanalysis corresponds to Lightfoot’s ‘radical restructuring.’

⁸⁷ This, in other words, is comparable to Wurff’s statement (cited above) that reanalysis is preceded by surface changes even though reanalysis does not lead to any changes in the surface manifestation.

⁸⁸ To help the reader understand what reanalysis is Harris and Campbell (1995: 383) include a footnote which relates reanalysis to the biological principle of preadaptation.

Newmeyer (1998: 242-243). However, Newmeyer also recognises other causes: an alternative interpretation might for instance be entailed or might follow from something having been used as an “exploratory expression”. So it seems opacity or ambiguity certainly can play a role in setting off reanalysis, but whether it has to is still not clear.

4.1.3 Reanalysis and Grammaticalisation - Synonyms?

As I observed in the introduction to this chapter, it has been noted that reanalysis and grammaticalisation are sometimes viewed as (near) synonyms.

The boundary between reanalysis and grammaticalization is sometimes not that sharp, however. The two terms have sometimes been used as synonyms or near-synonyms, but mostly they are explicitly kept apart, e.g., in the work of Heine and his associates. For although the two processes are closely related and reanalysis often involves grammaticalization and vice versa, they differ clearly with respect to the unidirectionality principle, which, as has already been mentioned, is an inherent quality of grammaticalization, but not necessarily of reanalysis. (Norde, 1998: 213)

Campbell (2001) has also noted the fact that the border between reanalysis and grammaticalisation has sometimes become too blurred and the concepts have merged into one. He even claims that Heine and Reh (1984) saw them as the same (Campbell, 2001: 143). But it is important to note exactly what Heine and Reh say. First of all, they do not say that they are absolutely synonymous, but only “largely synonymous”. And, if they saw them as absolutely synonymous it seems unlikely that they would have had one chapter on grammaticalisation and one on reanalysis, as in fact they do.

Reanalysis thus appears as a concept which is **largely synonymous** with our term ‘grammaticalization’ (Heine and Reh, 1984: 95, emphasis mine)

Interesting as well as odd is the fact that Campbell (2001: 144), a few pages after claiming that Heine and Reh (1984) see reanalysis and grammaticalisation as the same, recognises that they do in fact distinguish the two on the basis of unidirectionality – an example of Campbell’s occasional inconsistency.

Secondly, they in fact never claimed that **they** thought the two were even largely synonymous. They only say that *reanalysis* had been **used by Lord** (1976) to mean something “largely synonymous” to what **they** call *grammaticalisation* (Heine and Reh, 1984: 95-97). And Lord does seem to mean the same by *reanalysis* as they mean by *grammaticalisation*. Let us have a look at Lord’s and Heine and Reh’s definitions:

Reanalysis:

... a process by which a verb loses ‘semantic, morphological and syntactic properties and survives as a grammatical morpheme marking the relationship between clauses’ (Lord, 1976: 179)

Grammaticalisation:

With the term ‘grammaticalization’ we refer essentially to an evolution whereby linguistic units, lose in semantic complexity, pragmatic significance, syntactic freedom, and phonetic substance, respectively. This is the case for instance when a lexical item develops into a grammatical marker. (Heine and Reh, 1984: 15)

Reanalysis and Grammaticalisation:

In the present paper we wish to distinguish between the evolution of lexical or grammatical morphemes on the one hand and that of syntactic or pragmatic structures on the other. [...] We will reserve the label ‘reanalysis’ to the latter phenomenon, [...]. Although the two tend to be closely interrelated, there appears to be a need for discrimination. A major difference between the two can be seen in the fact that whereas grammaticalization is essentially uni-directional, this does not necessarily apply to reanalysis. (Heine and Reh, 1984: 95)

We must recognise that even if **we** would call the phenomenon that Lord describes *grammaticalisation*, we would also say that reanalysis was involved. So it could be possible that she is only wanting to refer to what we call *reanalysis* (in a rather broad sense). We should also note that she **does** mention the common signs of grammaticalisation: semantic, morphological and syntactic changes. Equally, we should recall that Langacker (1977: 62) said reanalysis affected syntactic, morphological and semantic structure. Lord (1976: 189) also speaks of reanalysis as “a ‘bleaching’ process in which verbs lose meaning and syntactic properties, remaining as grammatical morphemes marking relationships” which comes across as

a good description of a process of grammaticalisation. However, Lord is writing at a time, when grammaticalisation had only recently been revived and therefore varying terminology is quite natural since, for one thing, she may not be familiar with work that talks of grammaticalisation (this we can try to find out through her bibliography⁸⁹). And, even if she was aware of the term *grammaticalisation*, she might not have wanted to use it. Notably, it is not quite clear when the term *grammaticalisation* was actually reintroduced. Givón (1971a) certainly did not use it, however Carleton Taylor Hodge (1917-) (1970) did – but Hodge’s paper does not appear to have been very widely read, or at least not widely referred to. The first mention of it that I have come across was in Lehmann (1982 [1995]).

Furthermore, Heine and Reh (1984) note that Givón (1979a), has used the term *reanalysis* in a similar fashion to Lord (1976). He (1979a: 243) recognised that a change from a clitic to an “automatic agreement marker” could be called *reanalysis*. Apparently he also uses this term to refer to development among discourse constituents (Givón, 1979a: 209; cited in Heine and Reh, 1984: 95). However, in that same publication he also speaks of *grammaticalisation*, *syntacticisation* and *morphologisation*, so to him *reanalysis* might mean the same as it does to us today.

To show that Heine and Reh (1984) did not want grammaticalisation and reanalysis to be used as synonyms, they explicitly distinguish between the two primarily on the basis of unidirectionality, as noted by Campbell (2001), in the end. Another sign that they definitely do not want the two to be seen as one is to be found in their definitions of the two phenomena, which are not identical, and as mentioned above they also devote one chapter to each of them.

Heine et al (1991a: 215) also distinguish between grammaticalisation and reanalysis on the basis of unidirectionality, as I noted above that Norde (1998), for instance, has done. But Heine et al (1991b; 1991a) confuse us by claiming that there are **alternative** terms to *grammaticalisation* such as *reanalysis*, which can be understood as though they saw them as synonymous:

A number of alternative terms like reanalysis [...], syntacticization (Givón, 1979[a]: 208ff), semantic bleaching (see 2.3.1), semantic weakening

⁸⁹ From the bibliography in this article it seems as her likely sources for this concept may be Givón (1972a; 1972b), and possibly Li and Thompson (1973). *Grammaticalisation* as I noted above and as has been noted by Hopper and Traugott (1993) was not used by Givón in his early work on the concept.

(Guimier, 1985: 158), condensation (Lehmann, 1982:10/11), reduction (Langacker 1977: 103-107), subduction (Guillaume, 1964: 73-86), etc. are occasionally used as synonyms or near-synonyms, but in most cases they refer to specific aspects, like semantic or syntactic characteristics of grammaticalization. (Heine et al., 1991b: 149)

Considering the last part of the quotation it seems as though they do in fact want to distinguish the two, and do in this quotation only call attention to the fact that others have used the two in a manner, which would suggest that they are synonyms, more or less. Further on in their article (Heine et al., 1991b, section 3.1), it becomes clearer that this is what they meant to say:

In some other works “reanalysis” has been used as a near-synonym for grammaticalization (cf. Lord, 1976: 179), i.e. for the development from lexical to grammatical entities. (1991b: 167)

In (1991a) we never need to hesitate about their own view of the possible or impossible synonymy of grammaticalisation and reanalysis, since they clearly state that:

Perhaps one of the most spectacular effects that conceptual manipulation has on language structure can be seen in the reanalysis of linguistic structures. Some authors, therefore, have gone so far as to use the terms grammaticalization and reanalysis as synonyms or near-synonyms. One of them is Carol Lord (1976: 179), who refers to the development from lexical to grammatical entities as “reanalysis”. (Heine et al., 1991a: 215)

In Heine et al (1991b; 1991a) it is also clear that they do not accept Langacker’s definition of reanalysis completely, but would prefer to use the term *reanalysis* to refer “essentially” to what “falls under what Langacker (1977: 79) calls ‘syntactic/semantic reformulation’” (Heine et al., 1991b: 167):

We more or less adhere to this definition [i.e. Langacker’s (1977: 58)], although there are some problems with it. The term “surface,” for instance, is defined in a peculiar way, and, assuming that a watertight definition is possible, the question is whether indeed that “surface manifestation” remains unaffected by reanalysis.⁹⁰ (Heine et al., 1991a: 215-216)

⁹⁰ They note that Langacker is not absolutely clear in his discussion of the term *surface*. “He defines the ‘surface level’ as the ‘phonemic level of representation, together with indications of word

Campbell (2001) claims that Heine et al's view of reanalysis differs from the 'standard'. By "the standard definition" I assume Campbell is referring to Langacker's definition, which he appears to treat as the default or core meaning of reanalysis. We can therefore note that this difference is something that Heine et al call attention to on more than one occasion (see above) themselves.

The most "idiosyncratic" view of reanalysis according to Campbell (2001) is Haspelmath's (1998), who denies any connection between reanalysis and grammaticalisation, primarily as a way of proving that grammaticalisation is not an epiphenomenon.

... the large majority of syntactic changes are instances of 'pure' grammaticalization and should be explained within the framework of a theory of grammaticalization, without reference to reanalysis. A minority of syntactic changes are due to reanalysis, and they must be explained in different terms. Grammaticalization and reanalysis are disjoint classes of phenomena. (Haspelmath, 1998: 315) (Also cited in Campbell, 2001: 145, but the page reference is not correct there.)

The only explanation of how this could be possible, according to Campbell (2001: 145, 148), is if Haspelmath (1998: 330) were to change the meaning of *reanalysis*, and this Haspelmath also admits to having done:

The gradualness of word-class changes is recognized by some of the authors who describe these changes as reanalysis: Hopper & Traugott (1993: 111) note that in grammaticalization 'clear categorial discreteness is not in evidence, only a cluster of relationships on a cline from lexical to grammatical form', and Kortmann & König (1992: 684) even describe (15) as showing 'different degrees of reanalysis'. Clearly, these authors use the term *reanalysis* in a much broader way than I, meaning roughly 'any change in the structural description of a phrase'. I have no disagreement with these authors, but I would prefer to reserve the term for the narrower concept. (Haspelmath, 1998: 330)

boundaries, but with no indication of constituent structure or boundaries smaller than word boundaries (such as morpheme or clitic boundaries)' (Langacker, 1977: 61). The term *boundary* forms a key concept in Langacker's discussion, one of the two types of his syntactic reanalysis, which he calls *resegmentation*, is concerned entirely with boundaries. It remains unclear why, in particular, word boundaries belong to the surface level while all other types of boundaries do not" (Heine et al., 1991a: 275-276). It is likely that this has something to do with Langacker thinking that we are more aware of word boundaries than of other boundaries.

4.1.4 The History of Reanalysis

Harris and Campbell (1995) treat some of the history of reanalysis and grammaticalisation and syntactic change in general. They believe that these changes have had rather a long history with links back to the modists and earlier (Harris and Campbell, 1995: e.g. 16, 24). They also recognise the concept of reanalysis in the work of e.g., Bopp, Paul and Brugmann. Interestingly, Paul (1970 [1920, 1898]: 115, 299) is said to have called reanalysis *abweichende Neuerzeugung*, which he saw as “a principal mechanism of change” and perhaps more importantly in this chapter – as part of analogy (Harris and Campbell, 1995: 31)!

I have however not been able to find any clear discussion of reanalysis in the place that Harris and Campbell refer to in Paul, nor have I found the term *abweichende Neuerzeugung* on either of the two pages that they refer to (1920: 115, 299). Lyle Campbell (p.c. 2003) also regrets that he himself cannot find it there and thinks that it must probably have come from a different edition of Paul. Their reference to Paul is also a bit confusing, but Campbell appears to think that it was the 1920 edition they referred to (Campbell, p.c. 2003). However, although they say that they worked with the fifth edition, they also say the 1970 printing and the edition that was printed in 1970 in Tübingen was according to COPAC the eighth edition of a student edition (Paul, 1970), and in addition in square brackets they also include reference to the third edition from 1898.

Paul does discuss changes which could be classed as grammaticalisation and often also reanalysis today. For instance, he mentions examples of grammaticalisation such as how a reflexive pronoun has become a passive derivative in Scandinavian languages (1920: 237), and on a few occasions he also treats the development from a more lexical word to a function word (1920: e.g. 369-370), for instance, how prepositions and conjunctions have arisen for instance out of adverbs (1920: 369-370). (This will be treated more in chapter 5.) However, he also has examples of a change that is usually classed as reanalysis, but not grammaticalisation, nowadays, namely the development of new affixes whereby they adopt part of another suffix or part of the stem: *-assus* (e.g. *gudjinassus*) > *-nassus* (Paul, 1920: 245). Another example of reanalysis and grammaticalisation can be found on p. 299, which Harris and Campbell also refer to, where Paul discusses how

pronouns have been reanalysed as conjunctions and how two words have been reanalysed as one (Paul, 1920: 299).

Brugmann (1925: 7) is, by Harris and Campbell (1995: 31), said to have recognised three paths of grammatical change which all included reanalysis in a modern linguist's eyes. However, that does not make it clear what the concept of reanalysis (though not under that term) was seen to be, and how it relates to the present concepts of grammaticalisation and reanalysis. I would also say that what he lists are syntactic changes, although Harris and Campbell seem to draw a very thin line between what is syntactic and what is grammatical, cf. above in section 4.1.1. It is however true that there appears to be a concept similar to reanalysis underlying the statement about the three changes in Brugmann (1925):

Die so entstandene Gliederung des Satzes erfuhr mancherlei Schicksale hinsichtlich der Beziehungen, die zwischen den einzelnen Bestandteilen der Gruppe und zwischen ganzen Gruppen bestanden. Ich hebe hier dreierlei hervor. 1. Die Verschiebungen in der syntaktischen Gliederung. Teile von Gruppen werden mit Teilen von andern [sic.] Gruppen zu einer neuen Gruppe zusammengefaßt, und durch dieses Durchbrechen der ursprünglichen Gliederung bilden sich neue Konstruktionsweisen heraus. 2. Zwei gleichzeitig gebrauchte Konstruktionen werden gemischt, so daß eine dritte entsteht, die Bestandteile von beiden enthält. 3. Wörter, die in einer syntaktischen Beziehung zueinander stehen, für die es kein anderes Ausdrucksmittel gibt als etwa die Stellung im Satz oder die Betonungsart, werden nach Möglichkeit in formelle Übereinstimmung miteinander gebracht. Sogenannte Kongruenz.⁹¹ (Brugmann, 1925: 7)

None of these three types of change seem anywhere near grammaticalisation, except possibly the third kind (formal agreement arising between words in syntactic relation), although the fact that they can count as reanalysis even to this day seems clear.

It can be concluded that unfortunately, as far as the relationship between grammaticalisation and reanalysis goes, we are still at more or less the same stage as we were nearly twenty years ago, when Heine and Reh stated:

⁹¹ Translation: The sentence division that arose witnessed many fates with regard to the relations that existed between the single parts of the group and between the whole groups. Here I emphasise three kinds. 1. The shift in the syntactic division. Parts of groups are united with parts from other groups into a new group, and through this break of the original division new means of construction are created. 2. Two concurrently used constructions are mixed, so that a third arises comprising parts from both. 3. Words, which stand in a syntactic relation to one another, for which there are no other means of expression, such as the position in the phrase or the stress, are brought in formal agreement with one another if possible. So-called Kongruenz.

At the present stage of research it is still unclear how the relationship between grammaticalization and syntactic reanalysis is to be defined. (Heine and Reh, 1984: 97)

It seems possible that perhaps they used to be part of one concept and perhaps that would have been an easier and less confusing categorisation. But often it seems that the more we can specify something, the more categories we adopt (whether we are sure about the exact boundaries for them or not), the more we think we will be able to understand about the world, or, as in this case, about language.

4.2 Analogy

As I mentioned earlier in this chapter, analogy was treated by Meillet as a process parallel to grammaticalisation. He saw these two processes as the only processes that could lead to new grammatical forms:

Toutes les formes régulières de la langue peuvent être qualifiées d'analogiques; car elles sont faites sur des modèles existants, et c'est en vertu du système grammatical de la langue qu'elles sont recréées, chaque fois qu'on en a besoin. [...] alors l'analogie produit des formes nouvelles, indépendantes de la tradition. Et c'est dans des cas qu'on parle d'ordinaire de *formes analogiques*; il serait plus juste de dire : *innovations analogiques*.⁹² (Meillet, 1912; 1921: 130-131)

However, as noted in section 4.1, Hopper and Traugott (1993) are of a different opinion. In their eyes analogy is a **mechanism**, not a process, and on a par with reanalysis, rather than grammaticalisation (which to them is some form of process). Furthermore, they rather unjustly imply that this is what Meillet must also have meant (Hopper and Traugott, 1993: 32-62, esp. 48, 56).

Most linguists probably remember their first contact with analogy as being either in a lecture on linguistics in Ancient Greece or a lecture on the Neogrammarians. Analogy is clearly a very old concept, but it is also a term which

⁹² Translation: All regular forms of a language can be modified analogically; since they are created using existing models, and it is by virtue of the grammatical system of the language that they are recreated, every time there is a need for them. [...] thus, the analogy produces new forms, independent of the tradition. And it is in the case when one normally speaks of *analogical forms*; that it would be better to say *analogical innovations*.

has been widely applied in a less than homogeneous fashion, which probably easily happens with a term that is so closely connected with associations and relations. Sometimes, perhaps, we associate a bit too widely from the basic meaning and use of the term itself!

Analogy is a matter that has been debated by many others, and, although it does truly deserve further study, an in-depth study will not fit into the scope of this thesis. This section will rely heavily on what others have had to say on analogy and will only provide a brief summary with some parallels and comparisons to grammaticalisation.

4.2.1 *Analogy – Terminology and Concept*

There are almost as many suggestions for classifying analogical change as there are linguists who have written on the topic, but certain main lines repeat themselves. (Anttila, 1977: 66)

Anttila is clearly right in that there have been many different views on analogy, and he is most likely also right in saying that “certain main lines” have been repeated, although he does not mention which characteristics he sees this to be. We will see below how some linguists have used the term *analogy*.

Harris and Campbell (1995) only use the term *analogy* in one short section in their book and not under any of their three mechanisms of change. They mention it when considering reconstruction and the “devastating” effects analogy can have on syntactic reconstruction. It seems as though they consider it closely together with reanalysis, presumably because Lightfoot (1979: 164), for instance, has suggested that it can be based on reanalysis. However, Harris and Campbell (1995: 97-119) also speak of something which they have chosen to call *extension*. This is one of their three mechanisms of change, and, as was briefly mentioned above, this shows clear similarities to analogy (and also actualisation of reanalysis). They define it as follows:

Extension is a mechanism which results in **changes in the surface manifestation of a pattern** and which does not involve immediate or intrinsic modification of underlying structure. (Harris and Campbell, 1995: 51, emphasis mine)

This definition is extremely similar to Hopper and Traugott's definition of analogy:

Analogy, strictly speaking, **modifies surface manifestations** and in itself does not effect rule change, although it does effect rule spread either within the linguistic system itself or within the community. (Hopper and Traugott, 1993: 32, emphasis mine)

Hopper and Traugott speak of it not as not changing the rules, and instead Harris and Campbell speak of it as not modifying the underlying structure.

Harris and Campbell certainly realised that what they were calling *extension* was similar to what others had chosen to call *analogy*. However, they wanted to distinguish extension from analogy, primarily because analogy had been used in so many different ways:

Extension is not the same as analogy, though extension might be seen as part of analogy as traditionally defined in the linguistic literature. Since the term analogy has been used to cover so many different sorts of phenomena, we choose to avoid the term altogether. We speak here rather of "analogues," by which we mean a condition where a structural similarity exists between two (or more) items, or classes, or constructions, etc. The existence of the analogue often stimulates change through extension, but it may also prompt change through reanalysis or through borrowing; [...] (Harris and Campbell, 1995: 51, emphasis mine)

In other words, they simply change terminology in order to make it clearer. They redefine *analogy* and invent a new term for some of the changes which have often been referred to as analogy by others. This is perhaps even clearer in Campbell's (2001: 142) later recognition of the fact that he and Harris (Harris and Campbell, 1995) meant the same by *extension* as Hopper and Traugott (1993) meant by *analogy*. Isolating a small section, subtype, of what *analogy* can be used to apply to, is seen as a method of clarification in Harris and Campbell (1995). Consequently, the meaning of *analogy* or *analogues* in their work appears to be somewhat closer to the original sense of analogy, a sense of a relation of similarity, rather than a form of change.⁹³

⁹³ We should compare this discussion to the discussion above (section 4.1.1) of Campbell's (2001) criticism of Heine (1993a) for suggesting that *reanalysis* has been used to heterogeneously, a suggestion Campbell meets by pointing out that if we are to stop using the term *reanalysis* for this reason, *grammaticalisation* would also have to go.

It is good and indeed helpful to see the thought of analogues, as opposed to analogical **change**, expressed in words, because in some ways it often seems to be hidden at the back of treatments of analogy. Analogues or analogy, and reanalysis are frequently discussed as though they are part of each other or at least linked to one another, e.g. that an ‘analogue’/analogy exists and therefore reanalysis can occur, and this may in the end mean that we have a case of grammaticalisation... (see section 4.1.1 above).

Further evidence that Harris and Campbell mean the same by *extension*, as others mean by *analogy*, is apparent in something else that they say, which we can compare perhaps to Sturtevant’s paradox in phonology (i.e. “sound change is regular but creates irregularity, while analogy is irregular but creates regularity” (McMahon, 1994: 91)). They claim that syntactic reanalysis often (but certainly not always) leads to a more complex grammar, and that extension can rid the grammar of “exceptions and irregularities”, i.e. extension sounds much like McMahon’s (1994: 90-91) and Sturtevant’s ideas of analogy. Notably, the term *complex grammar* in conjunction with reanalysis, can also lead us to conclude that analogy should simplify grammar, since it is seen almost as an opposite to reanalysis here. This idea of analogy as grammar simplification has however been quite firmly dismissed (see 4.2.3 below).

In order to compare reanalysis and analogy, as well as extension and analogy, let us think back to the Finnish example that Timberlake (1977) and Campbell (1998) used (see also section 4.1.1):

Stage One: Reanalysis

näen miehe-m tule-va-m (Old Finnish)

I.see man-ACC.SG come-PTC-ACC.SG

“I see the man who is coming”

näen miehe-n tule-van (Modern Finnish⁹⁴)

I.see man-ACC.SG come-PTC

OR

I.see man-GEN.SG come-PTC

“I see the man who is coming”

⁹⁴ A sound change has changed final *-m* to *-n*.

Stage Two: Extension (or analogy)

näin venee-t purjehti-va-t (Old Finnish)

I.saw boat-ACC.PL sail-PTC.ACC.PL

näin vene-i-den purjehti-van (Modern Finnish)

I.saw boat-PL-GEN sail-PTC

(adapted from Campbell, 1998: 228-229)⁹⁵

Hopper and Traugott (1993: 56) criticise Meillet's definition of analogy as rather narrow, as they take up the old quest for a reason for why a particular member should be chosen as a model. They see a promising light however in the work of Kuryłowicz (Kuryłowicz, 1945-1949; 1949 [1973]) and Kiparsky (Kiparsky, 1968; cf. also 1974) (Hopper and Traugott, 1993: 57), who both attempted, however in different ways, to clarify analogy, or to explain how it works, Kuryłowicz by his six tendencies and Kiparsky by defining analogy as rule extension (in phonology). Transferred to syntax and morphology, the latter suits Hopper and Traugott quite well, since they see reanalysis as the only mechanism that "can create new grammatical structures", whereas analogy is claimed to often be "the prime evidence [...] that a change has taken place" – since it extends to cases which have not simply been reanalysed, but which also show surface change (Hopper and Traugott, 1993: 57). However, it seems as though they are not quite sure what analogy is, and they treat it as though it can mean slightly different things depending on what they need it to mean, making it clear that Harris and Campbell (1995) were correct in claiming that it has been used to mean many things.

Although analogy is best viewed as rule generalization, in practice it is often useful to maintain the term "analogy" when referring to certain local surface developments. (Hopper and Traugott, 1993: 59)

Other than Kuryłowicz, Manczak (1958) also tried to formalise how analogy happened, and which form was likely to be the model the change would take after, all in an attempt to make analogy less of a haphazard change. According to McMahon

⁹⁵ This example illustrates how ambiguity has arisen through sound change and lead to reanalysis in that the ending can then be interpreted as either accusative or genitive in the singular. Later this is extended to the plural.

(1994: 80) Kuryłowicz and Manczak sometimes make opposite predictions (cf. Manczak, 1958). Perhaps that is a sign of the difficulty in formalising and classifying these changes, as a consequence of their heterogeneity.

G. E. R. Lloyd (cited in Anttila, 1977: 40; Lloyd, 1966: 172) claimed that there had “never been general agreement in the definition of analogy” (Anttila, 1977: 40). In fact, we could note that this is much like in grammaticalisation and reanalysis. Which means that we are dealing with three terms with rather fuzzy histories of definitions and usage.⁹⁶

There seems to be close to general agreement that analogy is something that is quite universally used in language, in some way. According to McMahon (1994: 92) Leonard Bloomfield (cf. 1935 [1969]) and Hermann Paul saw analogy as a major mechanism in language acquisition (cf. Anttila, 1977: 43). Furthermore, Henning Andersen (1973) has related analogy to the innovative logical process of abduction, in language acquisition (1994: 92). Similarly, Anttila (1977: 18) said that analogy “feeds on abduction”, and with abduction Anttila also believes there is a chance of proving that all change is actually analogical, because he believes that “all change and learning must go through abduction” (Anttila, 1977: 20), which as we have already noted is believed to feed analogy. However, that it feeds analogy should not have to mean that it exclusively feeds analogy – perhaps it can also lead to other changes, e.g. reanalysis. Abduction also shows links to reanalysis – as noted by Traugott and Heine (1991a), for instance. But, perhaps the explanation to this is that reanalysis should basically be seen as a form of analogy, as suggested by Paul (1970 [1920, 1898]) according to Harris and Campbell (1995)? (cf. section 4.1.4)

The conclusions McMahon (1994: 96) draws regarding analogy may at first seem rather disappointing, and discouraging. She says that “[w]e have seen, then, that analogy is a strong force in linguistic change, but one which eludes formalisation.” But we should not give up because of this, instead McMahon believes we should keep fighting to find tendencies and laws and constraints, just like Kuryłowicz (1949 [1973]) and Manczak (1958; 1980) did:

However, this failure to entirely predict or explain analogy should not force us to abandon the concept. Instead, we should follow Kuryłowicz and Manczak in attempting to constrain the phenomenon, and also explore its

⁹⁶ Anttila himself also notes some confusion around the meaning of *analogy*, as can be seen in the first quote of this section (see also Anttila, 1977: 66).

connections both with surface-structure conditions like iconicity, and higher-order principles, including Humboldt's Universal and abduction. (McMahon, 1994: 96)

We should note that, similarly to Harris and Campbell's (1995) statement that Paul saw reanalysis as a form of analogy, Vincent (1974) tries to prove that analogy is useful by claiming that the useful form of analogy which is useful has also been called *reanalysis*:

... there is an area of language change for the understanding of which some concept of analogy is essential. This area has been given various names – analogy, false analogy, reanalysis, abduction – which one may consider of greater or less felicity, but the problem is not thereby eliminated. (Vincent, 1974: 436, emphasis original)

In many ways it feels as though grammaticalisation may be close to impossible to specify, define and pin down, and it is certainly impossible to predict. Reanalysis, however, may be possible to formalise to some extent at least and this is probably why there appears to be a preference to talk about reanalysis among formalists. But, as McMahon says about analogy, non-predictability and fuzziness does not mean that we should “abandon the concept”, neither analogy nor grammaticalisation. We must try to pin them down as far as we can, because we believe that they are important for our understanding of language change. This is true even if they turn out to be epiphenomena, as some formalists (in particular) have claimed of grammaticalisation now that they have finally dared to start tackling the topic.

4.2.2 The History of Analogy

As mentioned earlier (4.0), analogy has generally been presumed to have part of a controversy about language and its origins in ancient Greece (see e.g. Seuren, 1998; Taylor, 1986, for certain reservations regarding this). The big question was how language was constructed. And the presumed debate came to stand between people who could be classified as either analogists or anomalists. The main difference between these two camps was that the analogists claimed that language was regular, whereas anomalists argued for its irregularity (Robins, 1997: 25-28).

Analogy in ancient Greece was a “relation of similarity” (Anttila, 1977: 16):

‘Analogy’ was the Greek word for the mathematical proportion that in linguistics came to mean ‘regularity’, in contrast to anomaly (‘irregularity’), and it was translated into Latin as *proportio*, *ratio*, *regula sermonis* or kept as *analogia*. Analogy, or pattern, or structure, was thus the dominating principle guiding language learning, use, and saying new things not said before. (Anttila, 1977: 25, italics original)

Proportion is also a term that is often used for a diagrammatic explanation of a certain type of analogy that is sometimes still used today.

$$A:B = C:D$$

stān: stānes = sunne:X

$$X = sunnes$$

(McMahon 1994: 72)

réaction: réactionnaire = répression: X

$$X = répressionnaire$$

(Saussure, 1916 [1966]: 225)

The formula is used as a way of illustrating how an analogical creation could appear, i.e. what it could be based upon.

Later in history analogy became important to the Neogrammarians who saw it as one of the two major sources of change (sound change being the other). According to the Neogrammarians, it was also the basis of creativity in language and Vincent (1974: 427) claims that Neogrammarian analogy can be compared to the “generative grammarian’s system of rules”, in the sense that it explains the creativity of language users.

Subsequent students of the Neogrammarians’ work have however often noted a tendency among the Neogrammarians to treat analogy as a bit of a waste bin. In their study of phonological change they wanted to show that phonological change was regular, following exceptionless sound laws. But when irregularities and exceptions were found which they could not explain, these were explained away as analogical changes which had been caused by the forms ‘imitating’ other forms or expressions so to speak, on the basis of an association of some kind between the two forms or

expressions. The theory of regularity, notably, had become the theory which explained irregular change, changes that went against the sound laws. Was this when analogy started to be seen as a type of change, rather than an association pattern which could lead to change?

... the Neogrammarians saw sound change and analogy as interacting but opposing forces, a view summed up clearly in Sturtevant's Paradox, which says that sound change is regular but produces irregularity, while analogy is irregular but produces regularity. Analogy is therefore seen as a kind of housekeeping device, which resignedly picks up at least some of the mess made by the more impetuous sound change as it hurtles blindly through the grammar. (McMahon, 1994: 70)

Some nineteenth century scholars, including some Neogrammarians⁹⁷, discussed something they called *false analogy*, meaning “sporadic infractions of the regular sound laws” (Vincent, 1974: 428). It is of course this sense of analogy that has led many modern scholars to view analogy as a waste bin category (or ‘housekeeping device’), and this is one of the things that generative grammarians caught on to:

The latter categories (i.e. analogy and borrowing – NBV), in particular analogy, tend to become terminological receptacles devoid of explanatory power – catchalls for irregularities in the operation of ‘regular sound laws’. (King, 1969: 127; cited by Vincent, 1974: 429)

We can compare this to Saussure's *Cours* where he in fact praised the Neogrammarian treatment of analogy. Saussure worked in the historical comparative framework, and had been schooled in the Neogrammarian framework. Like them he saw analogy and sound change as the two factors in language which are at work in “evolution” (Saussure, 1972 [1985]: 223; 1983: 162):

Les premiers linguistes n'ont pas compris la nature du phénomène de l'analogie, qu'ils appelaient « fausse analogie ». [...] C'est l'école néogrammarienne qui pour la première fois assigné à l'analogie sa vraie place en montrant qu'elle est, avec les changements phonétiques, le grand facteur

⁹⁷ Saussure appears to have denied that Neogrammarians saw analogy as *false analogy* (see below).

de l'évolution des langues, le procédé par lequel elles passent d'un état d'organisation à un autre.⁹⁸ (Saussure, 1972 [1985]: 223)

We should compare this briefly to Meillet, who we noted above saw analogy and grammaticalisation as the two forces of language change. However, we also need to mention that Saussure was primarily concerned with individual words and sounds here. Elsewhere, he also spoke of agglutination in a sense which we could see as reflecting part of the process we might want to call *lexicalisation* and/or *grammaticalisation*:

L'agglutination consiste en ce que deux ou plusieurs termes originellement distincts, mais qui se recontraient fréquemment en syntagme au sein de la phrase, se soudent en une unité absolue ou difficilement analysable.⁹⁹ (Saussure, 1972 [1985]: 242)

Agglutination to him, like grammaticalisation to Meillet, is a factor which plays a role alongside analogy in language change. There are also other minor factors alongside sound change, analogy and agglutination, such as onomatopoeia and popular etymology, that he considers to be of little, if any, importance in comparison, (Saussure, 1983: 175). By comparison to what we mean by *grammaticalisation*, I understand Saussure's *agglutination* to mean a kind of reanalysis in the form of univerbation, which could happen in something that may be classified either as grammaticalisation or lexicalisation. Saussure (1972 [1985]: 246-247; 1983:178-179) later deals with something which is definitely a form of grammaticalisation, namely a passage from full lexical word, to preposition, to affix, etc. This he distinguishes from the previous factors of change that he has dealt with, which mainly concern word formation, whereas this concerns syntax. It seems that, although part of (what we would see as) grammaticalisation operates on the same 'level' as analogy, grammaticalisation itself does not in Saussure's work. Agglutination is the process

⁹⁸ Harris' translation: The early linguists failed to understand the nature of analogical phenomena, which they described as 'false analogy'. [...] The Neogrammarians were the first scholars to assign analogy to its rightful place, by showing that it is, along with sound change, the main factor in the evolution of languages, and the process by which they pass from one state of organisation to another (Saussure, 1983: 162). (However, we should note that Saussure speaks of **langues** and not of **langages**.)

⁹⁹ Harris' translation: Agglutination occurs when two or more terms originally distinct, but frequently joined together syntagmatically in sentences, merge into a single unit which is either unanalysable or difficult to analyse. (Saussure, 1983: 175)

which does and it is therefore something we recognise primarily as a **form** of reanalysis.

Bloomfield's (1935 [1969]) work on analogy gives us a rather clear idea of how analogy came to be seen as a form of change, rather than just a relation of similarity or proportion.

A vast number of such instances, from the history of the most diverse languages, leads us to believe that the analogic habits (§16.6) are subject to displacement – that at a time when the plural of *cow* was the irregular form *kine*, the speakers might create a regular form *cows*, which then entered into rivalry with the old form. Accordingly, this type of innovation is called *analogic change*. Ordinarily, linguists use this term to include **both the original creation of the new form and its subsequent rivalry with the old form**. Strictly speaking, we should distinguish between these two events. (Bloomfield, 1935 [1969]: 405, emphasis in bold mine)

We should also recognise that Bloomfield's concept of analogic change is rather broad, including morphological, semantic and also syntactic changes (Bloomfield, 1935 [1969]: 406-407). And, like Harris and Campbell (1995), he also uses the word "extension" for this change (Bloomfield, 1935 [1969]: 407).

Like grammaticalisation, analogy has been around for a long time, and it seems to have gone in and out of popularity much like grammaticalisation. It seems as though analogy reached a state when it was used rather fuzzily and very heterogeneously sometime shortly after the Second World War.¹⁰⁰ Some linguists looked at this topic and saw the confused state it was in and wanted to try to resolve the tangle. This aspect of the history of analogy parallels quite well that of grammaticalisation. *Grammaticalisation* today has reached a similar state. It has become widely studied and used, and been given a similar importance to that given to analogy by the Neogrammarians. However, the danger of a wider, more common usage is vagueness and dissimilar treatments and uses. This appears to have happened to analogy and has also happened to grammaticalisation.

Interestingly, we can see that people seem to be reaching similar conclusions about the two phenomena of change, grammaticalisation and analogy, in attempting to clarify them. One of the people who attempted to save and explain analogy was Kuryłowicz. He had realised that no matter how much we try to pin down what

¹⁰⁰ Based partly on a discussion with April McMahon (2001).

makes analogy happen, we will never be able to do this. What we can do is tell what is likely to happen **when** it occurs, because then there are only a few options:

Il résulte d'un système grammatical concret quelles transformations « analogiques » sont possibles (formules I – V). Mais c'est le facteur social (formule VI) qui décide si et dans quelle mesure ces possibilités se réalisent. Il en est comme de l'eau de pluie qui doit prendre un chemin prévu (gouttières, égouts, conduits) *une fois qu'il pleut*. Mais la pluie n'est pas une nécessité. De même les actions prévues de « l'analogique » ne sont pas des nécessités. Etant obligée à compter avec ces deux facteurs différents, la linguistique ne peut jamais prévoir les changements à venir.¹⁰¹ (Kuryłowicz, 1949 [1973]: 85-86)

We can compare this to grammaticalisation, which also appears to be difficult, or impossible, to pin down. However, like with analogy, we can predict what is likely to happen if grammaticalisation sets in, especially if we believe in the unidirectionality hypothesis (see chapter 3).

Kuryłowicz's metaphor is also similar to one used by Cowie (1995: 190) to explain grammaticalisation, and why it usually occurs in the same direction.¹⁰² Summing up the apparent similarities, we note that Kuryłowicz says analogy is like rain whose fall we cannot predict, although we can predict roughly which routes it will take when it does fall. Likewise, Cowie says of the process of grammaticalisation that it is like water in a river. We can predict that it will follow the rest of the water down the river, however every now and then we will come across a pothole which will cause some of the water to whirl around whilst most of it carries on down the river.

Water metaphors for language change seem quite popular. Lightfoot, for instance also says that:

Individual languages are on their own odyssey through time, and they are liable to change, sometimes dramatically; some of those changes reveal something about the nature of the language or about languages in general. [...] Heraclitus believed that everything is in flux, and consequently that we

¹⁰¹ Translation: It is the result of a concrete grammatical system that 'analogic' transformations are possible. (formulae I – V). But it is the social factor (formula VI) that decides if and to what extent these possibilities will be realised. It is thereby like rainwater which must take a predetermined route (gutters, sewers, pipes) *when it does rain*. But the rain is not a necessity. In the same way the actions predicted by analogy are not necessary. Being obliged to take into account these two different factors, linguistics can never predict the changes to come.

¹⁰² This was mentioned in ch. 3.

can never step into the same river twice, because things are always different. Now we may adjust our metaphor and think of a language's odyssey as flowing water. Water has intrinsic properties of its own, but it flows chaotically, not exactly the same from minute to minute. Sometimes water flows down a riverbed through rapids, over weirs and waterfalls; the Greeks called these things *catastrophes*. (Lightfoot, 1999: ix)

All three metaphors indicate a general feature of change – the fact that change cannot be predicted, but that we can often list the most likely (or the only) options for its path **if** it were to happen. This is not something that makes grammaticalisation and analogy particularly similar. Nevertheless it is still interesting to note, because in one sense it is highly significant. These two statements indicate a major problem that linguists have found with both of these processes, or phenomena or factors of change, namely that they cannot be predicted. We cannot say when or if they will occur. We cannot say why they occur at one time and not at another. We can only say what is likely to occur when they happen, or how they are likely to proceed after they have taken place. But even this is not definite – although some people would make it sound as though it is, e.g. through the Unidirectionality Hypothesis. This problem of predictability can be a major problem for someone who is attempting to show that linguistics is a science just like natural sciences, and who believes that this means our tools and formulations must be as exact and easily confirmed through experiments as in the natural sciences.

Anttila (1977: 1-2) claims that analogy lost ground around 1960 and became rather unpopular in linguistics, with some generative grammarians actually banning it. Around the mid-1960s transformational grammarians (e.g. King, 1969; Kiparsky, 1965; Postal, 1968) attacked analogy according to Anttila (1977: 16). Anttila (1977: 16) asserts that Chomsky also claimed (1966: 12) analogy was a “vague metaphor”, and earlier (1964) he is to have claimed that metaphor was only a “semi-grammatical phenomenon”. So what then was the status of analogy?

This was a reaction against the Neogrammarians and American structuralists who had attributed the creative aspect of language to analogy, one of the reasons for the generativists being that analogy is supposedly such a surface phenomenon. (Anttila, 1977: 16)

Of course, seeing as generativists explained language through deep structure, surface phenomena were not really important. However, they also claim that change

happens during language acquisition and surely surface structures are our data in acquisition.

Nevertheless, Anttila says that analogy lived on anyway, but partly under other names. It is clear that analogy was discussed by people working in the generative framework, such as Kiparsky, but it was generally discussed as a means of dismissing it.

In the 1970s, like grammaticalisation which was presumably revived at the beginning of that decade, analogy started to gain ground once more, albeit apparently still under other names. Anttila (1977: 3, 44) puts down the revival of analogy partly to the reintroduction of morphology and function in linguistics, something which occurred in the early 1970s. The early 1970s was a time of increased dissatisfaction with the transformational-generative framework and many people tried to move away from it (cf. Givón, 1979a) and it was also a time when morphology raised increasing interest.

In the mid-1970s the defenders of analogy were definitely back and Vincent (1974: 436) was one of those who did his best to make himself heard on behalf of analogy. Among other things, apparently, he divided people in linguistics into two camps, depending on how they had treated analogy, as occurring in competence or performance (cf. Anttila, 1977: 39):

Competence

Kuryłowicz
Kiparsky
Vennemann

Performance

The Neogrammarians
Sturtevant
Bloomfield
Manczak
Andersen
Anttila

The reason why analogy has been approached in these two different ways is, according to Vincent, that analogy in fact, lies somewhere between competence and performance. He sees it more as some form of “link” between them (cf. Anttila, 1977: 39; McMahon, 1994: 84). This, Antilla (1977: 39) says, is very much like what Saussure said, because he apparently had “analogy exactly between langue and parole” – which we all know are similar to competence and performance in some ways, the main difference being the individual nature of competence – performance, as opposed to the social, collective nature of langue - parole.

Clearly, even though analogy has been disproved of, rejected and seen as mysterious and unreliable, similarly to grammaticalisation (to paraphrase Meillet) it has never disappeared completely. And, just like *grammaticalisation* as a term, it has been used in a very diverse fashion.

4.2.3 Simplification?

In the latter half of the twentieth century, analogy was often seen as a kind of simplifying change.

If we accept the weaker interpretation of analogy as something that merely points the direction of possible change, then analogy becomes superfluous because simplification is enough. This weaker version of analogy would claim that *foot* and *child*, if they give up their old plurals, will become *foots* and *childs*. But since this is already predicted by simplification, analogy is unnecessary. For this reason, as well as for the others advanced in this section, we reject analogy. Grammar and simplification are enough. (King, 1969: 133-134)

However, McMahon (1994: 83), for instance, observes that analogy can also lead to complications of grammar:

Kiparsky (1978) and King (1969 [...]) associate analogical extension with simplification of the structural description of a rule, and analogical levelling with simplification of its structural change. (McMahon, 1994: 81-82)

Although some cases of analogical levelling and extension can be analysed as resulting from rule simplification or rule reordering, the discussion above should indicate that the Generative view of analogy is not always well supported by the facts. Furthermore, analogy may complicate, rather than simplify the grammar, ... (McMahon, 1994: 83)

As McMahon (1994) seems to admit, and as Vincent (1974) clearly shows, there are cases which could possibly be seen as grammar simplification. Vincent starts by questioning whether all cases of what we may want to call analogy, can be seen as grammar simplification, as King (1969) was seen to have claimed in the quote from McMahon above (Vincent, 1974: 432, 435). He thinks it seems as though there are certain changes that are similar to simplification but which still cannot be counted as that:

... there are cases very similar to those just mentioned which seem not to be instances of grammar simplification. Consider the attraction of the third declension neuter nouns with nominatives in -us – e.g. pectus, tempus, corpus – to the second declension. Here we are faced with a change apparently very similar to the previous one, but note that in this instance there is no pressure to close down the third declension, which remains fully productive throughout Romania. (Vincent, 1974: 432, underline original)

Clearly then we know that both language and change may be elusive and that we will never be 100 per cent sure of anything about language change. We will certainly never be able to predict it. Still most linguists appear to agree that there is a point in attempting to understand change as well as we possibly can, since it can tell us a lot about language. Therefore, it does not matter too much if we were to prove that grammaticalisation is an epiphenomenon as some have claimed, since (as at least some of them admit) it can still have a heuristic value. As Vincent (1974) says:

In language history, as in other types of history, there are quirks of fate which we cannot hope to explain. As Lass (1969) has pointed out, our task as historical linguists is, armed with a knowledge of the way synchronic linguistic systems work, to elucidate how a given language will accommodate a given historical accident, and it is at this second level of enquiry that the goal of developing predictive theories of language change should be pursued. (Vincent, 1974: 437)

4.3 Summary and Conclusions

The links between grammaticalisation and analogy appear to rest in function and functional grammar. They are both supposedly based in the subconscious world of associations, on metaphor and/or metonymy, a world difficult to tackle from a formal angle and therefore both were unpopular during the transformational-generative heyday, but started fighting their way back with the counter-formalism movement of ‘neo-functionalism’ in the 1970s.

It is not clear exactly what the links between analogy, grammaticalisation and reanalysis are. It seems that analogical patterns can lead to reanalysis, but it also seems reanalysis can lead to analogy. In fact there may often be a kind of cyclic pattern to their interaction. Furthermore, both of these, either in conjunction or separately, may lead to grammaticalisation.

It seems then as though grammaticalisation is seen as being on a different level to analogy and reanalysis in most current work on linguistics. However, this certainly is not true further back in the history of linguistics, where it has been seen as parallel to analogy (see e.g. Meillet, 1912) – and I have suggested that it is possible that reanalysis was not recognised as a distinct mechanism, but only as part of what was then called *grammaticalisation*. But, as we have seen above, we now recognise that grammaticalisation may involve reanalysis and we also recognise that there are cases of grammaticalisation which are not cases of reanalysis and vice versa.

Neither analogy nor grammaticalisation nor reanalysis can be predicted, but of the three it is reanalysis that is easiest to formalise. It was, at least partly, the problem of non-predictability that led people to want to dismiss analogy at one time and this can also be seen as a problem in work on grammaticalisation (cf. Givón, 1975, see also discussion in chapter 3). These are three indistinct concepts, three terms which have been used to mean slightly or more drastically different things by different people.

It remains to be asked – whether all kinds of change could be seen as analogy as suggested by Anttila (1977). Perhaps so, but that clearly depends on how we define analogy first of all, and it also depends on our feeling about categories. As Jespersen (1922 [1949]: 388, 391) says, “man is a classifying animal”. We seem to believe that the more we classify, the easier it will be to learn things, the easier it will be to further our knowledge and understanding of things. It therefore seems unlikely that we would want to see all change as analogy, even if it were possible.

Volume II

PART 3: THE HISTORY OF GRAMMATICALISATION

5. EARLY TREATMENTS OF GRAMMATICALISATION (THE NINETEENTH CENTURY)

5.0 Introduction

This chapter will present the findings from my studies of work done between the early nineteenth century (from Bopp's *Über das Conjugationssystem* (1816)) and Meillet's article (1912). The reason that I shall start with Bopp (1816) is that it has sometimes been seen as the first application of agglutination theory. However, I will actually go back further than Bopp's *Über das Conjugationssystem* since Bopp, and others who will be mentioned, occasionally dealt with earlier publications and I have considered it necessary to look at these too whenever possible. I have also included a look at Friedrich von Schlegel's famous publication from 1808, which also predates Bopp (1816). This I have included partly because of references to it in later publications, and partly because it was such an important text during the nineteenth century, a text that discussed different types of languages, as well as the Indian traditions that were to become so important in the development of Western thinking.¹⁰³

This chapter will present traces of thinking in concepts similar to and/or connected with what we would call *grammaticalisation* today, in the works of earlier

¹⁰³ Some scholars born in the nineteenth century will be discussed in the next chapter (chapter 6) because their most important work, as far as this thesis is concerned, was published during the twentieth century.

linguists. It will also include some discussion of what may have influenced these linguists to start thinking of language change in this way.

A table (table 5.1.1) will be included with a list of scholars with similar concepts to what we today call *grammaticalisation*, and possible terms that they may have used for closely related notions and for the mechanisms and processes related to grammaticalisation; e.g. reanalysis, semantic bleaching, phonological attrition and unidirectionality.

The chapter is likely to overlap to some extent with both previous and subsequent chapters at this point. Four main areas will be taken into account: Britain, the United States of America, Scandinavia and Germany. Germany is included here as one of the main areas due to its importance in nineteenth century language research. There will also be some comments regarding the situation in France, where Meillet had a strong position in the early years of the twentieth century.

After the first part where I discuss different linguists from the regions specified (section 5.1), I will sum up by presenting a final discussion regarding the influences and developments during this time, and discuss the similarities between the ideas of this period and the study of grammaticalisation at present (5.2).

5.0.1 Early Treatments of Grammaticalisation during this Period.

I would like to remind the reader once more, before I start looking at various linguists who were active in this period, that what I am looking for in their work is a notion of either:

- (1) the process of becoming part of grammar, of being entered in a grammar, or of changing in grammatical function.
- or
- (2) the process by which a word becomes a clitic, a clitic an affix, and an affix a synchronically unanalyzable part of another morpheme; including the processes of phonological reduction and semantic bleaching

Comparative linguistics was initiated in the early years in the nineteenth century with the work of Rasmus Kristian Rask (1787-1832), Jacob Grimm (1785-1863) and Franz Bopp (1791-1867). We also find that it is around the beginning of the nineteenth century that some, e.g. Lehmann (1982 [1995]), have recognised some of the earliest known predecessors to work on a concept very similar to grammaticalisation.

Lehmann starts his book by naming two eighteenth century figures, Étienne Bonnot de Condillac (1714-1780) and John Horne Tooke (1736-1812), as forerunners to these kinds of ideas. However, the next scholars to be mentioned are August Wilhelm von Schlegel (1767-1845) and Wilhelm von Humboldt (1767-1835), and later in his book Bopp is mentioned. Humboldt and Schlegel are referred to by Lehmann (1982 [1995]: 1) as some of the first evolutive typologists and he recognises that Schlegel in fact used some of the most common examples of grammaticalisation as early as 1818 in *Observations sur la langue et la littérature provençales* (1818) (Lehmann, 1982 [1995]: 1). But Schlegel is also to have noted the bleaching and expansion that words go through in this kind of process (cf. Lehmann, 1982 [1995]: 1). We will come back to this shortly (5.1.3).

Humboldt has been seen as the person who introduced the theory that later became called *agglutination theory*. This is also mentioned by Lehmann (1982 [1995]: 2-3), who gives a brief overview of Humboldt's work in relation to grammaticalisation. Lehmann also notes that Humboldt mentions semantic bleaching, like A. Schlegel, and in addition Humboldt recognised that phonological attrition tended to play a part in this form of change (but he did not use these words). When agglutination theory was first applied Lehmann believes that it was applied to the move towards stage III in Humboldt's evolutive typology. Humboldt's four stages of development of "grammatische Bezeichnung" are in the words of Lehmann (1982 [1995]: 2):

- I. "grammatische Bezeichnung durch Redensarten, Phrasen, Sätze": grammatical categories are completely hidden in the lexemes and in the semant syntactic configurations.
- II. "grammatische Bezeichnung durch feste Wortstellungen und zwischen Sach- und Formbedeutung schwankende Wörter".
- III. "grammatische Bezeichnung durch Analoga von Formen": here the "vacillating words" have been agglutinated as affixes to the main words. The resulting complexes are not "forms", unitary wholes, but only "aggregates", and therefore mere "analogs to forms".
- IV. "grammatische Bezeichnung durch wahre Formen, durch Beugung und rein grammatische Wörter". (cf. Humboldt, 1822: 54f.; [Humboldt, 1825 [1905]], on which Lehmann (1982 [1995]) bases this)

But it seems the sense of the term widened and that this is why Jespersen (1922 [1949]) finds the term not quite suitable and decides to replace it (cf. Lehmann, 1982 [1995]; Lindström, 2003 (forth)).

As I said above Lehmann (1982 [1995]: 3) also mentions Bopp, who is seen as the first one to have applied agglutination theory and who in fact published on the subject before Humboldt did. Lehmann notes that Bopp and Humboldt appear to have discussed this subject in letters they exchanged. Unfortunately, Lehmann does not tell us which letters he has consulted (if any) but he seems sure that Bopp and Humboldt did discuss these ideas in personal correspondence. But it is perhaps important to note that Bopp's main example is that personal endings of verbs in Indo-European can be derived from autonomous personal pronouns – an idea which Lehmann (1982 [1995]: 2) claims had then already been expressed by Condillac (1746; 2001).

Lehmann (1982 [1995]) also brings up Karl Brugmann (1849-1919), and August Schleicher (1821-1868) who are both said to have had ideas related to the agglutination theory. Furthermore, Lehmann (1982 [1995]: 3-4) mentions Georg von der Gabelentz's (1840-1893) introduction of the ideas of two forces to explain the reason we have a phenomenon like agglutination. He is also said to have contributed the idea of cyclic / spiralic change to studies of phenomena such as grammaticalisation (cf. Gabelentz, 1891). We shall return to this more below (5.1.5).

The next linguist to be mentioned by Lehmann (1982 [1995]: 4) is from the twentieth century. However, he was also active as a linguist in the nineteenth century. This was the historical linguist Antoine Meillet (1866-1936), who Lehmann and most others who are active in the field of grammaticalisation sees as the man who introduced the term *grammaticalisation*. This idea of Meillet as the coiner seems to have been first mentioned by Vincent (1980) and then by Lehmann (1982 [1995]), and has since spread and become accepted as general knowledge. We shall return to this issue below in section (5.2).

5.0.2 Agglutination Theory, Historical Grammar and Grammaticalisation

As I see it, it is important that we look at the general ideas of the time, especially in the countries which I aim to cover; in other words Britain, the United States, Scandinavia and, to some extent, Germany. I will attempt to give an overview of the ideas of some of the most important linguists in these areas at the time – focusing primarily on the scholars who worked mostly on the history of language, typology (which as Lehmann (1982 [1995]) notes was evolutive rather than synchronic at this

time), word formation and etymology. I will attempt to do this country by country, scholar by scholar, which means that I will have to jump back and forth in time. The scholars will be arranged by year of birth, which unfortunately might make the presentation even less chronological, but it still seemed like the most reasonable way of organising the data.

5.1 Germany

5.1.1 Wilhelm von Humboldt (1767-1835)

Wilhelm von Humboldt (1767-1835) has often been seen as one of the important scholars in the history of grammaticalisation (see e.g. Lehmann (1982 [1995]: 2), Heine et al (1991a: 6-7, 9) and Hopper and Traugott (1993: 18-21; 2003: 19-21)). His views clearly played a role in the development of agglutination theory, as suggested by Lehmann (1982 [1995]: ch. 1) for instance. However, he has also been claimed, by Wellander (1968: 141, see also 6.4.2 below), to have used the term *Grammatikalisierung*, although I have found no proof of this.

Humboldt (1822: 291) recognises a similarity between case endings and prepositions. He also claims that the grammatical signs cannot be both grammatical signs and content words at the same time (1822: 292), seemingly indicating that some form of ‘isolation’ or ‘split’ must have occurred between the two. It seems that what others have called *isolation* must first occur.

Humboldt believes that we can still tell that the grammatical forms in many of the most developed languages originated through *agglutination* (he uses this term) of meaningful syllables (1822: 295). He thinks agglutination could almost be the general way of creating new grammatical forms (1822: 295), which he considers to be clear from the signs languages use for grammatical categories:

Anfügung, oder Einschaltung bedeutsamer Silben, die sonst eigne Wörter ausgemacht haben, oder noch ausmachen,
Anfügung, oder Einschaltung bedeutungsloser Buchstaben, oder Silben, bloss zum Zweck der Andeutung der grammatischen Verhältnisse,
Umwandlung der Vocale durch Uebergang [sic] eines in den andren, oder durch Veränderung der Quantität, oder Betonung,
Umänderung von Consonanten im Innern des Worts,

Stellung der von einander abhängigen Wörter nach unveränderlichen Gesetzen,
Silbenwiederholung. (Humboldt, 1822: 295)¹⁰⁴

The most natural and suitable way of inflecting words, according to Humboldt, is however the attachment and insertion of **meaningless** elements, as well as the change of vowels and consonants (1822: 296). The most important and common of these, he claims, is *Anfügung* ('attachment') (1822: 299).

He also mentions yet another means of marking grammatical relations, one which was not usually treated together with agglutination during the nineteenth century, namely grammatical words such as prepositions and conjunctions (1822: 298, 302-303):

Es kommt aber zur Agglutination und Flexion auch noch eine dritte, sehr häufige Bildungsart hinzu, die man, da sie immer absichtlich ist, in dieselbe Classe mit der Beugung setzen muss, **nemlich wo der Gebrauch eine Wortform ausschliesslich zu einer bestimmten grammatischen stempelt**, [...] ¹⁰⁵ (Humboldt, 1822: 298, emphasis mine)

In accordance with Horne Tooke, whom he also refers to, Humboldt believes that these words seem to have originated in real words, words that denote objects (1822: 303).

Touching on the connection between grammatical forms and the development of language through different types, Humboldt notes that the further away from its origin a language is the more rigid is the bond between the affix and the main word (1822: 301). He also talks of this as a process of 'melting', which we will see also in some later scholars (1822: 301, cf. also 306, 308). This is yet another reason for him to conclude that *Anfügung* must have been the primary method used by all languages in their development of grammatical markers (1822: 301).

¹⁰⁴ Translation: Addition, or embedding of meaningful syllables, which used to make up own words, or which still do,
Addition, or embedding of meaningless letters, or syllables, only for the reason of [giving] a hint of grammatical conditions,
Change of vowels through the transition of one into another, or through a change in quantity, or stress,
Change of consonants inside the word,
Positioning, according to unchangeable laws, of words which are dependent on one another,
Reduplication of syllables.

¹⁰⁵ Translation: There comes however a third very common type of formation to agglutination and flexion, one which one must place in the same class as inflection, as it is always conscious, namely where the use of a word form is exclusively assigned to a particular grammatical [form], ...

In Humboldt we also see a recognition of some form of continuum. He asserts that the form words move closer and become affixes (1822: 306), which in connection with the rest of his writings means that we can get a cline from lexical content words all the way to affixes. But he actually says that at first one can still see where the word ends and the affix begins. It could be that he in fact recognises an intermediate step of clitics (cf. 1822: 306).

Similar to recent grammaticalisation research, Humboldt also noticed that the development of grammatical markers tended to be related to a certain weakening of the meaning and phonological attrition (1822: 306):

Dies wird wohl erleichtert durch verloren gehende Bedeutung der Elemente, und Abschleifung der Laute in langem Gebrauch.¹⁰⁶ (Humboldt, 1822: 306)

Humboldt does appear to have a sense of grammaticalisation similar to that of recent scholars. However, this makes him rather special during the nineteenth century, since others at this time usually did not link agglutination to the development of form / function words.

5.1.2 Karl Wilhelm Friedrich von Schlegel (1772-1829)

Friedrich Schlegel's (1772-1829) work is not usually mentioned in histories of grammaticalisation. However, both his brother and other scholars in the early decades of the nineteenth century who have often been mentioned in connection to grammaticalisation refer to him in their work.

The only linguistics work that F. Schlegel is known for is *Über die Sprache und Weisheit der Indier. Ein Beitrag zur Begründung der Alterthumskunde* (1808).¹⁰⁷ It was in this book that F. Schlegel introduced the two-class typology which was later to be expanded by both A. Schlegel and Bopp. He believed that there were basically two types of languages – inflectional and affixing/isolating – and he included Sanskrit, as well as most of the other modern Indo-European languages in the group of affixing/isolating languages (Schlegel, 1860). However, there were some Indo-

¹⁰⁶ Translation: These are surely simplified through the loss of meaning in the elements, and attrition of the sounds through long use.

¹⁰⁷ Other works by F. Schlegel are mainly concerned with literature and philosophy. After he moved to Paris in 1802 he started teaching philosophy and he became interested in studying Oriental studies, the latter of which led to the book mentioned here (website: *Karl Wilhelm Friedrich von Schlegel*).

European languages, e.g. Greek and Latin, that were inflectional, but which also used prepositions, although this was seen as an imperfection (Schlegel, 1860: 444).

It is important to note that *inflection* according to F. Schlegel meant an internal change in the root, used to indicate a grammatical relation. He never discusses how these forms came about, whereas the affixes which are used in the affixing/isolating type of languages are put down to particles and auxiliaries that have merged with the root. Sometimes these have merged to the extent that we almost think that they are inflectional forms (1860: 448). F. Schlegel (1860: 448) also claims that sometimes “the foreign particles inserted may be no longer traceable,” which resembles the notion of demorphologisation which has been discussed by Hopper and Traugott (1993: 164-166) and others.

F. Schlegel did not believe there were any means of looking further into the origins of the affixed forms. This he said “cannot be carried out without the assistance of an etymological skill and subtlety which must be unhesitatingly rejected in every scientific investigation or historical contemplation of the origin of language” (1860: 445). But still he believed that affixation has often given rise to inflections, although never in the Indian language Sanskrit (1860: 445). This statement also gives some indication of the general unhappiness with etymology at this time. Etymology had lost ground due to the many extremely farfetched examples of this type of scholarship in the past, but also due to the rise of natural sciences and the need to be scientific – in other words to use exact methods. Something which is still problematic today. For instance, some might question whether reconstructions can be seen as scientific.

Unlike his brother, as we shall see below (section 5.1.3), F. Schlegel does not appear to have had much trouble in thinking that languages can move from one type into the other. He believes that Greek and Latin are inflectional but that they also use some prepositions and that the modern languages of the Indo-European family make use of both affixes and auxiliary words. He also sees the possibility of moves from using affixes to mainly making use of auxiliaries and prepositions (1860:448-449). However, he believes that language was close to perfect from the beginning:

Such was the origin of language; simply beautiful in form and construction, yet capable of almost unbounded development; the union between the primitive roots, on which it is based, and the grammatical construction are

most closely cemented, and both spring from the same original source – a deep feeling, and a clear discriminating intelligence. (Schlegel, 1860: 455)

F. Schlegel's ideas clearly rest, to a large extent, on the idea that language was originally monosyllabic; consisting only of simple monosyllabic roots, which later were joined together. This idea was common at the time and related to the Sanskrit learning which had recently come into vogue.

5.1.3 August Wilhelm von Schlegel (1767-1845)

A couple of years after Bopp (1816) and before Bopp's *Analytical Comparison* (1820) was released, August Wilhelm von Schlegel (1767-1845) published a study of the origin of Provençal (1818). This brief study was written in French and mainly presents comments on the work *Choix des poésies originales des Troubadours* (Raynouard, 1816-1821), which we will look at below (section 5.2.2).

François-Juste-Marie Raynouard (1761-1836) was a scholar who had impressed A. Schlegel by picking up a thread that he himself had aimed to follow up and write about, namely the history of the formation of the French language (although Raynouard mainly dealt with the language of the troubadours) (Schlegel, 1818: 22). A. Schlegel was very pleased that someone else had thought of writing about this topic apart from himself. However, he did not agree with Raynouard's, in his eyes, exceedingly positive view of the development of languages from synthetic to analytic, and there were apparently some other points of disagreement too (Schlegel, 1818: 22, 25):

Il me semble cependant que M. Raynouard exalte un peu trop les avantages des langues analytiques. Plusieurs théoristes ont comparé le mérite relatif des langues anciennes et modernes, et Adam Smith donne la préférence aux langues modernes. Je l'avoue, les langues anciennes, sous la plupart des rapports, me paroissent bien supérieures. Le meilleur éloge qu'on puisse faire des langues modernes, c'est qu'elles sont parfaitement adaptées aux besoins actuels de l'esprit humain dont elles ont, sans aucun doute, modifié la direction.¹⁰⁸ (Schlegel, 1818: 25)

¹⁰⁸ Translation: It seems to me however, that M. Raynouard exalts the advantages of analytical languages a little too much. Several theorists have compared the relative merit of the ancient and the modern languages, and Adam Smith gives preference to the modern languages. I have to admit that the ancient languages seem to me far superior in most respects. The highest praise one can give to modern languages is that they are perfectly adapted to the present needs of the human mind, whose direction they have undoubtedly modified.

As noted by Lehmann (1982 [1995]: 1), A. Schlegel indeed does use some of the examples that have become paradigm examples of grammaticalisation – the articles, the future, the negation. He believes that the definite article can be derived from a demonstrative pronoun, the indefinite article from the numeral ‘one’ and that the French indefinite pronoun *on* derives from the noun *homme* (possibly through influence from German), and he also treats the use of auxiliaries to form past and future forms of verbs through periphrasis, the formation of the Romance future through affixation of *avoir* and the origin of the copula (or *verbe substantif*) (Schlegel, 1818: 28-29, 33, 34).

But not only that, A. Schlegel also notes the semantic bleaching (although not called that) that words go through in the development of analytic grammars:

C’est une invention en quelque façon négative, que celle qui a produit les grammaires analytiques, et la méthode uniformément suivie à cet égard peut se réduire à un seul principe. On dépouille certains mots de leur énergie significative, on ne leur laisse qu’une valeur nominale, pour leur donner un cours plus générale et les faire entrer dans la partie élémentaire de la langue. Ces mots deviennent une espèce de papier-monnaie destiné à faciliter la circulation.¹⁰⁹ (Schlegel, 1818: 28)

A. Schlegel notices the move from the specific to the more abstract (1818: 29-30). He also mentions that analytic languages make much use of auxiliary words, however he does not really treat their origin (1818: 30). And as in Raynouard (see further 5.2.2) we see some early signs of something similar to what Meillet (1912; 1915-1916 [1921]) was to call *renouvellement*. A. Schlegel’s example of this is of how pronouns and prepositions have had parts reinforced by other pronouns and prepositions:

Au lieu d’ALIQUIS, on disoit ALIQUIS-UNUS; au lieu de QUISQUE, QUISQUE-UNUS: ce qui s’est contracté ensuite en *aucun, chacun; assez* ne dit pas plus que SATIS; cependant il est formé de AD-SATIS: *dedans* signifie INTUS; mais il est formé de DE-DE-INTUS. Il y a une foule d’exemples de cette espèce, et qui ne laissent pas de sentir un peu la barbarie.¹¹⁰ (Schlegel, 1818: 30)

¹⁰⁹ Translation: The innovation which gave rise to analytical grammar is, in a way, a negative one, and the method uniformly followed in this respect can be reduced to one single principle. One strips some words of their meaningful energy, one leaves them with only their nominal value, in order to give them more general currency and to bring them into the elementary part of the language. These words become a kind of paper money destined to facilitate circulation.

¹¹⁰ Translation: Instead of ALIQUIS one said ALIQUIS-UNUS, instead of QUISQUE, QUISQUE-UNUS: that which was later contracted into *chacun, aucun; assez* does not say any more than SATIS; however it is formed from AD-SATIS: *dedans* means INTUS; but it is formed from DE-DE-INTUS. There is an abundance of examples of this kind, which does not give rise to [only] a little barbarity.

But not only that, this section also shows implicitly that A. Schlegel was aware that phonological attrition was part of this type of change.

A. Schlegel's (1818: 33) treatment of the Romance future is accompanied by clear references to Raynouard (cf. 5.2.2). He claims that the future forms, which he points out have not been derived from the Latin futures, are formed regularly through a composition of the infinitive of the (main) verb and the present tense form of the auxiliary *avoir*. However, from what he says, it seems that the auxiliary functions as a clitic in the language that Raynouard and A. Schlegel treats primarily, viz. old Provençal (or what it is now often called Occitan). Schlegel says that in the writings in Provençal the auxiliary is often separated from the main verb (*verbe principal*) by other words. He claims that this was first noted by Abbé [François Seraphin] Regnier [Desmarais] (1632-1713) (Regnier, 1706: 368f), and later cited by Jean Baptiste (de la Curne) de Sainte-Palaye (1697-1781) (1818: 33, 95). Bopp, as we shall see below (5.1.4), also mentioned Curne de Sainte-Palaye, and in fact their references are to the same publication by Curne de Sainte-Palaye, a paper published in *Mémoires de l'Académie des Inscriptions et Belles-lettres* (Curne de Sainte-Palaye, 1756 [1751]: 684).

Developing his brother's typology, A. Schlegel lists three types of language: languages without grammatical structure, languages that use affixes and languages with inflections (Schlegel, 1818: 14, 85). Interesting from a grammaticalisation point of view is the fact that Schlegel claims that he is *not* sure whether languages can move from one type to another (1818: 86). But he believes that inflectional languages can be of two types and that they may move from one subtype to the other, the two subtypes being analytic and synthetic languages (1818: 16):

L'origine des langues synthétiques se perd dans la nuit des temps; les langues analytiques, au contraire, sont de création moderne: toutes celles que nous connoissons, sont nées de la décomposition des langues synthétiques.¹¹¹
(Schlegel, 1818: 16)

But A. Schlegel, unlike his brother (according to Bopp (1820: 20) at least), realises that generally there are no purely synthetic or analytic languages (Schlegel, 1818: 17). He thinks the reason that we sometimes find that languages start to move

¹¹¹ Translation: The origin of synthetic languages disappears in the night of time, conversely analytic languages are modern creations and all these of which we know were born out of the decomposition of synthetic languages.

towards analytic forms, is that two synthetic languages have come in contact with one another (1818: 21-22).

Analytic languages are defined as languages that make use of circumlocutions in the form of using an article before nouns, personal pronouns before verbs, auxiliary verbs to form new tenses of verbs, prepositions rather than case forms, languages which express comparison through adverbs rather than inflection, etc, all examples that were also mentioned by Rask (1811; 1818a; 1818b), as we shall see below (5.5.1) and which make it clear that at this time both A. Schlegel and Rask were aware of a similarity in function or meaning between different forms of expression.¹¹²

The reason that A. Schlegel sees for the changes from synthetic to analytic differ somewhat from today's views, in that he appears to put this down mainly to language contact and in fact primarily to the influence that the Germanic tribes have had on the Provençal language (cf. 1818: 33-34). This is a difference between his and Raynouard's views, as Schlegel notes himself (1818: 34). Raynouard (1816a: 45, 83) doubted that Germanic (*théotisque*) grammar could have had any influence on the use of *aver*, *esser* as auxiliaries or definite articles in the Romance languages, something which may bring Raynouard closer to present theories of grammaticalisation than Schlegel was.

Finally, we can note that like Bopp both before and after A. Schlegel's *Observations* was published, A. Schlegel speaks of roots of words. However, unlike Bopp he does not appear to see roots as something rather abstract. Instead he says that in the first type of languages, words are (sterile) roots (Schlegel, 1818: 14).

5.1.4 Franz Bopp (1791-1867)

In 1816, Franz Bopp (1791-1867) published *Über das Conjugationssystem der Sanskritsprache in Vergleichung mit jenem der griechischen, lateinischen, persischen, und germanischen Sprache* (Bopp, 1816), in which he discussed his theory of roots for the first time. There he also discussed his idea that the personal endings of verbs could be derived from personal pronouns (cf. Bopp, 1816). A few years later he published an essay in English with some thoughts on this, *Analytical Comparison* (1820; 1974). In this essay, which may have been the first introduction

¹¹² However all examples were not mentioned in all three publications by Rask, see 5.5.1.

for many English and American scholars to the ideas of Bopp, he briefly relates his notions of roots. Bopp believed that all words are derived from monosyllabic roots, a theory which he claims to have got from Indian grammarians (1820: 8; 1974: 19). Because of this idea, Bopp believes that grammatical modifications can only be expressed by “foreign additions” and never through changes to “original materials”, in other words internal changes (1820: 10; 1974: 20).

We must expect that in this family of languages the principle of compounding words will extend to the first rudiments of speech, as to the persons, tenses of verbs, and cases of nouns, &c. (Bopp, 1820: 10; 1974: 20)

This statement appears to include the basic ideas behind grammaticalisation, and the basic prototypical examples of grammaticalisation.

Bopp believes that his views of the origin on grammatical forms is opposed to those of Friedrich von Schlegel who had claimed (Schlegel, 1808; cf. 1860) that languages can use one of two methods to express grammatical relations, inflection (which in his eyes meant an internal modification) or suffixes. F. Schlegel also claimed that the old Indo-European languages made use of the former, which Bopp found wrong, since first of all he believed that languages could use both methods, and secondly he believed that suffixation was the method used most in **all** languages (1820: 10; 1974: 20).

In 1845 an English translation appeared of Bopp’s *Vergleichende Grammatik* (Bopp, 1845) in an attempt to inform British scholars of “the full extent of the progress which has been effected, and the steps by which it has been attained,” which up until then had been “imperfectly appreciated by this country [Britain]” (1845: i). Through comparison of different related languages, Bopp aimed to find out more about the origin of grammatical forms (1845: v). In this publication as elsewhere, he distinguishes first of all between two types of roots, verbal roots (which nouns and verbs are derived from) and pronominal roots (which give us pronouns, all **original** prepositions, conjunctions and particles) (1845: 96). These ideas are similar to those discussed by Indian grammarians, but they had believed that there was only one type of roots, viz. verbal. Like the Indian grammarians, however, Bopp also strongly believed that roots were monosyllabic (1845: 96-97).

Bopp is certain that the personal endings on verbs stem from personal pronouns, but he does not claim to be the first to have had this idea. He claims, for

instance, that Sheidius (presumably referring to Everard(us) Sheid(ius) (1742-1794)) had shown that this was true of Greek (1820: 11, cf. 15-16; 1974: 21, cf. 24). Bopp also believes that verbs can become used in a more abstract sense, such as in his example of the Spanish copula *estar*, derived from the Latin verb for 'sitting, lying'. But he also goes further and claims that this verb may form part of verbal inflections. One example that he uses is the verb form *potest*, where *pot-* is said to be the attribute, *es-* the copula, and *t* the subject (1820: 13-14; 1974: 22-23). This may remind us of ideas of the Port Royal in the mid-seventeenth century and of Bishop John Wilkins (1614-1672), even though, as Robins has pointed out, it is admittedly very different still from that in that there is a difference between explaining something logically or structurally and explaining something historically although these are occasionally confused (cf. Robins, 1997: 138, 140-145):

Nouns and verbs are integrals; in Wilkins's systematisation, which is more explicit and worked out as part of his philosophical grammar, verbs are not given a separate class, but are regarded as nouns adjective (active, passive or neutral (intransitive)) always in association with or containing in their own form a copula (e.g. *lives = is living; hits = is hitting*). This analysis is similar to that of the Port Royal grammarians. (Robins, 1997: 138, emphasis mine)

Verbs are properly words that 'signify affirmation' and, in other moods, desire, command, etc. This returns the Port Royal grammarians to **an analysis suggested by Aristotle of all verbs other than the copula, to be, as logically and grammatically equivalent to this verb plus a participle**, making *Peter lives (Peter is living)* structurally analogous with *Peter is a man*; the categories of intransitive and transitive (and active and passive) are said properly to belong not to the words commonly called verbs but just to the 'adjectival' element in them.

This analysis, it should be noted, is not an alleged historical explanation, nor is it a surface description of verbal morphology, as Bopp was later to try to make it; it was the positing, in modern terms, at a deeper structural level, of elements that in actual sentences were represented conjointly with other elements. (Robins, 1997: 142, emphasis mine)

Bopp recognises that there has been a move in some languages from synthetic to analytic, and he also mentions the continuous bleaching in the semantics of affixes:

It is the genius of modern language to express, by separate words, what in ancient languages was united into one body. In Sanskrit, Greek, and Latin, &c. the pronouns, for instance, are suffixed to the verb, but in English, German, French, &c. they are placed separately before, and where the

pronouns, formerly united with the verb, have left some remaining traces, they have lost their significance. (Bopp, 1820: 24; 1974: 30)

Bopp also uses the example of the French future, where he claims that forms of the auxiliary verb *avoir* ‘(to) have’ are attached to the infinitive (1820: 46-47; 1974: 46), just as we do today. This particular example is used to back up his own idea that the future forms in Sanskrit can also be derived from the attributive root plus the ‘substantive’ verb (cf. the copula) (1820: 47-48; 1974: 46-47), which appears to indicate that the idea regarding the French future was already well known and considered general knowledge at the time. In a footnote (Bopp, 1820: 47; 1974: 46) Bopp refers us to a few French writings regarding the development of the French future, a paper by Curne de Sainte-Palaye (Curne de Sainte-Palaye 1756 [1751]) (see 5.2.1), but also to A. Schlegel (1818) (see 5.1.3) and Raynouard's *Grammaire de la langue des troubadours* (1816a) (see 5.2.2).

It appears that we can see traces of what was to become the unidirectionality hypothesis in the late twentieth century in Bopp's work. This we see traces of when he says that “[h]ad the verb **facio**, and all nouns of the same root become obsolete, then the word **ficus**, and **ficium** [...] would probably have been called by grammarians inflections or terminations; but what are called **inflections are mostly distinct words, whose origin and primitive meaning is obscure**, or not sought for” (Bopp, 1820: 48; 1974: 47, emphasis original). This bears a strong resemblance to what we nowadays sometimes refer to as a weak formation of a unidirectionality hypothesis, whereby all inflections are not said to stem from autonomous lexical items, but certainly most of them.

Bopp also notes that this kind of change may involve increased bonding (cf. Lehmann, 1982 [1995]), not expressed in as many words, but he notes that Curne de Sainte-Palaye has shown that the earlier forms of the French future accepted articles, for instance, between the main verb and the auxiliary, and Bopp thinks he can show similar changes occurring in Sanskrit (Bopp, 1820: 50; 1974: 49).

A last example mentioned in Bopp (1820; 1974), which resembles grammaticalisation, is that the Latin form *amaris* (< *amasis*) might include the pronoun *se*. This idea was later discussed and dismissed by Jespersen (1922 [1949]), as we shall see in section 6.5.1, and it appears that Bopp is also sceptical of such an origin for this form, even though he does not suggest any alternatives (1820: 62; 1974: 58).

Humboldt wrote a letter and critique of Bopp's *Analytical Comparison*, which he sent to the author in 1821 (Humboldt, 1821 [1974]). Humboldt appears to be both impressed by and interested in Bopp's work. Happy that Bopp has been able to prove that Sanskrit derived its grammatical forms only through agglutination (Humboldt, 1821 [1974]: 61), thereby, according to Humboldt, proving F. Schlegel wrong to distinguish between languages with affixes and languages with inflection – which had to be a mistaken analysis since they believed that Sanskrit and all old Indo-European languages were inflectional, in other words had internal flexion, rather than affixed flexion. But they believed that Sanskrit had created its grammatical forms through agglutination.

Humboldt also expresses a belief in the creation of case endings from prepositions, and his example is the genitive case ending *-s*, which he believes stems from the prepositions *aus* (Humboldt, 1821 [1974]: 64). But we shall return to Humboldt's ideas above (section 5.1.1).

Bopp's own comments on F. Schlegel's two types of languages, in comparison to Humboldt's comment above, is also quite sceptical of F. Schlegel's categorisation (1845: 99):

As the Semitic roots, on account of their construction, possess the most surprising capacity for indicating the secondary ideas of grammar by the mere internal moulding of the root, of which they also make extensive use, while the Sanscrit [sic] roots, at the first grammatical movement, are compelled to assume external additions; so must it appear strange, that F. von Schlegel, while he divides languages in general into two chief races, of which the one denotes the secondary intentions of meaning by an internal alteration of the sound of the root by inflexion, the other always by the addition of a word, which may by itself signify plurality, past time, what is to be in the future, or other relative ideas of that kind, allots the Sanscrit [sic] and its sisters to the former race, and the Semitic languages to the second. (Bopp, 1845: 99)

F. Schlegel's explanation of this, however, is also provided. F. Schlegel (1808: 48) asserts that languages with affixes may appear inflectional sometimes due to what we would today call *demorphologisation*. Schlegel says: “[t]here may, indeed, arise an appearance of inflexion, when the annexed particles are melted down with the chief word as to be no longer distinguishable” (Schlegel, 1808: 48, cited in Bopp 1845: 99; cf. Schlegel, 1860: 448). Schlegel's reason for assuming this to be the case is that he believes it is clear from other elements in the language that it is agglutinating and not inflectional (Schlegel, 1808: 48, cited in Bopp 1845: 99-100).

But Bopp believes that Sanskrit and other Indo-European languages show as many signs in that case of agglutination and should then also be assumed to be agglutinating rather than inflectional (Bopp, 1845: 100).

Bopp believes that all languages make use of agglutination, although he does not use the word *agglutination* (1845: 99-100). And he definitely believes that he can prove that Sanskrit has made use of agglutination. An example of this is the personal endings on verbs (Bopp, 1845: 100-101). Although he realises that he may not be able to prove the origin of all grammatical affixes, even though this is something he hopes to do, the principle still holds, in his view:

But even if the origin of not a single one of these inflections could be traced with certainty, still the principle of the formation of grammar, by external addition, would not, for that reason, be less certain, because at the first glance, in the majority of inflections, one discovers at least so much, that they do not belong to the root, but have been added from without. (Bopp, 1845: 101)

His comment on A. Schlegel's work (1818) makes it even clearer that Bopp does not believe in a typology of strictly different kinds of languages. He claims that A. Schlegel essentially agrees with his brother's (i.e. F. Schlegel's) views regarding language typology. Although A. Schlegel speaks of three types of language and believes that inflections are "foreign additions", the difference between them and other additions is that they have no meaning on their own (Bopp, 1845: 101-102). Sanskrit additions do have a meaning since they can also occur in isolation, whereas Semitic additions do not have any meaning. This is why Bopp concludes that a synchronic typology like A. Schlegel's may be useful after all (Bopp, 1845: 102-103).

A second touch of unidirectionality¹¹³ may be seen in Bopp's explanation of the creation of nouns from monosyllabic roots with added syllables that probably once had meaning:

From the monosyllabic roots proceed nouns, substantive and adjective, by the annexation of syllables, which we should not, without examination, regard as not, *per se*, significative and, as it were, supernatural mystic beings; to a passive belief in whose undiscoverable nature we are not willing to surrender ourselves. It is more natural to suppose that they have or had meaning, and

¹¹³ The first sign of unidirectionality being the idea that inflections are derived from free words (see above).

that the organism of language connects that which has meaning with what is likewise significative. (Bopp, 1845: 120-121)

But even though he expresses a strong belief in all inflections deriving from roots of some form, Bopp also admits that there are “addition[s]” that are purely phonetic in that:

Sanskrit [sic] roots which end with short vowels [...] are, in compounds of this kind, supported by the addition of a *t*, which so much the more appears to be a simple phonetic affix without signification, that these weakly-constructed roots appear to support themselves on an auxiliary *t* before the gerundial suffix *ya* also (Bopp 1845: 122-123)

He also lists similar examples of the use of *t* from Latin and German.

5.1.5 Georg von der Gabelentz (1840-1893)

Georg von der Gabelentz (1840-1893) is often recognised as another forerunner of grammaticalisation studies. He believed that language change can proceed in a spiral affected by the two forces of ease of articulation and ease of perception, or distinctness, something which has clear relations to Meillet’s sense of *renouveau* / *renewal* (Meillet, 1915-1916 [1921]). In his *Die Sprachwissenschaft* (Gabelentz, 1891), one section is devoted to agglutination theory. In that section, one gets the impression that he could have subscribed to the strong unidirectionality hypothesis if he had been writing today:¹¹⁴

Was heute Affixe sind, das waren einst selbständige Wörter, die nachmals durch mechanische und seelische Vorgänge in dienende Stellung hinabgedrückt wurden.¹¹⁵ (Gabelentz, 1891: 250)

It seems from the citation above that von der Gabelentz clearly had a notion similar to what we today would call *grammaticalisation*. He speaks of the

¹¹⁴ This may seem at odds with his idea of a spiral of change. However he claims that all agglutination processes lead in the end to new states as isolating languages. New periphrastic forms then take over again and continue towards a new state as an agglutinating language in parallel with the last set of changes (cf. von der Gabelentz 1891:251).

¹¹⁵ Translation: What today are affixes were once autonomous words, which later through mechanical and psychological processes became posited in an auxiliary position. (1891:250)

development of functional affixes, a category under which we would not include both derivational and inflectional morphemes, in short all affixes.

In an attempt to cover the history of grammaticalisation, the fact that von der Gabelentz asserts that agglutination theory was part of the general knowledge of linguists at the time is also of interest. The importance of this is due to the fact that there are strong similarities between the concept of grammaticalisation and the concept covered in agglutination theory, which means that a history of agglutination also has implications for the history of grammaticalisation (cf. Heine et al., 1991a; Lehmann, 1982 [1995]). However, agglutination theory usually appears to have been a more general notion (in some ways) than grammaticalisation is today, more to do with form than function. This is an important difference when one is talking about unidirectionality. Agglutination is unidirectional by definition, even by its term – and a move from an inflectional morpheme or derivational morpheme to an autonomous word could never be part of the same process, even if such a move was possible. In other ways the notion of agglutination is much more narrow than grammaticalisation, since it only deals with affixes and never with any other means of expressing grammar, such as function words or word order for instance.

At the end of the nineteenth century, the ideas of unidirectionality in the development of grammatical elements appear to have been stronger than now – von der Gabelentz seems to claim that all of the comparative and historical linguistic scholars at the time would have subscribed to a strong unidirectionality hypothesis as far as the development of formative elements is concerned at least:

... im Wesentlichen dürfte die sogenannte **Agglutinationstheorie**, wie sie heute wohl von allen Indogermanisten angenommen ist, unumstösslich und gemeingütig sein; alle Afformativen waren ursprünglich selbständige Wörter.¹¹⁶ (Gabelentz, 1891: 251)

It might be useful to consider how much this idea had to do with older studies of etymology and the thoughts created by the eighteenth and nineteenth century ideas of the origin of language and its evolution. For instance, Condillac suggested that for us to understand the signs they must all be derived from the natural gestural and

¹¹⁶ Translation: ... essentially the so called Agglutination theory, as it seems to be understood by all Indo-European scholars nowadays, should be irrevocable and general, all affirmatives were originally autonomous words. (1891:251)

vocal signs which man first used (Harris and Taylor, 1997: 150). Similarly, Horne Tooke believed that “directly or indirectly (i.e. by means of abbreviation), every word stands for a simple idea” (Harris and Taylor, 1997: 165). Unfortunately, I do not have the time and space to explore this issue any further at present. It is also possible that linguists during the nineteenth century were primarily interested in word formation more generally. Or that they at least clearly distinguished the creation of affixes, for inflection or derivation, from the creation of other means of expressing grammatical relations.

5.1.6 Berthold Delbrück (1842-1927)

Berthold Delbrück (1842-1927) is perhaps most famous for his work on the history of syntax, a topic which is often seen as related to grammaticalisation since syntax expresses grammatical functions even though some grammaticalisationists today would deny that word order can be grammaticalised (cf. Hopper and Traugott, 1993).

Delbrück published an introduction to linguistics or the study of language in 1880, revised 1884 and 1893, and an English translation of the first edition was published in 1882. The book contains a chapter where Delbrück discusses the so-called *Agglutination Theory*, which he basically adheres to. He recognises Bopp as the founder of the theory, and impressively the chapter is quite rich in references both to scholars and to specific publications.

The first thing of importance in the chapter on agglutination theory is the fact that Delbrück looks upon agglutination theory with a critical eye and interestingly he also compares it to other theories that were around at the time, what he calls *evolution theory* (*Evolutionstheorie*) and *adaption theory* (*Adaptionstheorie*). He gives a basic review of the two theories, and refers to some of the main names that have supported them. F. Schlegel and A. Schlegel, for instance, are both recognised as believing in the evolution theory idea that the inflectional morphemes involved first and then the pronouns evolved from the inflections. The basic means of comparison is how the origin of the personal endings of verbs is treated, since this is an example of something which most scholars then appear to have seen as an obvious link (cf. Delbrück, 1882 [1989]: 62). Another scholar who is considered as an adherent of the evolution theory is Christian Lassen (1800-1876) (Delbrück, 1882

[1989]: 62). But a problem with this theory is that according to Delbrück it has not really been tried out in practice (Delbrück 1882 [1989]: 62).

Modern scholars are likely to see the idea of inflectional morphemes giving rise to personal pronouns as directly counterdirectional, since, as discussed in chapter 3 above, many grammaticalisationists have claimed that grammaticalisation can only be a change from lexical to grammatical elements. Sometimes it has even been claimed that all grammatical items would have to start off as lexical items, something which is obviously contradicted by this theory. So, according to grammaticalisationists today, this theory would either not be possible, or if seen as possible, it would at least not count as grammaticalisation.

The adaptation theory appears to have been mainly an idea of Alfred Ludwig (1832-1912), proposed in three different publications (1867; 1871; 1873) according to Delbrück (1882 [1989]: 66). Ludwig believed that pronouns and personal endings had both arisen independently and only later come to resemble one another (Delbrück, 1882 [1989]: 62, 66). But he also believed that the personal endings had not actually been personal endings to start with, but that they might rather have been stem-forming affixes which had nothing to do with a specific grammatical person (Delbrück, 1882 [1989]: 67). Apparently he claimed that the same was true of the noun inflection of case (Delbrück, 1882 [1989]: 67).

The idea that a stem-forming affix could develop into an inflectional morpheme seems counterdirectional to us today, although not quite in the same way as personal endings evolving into pronouns, as proposed by the evolution theorists, seems counterdirectional. And in fact there are statements which very clearly resemble statements regarding the ‘recycling of junk’ that Roger Lass for instance has discussed since the 1990s:

This process of word-formation was gradually arrested, and beside it appeared another tendency, namely, to **turn to account those forms which were divested of their original meaning**. In the beginning men neglected to especially characterize *agens*, *actio*, *actum*, and contented themselves with the employment of demonstration, which was evidently used at that time to a very large extent. As soon, however, as language possessed suitable material, it went to work (although by no means consistently) to introduce this distinction, which promotes perspicuity of speech to such an extraordinary degree. When this differentiation had reached a certain point, there was undoubtedly another inclination to indicate the relations of number and case; but even for this purpose **only existing materials were used, and we must**

not suppose that a grammar was created. (Quoted from Ludwig (1871)):§19 in Delbrück (1882 [1989]: 67-68), emphasis mine.)

Delbrück is not keen on either of the two alternatives to agglutination theory, but prefers Bopp's agglutination theory, even though he can see some problems with this too. He often points out that we cannot know for sure what were the origins of forms or the number and type of roots (e.g. 1882 [1989]: 92, 94-95, 99). Although the personal endings and personal pronouns were clearly in focus and must have been the main issue of discussion at the time, Delbrück also introduces some other examples. He looks at ideas regarding the evolution of case forms, for instance, the idea that there was a move from a stem formative to case ending was apparently sometimes adopted by Georg Curtius (1820-1885), and usually by (Henri Joseph) Abel Bergaigne (1769-1820) (1875: 358-379) and always by Ludwig who ascribed to adaptation theory. Another possible derivation of case forms was of course by affixation of "pronominal, or pronominal and prepositional elements" directly in the function of case markers (Delbrück, 1882 [1989]: 92).

According to Delbrück, inflectional endings arose gradually in the second (the inflectional) period of language development, and since they arose gradually that must mean there are subsections into which the inflectional period can be divided. However, he finds it unlikely that we can specify what these subperiods were exactly (1882 [1989]: 100).

His conclusion as to the position of agglutination theory in the study of language, is "that the principle of agglutination is the only one which furnishes an intelligible explanation of the forms" (1882 [1989]: 101). He claims that none of the other ideas he has come across can be called a principle (1882 [1989]: 101). In other words, agglutination theory according to Delbrück is not only a theory, but a principle. Unfortunately, he ends on a partly negative note concerning the future studies on this principle:

Since, now, after our whole discussion the principle of agglutination is all that survives, the question arises whether it would not be better to relinquish philological metaphysics altogether, and confine ourselves to what can be really known ... (Delbrück, 1882 [1989]: 101)

Still, he believes that this form of change will continue to interest scholars:

But I do not believe that this view will become general. The attempts to analyze the parts of speech do not, after all, depend upon the arbitrary decisions and fancies of scholars, but are founded upon certain linguistic facts (as, for example, the resemblance of the personal and stem-forming suffixes to certain pronouns, and the like), and therefore will probably be repeated in the future. (Delbrück, 1882 [1989]: 101-102)

5.1.7 Hermann Paul (1846 - 1921)

Hermann Paul (1846-1921) wrote quite a lot about changes that remind us of grammaticalisation and reanalysis in the fifth edition of his *Prinzipien* (Paul, 1920). Not only did he discuss what had been called *agglutination*, which he called *Komposition* (e.g. Paul, 1920: 325), but he also discussed how sound changes could be given grammatical function (e.g. 1920: 325), how parts of the stem could be given a grammatical function (e.g. Paul, 1920: 215), and the development of full words into function words (e.g. Paul, 1920: 366-370)

Paul notes that when full words become function words the origin of the word is “verdunkelt”, i.e. it becomes opaque. He also notes that words can go through what he (and many others) refers to as *Verschmelzung*, when they melt into one, and that they have to be isolated from their origin to be seen as function words or flexions. In other words, what we now call a *split* or *divergence* must occur (cf. Paul, 1920: 330-331, 369).

One of the examples Paul discusses is the Scandinavian, passive which has developed from a third person reflexive pronoun (Paul, 1920: 237). He also mentions how adverbs tend to stem from case forms of nouns or alternatively from prepositions and case forms of nouns, and he says that this is likely to have been how adverbs have always been formed although at first there was no flexion and then they will naturally have been formed from stems rather than case forms (1920: 366). Similarly, prepositions are noted to stem from adverbs sometimes, for instance, German *nach* ‘to, after’ from an adverb *nahe* ‘after’¹¹⁷ (1920: 369-370).

The examples mentioned so far are clearly typical examples of grammaticalisation. However, as I mentioned earlier, Paul also has some examples of how function elements can develop out of sound changes and from parts of stems.

¹¹⁷ Naturally Paul does not give any translations for these words and an adverb *nahe* is not to be found in *Langenscheidts Großes Schulwörterbuch Deutsch – Englisch*. (1992). Similarly, in *Duden Deutsches Universal Wörterbuch* (2003) *nahe* is only listed as an adjective and noun. However there is an adverb *nahebei* which means ‘nearby’.

This sounds like something that could be classed as *exaptation* by scholars today, and a look at the examples shows that the examples are also in part exactly the same examples as those that tend to be used to illustrate exaptation. Paul says that parts of some noun stems have been reinterpreted as a plural marker (-er) in German (*Rad – Räder, Mann – Männer*), and parts of other noun stems (-n) as a weak oblique marker (*Name - Namen, Frau - Frauen, Herz - Herzen*) (Paul, 1920: 215). He also mentions the development of umlaut plurals, which is noted to have arrived at its new function differently than the ablaut difference between imperfect and aorist, in that it happened when the other signs of plural disappeared (1920: 325). Unfortunately, he does not go into this in any depth.

As we have already mentioned briefly in chapter 4, Paul has also been recognised to have discussed reanalysis, although not under that term or any other term. One form of reanalysis that he discusses is how endings can change by adopting parts that occur before them: Gothic *-assus* > West Gmc. *-nassus*, etc. (1920: 245-246).

It seems that Paul has some sense of a connection between the development of inflectional morphemes, derivational morphemes (and notably, both from independent words and from other sources) and function words, in that for instance he lists different ways of creating groups of words in his chapter on the development of derivation and flexion (1920: chapter 19). He also refers to something which resembles a continuum or cline when he says that “[w]as man vom Standpunkte des Sprachgefühls ein Kompositum nennen darf, liegt in der Mitte zwischen diesen Punkten [Simplex – Komplex]”¹¹⁸ (1920: 346). It is also to be noted that he claims that only one’s intuition about language can tell us where to draw the line between being a member of a compound and a suffix, but that it is absolutely clear that it is a suffix when it can be extended analogically to words with which it could not originally appear (1920: 348). This is similar to the more recent comment by Hopper and Traugott (1993) that we can only see that reanalysis has occurred after the change has been extended through analogy.

Paul believes that the process of creating new suffixes is an old process, but also one that still takes place (1920: 349). He sees no strict dividing line between inflectional morphemes and derivational morphemes. He also believes that they have

¹¹⁸ Translation: what one can call a composite from the point of view of one’s language intuition, lies in the middle between these points [a simple word – a complex word]

both arisen in the same way (1920: 349). Like so many others he lists the example of the Romance future, and he also mentions that pronouns have been affixed to tense forms in Bavarian dialects, notably not mentioning Indo-European languages in general (1920: 349). However, there are in fact not that many examples from the development of inflectional morphemes since Paul believes that everyone knows very well that this change has occurred and has much material on the matter available to them (1920: 349).

One last thing that I think we should note in Paul's work is that he finds reason to stress that it is only the first time the inflectional or derivational morpheme is used that it is developed through composition from autonomous elements, and that later it occurs only through analogy with that first form and later analogical formations (1920: 350).

5.1.8 Karl Brugmann (1849-1919)

According to Karl Brugmann (1849-1919), the Indo-European (or *Indo-Germanic* in his terminology) languages are inflectional, although the inflections are not original but have been developed only gradually (Brugmann, 1888: 13).¹¹⁹ Brugmann assumes that a "root-period" precedes the period when inflections, i.e. suffixes, arise (1888: 14). His description of the "process" (1888: 14), or "principle"¹²⁰ (1891: 1), by which we have gained suffixes in the form of grammatical inflections and word formation devices is that they arose through "*Zusammenrückung*" (1889: 1) / "*juxtaposition*" (1891: 1), "*Compositionsprocesse*" (1889: 1) / "*composition*"¹²¹ (1888: 14; 1891: 1-2) and "*Verschmelzung*" (1889: 1) / "*fusion*" (1891: 1), "*Zusammenwachsen syntaktischer Wortkomplexe*" (1889: 1) / "*coalescence of words grouped in some syntactic relation*" (1891: 1).¹²²

¹¹⁹ Note that the English translation from 1888 incorporates the corrections of the mistakes listed at the end of the German edition, as well as some other changes suggested by Brugmann himself (Brugmann, 1888: ix).

¹²⁰ There is no talk of this being a 'principle' in the original (cf. Brugmann, 1889: 1), only in the translation.

¹²¹ In the German original for the first instance of 'composition' in the translation it says: "Die einheitlich gewordenen Formen gaben Bildungstypen ab, [...]" (1889: 1) where there is no word like 'composition'.

¹²² The term *coalescence* (Brugmann, 1891: 1), is the term that Jespersen (1922 [1949]) later used when he suggested a new term instead of *agglutination theory*.

What we understand by word-formation and inflexion arose by composition, that is, by the following process: a group of words which formed a syntactical complex was fused into a unity, in which the whole was in some way isolated in relation to its elements. (Brugmann, 1888: 14)

It is equally impossible to draw a hard and fast line between a compound and a simple word. One part of a compound is reduced or degraded to a suffix or prefix, or generally to an inflectional element, by losing in some way or other its connexion with the simple word in the consciousness of the speaker. This isolation may affect the meaning only [...] or it may affect only the form [...] or it may affect both at once [...]. (Brugmann, 1891: 5-6)

Was man unter den Namen Stammbildung und Flexion zusammenfasst, beruht, wie schon Bd. I § 13 und 14 bemerkt wurde, auf Zusammenrückung und mehr oder minder inniger Verschmelzung ursprünglich selbständiger Elemente.¹²³ (Brugmann, 1889: 1)

Clearly, Brugmann does not only have the evolution of grammatical elements in mind, but all forms of univerbation whether there is a grammatical or purely lexical result. His statements seem to confirm what von der Gabelentz said about his contemporaries, in that here too we seem to have someone with a rather unidirectional view of this process which could occur in the latter stages of agglutination.

It may be a bit difficult to understand what Brugmann means by what appears as *isolation* in the translation. But this appears to have been a common concept at the time as we see in some of the other scholars in this chapter. Brugmann stresses the fact that the isolation is something which happens slowly and gradually, and it is therefore impossible to say exactly when a phrase becomes a compound (1891: 4), just as it is often noted today that grammaticalisation is a gradual process. It is also important to note that today a *split* (in the form of ‘isolation’ of meaning and function) can occur not only between a full word and an affix developed out of that full word, but also at the full word level. Brugmann recognises that it may be hard to distinguish between compounds and simple words, since it is hard to say exactly when a member of a compound has been reduced to a suffix or prefix (1891: 5-6). This difficulty arises from the fact that the “isolation” sometimes only affects the

¹²³ Translation Rouse Seymour Conway, W. H. D.: All the developments [sic] of language denoted by the terms Stem-formation and Inflexion are based upon one common principle, the juxta-position and more or less intimate fusion of elements which were originally independent. (Brugmann, 1891: 1)

meaning, whereas at other times it only affects the form and occasionally it affects both (1891: 6).¹²⁴ This “change of function is very gradual” (1891: 7).

Another thing that is clear from Brugmann’s text is that this phenomenon of composition and fusion did not only occur in the Indo-European parent language, but also in the separate languages that sprung from that source. With his comment that this also happened “in historical periods” he appears to be saying that it is both an evolutionary and a developmental form of change (cf. Brugmann, 1888: 14). His example of this includes, as expected from the wide statement above which included inflection as well as word formation, both examples that are sometimes used for grammaticalisation (Fr. *-ment*) and examples which are used for both lexicalisation and grammaticalisation (Germ. *-heit*) (cf. 1888: 14).

He distinguishes between “word-forming suffixes” (“derivation[s]” (1888: 16)) and “inflectional suffixes”. However, it is very clear that although he thinks their functions are different, he sees a similarity of form which originates in a similarity in origin – viz. the same process (1888: 15). This is probably due to the fact that Brugmann also realises that it is sometimes difficult to distinguish the two kinds of suffixes. In fact he claims that “[i]t is impossible to draw a sharp line between the two species of suffixes” and the reason he gives is both interesting and seemingly counterdirectional (according to the unidirectionality hypothesis) from our modern point of view:

... since many an element, which was originally only stem-forming, has come to be treated on the same level with word-forming suffixes. Cp. e.g. Lat. *legimini*, whose final part *mini*, felt by the Romans as a personal ending (like *-mur* etc.), contains the stem-forming suffix *-meno-* [...]; properly *legimini estis* [...] (Brugmann, 1888: 15)

Some inflectional morphemes¹²⁵ are very old and it is (close to) impossible to derive their origins. However, there is one type of inflectional morphemes which one is quite sure of the origin of, according to Brugmann, namely some of the personal endings. These were thought to be derived from pronouns, such as *-m* from *me* (1888: 15).

¹²⁴ The examples given are Lat. *-iter*, Old Ir. *bith-*, Mod.HG. *-tel*, Mod.HG. *-lich*, [Eng. *-ly*, *-red*. – *ship*, *-ric* seemingly added by the translator].

¹²⁵ I do not mean to say that the concept of a morpheme existed during the nineteenth century, but I shall be using the modern concept of morpheme when talking about the development of inflectional and derivational affixes / clitics so that I do not have to go into whether it is an affix or a clitic.

Brugmann realises that not every single word that appears with an inflectional morpheme or derivational morpheme has been formed through composition and fusion, but rather just the first ones with that inflectional or derivational morpheme, and later these forms have spread by analogy to other words (1888: 16; 1891: 10). To some extent this can be compared with the extension often talked about today, or analogy in the words of Hopper and Traugott (1993) for instance.

As people get used to the forms with the suffix, they may misinterpret where one morpheme ends and another starts, and we may thereby get new productive suffixes, such as Germ. *-keit* (1888: 16). This brings Brugmann to the conclusion that we can never be sure whether a suffix has existed as an independent word of (approximately) the same shape (1888: 16). He also recognises that sometimes two suffixes may become interpreted as one (1888: 16-17), cf. *univerbation* and *demorphologisation* of today. This might seem to indicate that Brugmann would not have subscribed to the unidirectionality hypothesis of today.

Still it seems as though Brugmann's ideas would agree with a weak unidirectionality hypothesis to some extent. He says:

The reverse process does not often occur, i.e. when an element once a suffix or a prefix is raised in the speaker's consciousness to the dignity of a compounded word; but we find it e.g. in Mod.H.G. *leu-mund* 'repute, character' (popularly connected with *mund* 'mouth') = Skr. *śrō-mata-* (...), *brō-sāme* 'the crumb of the loaf' (influenced by *sāme* 'seed') from O.H.G. *brōsma*, [...] (Brugmann, 1891: 7)

In fact, Brugmann's statement is even stronger than von der Gabelentz's since he makes a statement about the unlikelihood that there will reversals, and not just the unlikelihood that formative elements will spring from other sources. However, like Givón (1975), when speaking of the possibility of a reversal preposition > verb, Brugmann claims that it is **rare, not** impossible for a process to work in the reverse direction to univerbation. It is clear from the quotation above that, although Brugmann recognises that there is a main direction of change, he is aware that there are examples of changes that have occurred in the opposite direction. Thus, he only seems to believe in a directional **tendency!**

Brugmann considers words with suffixes as a form of "compounds" (1891: 3). Even though he can see that it may be good to restrict the term *compound* to forms "consisting of two members", he says that a compound is a "simple word" when one

of the members has lost its “connexion in the mind of the speaker with kindred words which are uncompounded,” in other words when they have become what he above called “isolated” (1891: 3). There are a few different results that are possible when a compound becomes a “simple word”. Either they may seem “absolutely simple”, which would mean in the words of later grammaticalisationists that univerbation and demorphologisation have occurred, or the isolated element may be bleached and become a suffix or prefix (1891: 3).

The term *bleaching* is not used by Brugmann. However, he speaks of the element “los[ing] its full meaning” (1891: 3). He also talks of a group of compounds which are so-called “*obscured compounds*” (1891: 7-10). These compounds contain members that are no longer etymologically clear, and the reduced member no longer occurs independently. Such an example, according to Brugmann, is OHG *hiutu* ‘today’, MHG. *nicht* ‘nothing’ and according to the translator’s addition also Eng. *huzzy* (1891: 10).

An example which one rarely sees today, apart from that personal endings should be derived from personal pronouns, is that the Latin future should be derived from autonomous words. But Brugmann claims that the Latin future ending *-bō* stems from IE **bhu-ō* (from *bheu-* ‘become’) (1891: 10). Brugmann also recognises that a form that has developed is not likely to remain forever. Forms lose their productivity and die out, and remain only in some frozen forms (cf. Brugmann, 1891: 10-11). He believes we must try to explain why this happens (1891: 11).

Another kind of compounding used to express grammatical information is reduplication, which, as Brugmann notes, can be used to express repetition or duration (1891: 12). He also observes that it is rare for both forms to remain intact, but instead often the first is “abbreviated” or “debased”, in other words phonetically reduced (1891: 13).

Ablaut is also discussed briefly, a phonetic change which Brugmann explains came about through shifting accents and which originally had nothing to do with a grammatical function. The fact that it is recognised to have had no grammatical functions to begin with is very important in a comparison with later work on grammaticalisation. Later, ablaut acquired meanings, such as the use to express the perfect (1891: 15-16). The fact that ablaut could go from no function to acquiring

function, and not from lexical to functional, is of great importance in comparison with the unidirectionality hypothesis of grammaticalisation.

Brugmann goes on to mention how differences brought about through the phonetic change called *umlaut* came to bear meaning differences (1891: 16). These kinds of change whereby ablaut and umlaut have received grammatical functions might today be called *exaptation* or *regrammaticalisation* (cf. chapter 3). However, Brugmann does not attribute any particular name to these changes.

To sum up Brugmann's ideas:

We have seen in the foregoing pages that the Indo-Germanic formative system in all its branches is really based upon composition. This being the case, the task of systematic morphology is to exhibit, first the processes of composition which gave rise both to what are usually known as compounds, and to all formations containing elements of the nature of suffixes or prefixes; and secondly, the development of the analogical formations which are associated with these. It is clear however that this task can only be very imperfectly accomplished. (Brugmann, 1891: 18)

The quotation shows that **part of** grammaticalisation is called *composition*. However, this is of course a very general term, and perhaps it is not meant to be understood as something that should be used as a term. Perhaps it is simply a way of describing what it is using everyday vocabulary. But then terminology is also formed from everyday language, sometimes consciously and sometimes unconsciously. It also means that this process does not only include the origin of inflectional morphemes, but also derivational morphemes and even compounds. The last of which would definitely not count as grammaticalisation and the the origin of derivational affixes would only sometimes count as grammaticalisation. We should also note that so far there have been no comments on the origin of other grammatical items, such as particles, conjunctions, prepositions, auxiliaries – which would also count as a form of grammaticalisation today.

It is worth stressing that Brugmann notes several times that we cannot be sure that a formative element ever existed as an independent word, nor can we be sure that all the sounds that are now part of it were always part of it (i.e. that it is only one previously independent word, not two). Interestingly, one of his two examples of formations which scholars at the end of the nineteenth century felt reasonably sure they knew the origin of, would now be considered more **uncertain**, viz. that some personal endings stem from personal pronouns (cf. Brugmann, 1891: 19). His other

example is reduplications (Brugmann, 1891: 19) and therefore quite different from the classic example of formative elements, or affixes derived from independent words that are usually studied as grammaticalisation.

5.2 France

5.2.1 Jean Baptiste de la Curne de Sainte-Palaye (1697-1781) & Abbé François Seraphin Regnier Desmarais (1632-1713)

Jean Baptiste de la Curne de Sainte-Palaye (1697-1781) was referred to by Bopp (1820) with regards to the origin of the French future. However, Curne de Sainte-Palaye does not say much about this and it is the only example he mentions of a change we would class as grammaticalisation. The reason he brings up this example appears to have been that he thought it illustrated the similarity quite well between French, Italian and Spanish. The source for his information on this change is clearly Abbé François Seraphin Regnier [Desmarais] (1632-1713) to whom he also includes a reference:

Je finis par une observation grammaticale peu importante en elle-même; mais qui servira d'une nouvelle preuve à la conformité des langues Française, Italienne & Espagnole, & justifiera encore la remarque d'un de nos plus célèbres grammariens sur la formation de notre future. Elle se fait, suivant l'abbé Regnier, par la jonction ou réunion du temps présent de l'indicatif du verbe auxiliaire *avoir*, & de l'infinitif; *j'aimerai*, *tu aimeras* [...] Il fait l'application du même principe aux verbes Italiens & Espagnols, à quoi j'ajouterais que la formation du futur imparfait du subjonctif *j'aimerois*, se fait pareillement de la jonction de l'infinitif avec l'imparfait de l'indicatif du verbe *avoir*, que l'on a syncopé & dont on n'a conservé que la finale.¹²⁶, ¹²⁷,¹²⁸ (Curne de Sainte-Palaye, 1756 [1751]: 684)

¹²⁶ Long s has been replaced by regular s.

¹²⁷ In the margin Curne de Sainte-Palaye has a more precise reference to abbé Regnier's *Grammaire Française* p. 368f. (cf. Regnier Desmarais, 1706, another edition of the grammar but from the same year and the page number seems to apply to that too.)

¹²⁸ Translation: I will finish with a grammatical observation which is of little importance in itself; but which will serve as renewed proof of the similarity of the French, Italian and Spanish languages, and justify yet again the remark made by one of our famous grammarians on the formation of our future. It is formed, according to abbé Regnier, through the addition or reunion of the indicative present of the auxiliary verb *avoir* ['to have'], and the infinitive: *j'aimerai* ['I will love'], *tu aimeras* ['you will love'] [...] The same principle is used for Italian and Spanish verbs, to which I will add that the formation of future imperfect of the subjunctive *j'aimerois*, is formed in a parallel manner by the addition of the indicative imperfect of the verb *avoir* to the infinitive, [the verb *avoir*] has been syncopated and one has only kept the final part.

It is important to note that Curne de Sainte-Palaye adds a comment of his own, and does not only repeat what he has found out from Abbé Regnier. He draws a parallel between the creation of the future forms and the creation of the future imperfect forms. In addition, he also remarks that the form of *avoir* used in the future is not firmly agglutinated to the infinitive. Instead he notes that an article, particle or pronoun can sometimes appear between the two verbs (Curne de Sainte-Palaye, 1756 [1751]: 684). A sign which, if we compare it to more recent discussions of grammaticalisation, could be seen as indicating a less advanced stage of grammaticalisation, in that it shows less bonding between the forms (cf. Lehmann, 1982 [1995]).

Going back then a step further to see what Abbé Regnier [Desmarais] says, we find that he does not say much more. He simply says that the future is formed from the present tense of the auxiliary *avoir* ‘to have’ and the infinitive (of the main verb). However, he also remarks on the fact that this is not quite true of the first and second person plural:

Au reste, ce qui fait la formation de ce *Futur*, n’est proprement autre chose, que le *Temps present* [sic] de l’Indicatif du Verbe auxiliaire *avoir*, joint à l’Infinitif, comme on le peut voir dans les trois personne du *Nombre singulier*, *j’aimerai*, *tu aimeras*, *il aimera*. Il est vray que dans la premiere [sic] & dans la seconde personne du pluriel, le *Temps present* de l’Indicatif du mesme [sic] Verbe n’est pas mis dans toute son estenduë; mais cela vient de ce qu’autrefois on a dit, *nous ons* & *vous ez*, pour *nous avons* & *vous avez*; ainsi qu’on le peut encore juger par la troisième [sic] personne du pluriel, où on a conservé *ils ont*.¹²⁹, ¹³⁰ (Regnier Desmarais, 1706: 368, italics original.)

We also find Curne de Sainte-Palaye’s comment regarding the occasional separation of the two verbs in the future included in Régnier (Regnier Desmarais, 1706: 369), and he gives some examples of how a particle is sometimes used between the auxiliary and the main verb, then placing the auxiliary before the main verb.

¹²⁹ All long s’s have been changed to regular s.

¹³⁰ Translation: For the rest, that which makes the formation of this *Future*, is not properly anything other than the present tense of the indicative of the auxiliary verb *avoir* [‘to have’], added to the Infinitive, as one can see into the three persons of the *singular number*, *j’aimerai* [‘I will love’], *tu aimeras* [‘you will love’], *il aimera* [‘he will love’]. It is true that in the first and in the second person of the plural, the present tense indicative of the same verb is not used in all its extent, but this arises because in the past one has said, *nous ons* & *vous ez*, instead of *nous avons* & *vous avez*, as one can still judge from the third person plural, where one has kept *ils ont*.

De plus, ce qui prouve indubitablement cette formation, c'est que les Espagnols, pour exprimer le *Temps future*, separent quelquefois les deux Verbes l'un de l'autre, tantost en disant *amar hemos*, *amar heys*, & tantost en mettant l'auxiliaire devant avec la particule *de* comme *has de amar*, *hemos de amar*.¹³¹ (Regnier Desmarais, 1706: 369, italics original)

5.2.2 François-Juste-Marie Raynouard (1761-1836)

In François-Juste-Marie Raynouard's (1761-1836) six-volume work on the troubadours' *Choix des Poésies des Troubadours* (Raynouard, 1816-1821), the first volume contains a section called *Recherches sur l'origine et la formation de la langue romane* (1816a), which was later referred to by A. Schlegel (1818).

Raynouard believes that cases have been replaced by prepositions and that a Latin demonstrative pronoun has developed into articles in order to indicate which words are nouns, as these had lost their case endings (1816a: 23-25, 38, 41, 46). Of course the fact that he saw that cases and prepositions could fulfil the same purpose does not mean that he recognised something like grammaticalisation. However, it shows that he recognises the possibility that there may be different ways of expressing grammatical relations. And his comment regarding demonstrative pronouns and definite articles bears much resemblance to grammaticalisation and would normally be classed as such by today's scholars. Something else which is more in line with what we call grammaticalisation comes across in a brief discussion of the origin of the indefinite pronouns *om* (< Lat. *homo*) and *un* (< Lat. *unus*) (1816a: 63).

There are no signs in this work of the idea that the Latin personal endings on verbs should have been derived from personal pronouns, which is interesting since Bopp discussed that idea in the same year, and it had presumably already been discussed in the eighteenth-century by Condillac (cf. Lehmann, 1982 [1995]: 1). Since there is no reference to this in Raynouard, there is a possibility still that Bopp was one of the first to discuss this in nineteenth-century Europe. One of the first since it appears that Rask may have introduced the idea in Scandinavia a few years earlier than Bopp's discussion of it (cf. section 5.5.1).

¹³¹ Translation: Furthermore, that which proves this formation beyond doubt, is that the Spaniards, to express the *future tense* sometimes separate the two verbs from one another, sometimes saying *amar hemos*, *amar heys*, & sometimes putting the auxiliary before with a particle *de* as in *has de amar*, *hemos de amar*.

Raynouard also treats various forms of periphrastic verb conjugation (1816a: 70-76). Among these he also treats the *futur simple*, which he claims is formed from the present of the Romance¹³² infinitive and the present of the verb *avoir* (1816a: 70). It is interesting that he claims this is a simple form, but still claims that the second part is the present tense of *avoir*. This seems like a clear sign that he believes that the two parts have fully merged. He also says that the present conditional¹³³ is formed in a similar manner from the imperfect of *aver* added to the present infinitive of verbs, whereas the future conditional is said to be formed through periphrasis (Raynouard, 1816a: 72).

For some reason Raynouard varyingly refers to the French *avoir* and the Provençal *aver*. It is not clear why and it is possible that this is a simple slip of the pen. It feels as though he sees these processes as synchronic and not as diachronic explanations of how the forms have become what they are:

J'ai précédemment observé que le futur de l'indicatif et le présent du conditionnel avaient été formés par l'**adjonction** du présent de l'indicatif du verbe AVER, ou de la finale de son imparfait, au présent de l'infinitif des verbes.¹³⁴ (Raynouard, 1816a: 81, emphasis mine)

Cette manière très-remarquable de composer ces temps offre une circonstance qui l'est également, et qui constate toujours plus évidemment l'identité de la langue romane et des autres langues de l'Europe latine.¹³⁵ (Raynouard, 1816a: 82)

The fact that he claims that the future is a simple form, but still explains where it derives from, indicates that he also has considered some things historically. Furthermore, as we see in the second quotation above, he says that this is something remarkable and we cannot help but think he means unusual. But possibly he only means that it is fascinating. Have we not all been fascinated when we realised that part of a word, such as the Romance future ending, originates in an independent word?

¹³² The term Raynouard uses is *roman*, "*l'infinitif roman*" (Raynouard, 1816a: 70).

¹³³ The term Raynouard uses is *conditionnel* (Raynouard, 1816a: 72).

¹³⁴ Translation: I have previously observed that the future indicative and the present conditional have been formed by adjoining the present indicative of the verb AVER, or of the end of its imperfect, and the present of the infinitive of the verbs.

¹³⁵ Translation: This very remarkable manner of composing these tenses offers a situation that is equally that [i.e. noteworthy], and which certifies ever more clearly the identity of the Romance language and the other languages of Latin Europe.

It is not only the descendants of Latin *esse* and *habere* that are discussed as auxiliaries, but also *anar* (1816a: 85-86), which he appears to claim formed the ending in e.g. *descapt-an*, for instance, in *Cum el es velz, vai s'onors DESCAPTAN...* In other words, it is written as one word together with the main verb and should probably be seen as a clitic or possibly an auxiliary. So far I have not found any recent comments on the origin of such a construction.

In a section about prepositions, adverbs and conjunctions, Raynouard discusses the origin – or perhaps renewal – of some prepositions and the formation of some adverbs, conjunctions and negations (1816a: 87-91):

davan, devant (PREP) < *de ab ante*
aprob (ADV) < *a* (PREP) *prope*
dereer (ADV) < *de* (PREP) *retro*
y/i (ADV) < *ibi*

But he also mentions the formation of adverbs by joining the noun *mente* (ablative absolute) to an adjective (1816a: 95), and this he says is the way that most adverbs in the *langue romane* are formed (1816a: 95-96). To this day this remains a popular example of grammaticalisation (cf. e.g. Hopper and Traugott, 1993).

An even more common example of grammaticalisation today is the French negation *ne... pas*. Raynouard comments on the common use of what he calls “négations explétives” together with the “négations ordinaires”, by which he means the use of *mie, gens* etc together with the negation *ne* (1816a: 100-101). However he does **not** mention the use of *ne... pas*. He also does not point out explicitly that these “négations explétives” originated in nouns, although implicitly this seems clear from the fact that he mentions which Latin word they stem from, e.g. *mie* from *mica*, *gens* from *gens* (in the sense of ‘someone’).

In 1816 Raynouard published a book called *Grammaire Romane ou Grammaire de la Langue des Troubadours* (Raynouard, 1816b). As expected it contains similar comments to the work discussed above, and like that it includes examples of what we would now call *grammaticalisation*. Once again he notes the development of the Latin demonstrative pronoun *ille* into a definite article (1816b: 13-14), and here unlike in the book discussed above, he also mentions that many pronouns become affixes, just as Bopp (1816; 1820) and others claim around the same time (Raynouard, 1816b: 91). Of this, he gives the same examples that are later repeated in the literature, *m* being derived from *me, mi*, *t* from *te, ti*, *s* from *se, si, ns*

from *nos* and *vs* from *vos*, even going a bit further than normal in this list in that he includes the last two forms *ns* and *vs*.

Also here the origin of the indefinite pronoun *hom* (Fr. *on*) is mentioned (1816b: 146), as well as the fact that *aver* and *esser*, *estar* function as auxiliaries (1816b: 167). He also shows that *aver* can be used as an affix and it is not quite clear whether he distinguishes such a use from periphrastic auxiliaries (1816b: 176, 221). And he speaks of this as an *adjonction* ‘addition’, but still he claims that they do not have to be joined together (1816b: 221). Interestingly, in this volume there are also explicit claims that conjunctions can be derived from adverbs and prepositions, and that prepositions can also sometimes turn into adverbs, the latter of which seems on the verge of being something that would be seen as counterdirectional in the early twenty-first century (1816b: 247). And once again he states that the old Latin prepositions, adverbs and conjunctions are often joined with an additional element in their use in the *langue romane*, which we can compare to what Meillet (1912; 1915-1916 [1921]) and others have called renewal / renouvellement:

intus > *intz/ins*
 de intz/ins > *de ins* > *dans*
 de intz/ins > *de dins* > *dedans*
satis > *satz*
 a satz > *asatz* > *assez*
versus > *vers*
 de vers > *devers*
 en vers > *envers*
(Raynouard, 1816b: 248)

Similarly, Raynouard also calls our attention once more to the fact that negations often include “particules explétives”, as well as the negation *non*, to increase its force. However, this time he also mentions the common example of the particle *pas* (Raynouard, 1816b: 333).

5.3 Britain

5.3.1 The Reverend Richard Garnett (1789-1850)

The Reverend Richard Garnett (1789-1850) read a paper on the origin of the Sanskrit and Greek augment to the Philological Society in May 1844. This was later

published in its *Proceedings* (Garnett, 1844). In the paper he gives us a view of how the history of this little grammatical particle had even by then gone through quite a few theories of explanation. However, all but one of those he relates are within agglutination theory (although he does not use that term). He mentions that the Greek augment has been related to the copula, but also to the reduplicative prefix of the perfect, the latter of which would not count as agglutination theory. He also mentions that it has been seen as identical to the negative prefix *a/an* but that this theory, proposed by Bopp, was later modified by the same to say that the negative prefix and the augment stemmed from the same demonstrative root. It is the last theory which Garnett himself favours (Garnett, 1844: 266).

He compares the Greek and Sanskrit augment to the Celtic (esp. Welsh) preterite prefix *a-* which he believes originally had the meaning ‘there, then’ (Garnett, 1844: 266-267, 271). And as he proves what he believes to be the most probable origin of the augment he also looks at many other Indo-European, and non Indo-European languages, to see if they have anything similar which could confirm that the Greek and Sanskrit augment stems from a demonstrative root (Garnett, 1844: 266-270).

5.3.2 Francis Henry Trithen / Friedrich Heinrich Trithen (1820-1854)

Francis Henry Trithen (1820-1854), an Orientalist at Oxford, is known to have been in contact with the famous German linguist August Friedrich Pott (1802-1887), through letters that are kept at the Universitäts- und Landesbibliothek Sachsen-Anhalt (see the website from Universitäts- und Landesbibliothek Sachsen-Anhalt). In an article from the mid-nineteenth century, Trithen comments on how the past tense is formed in some Indo-European languages (Trithen, 1854). He claims that in Old Slavic one way of expressing the past tense is by “union of the verb substantive with the participle”, something which in Polish is reported to result in a suffix (Trithen, 1854: 273). Similarly he claims that Bopp has proved that the Latin inflectional ending *-bam* is related to the Sanskrit verb *b’hū* ‘to be’, and Trithen believes that in Sanskrit it is also the auxiliary verbs *asa / b’ū* ‘to be’ and *kr* ‘to make’ that are attached to verbs to form the preterite (1854: 273-274). In other words, Trithen’s work also included uses of agglutination theory.

5.3.3 Friedrich Max Müller (1823-1900)

Friedrich Max Müller¹³⁶ (1823-1900) believed that there was a semantic change at work in language whereby linguistic items lost some or all of their meaning. This is clear from the following statement: “I must ask you at present to take it for granted that everything in language had originally a meaning” (1862: 42). It also seems that he had a rather strongly unidirectional view, in modern grammaticalisation terms, in that he says that **everything** in language originally had a meaning. In other words also inflectional and derivational morphemes must have had meaning even if they do not seem to have much meaning, apart from their grammatical function, now. Similar ideas are also repeated several times in the first series of *Lectures* he gave at the Royal Institution, cf.:

The fact that every word is originally a predicate¹³⁷ – that names, though signs of individual conceptions, are all, without exception, derived from general ideas – is one of the most important discoveries in the science of language. (Müller, 1862: 390)

All roots were originally full, whether predicative or demonstrative, [...] (Müller, 1862: 393)

Chinese commentators admit that all empty words were originally full words, just as Sanskrit grammarians maintain that all that is formal in grammar was originally substantial. (Müller, 1862: 394)

Müller not only recognised that there could be a development of grammatical terminations from something else, but he also recognised the semantic and phonetic aspects of the change as well as the similarity between grammatical expressions in one language (and their constructions) and lexical constructions in another (and their constructions).

Let us first have a look at one of the places where he mentioned the phonetic and semantic attrition involved in the development of grammatical inflections, to put it in present-day linguistic terminology, he says:

And here again, as long as these words are **fully understood** and kept alive, they **resist phonetic corruption**; but the moment they **lose**, so to say, **their presence of mind, phonetic corruption sets in**, and as soon as phonetic

¹³⁶ I will here concentrate on two of his books, Müller (1862, 1868). However I have also consulted some of his other publications more briefly (1875, 1901).

¹³⁷ Müller opposed predicates to particles much like we oppose *content words* to *function words*

corruption has commenced its ravages, those portions of a word which it affects retain a **merely artificial or conventional existence** and dwindle down to **grammatical terminations**.” (Müller, 1862: 45, emphasis mine)

There are several interesting points in this quotation. First of all he talks about the relation between the semantic and phonetic status of the word, but he also mentions that the words become grammatical endings and that this is a form of ‘conventional existence’, just as we today sometimes talk of *conventionalisation*. Hopper and Traugott (1993: 64-65, 201) have also talked of something called *routinisation / idiomatisation*, by which they mean that speakers tend to repeat expressions they have come across before, rather than create new expressions.

Regarding the relation between inflectional and derivational morphemes, Müller compares the Chinese plural *gin-kiai*¹³⁸ which he claims literally to mean ‘man-whole / totality’ and the English *man-kind*, for instance (Müller, 1862: 45), one a compound and the other a word which includes a derivational morpheme.

At this time *agglutination* had already been introduced as a term for this process of change and Müller (1862; 1868) also uses it, but he also speaks of *gluing*, *addition* (which however may be different, see below) and *coalescing*. However the latter is seen as step further than agglutination, something which perhaps resembles what we would now call *demorphologisation*.

... **Agglutination** [...] This means not only that, in their grammar, pronouns are **glued to** the verbs in order to form the conjugation, or prepositions to substantives in order to form declension. [...] for in Hebrew as well as in Sanskrit, conjugation and declension were originally formed on the same principle. [...]

In the Aryan languages the modifications of words, comprised under declension and conjugation, were likewise originally expressed by **agglutination**. But the component parts began soon to **coalesce**, so as to form one integral word, liable in its turn to **phonetic corruption** to such an extent that it became impossible after a time to decide which was the root and which the modificatory element. (Müller, 1862: 297, emphasis mine)

It is also clear here that Müller believed that both personal endings on verbs and case inflections on nouns could be derived through agglutination, which was possibly followed by the words *coalescing* into one word.

Other words which Müller treats are the Romance adverbs, which are formed by a derivative from the Latin *mente* (ablative of *mens*). This shows quite a lot of

¹³⁸ some diacritics missing

phonetic decay in French, and is no longer felt to be an independent word, whereas in Italian the word has not yet been reduced phonologically, but also there it is no longer an autonomous word (Müller, 1862: 46-47).

Müller also comments on the Cochinese means of forming past and future through the addition of particles, such as *da* ‘already’ being used to indicate the past. It is however possible that by *addition* he does not mean *agglutination* but rather *juxtaposition* since he is talking about Chinese which is primarily an isolating language.

Like von der Gabelentz and many other linguists, Müller observed the fact that there are two processes at work in these kinds of changes in language, one which could be said to lead to growth and one to decay:

We are accustomed to call these [phonetic] changes the growth of language, but it would be more appropriate to call this process of phonetic change decay, and thus to distinguish it from the second, or dialectic process, which we must now examine, and which involves, as you will see, a more real principle of growth. (Müller, 1862: 48)

Similarly, Hopper and Traugott (1993) choose to concentrate on the role of speaker-hearer interaction in language change, and the role of “maximisation of economy or ‘simplicity’” (Hopper and Traugott, 1993: 63).¹³⁹ Referring to Dressler (1985), they say that “the motivations of simplicity and informativeness are inevitably in competition in the individual language user”, which means that “problem solving” and “conflict” are part of language change (Hopper and Traugott, 1993: 64). This is a continuation of the two forces noted by Gabelentz (1891), which he called *Bequemlichkeitstrieb* (“indolence, ease”) and *Deutlichkeitstrieb* (“distinctness”) (cf. Heine et al., 1991a: 8).

5.3.4 John Earle (1824-1903)

In the second edition of Earle’s *The Philology of the English Tongue* (Earle, 1873) there are quite a few brief mentions of the origin of grammatical words, auxiliaries, copulas, etc. He mentions that *be* appears to stem from a sense of growing, that the French *été* stems from Latin *stare* ‘stand’, and he also briefly treats the **gradual** passing of *will* from being a *presentive* word into a *symbolic* state, which is important

¹³⁹ Based at least partly on Langacker 1977:101-6

since ‘gradual’ is an important key-word in grammaticalisation studies at present (Earle, 1873: 271, 283). Unlike Beths, who has recently treated the history of *dare*, Earle also believes that this auxiliary stems from a concrete verb which has been bleached (Beths, 1999; Earle, 1873: 283). This is the way most auxiliaries have been thought to have developed. However, Beths (1999) has claimed that *dare* is a counterdirectional exception.

Like Sayce (5.3.6) and many other linguists around this time, Earle relates the Germanic weak preterite ending to *do/did*. He does so with a reference to Max Müller (1861: 219) (Earle, 1873: 290). The same idea is mentioned again, as a footnote, in the fifth edition, but without reference to Müller this time, instead Bopp and Grimm are mentioned (Earle, 1892: 294-295 fn).

Earle (1873) is primarily a description of the English language at a specific point in time, rather than a description of its development. However, he includes brief comments on some prepositions which he points out are derived from nouns, e.g. Eng. *till* which he claims is related to German *Ziel* ‘goal, mark, aim’ (Earle, 1873: 484; 1892: 509). He also observes that conjunctions can be derived from prepositions, pronouns or compounds consisting of prepositions and nouns, nouns, adverbs, etc (Earle, 1873: 490-498; 1892: 516-523).

In his treatment of compound prepositions, Earle also appears to have some sense of language renewing itself continuously. He notes how two prepositions, for instance, can come to occur each on one side of a *presentive word*, in other words what we might call a *content word*. This word can then get gradually taken up in a prepositional expression. Eventually, this may become the only context in which the word is used in the language (Earle, 1892: 515).

Interestingly, Earle mentions Horne Tooke a few times in both the second and the fifth editions of his *Philology* (1873; 1892). He also uses one of Horne Tooke’s rather well known and sometimes seemingly farfetched examples (although he does not mention that Horne Tooke used this example):

Conjunctions formed from verbs, or containing verbs in their composition. The first place here is claimed by the old familiar *if*, Saxon *gif*, imperative of the verb *gifan*, to give. (Earle, 1873: 498-499, italics original)

Earle admits that this conjunction may also have another source, but appears to see the two sources as cooperating in some sense, rather than competing (Earle, 1873:

499 fn.). One of those who has pointed out how unlikely it is that OE. *gif*, Mod.E. *if*, should be related to *gifan* is Sweet (cf. 5.3.7).

The older school of philologists regarded form-words as arbitrary inventions made for the express purpose of showing grammatical relations. One of the earliest and most energetic opponents to this view was our countryman Horne Took [sic.], whose *Diversions of Purley*, first published about 1770, is an attempt to show that even prepositions and conjunctions once had a definite independent meaning, and are simply worn-down forms of full-words – a view which is now generally accepted. Thus he connects *if*, Old English *gif*, with the verb *to give*, making out that *if* originally meant ‘given (or granted) that.’ Although we know now that this view is incorrect, and that *if* is really formed from an old noun meaning ‘doubt,’ we cannot be severe on Horne Took [sic] for this and the other mistaken etymologies in his book; as regards *if*, he was misled by the Scotch form *gin*, which, however, really seems to owe its *n* to association with the participle *given*. (Sweet, 1900 [1930]: 43)

However, this view, presented by Sweet as a slight criticism of one of Horne Tooke’s examples, was published nearly 30 years after Earle’s book, so Horne Tooke’s views may still have been considered correct at the time Earle published his views.

If we sidetrack marginally, we find that Earle also recognised changes which today would probably be labelled as *reanalysis*, such as when he talks of how people interpret *handy-* in *handywork* as the adjective ‘handy’, eventhough, the *y* actually used to be part of the second part of the compound, viz. *work*, as in *ywork* a form stemming from *geweorc* (Earle, 1873: 560). This could be seen as an example of a process which, **when** it occurred between two separate words, was later to be called *metanalysis* (cf. section 6.5.1 on Jespersen).

Earle recognises that grammatical inflectional morphemes stem from previously autonomous words, and refers to their growth out of *composition* (Earle, 1873: 562):

Out of composition has grown, and by insensible modifications developed itself, that phenomenon so interesting to the philologist, and so frequent in his discourse, namely, FLEXION. The origin of flexion appertains to this eldest group of compounds; but for the action and behaviour of flexion when once established, we may go to the second or middle order of compounds; and indeed, we may speak more generally, and say: – *Flexion occupies the middle zone of the whole sphere of human language as it is historically known to us.* (Earle, 1873: 562, italics original, emphasis (bold) mine)

In the fifth edition of *The Philology of the English Tongue* (Earle, 1892), the origin of flexions is no longer as obvious. There, Earle discusses the origin of the

personal forms – possibly because the accepted view of their origin has come under attack and can no longer be viewed as common knowledge. The fact that the Boppian theory was being questioned at the time is also something Earle sees reason to mention, firstly with a brief mention in the running text and secondly with a longer treatment in a footnote (Earle, 1892: 260):

The six persons are thus exhibited by Curtius in the way of a scientific restoration: the root DÂ means give, *da-ma* give-I, *da-twa* give-thou, *dâ-ta* give-he [...] *Zur Chronologie der Indogermanischen Sprachforschung*, von Georg Curtius, Leipzig, 1873. **This theory has of late years been vigorously attacked.** Professor Sayce, in an article entitled ‘The Personal Endings of the Indo-European Verb’ writes: - ‘But is the relationship between the personal terminations of the verb and the personal pronouns really a fact? I also once thought so, but further study has convinced me that I was wrong. When we try to analyse the terminations of the verb, we find that they connect themselves, not with the personal pronouns, but with the suffixes of the noun.’ *Internationale Zeitschrift für Allgemeine Sprachwissenschaft*, Band I, Heft I, Leipzig, 1884. ‘The resemblance between the personal endings of the verb and the personal pronouns turns out, when closely examined, to be really illusory.’ *id.* **If this view prevails, it will cause a revolution in philological habits of thought.** Nouns will no longer rank as the derivatives of verbs, but vice versâ. **The new theory is that person-endings are adaptations of nominal suffixes**, and that nouns date before verbs. I quote again: ‘Against the hypothesis of a nominal origin of the forms of the finite verb no syntactical objection can be raised. [...]’ ‘The meaning of the sentence was determined by the context and the order of the words which composed it; it was only gradually that this meaning came to be transferred from the context and attached to the terminations of the words.’ *id.* (Earle, 1892:260 fn., italics original, emphasis (bold) mine)

It is interesting here that Earle refers to Sayce’s 1884-publication (1884), eight years after its publication, and indicates that the debate is still going on, and its effects on philology are therefore not clear yet.

Earle does not only treat the origin of **inflectional** morphemes as one of compounding or composition, but also derivational morphemes such as *-hood*, *-ness*. In discussing these he also makes it clear that the parts of the composition that have become inflectional or derivational morphemes have been reduced semantically. He says that they become “symbolic” through a “symbolising process”, and that they are now “merely an abstract collective sign” (Earle, 1873: 563).

Like others writing around the turn of the century, Earle had the feeling that perhaps synthetic languages did not stem from analytic. He felt that what he called *symbolism*, but which I understand to mean autonomous grammatical forms,

periphrasis, is something of a more recent nature than inflectional morphemes (cf. also e.g. Jespersen, 1922 [1949]):

Although we cannot pursue our research so far up into antiquity as to arrive at a station where inflexions exist without symbolic words, yet we have sufficient ground for treating flexion as an ancient, and symbolism as a modern phenomenon. One reason is, that in the foremost languages of the world, flexion is waning while symbolism is waxing. Another consideration is this, that after the growth of the symbolic element, the motive for flexion would no longer exist. (Earle, 1892: 249)

In some ways it seems as though Earle did not see the spiral which others (e.g. Gabelentz, 1891) could see, but he spoke of a form of renewal of prepositions whereby content verbs were taken up in prepositional compounds. He also mentions that inflectional morphemes originate in words which have developed some *symbolism* and which have then become attached to a content word, which seems to indicate a possible spiral or cycle after all:

The inflexions are partly words which, having made some progress towards symbolism, and having lost accordingly in specific gravity, have been attracted by, and at length annexed to, the denser substance of presentive words. How far this will suffice to account for Flexion as a whole, and how far it may be necessary to fall back upon causes of a more primitive, rudimentary, and physical nature, is a question yet unsettled. That inflexions are the remains of attached words, is what constitutes the theory of Agglutination. (Earle, 1892: 249)

Earle's terminology for phenomena such as the formation of grammatical inflections concurs with that of other linguists around the same time. He uses the term *agglutination*, but he also speaks of *condensation*, *symphytism*, *compound*, *adaptation*, and words being *annexed to* another.

5.3.5 Frederic William Farrar (1831-1903)

Frederic William Farrar (1831-1903) believed that in Indo-European and Semitic languages one could see a tendency for change from synthetic to analytic structure (Farrar, 1868: 1). This was a problem to him, since at the same time he could not help but believe that there should at some point have been a move from analytic to synthetic. And he also hinted at the possibility that language might be cyclic (Farrar, 1868: 2):

But vast as is the array of linguistic evidence to establish the reality of this tendency in the two chief Families of Language during historic periods, are we not inevitably compelled to assume the existence of *another and prehistoric* tendency to advance from analysis to synthesis? Even if there be no remaining traces, or none but the most evanescent and fugitive traces of such a change, are we not logically forced to *assume* a *cylical* tendency of language? (Farrar, 1868: 2, italics original.)

After examining the matter, by consulting the different types of languages, Farrar (1868) concludes that although it seems that grammatical elements stem from full words, and that there has been an agglutinating tendency in the past, we have no way of proving such a history and origin of language. This is so even though he says “*agglutination*” is in fact part of three quarters of the world’s languages (Farrar, 1868: 9).

5.3.6 Archibald Henry Sayce (1845-1933)

Archibald Henry Sayce (1845-1933) was a well-known British orientalist at Oxford who, apart from his publications on e.g. Babylonian literature, religion; the Hittites; and the Assyrians; also published a book called *Principles of Comparative Philology* (1875) and one called *Introduction to the Science of Language* (1880) (for more information see the webpages listed under *Archibald Henry Sayce*). He appears to have been rather critical of the state of comparative philology around 1875. He complains about the confusion in the use of terms, and notes the fact that derivational and inflectional morphemes, are simply tools that we have invented. He complains even more about the idea that languages can change their type (e.g. Sayce, 1875). This is one thing which makes him a good source for finding out more about the state of linguistics in Britain around this time, since he tells us what he thinks, and what is commonly believed, and he often criticises the current ideas in a clear and useful manner. Apart from this he is also happy to say when he himself has gone from one opinion to another.¹⁴⁰

One thing that is very clear to him, as I said above, is that languages cannot change type, and Indo-European (or *Aryan* as he calls it) according to him is an inflectional language and always has been (cf. Sayce, 1875: xii). Later he becomes

¹⁴⁰ In Sayce (1875: 396-401) an appendix is included where he treats some of his latest ideas on the origin of the Indo-European inflectional forms.

highly critical of the idea that agglutination might be a means of getting new flectional forms¹⁴¹, but before the mid-1880s he still appears to have believed that some of the inflectional morphemes stemmed from autonomous words, as we shall see below. However, Sayce was also clear on the fact that he thought there were other means by which language had created new inflectional forms (cf. Sayce, 1875: xii-xiii):

It hangs together with the attempt to transmute all the case-endings from the very first into pronominal or, at any rate, independent words. So far as I can see, many of the flections were formative suffixes before they were turned to their later use. (Sayce, 1875: xiii)

In the above citation, we see a suggestion that flections may stem from other ‘*formative suffixes*’, a comment which resembles what present-day grammaticalisationists sometimes refer to as *exaptation*, turning linguistic items into use in new functions (see chapter 3), cf. also:

When the conception of the locative case, for example, first arose in the mind of the Aryan, he selected some formally existing but hitherto meaningless suffixes to express the new relation, and so **turned a mere phonetic complement, a mere formal sound, into a grammatical inflection**. (Sayce, 1875: 155, emphasis mine)

The idea of the instrumental case, for instance, must have been obtained from a deeper analysis of the sentence, which all along implicitly contained it; and then **some already existing ending or suffix was set apart to express it**. (Sayce, 1875: 157, emphasis mine)

I have long believed that an unprejudiced and thorough-going examination of the Aryan declension would show that its origin was similar to that of the Semitic noun, **the cases being differentiated as the need for them arose out of various more or less unmeaning terminations or ‘suffixes of derivation’ if the latter phrase be preferred**. M. Bergaigne [(Bergaigne, 1875)] has made it clear that this is the fact, and has thus provided a way of escape for believers in pronominal roots out of the difficulties in which they are involved. (Sayce, 1875: 396-397, emphasis mine)

... the only logical conclusion that can be drawn from the results of the author’s researches is, that so far as the declension of the noun is concerned it has grown out of a process of **adaptation** and not of agglutination. As he remarks very justly, we cannot assign the formation of the cases to the same process as that whereby they have been *replaced* in the later stage of analysis; [...] (Sayce, 1875: 397, italics original, emphasis in bold mine)

¹⁴¹ Flectional forms are defined as forms that “denote the relations of grammar, or rather the relations that exist between the different parts of the sentence” (Sayce, 1880: 393-394).

Sayce also recognises that phonetic changes, such as ablaut and umlaut, can develop grammatical functions, another example of something which may be viewed as *exaptation* today:

Thus the Teutonic idioms have **adapted** the *ablaut* or change in the vowel of the root to the expression of the distinction between the tenses of the verb, thus making it inflectional; while it remains in Sanskrit a mere phonetic unmeaning modification of the vowel, the mechanical result of the accent. (Sayce, 1875: 165, italics original, emphasis (bold) mine)

A difference of vowel which was originally purely phonetic has been **adapted** to distinguish between singular and plural in the English *man* and *men*, between transitive and intransitive in Greek verbs in *-οω* and *-εω*¹⁴². (Sayce, 1880: 385, italics original, emphasis (bold) mine)

It is interesting to see that Sayce talks about the ablaut having been *adapted* which of course also bears some resemblance to *exaptation* as a term. Moreover, *adaptation* was also used by others at this time, usually with a rather specific meaning of endings being adapted into a use as pronouns (cf. section 5.1.6). But what is the difference between *adapting* and *exapting*? One of the OED Online's definitions of 'adapt' is "[t]o alter or modify so as to fit for a new use" (Oxford English Dictionary, 1989a). 'Exapt' or 'exaptation' is not listed in the OED Online (2nd edition), but according to Lass the most important aspect of exaptation is that it gives something a new function (Lass, 1990). It seems then that Sayce and others in the late nineteenth century had already observed one of the counterdirectional changes which have caused such debate lately among grammaticalisationists.¹⁴³

The term *adapted*, however, was not restricted to cases such as these, which we would call *exaptation*. Sayce also, at least on one occasion, uses it when talking of the compounding of pronouns with verbs and their development into suffixes (Sayce, 1880: 395), a case which today would probably count as grammaticalisation.

The way Sayce sees it, even if it is the case that certain flectional forms stem from autonomous words, that does not mean that the Indo-European languages have proceeded from an agglutinative to an inflectional stage. Conversely, it means that

¹⁴² some diacritics are missing on the Greek words.

¹⁴³ The words both include the Latin stem *aptāre* meaning 'to fit', and 'adapt' also contains the prefix *ad-* meaning 'to', whereas 'exapt' contains the prefix *ex-* meaning 'out of, from'.

they have always been flectional and that this is why such forms are formed – by analogy with the old ones:

When will it be recognised that the growth of most of our present flections out of independent words indicates not a primitive agglutination, but a preexisting inflectional instinct or analogy, [...] (Sayce, 1875: xix)

It may be that all the inflections of Schleicher's parent-speech will yet be traced back to independent vocables; but this, improbable as it is in the highest degree, will only show that the new suffixes, as soon as they became grammatical signs, were modelled after a fore-existing pattern; they imply that **the language was already inflectional**, and inclined to assimilate everything which modified the meaning of a sentence to the prevailing inflectional type. (Sayce, 1875: 160, emphasis mine)

Sayce sees the driving force behind the change as one of analogy with the existing linguistic system.

But if Indo-European languages have inflections and have developed new inflections simply because they have always been flectional, what about the other languages, the non-flectional languages, e.g. agglutinative languages? According to Sayce, these do not have any inflections, nor could they ever develop them:

The agglutinative languages do not express the relations of grammar by pronominal suffixes – indeed, it is hard to see how they *could* do so – but by the help of postfixed substantives and verbs or participles, each with a definite signification of its own. (Sayce, 1875: 163, italics original)

It is not only flectional forms that Sayce takes the time to discuss. He also considers the origin of more autonomous functional items, such as conjunctions and prepositions, which he recognises may stem from demonstrative pronouns and nouns respectively (Sayce, 1875: 135). Later he also notes that adverbs could sometimes become prepositions (Sayce, 1880: 437). He also believes that the definite article, when it exists in a language, is always derived from a demonstrative pronoun (Sayce, 1880: 421-422). However, he also mentions the fact that there are people who have suggested other origins for some definite articles. Auguste Dozon (1822-1891), he says, has suggested that the Albanian article might be derived from an earlier suffix (Sayce, 1880: 424 fn.).

Considering Chinese, Sayce also notes that the genitive (among other things) can be marked by a relative pronoun *ti* which he claims stems from a noun meaning 'place' (Sayce, 1880: 224). In a more general statement he also observes that what

Earle (cf. section 5.3.4) called *symbolic words* and what the Chinese called *empty words* are words “which have been stripped of their original nominal or verbal signification, and applied as auxiliaries and helpmeets to express the relations of a sentence” (Sayce, 1880: 23).¹⁴⁴ Today this statement sounds similar to the common talk of semantic bleaching as part of grammaticalisation. In addition, he recognised that function words often undergo phonetic reduction:

Constant use and close amalgamation with other words tend to attenuate symbolic words, and cause them to be especially affected by the action of phonetic decay; hence it is that pronominal roots consist for the most part of open syllables like *ka, na, ma, ta*. (Sayce, 1880: 24, italics original)

One type of inflectional forms which I mentioned were important in Bopp’s work on the origin of grammatical forms (see above section 5.1.4), were the personal endings of verbs. When Sayce was 30 years old he had similar beliefs, and so he stated, that “[t]he inflections of the verb in Aryan as well as in Semitic can be traced to the attachment of the objective case of the personal pronouns to the root or base, [...]” (Sayce, 1875: 148). However, his views later changed and he became one of the first in Britain and in Europe to contradict this view. Already in 1875 he realised that there were optional views, which he incorporated in footnotes, e.g. comments on Rudolph (Georg Hermann) Westphal’s (1826-1892) (1873: xxiii ff) opposition to *agglutination theory*:

... the person-endings of the verb are the originals out of which the personal pronouns have been afterwards elaborated by a process of analysis and differentiation. (Sayce, 1875: 148-149)

But still, at that point Sayce was not ready to dismiss *agglutination theory* completely. He believed it could be proved, at least for the verb, as he thought could also be proved for the Semitic verbal flexion (Sayce, 1875: 149). Although he also recognises that Westphal makes some good points (argument 3 and 4 cannot be contradicted according to Sayce), which indicates that he is not completely happy with the theory of agglutination:

His [Westphal’s] arguments against the ordinary agglutination theory of the origin of verbal flexion are: (1) that none of the existing forms of the third pers. sing., for example, numerous as they are, represent what the

¹⁴⁴ Sayce himself opposed *symbolic words* to what he called *presentative words* (cf. Sayce, 1880: 25-26).

agglutination theory assumes as the primitive pronoun-termination – *ti* in the present, *t* in the first preterite, and *tu* in the imperative instead of the hypothetical *ta* – and we are not justified in assuming the existence of a form which is never found in any of the many Aryan dialects, and must on the contrary have branched off into three distinct varieties; (2) that the change of the hypothetical *tata* into the deviating *tai*, *ta* and *tau* of the Atmane-pada present, first preterite, and imperative is unparalleled and unwarrantable; (3) that no sign of the third person can be discovered in the *n* of the third pers. pl. (*nti*, *nt*); (4) that the explanation of the fulcrum-vowel (as in *bhav-a-ti*) as a demonstrative is absurd, since a demonstrative would have no sense in such a position; and (5) that if the pronouns had been prior to the verbal endings, the latter would have been formed by means of the nominative and not the objective case of the pronouns, whereas as a matter of fact the nominative case of the pronouns (*aham*, *ego*, for instance) is later than the oblique cases and posterior to the flection of the verb. (Sayce, 1875: 149, italics original)

Five years later, Sayce still believed that the personal endings on verbs in Indo-European languages **might** stem from personal pronouns. However, he was perhaps a bit more uncertain when he said that it was “**highly probable** that the person-endings of the Aryan verb *as-mi*, *a(s)-si*, *as-ti*, [...] are but the personal pronouns closely compounded with the verbal stem” (Sayce, 1880: 392, italics original emphasis (bold) mine).

In 1884 however, Sayce published an article in the German journal *Internationale Zeitschrift für allgemeine Sprachwissenschaft*, entitled ‘The Person-endings of the Indo-European Verb’ (Sayce, 1884), where he firmly dismisses the agglutination theory explanation of where the personal endings of verbs come from. In other words, his hesitation had now become a firm belief that the old theory was not right. He does not only attack the theory of the origin of the personal endings, but also uses this to attack the whole theory of agglutination:

Professor DELBRÜCK, in his interesting EINLEITUNG IN DAS SPRACHSTUDIUM, rests his defence of the agglutination theory of BOPP on the assumption of a relationship between the personal terminations of the verb and the personal pronouns. Every theory on the origin of inflection must start, he says, from the ‘Thatsache einer die Erklärung durch Zufall ausschließenden Ähnlichkeit zwischen einigen Personalsuffixen und Pronominibus’¹⁴⁵ (p. 70). Why we must start from this particular ‘fact’, he does not explain, tho’ I suppose he had in mind CURTIUS’ attempt to show that verbal flexion is older than nominal flexion, an attempt which, as I have elsewhere endeavored to prove, seems to me the exact converse of what is warranted by the evidence. Nor does he explain why we must accept as a ‘fact’ what has been denied by more

¹⁴⁵ Translation: the fact of a similarity between certain personal suffixes and pronouns, which explanation through accident is excluded (cf. Delbrück, 1882 [1989]: 71)

than one scholar. Accepting it as a ‘fact’, however, as I myself formerly did, it by no means follows that we are compelled to go further and accept the agglutination theory. (Sayce, 1884: 222)

One reason why Sayce cannot accept this theory is his belief that nominal inflection is older than verbal inflection. He also believes that languages remain true to their type and never change type. Therefore we first need to try to explain the origin of the nominal declension before we can tackle the history of the verbal inflections (see e.g. Sayce, 1884: 222). However, since Sayce does not think that languages change type there should never be any possibility of finding out what the origin of the first nominal forms was, because they must – presumably – have been there from the beginning.

Further evidence for Sayce’s view that the nominal endings are older than the verbal inflections, could possibly be his new theory on the origin of the personal endings of verbs – which he now believes are clearly connected with the nominal suffixes (Sayce, 1884: 222).

Present-day grammaticalisationists often discuss examples such as the Romance future and similarly Sayce also discussed this. In addition he mentioned the Latin imperfect and future forms, which he believed to be compounds including the verb *fuo* ‘to exist’, and the Germanic (*Gothic*) perfect:¹⁴⁶

The Latin imperfect and future in *-bam* and *-bo* seem to be compounds of the verbal stem with the verb *fuo*, ‘to exist,’ like the perfect in *-ui* or *-vi* (*fui*), while the pluperfect *scripseram* is a combination of *eram* or *esam* and the perfect *scripsi* (itself formed from the verbal stem *scrib-* and the old perfect *esi* of the substantive verb ‘sum’). So, too, the form *amavissem* is just as much a compound of *amavi* (*ama+fui*) and *essem* (*es+siem*) as is *amatus sum* of the passive participle and the substantive verb. If we turn to our own language we can trace our perfects in *-ed* back to the Gothic amalgamation of the verb with *dide*, the reduplication perfect of the verb *do*, while the origin of the French *aimerai* in the infinitive *aimer* (*amare*) and the auxiliary *ai* (*habeo*) is as plain as that of the Italian *dármelo* (‘to give it to me’) [...] (Sayce, 1880: 393, italics original)

Although Sayce could at one time see that verbal endings could possibly stem from autonomous words, he saw no way of proving the same for most of the nominal declension:

¹⁴⁶ There is no mention of the origin of this inflection in the OED, however it is compared to Latin participles in *-tus*.

All this would apparently tend to show that flection did not originally belong to the verb, and that there was a time when its several relations of time and mode and person were each expressed by independent words. The analysis thus successfully carried out in the verb has been applied to the noun, but the results here have not been so decisive. One or two of the case-endings have been identified with prepositions, or in this case postpositions, [...] (Sayce, 1875: 150)

The very existence, then, of classificatory suffixes due to composition in our Indo-European idioms implies the existence of earlier suffixes for which we cannot claim a similar origin. We have already seen that this is the case with many of the suffixes which serve the purposes of flection; though the person-endings of the verb go back to separate words, **every attempt to discover such a derivation for the principal case-endings has ended in failure.** (Sayce, 1880: 402, emphasis mine)

At the same time it seems that Sayce believes that Bergaigne had proved that some cases can be derived from “adjectives used adverbially” (Sayce, 1880: 418). He also recognises that in agglutinative languages of Western Asia (his example is from Akkadian) postpositions stemming from verbs can be added to words to mark grammatical relations (Sayce, 1880: 420).

Sayce also mentions derivational morphemes, which to him are only theoretically different from inflectional morphemes. He sees the two as developing in the same way. They have arisen out of autonomous words, this has always been the case when we have been able to trace their historical path of change (Sayce, 1875: 164; 1880: 396). However, in 1880 he stresses that this has not necessarily always been the way they developed:

But it does not follow, as a good number of writers on language have assumed, that because some of the classificatory suffixes are examples of composition, all of them are so, any more than in the case of flection and the flectional suffixes. (Sayce, 1880: 397)

The rather sceptical and critical Sayce was also not happy with the way what we now call *the uniformitarian hypothesis* was applied. He dismissed any possibility of inference from the present **forms** to the past **forms** it seems:

I, for one, hold the development theory to be a false though attractive assumption, simply because all science must rest on the law of the uniformity of nature, and consequently the formative *principle* at work in modern times must be of the same character as that at work in the earliest period. To infer that because the later formative *elements* are of a certain nature, the older formative elements must therefore be of the same nature, is in the highest

degree illogical; indeed, it directly contradicts the very hypothesis Professor Whitney is maintaining, since the formative elements of an agglutinative language are wholly different from those of an inflectional language. To say that an agglutinative suffix is identical with a flexion is to confound two very different and unlike things. (Sayce, 1875: 166-167 fn.)

Towards the end of the quotation here it seems as though he claims that others have thought the uniformitarian principle applied to more than it was actually meant to apply to. The uniformitarian principle, as Deutscher (1999) has stressed, applies to forces and principles rather than to states and types (cf. chapter 3). Therefore it should not be applied to comments regarding the state of certain formatives.

Similar to Meillet, Sayce also recognises that when word order, or *position* as Sayce calls it, is used to express grammatical relations, or when *accent* is used in similar manners, they must be seen as grammatical means of expressions just like other grammatical forms (Sayce, 1880: 383) (although, Sayce's way of expressing this is by saying that they must then be seen as part of morphology).

Sayce's terminology includes the term *agglutination theory*. However, he usually speaks of *composition*, words being in a *compound* or being *glued together*, and he also uses the term *amalgamation*. Further, as I have shown above, he occasionally speaks of *adaptation* which in many ways comes close to *exaptation* in present grammaticalisation studies. However, it includes examples which we would not count as *exaptation* now, Sayce also uses it, at least once, for an ordinary example of agglutination. So maybe it was a wider concept than what is now called *exaptation*? A concept that perhaps covered both the development of grammatical items from lexical elements, and including both grammatical words and affixes, and in addition to this, including developments from sound changes to grammar, or from parts of affixes to grammar or from more or less meaningless affixes to grammar. Perhaps it was not a concept at all – maybe Sayce and others had simply noted that words, sounds, and parts of words could be adapted to new uses.

In one sense, one could say that they could be grammaticalised. In another, one could say that they only noted a change without being sure whether to see it in any sense as 'one'. What is clear however is that what we now call *exaptation* was included.

5.3.7 Henry Sweet (1845-1912)

In 1900 Henry Sweet (1845-1912) published an introduction to historical linguistics (Sweet, 1900 [1930]), which in part suggests that Sweet may have been inspired by John Horne Tooke (1736-1812) (cf. Sweet, 1900 [1930]) (cf. Horne Tooke, 1798-1805 [1968]) (cf. Lindström, 2003):

The older school of philologists regarded form-words as arbitrary inventions made for the express purpose of showing grammatical relations. One of the earliest and most energetic opponents to this view was our countryman Horne Took [sic], whose *Diversions of Purley*, first published about 1770, is an attempt to show that even prepositions and conjunctions once had a definite independent meaning, and are simply worn-down forms of full-words – a view which is now generally accepted. (Sweet, 1900 [1930]: 43)

Although Sweet claims this is the general view at the time of writing (in other words around the turn of the last century), he also admits that it is not accepted by everyone. And in fact it would seem that this was no longer the generally accepted view, if we look at the work of Jespersen, for instance (see section 6.5.1). As for Horne Tooke's role as an inspirer, perhaps we would be more correct to see him as a forerunner Sweet recognised after having learnt of the theory from his own contemporaries?

Sweet also recognises that grammatical elements do not have to carry on down the drain-like cline towards zero, or a frozen state, but that they can in fact start moving in the opposite direction:

It may happen that an inflectional element, instead of becoming more and more a part of its stem till at last, perhaps, it disappears altogether, may pursue the opposite course or development, and even regain something of the formal independence of the free particle or full-word of which it is the descendant. (Sweet, 1900 [1930]: 46)

So, although Sweet thinks that grammatical elements **usually** originate in worn down full words, he thinks it is possible to have changes in the opposite direction, at least as far as form is concerned, from affix to free particle.

Unlike Sayce, Sweet still believes that the personal inflections on verbs can be derived from personal pronouns, as many had done before him:

Although we still know very little of the origin of the Aryan inflections, we know that the personal inflections of the verb are simply personal pronouns that have lost their independence. (Sweet, 1900 [1930]: 50)

This is particularly notable though, since Sweet was writing at a time when this view had started to be frowned upon and was about to be discarded. Sweet in other words, was just as ‘old fashioned’ as Whitney (see section 5.4.1) and does not appear to have taken in the latest findings on this subject.

Some of the other examples of agglutination Sweet uses include the French future and the Scandinavian passive where an *-s* stemming from the reflexive pronoun *sik* is added to the active form of the verb:

We can see the development of inflection out of independent words which have lost their formal independence in such forms as the French future *parlerai* from Late Latin *parabolare habeo* ‘I have to speak,’ and the modern Scandinavian passive formed by adding *-s* to the corresponding active forms, the *s* being a shortened form of Icelandic *-sk*, as in *būask* ‘prepare oneself,’ whence the borrowed English *to busk*, the *-sk* again being only a shortening of *sik* ‘oneself’. (Sweet, 1900 [1930]: 44)

However, he also mentions that postposition, pronouns, demonstratives, particles and nouns can develop into case endings (Sweet, 1900 [1930]: 107-108, 110-111).

Furthermore, it should be noted that an important part of Sweet’s discussion of the development of grammatical words and inflections is that for a linguistic item to count as an inflectional or a derivational morpheme it must be *isolated* from the word it stems from (see e.g. Sweet, 1892: 182, 197-198; 1900 [1930]: 42).

Mere obscuration without isolation is not enough to constitute a derivative or inflection. Thus the (l) in (hijl) = *he will*, does not constitute an inflection, because it is added indifferently to all words, and because we can change the unemphatic (hijl) into the emphatic (hij wil), and so break up the connection between the two words and restore the original full form of the (l). (Sweet, 1892: 197)

Notably, Sweet certainly has an understanding of the fact that grammatical devices do not only develop from the lexicon, but can be a matter of making use of earlier phonological changes also, for instance, such as the ablaut of the strong verbs in the Germanic languages (Sweet, 1900 [1930]: 45). This is a near-parallel to what we now call *exaptation*, in modern terms this could be seen as exaptation in the form of reanalysis of a formerly word-internal phonologically determined sound as a grammatical device. This also makes it clear that it is not only the form that can start

moving in the other direction, but that there can also be functional changes in **non-**lexical items.

A noteworthy point about Sweet's work is that here we actually find a connection between independent content words (*full words*), function words (*form words*) and inflectional and derivational morphemes. He seems to have had a sense of a continuum from the independent lexical item to the more grammatical functional item, be it a derivational or an inflectional morpheme. Among the early scholars discussed in this thesis it is only Humboldt who appears to have had a similar idea before Sweet. Sweet also clearly saw a strong parallel between inflectional and derivational morphemes and explicitly stated that formally they were the same, but that there was also a strong connection between function words / form words and inflectional morphemes – in that there was not “necessarily any formal distinction between” them (Sweet, 1900 [1930]: 44), by which I suppose he is referring to their formal function rather than their form.

I have looked at three of Sweet's publications for my thesis (1892, 1898, 1900 [1930]). However, I have only found parallels to grammaticalisation in two (1892, 1900 [1930]), the third being the second part of his *New English Grammar* (1898) which concentrated on syntax. In both of the other publications there are clear references both to *full words* developing into grammatical words / form words, but also to agglutination giving us new inflectional and derivational morphemes (Sweet, 1892; 1900 [1930]). I shall start by treating the two separately.

There appears to be more comments on the origin of grammatical words than on the origin of inflectional and derivational morphemes in Sweet's work, although he certainly treats both. This may be quite understandable seeing as he was treating the English language, which at present, and for a rather long part of its more recent history, has been more concerned with ridding itself of inflectional morphemes than with creating new ones.

The examples of function words that he mentions, have developed out of formerly content words, are verbs, adjectives and nouns developing into prepositions, demonstrative pronouns developing into definite articles, full verbs becoming “*link verbs*” (italics mine) and then continuing to develop into “mere grammatical devices” (Sweet, 1900 [1930]: 43, 53, 55-56, 89). The last example also shows a rather clear sense of what among the grammaticalisationists at the end of the century was to be called a *cline*. Furthermore, that is not the only time when the reader feels that Sweet

had a sense of a continuous cline-like development of grammatical devices. He also notes:

It may happen that an inflectional element, instead of becoming more and more a part of its stem till at last, perhaps it disappears altogether, may pursue the opposite course of development, and even regain something of the formal independence of the free particle or full-word of which it is the descendant. This has happened with the genitive ending in English. (Sweet, 1900 [1930]: 46)

The above citation makes it clear that he had a concept of a cline all the way from the lexical content item to the inflectional item and even to zero, just like the clines of today. The possible difference is that it seems he would have included derivational and inflectional morphemes on the same cline, something which, Hopper and Traugott (1993) have not wanted to do. Instead they speak of two clines, a *cline of grammaticality* and a *cline of lexicality*, the first ending in an inflectional morpheme (or zero) and the second in a derivational morpheme.

It is also worth noting that Sweet says this cline can be reversed, which fits in with the definition Norde (2002: 47-48) has given of *degrammaticalisation*, and could therefore possibly count as evidence that Sweet did not believe that this kind of change was unidirectional. This provides us with more evidence that Sweet saw the possibility of changes moving in the opposite direction of the ordinary cline. Even more evidence rests in that Sweet believed that grammatical markers could appear by other means than through the use of formerly lexical items, something we find in his discussions of umlaut and ablaut:

In the corresponding English plural *feet*, the old *-i* after causing a similar mutation (p. 22) of the preceding vowel was at last dropped entirely, so that the inflection is now marked by vowel-change only. The ‘gradation’ of our strong verbs by which we distinguish such forms as *sing*, *sang*, *sung*, is a striking instance of how sound-changes which were originally accidental – in this case the result of the stress falling on different syllables in different inflections of the verb – have come to have a definite grammatical inflectional function. (Sweet, 1900 [1930]: 45, cf. also p. 106)

This kind of change, like that of *adaptation* discussed above, resembles what is today variously called *exaptation*, *regrammaticalisation* or *functional renewal* (cf. Brinton and Stein, 1995; Greenberg, 1991; Lass, 1990; Vincent, 1995; chapter 3 above).

There does not seem to be any term that Sweet applies to the whole change we now call *grammaticalisation*, and which he also discusses in a way that very much

resembles the discussions of this phenomenon at present - he clearly has a concept of all of these changes forming a whole in some way. He occasionally uses the term *agglutination* or *composition* for the joining of words into compounds and the further development into inflectional and derivational morphemes. He also speaks of verbs as *sinking* (Sweet, 1900 [1930]: 43) when they develop more strictly functional characteristics such as in becoming copulas or auxiliaries, but his concept of the cline-like development receives no term.

5.3.8 Lionel Graham Horton Horton-Smith (1871-1953)

Lionel Graham Horton Horton-Smith (1871-1953) joined St John's College as a Classical Tripos student in 1889 or 1890 and received his BA 1893 (Colbert, 2003). Horton-Smith was initially mainly interested in classics and philology and wrote some minor publications on these subjects. However, in 1896 his interest turned to law and he was elected M'Mahon law scholar (Colbert, 2003).¹⁴⁷ Shortly after receiving his BA, Horton-Smith published an article in which he discusses the Latin gerund and gerundive as an example of agglutination (but called by no term) (Horton-Smith, 1894). Following Brugmann, Horton-Smith believes that the *-n-* in the gerundive suffix *-ndo-* may stem from the Proto-Italic accusative infinitive *-m*, to which Horton-Smith believes *-do* was attached in Latin as a descendent of the root $\sqrt{d\bar{o}}$ ¹⁴⁸ 'give' (Horton-Smith, 1894: 196, 198, 202).

Venum do, originally = 'I give as a selling or a sale,' *venum* being in apposition to the object of *do*, or it may be equally well explained 'I give a selling (i.e. the sale of) some object,' e.g. *Si pater filium ter venum duuit (? or davit) filius a patre liber esto*, Leg. XII Tab., 'If a father thrice gives his son as a sale' (or 'the selling of his son'). Hence *venum do* became gradually regarded as a unity = 'sell.' We find it both uncompounded and also compounded as *venumdo* or just like the gerundive forms) *venundo* (cf. *vendo*), an abbreviated form for *venum do*, and *veneo* for *venum eo* 'I come as a sale,' i.e. 'am on sale'. (Horton-Smith, 1894: 209)

Another example of the same type of change, also mentioned by Horton-Smith is the Romance future, one of the paradigm examples of grammaticalisation. He explains the Romance form as an "infinitive governed as object by *habeo*" (Horton-Smith, 1894: 211), which is similar to the gerundive example that he himself is

¹⁴⁷ He was awarded an MA in 1897, the same year he was called to the bar by Lincoln's Inn (Colbert, 2003).

¹⁴⁸ The $\sqrt{\text{ }}$ -sign is a standardised way of showing that what follows is a root (Schaefer, 2003, pc).

trying to explain in that he believes that it also consists of an infinitive in the accusative governed by another verb (*do*).

Linguists	Grammaticalisation?	Unidirectional?
Max Müller	Agglutination (<i>agglutination, gluing, addition, coalescing</i>)	Yes
Archibald Henry Sayce	agglutination adaptation content words > functional words (<i>agglutination, amalgamation, compound, glued together, adaptation, composition</i>)	No. (adaptation)
John Earle	agglutination <i>presentative > symbolic words</i> (<i>agglutination, condensation, symphytism, compound, adaptation, annexed</i>)	No? (adaptation)
Henry Sweet	grammaticalisation	No / weak unidirectionality ¹⁴⁹

Table 5.3.1 *Nineteenth Century British Linguists's views regarding the development of grammatical forms.*

¹⁴⁹ Sweet mixes strong unidirectionality statements which statements that claim that the change can sometimes be reversed and may therefore be classified as someone who believes in Weak Unidirectionality.

Linguist	Examples
Max Müller	personal endings of verbs case endings Romance adv. <i>-ment(e)</i> Chinese particles: <i>da</i> 'already' > PAST
Archibald Henry Sayce	umlaut, ablaut > grammatical elements demonstrative pronoun > conjunction noun > preposition adverb > preposition demonstrative pronoun > definite article noun > relative pronoun (personal pronouns > person endings on verb) Latin future, imperfect Gothic (Gmc) weak preterite > <i>did</i> French future word order accent nominal endings > personal endings
John Earle	<i>will</i> : content word > function word <i>dare</i> : content word > function word Germanic weak preterite < <i>did</i> noun > preposition, cf. Germ. <i>Ziel</i> - Eng. <i>till</i> prepositions, pronouns, nouns, adverbs > conjunctions <i>if</i> > IMPERATIVE of <i>gifan</i> 'to give' (cf. Horne Tooke) personal pronouns > personal endings?? nominal endings > personal endings?? noun > derivational ending
Henry Sweet	verbs > link verbs > gram. devices verbs, adjectives, noun > prepositions demonstrative pronoun > definite article umlaut, ablaut personal pronoun > personal ending French future Scandinavian passive <i>-s</i> < refl. pron. <i>sik</i>

Table 5.3.2: Examples used by the Nineteenth-century British linguists in this study.

5.4 The United States¹⁵⁰

5.4.1 William Dwight Whitney (1827-1894)

Otto Jespersen (1922) mentions William Dwight Whitney's (1827-1894) interest in the development of grammatical forms. The book he cites from was published in 1875, when Jespersen himself was still too young to have read it. And we know for a fact that Whitney also treated this subject in 1867.

Whitney was educated partly in Europe and kept up to date with what was happening in linguistics across the Atlantic. He was an influential and very important character in the history of American linguistics, being one of the first scholars to work within the newly institutionalised linguistics in America (Andresen, 1990). His interest in language change was partly inspired by a general interest in evolution, and he also learnt much from his brother who was a geologist. Geology, one should note, is somewhat like biology when it comes to directional tendencies. It does not seem likely that there could really be any non-directional changes in geology, since stone, for instance, that has been worn away cannot come back (although there can be new creations of drop stones in lime stone areas). This makes it interesting to see where Whitney stands in relation to agglutination theory and unidirectionality.

As is more or less expected his views are unidirectional, and although it has lately been pointed out that this may seem to go against the uniformitarian principle (cf. Lass, 2000), a principle which Whitney has been considered to have introduced into linguistics (cf. Christy, 1983), Whitney has no problem combining unidirectionality and uniformitarianism. He has no problems assuming that the first stage in language was a monosyllabic isolating root state even though such a language type does not seem to exist today (Whitney, 1867 [1973]; 1994 [1875]). The reason this is acceptable to Whitney is probably that for him uniformitarianism only applied to processes, and not to states (cf. Deutscher, 1999).

Whitney believed that there was a force in language (in a universal sense) which makes it move in one predetermined direction. However, he also recognised

¹⁵⁰ My treatment of the nineteenth-century history of grammaticalisation in the States is based on my reading of Whitney and consultation of the *Journal of the American Oriental Society* and the *American Journal of Philology*.

that it is possible for other directions to occur sometimes, but that we cannot predict what will happen, what will change and how. We can only say what is most *likely* to happen, because evidence may tell us that something has usually (or even always) been the case up until now. Therefore, inductive reasoning is likely to tell us that the result will most probably be the same this time (cf. Whitney, 1994 [1874]: 50). Whitney, believed in unidirectionality, but he also saw the possibility of countermoves. Thus his views were rather weak compared to those of many grammaticalisationists today.

5.4.2 Maurice Bloomfield (1855-1928)

Maurice Bloomfield (1855-1928) was a professor of Sanskrit and Comparative Philology at Johns Hopkins University in Baltimore, Maryland. In the 1890s he published at least two articles on *adaptation*, a process which we have also come across in some of the writings of European scholars (see Delbrück and Sayce), but in his paper it seems even clearer that this would have to be compared to what we call *exaptation*:

The term adaptation is used here to designate the infusion with some definite grammatical or lexical value, of a formal element originally either devoid of any special functional value, or possessed of a value which has faded out so completely as to make this infusion possible. (Bloomfield, 1891: 1)

The definition provided by M. Bloomfield makes the similarity clear, but perhaps his examples which include umlaut and ablaut as well as the example of stem formatives becoming plural suffixes makes it even more obvious (Bloomfield, 1891).

Notable is the fact that M. Bloomfield clearly believes that adaptation concerns both derivative elements and inflectional elements, and also that according to him a form has been adapted even if it is only used once, i.e. with very shortlived productivity. Such examples are not usually discussed as exaptation at present, but perhaps more commonly as *lexicalisation*. For instance, M. Bloomfield's (1895: 410-411) examples of *idolatory* giving *symbolatory*, would compare to the discussions in e.g. Ramat (1992) of *Watergate* > *petrolgate*, as lexicalisation.

In his second paper, M. Bloomfield provides much more discussion of the concept of *assimilation* which is a form of analogical extension, whereby words in the same group tend to become more and more alike. This would **not** then count as

exaptation, normally, since it could not necessarily be seen as a productive morpheme with a new function or meaning. Bloomfield (1895: 419) also says that “[t]he process borders, in fact, upon popular etymology on the one hand, and symbolic association on the other” two processes which we would probably want to keep distinct from exaptation. This may however be a first step towards adaptation, if I understand Bloomfield correctly (Bloomfield, 1895: 410).

5.4.3 Edwin Whitfield Fay (1865-1920)

Edwin W. Fay (1865-1920)¹⁵¹ was a rather active contributor to the *American Journal of Philology* in the last few years of the nineteenth century. Among many articles on historical linguistics he published a two-part article on agglutination and adaptation (Fay, 1894; 1895) with at least one more follow-up note (Fay, 1896).

Fay’s discussion of the origin of various inflections makes it clear that he believes that adaptation has played an important role in the development of language and that this process of adaptation can be compared with what we now call *exaptation*:

Adaptation of abandoned forms to new needs is a regular process of linguistic economy (cf. my remarks on ‘Linguistic Conservation of Energy,’ *Mod. Lang. Notes*, IX, col. 268). (Fay, 1894: 428)

It seems as though Fay believes that both adaptation and agglutination have been part of the development of language, similar to how we now speak about both grammaticalisation (which often is considered unidirectional) and exaptation (which is sometimes referred to as non-directional). One of his comments on agglutination makes it clear that his acceptance of the more recent theory of adaptation does not mean that he has dismissed the old theory of agglutination:

The study of agglutinative groups has brought us to this point: inflexion has developed in the Aryan speech from primitive action-nouns + demonstrative stems, finally lost to consciousness as inflective endings; verbs and nouns proceed from a common stem-background, and case and person and mode-signs from common agglutinative groups of stems + demonstratives. The categories of verb- and noun-inflexion ought then to coincide: so for the verb the category of gender has been set up. (Fay, 1894: 440)

¹⁵¹ Unfortunately, I have not been able to find out very much about E. W. Fay. From his COPAC (www.copac.ac.uk) records it would seem that he was an American Indo-Europeanist who wrote mainly on Latin and Sanskrit literature, however he also published a history of education in Louisiana.

The last comment on how inflections in nouns and verbs coincide and how therefore verbs have gender shows a big difference to modern ideas. He also says that nouns have the concept of person, which also seems odd to us now. The citation above also makes it clear that Fay seems to believe in the Boppian or Sanskrit division into verbal roots (verbs and nouns) and pronominal roots.

Some of Fay's examples seem rather far-fetched, but he does his utmost to explain them step by step and he clearly shows a belief in both agglutination theory and adaptation, although agglutination seems to be seen as the more basic process:

I seek to prove that in the fut. *-bit* we have composition with *-dhē-*. According to this theory a fut. *calebit* must have been at one time **calefit*. This constructed form is in actual existence (save for the quantity of the *e*) in the sense of 'is made warm,' a pr. pass. to *calefacio*. When the form **calefit* was in this state, its termination was associated with *fit*, 3d sg. pass. to *facio*, and forms like *calefacion* created, but this did not keep the form from passing on to *calebit*. I thus endow my verb with the form *calet* 'is dry' and **calefit* 'is made dry'; they were adapted to different uses, and *calebit* became a fut., helped to this, perhaps, by the form *erit* (infra., p. 21); but certainly no English-speaker would find it hard to believe that the fut. sense has developed directly from the pres. (Fay, 1895: 9-10)

Fay also discusses the ever haunting example of the Germanic weak preterite. He believes that this can be derived from the possibly originally demonstrative root $\sqrt{dhē}$ meaning 'put' (Fay, 1895: 2, 15). Much more than this he does not say about this inflection. However, he mentions some of the possible objections that have already been dismissed and he also mentions what he sees as the greatest argument against the idea, which is the Old Irish preterite *-t*. But still he dismisses this objection (Fay, 1895: 15-16).

Fay mentions the common example of the development of negations. In trying to find the origin of the Latin form *nihil*, he compares this to Eng. *naught* / *not*, and French and Italian negations and concludes that the latter part of *nihil* must mean:

It is to be noted also that Lat. *non*, the ordinary negative, is a compound of *ne* + *unum*; and compound negatives meet us in French *ne-point*, Ital. *non-punto* 'not at all.' We may seek, therefore, in **-elum* for the meaning of 'whit, bit.' (Fay, 1897: 462)

5.4.4 *Journal of the American Oriental Society*

A look through the *Journal of the American Oriental Society* (founded 1842 and according to their website the oldest American learned society devoted to a specific field (see the website of the American Oriental Society)) makes it quite clear that ideas regarding agglutination were common during the nineteenth century in that there are quite a few people mentioning hypotheses based on such an idea in their papers, many of which are according to the time non-professional linguists, missionaries, etc. Although there were not that many articles that explicitly stated in their title that they dealt with the history of a language, many of the papers on various ‘Oriental’ languages include mentions of the history of some of the items in the language.

The Reverend Francis Mason¹⁵² (1799-1874) read a paper in May 1853 on the Talaing Language (Mason, 1853 [1854]). He noted that the language made use of affixes and particles for grammatical distinctions and in comparison, he also said that the particles used with nouns would usually be prepositions in Western languages (Mason, 1853 [1854]: 281). Unfortunately, he usually makes no comment regarding the possible origin or any other uses that the particles may have.

A couple of years later the Reverend Lewis Grout¹⁵³ (1815-1905) read a paper which looked at the small grammatical elements, in particular prepositions, conjunctions, etc., in Isizulu and languages cognate with it (Grout, 1858 [1860]). He lists the various function words together with a few words on their origin and/or how they can also be used, giving some idea of how they have developed. *Ku*, for instance, is said to be a preposition meaning ‘to, from, in, with’ but it is also used as a prefix in adverbs and with the prefix *u-* it forms the infinitive marker (Grout, 1858 [1860]: 130). He also makes a comment regarding the Hebrew preposition *b*, which he claims that many scholars believe can be derived from the Hebrew noun *beth* ‘house’ (Grout, 1858 [1860]: 132). Similarly, there are comments on adverbs, such as *pezulu* ‘over, above’ which he believes stems from the preposition *pa* ‘near, at’ and the noun *izulu* ‘sky, heaven’ (Grout, 1858 [1860]: 133). As a more general

¹⁵² Missionary of the American Baptist Missionary Union in Burma.

¹⁵³ Missionary of the American Board of Commissioners for Foreign Missions in South Africa.

statement he also says that “many of the prepositions were originally nouns” (Grout, 1858 [1860]: 140).

A paper which is of a more historical-comparative character was presented by the Reverend Edward Webb¹⁵⁴, ¹⁵⁵ (Webb, 1861 [1862]). In saying a few words on agglutinative languages he also makes a comment on the gradual process of agglutination:

In this class [i.e. agglutinative languages], grammatical relations are expressed by affixes or suffixes appended to the root or compounded with it. These agglutinated particles have in the Indo-European languages been gradually melted down into inflections, and sometimes even blended with the root. (Webb, 1861 [1862]: 279)

It is interesting to note that Webb speaks of the particles having ‘melted down’, an expression which we have seen in some of the German scholars mentioned above and we will see a similar Swedish expression (quite possibly calqued from German) in section 5.5. He also comments more explicitly on the development of postpositions and auxiliary words into inflectional morphemes in Indo-European (1861 [1862]: 284).

5.4.5 *American Journal of Philology*

The *American Journal of Philology* (first published in 1880) has published many papers within historical linguistics, some of which show a clear relation to the subject of this thesis. I have treated the articles on adaptation by Fay above, and papers on the history of the gerundive by L. Horton-Smith have been treated in the section on British scholars since he worked in Cambridge, England. But it is important to note a few more papers which include views which are reminiscent of grammaticalisation.

F. D. Allen¹⁵⁶ makes the interesting comment that “[a]dverbs are *fossilized* cases, so to speak, of dead (or living) nouns”, which seems to indicate that he may have had an interest in grammaticalisation in the sense of ‘how do grammatical items develop’ (Allen, 1880: 129). Unfortunately, the rest of the paper does not seem to

¹⁵⁴ Missionary of the American Board of Commissioners for Foreign Missions in Southern India.

¹⁵⁵ I have not been able to find out when he lived exactly.

¹⁵⁶ Probably Frederic De Forest Allen (1844-1897).

include anything of much interest for research in the vein of agglutination / grammaticalisation.

B. F. O'Connor¹⁵⁷, treated the frequent example of the French negative, only with concentration on the particle *mie* rather than *pas*. He relates how the French negative *non/ne* has become weakened and therefore needed to be reinforced by various particles, e.g. *mie*, *pas*, *point* which all represented small quantities (O'Connor, 1881). The way he sees it these particles started out as nouns which became used as adverbs together with the negation (1881: 210).

There was also a paper published by Edward Henry Spieker (1859-?) which is worth a closer look, this treated the introduction of direct speech by conjunctions (Spieker, 1884). He notes that these conjunctions often stem from demonstrative pronouns, as for instance in Germanic languages, but that it can also have been “a relative pronoun or a relative adverb of manner” (1884: 221-222).

There are also occasional articles on more exotic, Asian languages. In an article by John Avery (1837-1887) the Ao Naga language in Assam, a Northern district of India, Avery believes that inflectional morpheme for the present indicative (*-er*) of the language may be derived from the verb ‘to be’ (1886: 353). He also comments on at least one verb which has acquired an auxiliary sense, where he notes that “it [*dok* ‘to appear’] shows a tendency to become a merely formative element” (Avery, 1886: 356). Similarly, he remarks on the possibility that other verbal modifiers may also have their origins in verbs (Avery, 1886: 357). Part of Avery’s reason for producing these examples is to question the idea that classes all “rude tongues between Tibet and Burma” can be categorised as monosyllabic, in that he can prove that this language shows clear signs of some agglutination at least (Avery, 1886: 361).

5.4.6 Summary

Scholars in both Britain and the States touched on agglutination theory during the nineteenth century. And towards the end of the century there seems to have been a feeling that agglutination was not as generally accepted anymore and discussions of other processes started to sneak their way in. There is talk of adaptation, either in a very general sense or similarly to what we now call *exaptation*. Towards the end of the century, there also starts to appear discussions of a cline from the lexicon to the

¹⁵⁷ I have not been able to find out more exactly who he was or when he lived.

grammar, including an intermediate step between lexical full words and affixes which function words tended to fill. We already saw this before in Humboldt (1822) but it does not seem to have been picked up on at first. It appears again in Sweet's work and we feel that there is a sense of grammaticalisation starting to appear that includes both grammatical words and affixes.

Linguists & Publication	The Origin of Grammatical Items?	Unidirectionality? (UD)
<p>W. D. Whitney <i>Language and the Study of Language</i>, 1867.</p>	<p>YES. “the elaboration of formative elements out of words possessing independent significance” (1867:136-137)</p> <p>“since all known words have been constructed by putting together previously existing items of speech, the combination of old materials into new forms, the making of compounds, with frequent accompanying reduction of one of their members to a merely formal significance, is a very prominent part of the mechanism of language, one of the most fundamental and important of the processes by which are carried on its perpetual growth and change, its organic development.” (1867 [1973]:67)</p>	<p>YES. “No inconsiderable number of the formative elements of our tongue, in every department of grammar and of word-formation, can be thus traced back to independent words, with which they were at first identical, out of which they have grown. It is true, at the same time, that a still larger number do not allow their origin to be discovered. But we have not, on that account, the right to conclude that their history is not of the same character. In grammar, as everywhere else, like effects presuppose like causes.” (1867 [1973]:66)</p>
<p>W. D. Whitney <i>Life and Growth of Language</i>, 1875.</p>	<p>YES. “in the <i>lice</i> of <i>sôthlice</i> we have the full case-form of a compounded adjective, out of which has been made later the adjective and the adverbial suffix <i>ly</i>. Here is illustrated another department of the action of the abbreviating tendency; it is essential to the conversion of what was once an independent word into an affix, an appended element denoting relation.” (1875:52)</p>	<p>YES. “Now it is by no means all, or even the largest part, of our existing formative elements, suffixes of derivation and inflection, of which the origin in this method can actually be proved; and if we are to believe nothing respecting language which does not rest on positive evidence, we shall never make the principle of combination go far toward explaining the growth of language. (1875:124)</p>

Table 5.4.1: Nineteenth Century American Linguists's views regarding the development of grammatical forms.

5.5 Scandinavia

5.5.1 Rasmus Kristian Rask (1787-1832)

Rasmus Rask (1787-1832) won a prize for his essay *Undersøgelse om det gamle nordiske eller islandske Sprogs Oprindelse : et af det kongel. Danske Videnskabers-Selskab kronet Prisskrift* (Rask, 1818b; 1993 [1818]), which although written earlier and submitted in 1814, was only published in 1818. It has been recognised that he mentions the derivation of personal endings from pronouns, just like Bopp did around the same time¹⁵⁸ and similar to Condillac in the eighteenth century (cf. Lehmann, 1982 [1995]: 1). Although Rask in his prize essay is incredibly good at referring to works by others, the names Bopp and Condillac never appear neither in the prize essay nor in the two versions of his introduction to Icelandic / Old Norse (*Vejledning til det Islandske eller gamle Nordiske Sprog* (Rask, 1811), Swedish translation (slightly reworked) (1818a) as far as I can tell.

In his prize essay (1818b) Rask makes quite a few statements that may remind today's linguists of grammaticalisation or processes and mechanisms connected or related to grammaticalisation – I am thinking of phonological attrition, semantic bleaching, bonding, extension, reanalysis, etc. Several times he says that certain verb endings, e.g. personal endings, were derived from personal pronouns (cf. 1818a: 98, 99, 243). He also says that the Gothic¹⁵⁹ (his term for what we now call Germanic languages) future is formed through auxiliary words (1818b: 140; 1993 [1818]: 118). His discussions of the passives (or reflexives) in several languages is another example of a discussion that resembles grammaticalisation discussions in the present, and an example that has been mentioned in later work on grammaticalisation as we shall see below.

¹⁵⁸ Bopp's first text to have mentioned this is probably his publication from 1816 (Bopp, 1816), so if he wrote it just before it was published then Rask wrote about this earlier. But it could also be that Bopp wrote his work much earlier than its date of publication. However, similarly it is of course possible that Rask had this idea long before his published mentions of it. So it is hard to say who was first.

¹⁵⁹ Rask distinguishes between Gothic and Germanic languages. He dislikes the use of Germanic to designate Germanic languages since it bears too strong a connection to German and also because if we use it for all Germanic languages what should we then call the 'truly' Germanic languages, i.e. the West-Germanic. So Gothic (*gotiska*) languages means Germanic if translated to modern terminology, and Germanic (*germanska*) means West Germanic, except when he discusses the use of the term Gothic as opposed to Germanic (Rask, 1818b: 70-71).

Rask claims that the reflexive form of verbs in Russian is formed by adding –*sã* or –*s*’ after the active form of the verb. He also mentions that in Polish the autonomous form *się* is used. Both of these forms are claimed by Rask to be the accusative of the reflexive pronoun in the third person, but used for all persons (Rask, 1818b: 156; 1993 [1818]: 132), in other words having gone through extension in our modern terms. In Lehmann’s terms we could also say that it has gone through a form of obligatorification, since paradigmatic variability has decreased in that the form can no longer vary depending on the person (cf. Lehmann, 1982 [1995]: e.g. 164). Rask also notes that this is what has happened in the Scandinavian languages, where the passive form is of exactly the same origin, according to him (1818b: 156; 1993 [1818]: 132). The same situation is similarly noted in Lithuanian reflexive verbs (1818b: 171; 1993 [1818]: 146). Furthermore, Rask believes that the passive in Greek tends to have a reflexive meaning and that this is probably due to the fact that it seems to originate from such a function and that the personal endings are variants of personal pronouns (1818b: 198, 295; 1993 [1818]: 171, 258).

It is interesting to see how Rask understood the Icelandic or Scandinavian passive. He claims (Rask, 1818b: 293-294; 1993 [1818]: 256) that in Icelandic passives –*st* is “appended to the active form throughout.”¹⁶⁰ He also points out that none of the Germanic¹⁶¹ languages (his terminology, by which he means West Germanic) has a passive form, but instead they use periphrastic forms (1818b: 293-294; 1993 [1818]: 256-257). In Old Norwegian (–*asc* or) –*sc* were used rather than –*st*, according to Rask, and this form is assumed to come from *sic* through what he calls a “contraction” (Da. *sammentrukken* [verb] ’push together’) (1818b: 274; Rask, 1993 [1818]: 256-257). He also claims that sometimes there are some examples where pronouns from the first or second person, rather than the third, are used instead, and gives the example *hugda-mc* (1818b: 274; 1993 [1818]: 257). This, to modern grammaticalisationists, would be a sign of slightly less advanced grammaticalisation, as one form has not yet extended to cover all varieties, or as in this case, all persons.

¹⁶⁰ Original: “...som overalt hænges til Handleformen...” (translation above by Niels Ege).

¹⁶¹ This is Ege’s translation of *germanska* in the Raskian sense of West Germanic, see footnote above regarding the distinction that Rask makes between Gothic and Germanic (cf. Rask, 1818b: 293).

It is notable how Rask explains the order in which these changes concerning the passive have occurred, and I will therefore quote this in full and then point out a few parallels to the more recent work on grammaticalisation:

This *sc*, which accordingly is the oldest and the most original form, is clearly a contraction of *sic* σ : Dan. *sig*, the accusative of *sá sú*, or if one prefers in the genitive, of *sín* (Lat. *sui*); just as one occasionally finds *mc* for *mic* (me) appended to the verbs, as in *hugda-mc* (σ : *hugda-mig*) I thought, it seemed to me. But when the vowel *i* disappeared and the word became unrecognizable, the origin was forgotten and it was extended to all persons [...] But by shifting into this meaning of an ordinary relative word, or rather ending, it eventually changed its form when pronounced rapidly from *sc* to *st*, just as e.g. German *Damast* has come from *Damascus*, and the like; in the end, even *t* was lost and just *s* remained, eventually passing from the relative or, as it is called in the verbs: reflexive, meaning into that of the passive.¹⁶² (Rask, 1818b: 274; 1993 [1818]: 257)

We might not agree completely with what he says here, with his derivations and hypotheses regarding sound changes that can occur and have occurred. Still, we can see that (1.) he recognises an extension of use, (2.) this extension is partly caused by reanalysis, and (3.) that the reanalysis is caused partly through sound change, and (4.) that the semantic change (which in fact appears to resemble a form of bleaching if we like) led to further phonological attrition. I am aware that I am using present day terminology here, but I hope this is not taken anachronistically but simply as what it is – a comparison with how we would have described this today. This of course does not mean that Rask had these concepts.

Rask even manages to find a parallel in French to the Slavic and Lithuanian reflexive verbs and the Scandinavian passives, viz. in the use of the pronoun *se* in forms like *s'appelle* (1818b: 295; 1993 [1818]: 257-258).

Another common example of grammaticalisation mentioned by Rask is the origin of definite articles. In Lithuanian they are seen as derived from pronouns.

¹⁶² Original (Translation above by Niels Ege): “Dette *sc* som altsaa er den ældste og oprindeligste Form, er klarligen sammentrukken af *sic* σ : *sig*, Gjenstandsform klarligen af *sá*, *sú*, eller om man vil, i Ejeformen af *sín* (lat. *sui*); ligesom man og stundom finder *–mc* for *mic* (mig) vedhængt Gjærningsordene, saasom: *hugda-mc* (σ : *hugda-mig*) jeg tænkte, troede mig. Men da Selvlyden *i* bortfaldt af Ordet blev ukjendeligt, har man glemt Oprindelsen og udstrakt det til alle Personer, aldeles ligesom Grækerne tilsidst udstrakte deres [...] til alle Personer isteden for [...] og [...]. Men ved at gaa over til denne Betydning af et almindeligt tilbagevisende Ord, eller rettere Endelse, forandrede det tilsidst ved hurtig Udtale sin Form fra *sc* til *st*, ligesom f. Eks. af *damscus* er kommet det tyske *Damast* o. desl., tilsidst tabtes endog *t* og blot *s* blev tilbage, som fra den tilbagevisende, eller, som man kalder det i Gjerdingordene, tilbagevirkende Betydning, efterhaanden gik over til den lidende.” (Rask, 1818b: 274)

These are articles, unlike the Scandinavian ones, attach to adjectives rather than nouns, but like the early Scandinavian article they are independent enough to continue taking inflections according to case and number as does the adjective – in other words there is double declension (1818b: 168; 1993 [1818]: 143). Rask recognises that the definite article tends to stem from a demonstrative pronoun (Rask, 1818a: 93).

There is no direct mention of the derivation of personal endings from pronouns in the work from 1811 (at least not in the Swedish edition (1818a)), an example mentioned in many other works during the nineteenth century. Rask mentions that pronouns and verbs can occasionally be combined, but claims that this is most common in the imperative form of the second person singular (1818a: 165). It appears that he thinks that this is what most people do in the spoken language - however he finds that they sometimes write the two separately. Since he makes no mention of the personal endings on verbs, it seems as though he probably has not had the idea that these might stem from autonomous pronouns. There are other ideas that bear some resemblance to grammaticalisation. He mentions that some adjectives use periphrastic forms to form the comparative and superlative forms and that this is comparable to endings marking comparison. However, he says nothing about the development of the various comparative and superlative forms (1818a: 116-117). Another thing Rask (1818a) mentions is the etymology of the negation *ecki* which he sees as a compound of *eitt* 'one' and *-gi/-ki* a negative suffix. This is interesting in relation to Meillet (1912) where Meillet uses the origin of several Indo-European negations as examples of grammaticalisation.

Further comments concerning the verb relate to the question of mood and tense. The fact that Icelandic cannot form more than two tenses with inflections is noted by Rask and he claims that this is solved through the use of auxiliary verbs, such as *mun*, *skal*, *hafa*, *vera*. Unfortunately, there are no comments with regard to how these verbs might have been used before they developed into auxiliaries, if they have gone through any semantic changes, phonological changes, etc., as we might have hoped there would be if we wanted to be able to see a sense of grammaticalisation as a process of development.

The next near-parallel to grammaticalisation we find in Rask is the fact that he notes that inflections can sometimes be replaced by what he calls particles (Sw.

partiklar (1818a: 172), Da. *Smaaord*¹⁶³ (1811: 146)). The examples he mentions is the Latin inflections (case inflections), which have been replaced by two prepositions in French and Italian, along with the fact that all personal endings on verbs have been replaced by three or six pronouns in Danish (1818a: 172). Note however that Rask does not claim that the endings have been derived from prepositions or pronouns, only that prepositions, particles and pronouns may play the same role as inflections. He appears quite positive about these changes, but claims that when one lacks derivational morphemes and cannot use compounds as much this is more problematic since it will not lead to simplification but to irregularity and lack of character:

The newer languages seem to have won more simplicity, ease and clarity, than they have lost in brevity and freedom of inversion, and thereby have compensation for their loss, but lack of derivative syllables and decrease in compounds cannot be replaced in any other way, than by accepting foreign words which make the language irregular and characterless.^{164, 165} (Rask, 1818a: 172)

Since word formation is closely associated to one kind of grammaticalisation, namely the affixation of words, or agglutination, it is important to see what Rask's views are on this issue. He mentions two forms of word formation, derivation (Sw. *derivationen*, Da. *Afledning*) and compounding (Sw. *sammansättningen*, Da. *Sammensætningen*¹⁶⁶), and under derivation some of his examples show that prefixes may be derived from prepositions (1811: 148-178; 1818a: 174-209):

For- < an old preposition from which the preposition *fyrir* was later derived.
e.g. *forfaðir* 'forefather', *forðaema* 'condemn', *foreyða* 'destroy completely'.
Ör-, *er-* < a preposition *úr*.
e.g. *örvaenta* 'dispair' (V), *örstuttr* 'very shorttime'.

But Rask also mentions some derivations that are derived from adjectives, such as:

¹⁶³ In the Danish version from 1811 Rask also mentions that instead of inflections one can use word order (Da. *Stillingen af Ordene* (lit. position of the words)) and also in other ways.

¹⁶⁴ The vowel representations <ae> and <oe> have been used where Rask uses an a with a superscript e, and an o with a superscript e respectively. By superscript I here mean that the letter is placed directly above the other letter.

¹⁶⁵ Original: "De nyare språken tyckas hafva vunnit mera i enkelhet, laetthet och tydlighet, aen de foerlorat i korthet och frihet till inversioner, och hafva således skadestånd foer sin foerlust, men brist på derivationsstafvelser och inskraenkning i sammansættningar kan på intet annat vis ersættas, aen genom upptagande af främmande ord, hvilka goera språket ojemt, oregelbundet och karaktersloest; ..."

¹⁶⁶ The second <s> should be a long s according to Rask's spelling (Rask, 1811: 178).

al(l)- < allr ADJ.

e.g. *alvitur* 'omniscient', *allgóðr* 'quite good' (Rask, 1818a: 177)

Occasionally, Rask's examples can be compared to some examples of lexicalisation followed by grammaticalisation in Lehmann (2002). Rask (1811: 175) claims that adverbial expressions can be expressed by some prepositions together with the cases they govern, or by adjectives (*tillægsord* (1811: 175)) or nouns (*navenord* (1811: 175)) in a certain case form. He also says that this kind of construction can give rise to new compounded prepositions and conjunctions or particles (*Smaaord* (1811: 175)) (Rask, 1811: 175; 1818a: 203), such as:

(til) handa einum 'for/so someone'

(til PREP, *handa* NOUN GENITIVE PLURAL)

á hendr þeim 'against them'

(á PREP, *hendr* NOUN ACCUSATIVE PLURAL)¹⁶⁷

Rask's concept of grammaticalisation, if he had one, was perhaps similar to Franz Boas's (cf. chapter 3.3.1). Rask recognised a difference in how different languages express certain things. He even observed that one language may sometimes have the option of expressing things by different means:

Word formation can be done in two ways, namely by derivation and by compounding; of these derivation resembles Formchanges [inflection] most. The limit between word formation and form changes is probably not so well distinguished, so often one finds that what belongs to one in one language, in another language it is part of the other, occasionally one even finds both means used in the same way in the same language to express the same thought.¹⁶⁸, ¹⁶⁹ (Rask, 1811: 148-149)

Having seen that Rask believes that inflectional and derivational morphemes are not always easy to distinguish, it may also be interesting to have a very quick look at his differentiation between compounds and derivations. There is something to this description which shows some of the difficulty in drawing a line between these

¹⁶⁷ Thank you to Dr Yair Sapir, Department of Scandinavian Languages, Uppsala University for confirming that the forms have been correctly classified according to number and case.)

¹⁶⁸ I have not been able to represent the text exactly as it was printed. Long s has been changed to short s, long a has been written as two separate a's rather than as a diagraph.

¹⁶⁹ Original: "Orddannelsen kan ske paa tvende Maader, nemlig ved at aflede og ved at sammensætte; af disse har Afledningen mest Overensstemmelse med Formforandringen. Grændsen imellem Orddannelsen og Formforandringen er vel ikke saa bestemt afpæles, at man jo ofte finder hvad der i et Sprog hører til hin, i et andet Sprog at høre til denne, ja endog stundum i et og samme Sprog finder begge brugte ligegyldig til at udtrykke den samme Tanke"

two categories (cf. also the notion of *isolation* which appears in the later writings of e.g. Brugmann as we saw in 5.1.8):

But both the main and the auxiliary parts may be autonomous words, if one of them disappears out of use outside the compound it [the word] will be a derivation and that is precisely the external difference between derivation and compound.¹⁷⁰ (Rask, 1811: 179)

It does not actually seem as though Rask had a real concept of grammaticalisation. Rather the similarities we see to changes that would be classed as grammaticalisation, are listings of changes that have occurred and Rask has tried to follow them etymologically as he believed it was his duty to do so. But there is no attempt at a generalisation regarding the way that languages change, or that grammar evolves and develops, there is no absolute consistency in the terminology – terms which can be translated as appended, contraction, postfixing are all being used. There is also no sign that he linked the development of grammatical words, such as prepositions and conjunctions, to the development of inflections as grammaticalisationists today would usually do.

5.5.2 Johan Nikolai Madvig (1804-1886)

Johan Nikolai Madvig (1804-1886) is probably most famous for his Latin grammar (1841) which dominated Latin teaching in Europe for many years (Hovdhaugen et al., 2000: 261). But he was also one of “the founders of the critical method in philology” (Hovdhaugen et al, 2000: 150). He wrote mainly in Danish, but that did not stop him from being an internationally recognised scholar (Hovdhaugen et al, 2000: 150).

According to Madvig, grammatical signs (*grammatikalske Betegnelser*¹⁷¹) include inflections (*Bøining af de bencævrende Ord*), function words (*hjelpende Ord*) and word order (*Ordstilling*) (Madvig, 1856: 7). He also sees that prepositions and word order can fulfil the same role as cases in other languages, while noting that some prepositions can be affixed and thereby stand in between ordinary prepositions and case inflections (Madvig, 1856: 25-26). An example occasionally mentioned in

¹⁷⁰ Original: “Men saavel Hoved- som Bidelene maa være selvstændige Ord; er en af dem gaaet af Brug uden for Sammensætning, bliver det for saavidt et Afledsord, og dette er netop den udvortes Forskjel imellem Afledning og Sammensætning.”

¹⁷¹ Long s and Gothic k has been changed into regular <s> and <k>.

recent work in grammaticalisation is the development in French of a question particle *ti* (see e.g., Harris and Campbell, 1995: 65-66¹⁷², Campbell, 2001: 132¹⁷³). This is also mentioned by Madvig, as a way of showing how easily a new inflection may arise even when we do not realise that one could possibly be created or even useful:

The question (about yes or no) is in our family indicated through word order and special particles; if one wants to see how it could lead to an inflection, one only needs to look at the French: *Le roi vient-il*; a continued weakening in the pronunciation of the pronoun and its melting together with the verb will have created a question mode; and this really exists in other language families.¹⁷⁴ (Madvig, 1856: 28)

Notably, this citation includes a comment on the fact that the change involves phonetic weakening, and we also note the expression that the pronoun and the verb ‘melt together’ – which seems to have been a common metaphor of the time as we have seen already above.

Madvig claims it is clear that the (seemingly) meaningless syllables were not always without meaning, but used to have meaning which was how they came to fulfil the role of indicating the relations and forms of concepts. In other words he has a sense that this kind of change involves semantic weakening:

All inflection is, with the exception of the completely subordinate inner (not only consecutive) sound modification in words, arisen through agglutination of originally autonomous, and to this reduced words (words, as they were in the oldest stage of the language, in all nakedness and indefiniteness).¹⁷⁵ (Madvig, 1856: 56)

Madvig believes that all affixed morphemes or inflectional morphemes have arisen through agglutination.¹⁷⁶ However, he recognises that inflection can also consist of internal changes which cannot have arisen in this way. Madvig claims that

¹⁷² They call this *reanalysis* and *extension*, not *grammaticalisation*.

¹⁷³ He calls this *reanalysis* and *extension* and notes that this is a grammatical item without a lexical origin.

¹⁷⁴ Original: “Spørgsmaalet (om Bejaen eller Benegtelse) betegnes i vor Æt ved Ordstilling og særlige Partikler; vil man see, hvorledes det kunde frembringe en Bøiningsform, behøver man blot at betragte det franske: *Le roi vient-il*; en forsat Svækkelse i Udtalen af Pronominet og dets Sammensmelten med Verbet vilde have frembragt en Spørgemodus; og virkelig findes i andre Sprogætter en saadan.” (Madvig, 1856: 28)

¹⁷⁵ Original: “Al Bøining er, med Undtagelse af den aldeles underordnede indre (ikke blot consecutive) Lydmodification i Ordene, opstaaet ved Agglutination af oprindelig selvstændige, hertil nedsatte Ord (Ord, som de vare paa Sprogets ældste Trin, i al Nøgenhed og Ubestemthed).” (Madvig, 1856: 56)

¹⁷⁶ cf. also Hovdhaugen et al, 2000: 263, where it is noted that the Norwegian scholar Ludvig Cæsar Martin Aubert (1807-1887) criticised Madvig and Bopp for their idea that case inflections could be derived from autonomous words (see Aubert, 1843, cited by Hovdhaugen et al, 2000: 263).

in **some** cases we can prove that there are agglutinated parts, as in the personal endings of verbs in the older Indo-European languages and in Semitic languages. And we can do so even more easily with the later creations, such as the passive in Scandinavian languages, the pluperfect in Latin. In addition we should note that we can see both agglutinations that have been completed and those that have not been completed, an example that he gives of the latter are the Hebrew prepositions (Madvig, 1856: 56-57).

Furthermore, Madvig remarks on the fact that as he sees it derivational morphemes can arise in the same way. He sees this as an “analogous reduction of originally autonomous words into signs for an understanding of a conception of a certain function”¹⁷⁷ (1856: 57). His examples of this include the frequently mentioned French adverbial derivative *-ment* which, as he says, stems from the ablative *mente*. He claims that Friedrich Christian Diez (1794-1876) discussed this in his *Grammatik der romanischen Sprachen II* (p. 382) (Madvig, 1856: 57), and he goes on to say that among contemporary scholars of language, no one doubts that derivational and inflectional morphemes stem from originally autonomous words. They are all too eager to find the origins of suffixes (Madvig, 1856: 57). It seems this is yet another indication that agglutination theory naturally led to a unidirectional view of affixes. However, it is also clear that this did not necessarily involve a unidirectional view of all that we would now call grammaticalisation, see for instance the comment on internal inflection above.

The common Scandinavian example of the passive is treated more thoroughly by Madvig in the second part of his treatment of the development and form of grammatical signs (Madvig, 1857). Based on Rask, Madvig claims that the Scandinavian passive can be compared to the German *sich* in *Die Sache macht sich leicht*, from which he goes on to explain that:

... our passive ending is originally the pronoun **sig** (*sik*); but in Old Norse and from there in Danish and Swedish this is so happily forgotten, that one has the third person reflexive pronoun on its own, which has become a general passive form for all persons and with complete and pure passive meaning¹⁷⁸ (Madvig, 1857: 21-22, different styles original)¹⁷⁹

¹⁷⁷ Original: “analog Nedsættelse af oprindelig selvstændige Ord til Mærker for Opfatning af en Forestilling i en vis Function” (Madvig, 1856: 57)

¹⁷⁸ Original: “vor Passivendelse er oprindelig Pronomenet **sig** (*sik*); men i Oldnordisk og derfra i Dansk og Svensk er heraf ved lykkelig Forglemmelse af, at man havde tredie Persons reflexive

Here once again we have a nineteenth century scholar who puts emphasis on the fact that **one** of the original forms has been generalised as the sign of the passive for all persons and numbers in Scandinavian languages. Just as today we may suggest that extension tends to be involved in grammaticalisation.

5.5.3 *Esaias (Henrik Wilhelm) Tegnér the younger (1843-1928)*

One thing that made me especially interested in Esaias Tegnér's (1843-1928) work was the fact that Jespersen (1922 [1949]) mentioned that Tegnér the younger had treated agglutination theory in one of his works (*Språkets Makt öfver Tanken* (1880) 'the power of language over the mind'). This was a theory which Jespersen himself was not completely in favour of (see Lindström, 2003 (forth), and section 6.5.1). However, Tegnér is also a rather well known Swedish linguist, who was active around the time the Neogrammarians were at the height of their productivity, and he would have been guaranteed to be mentioned in my study even if Jespersen had not explicitly said that he had treated agglutination theory.

Tegnér appears to have had a sense of what was then called *agglutination*. However, he does not use that particular term, nor any other term really. He questions why the personal endings of verbs should derive from personal pronouns (1880: 49):

Why should then, e.g., the obscuration of enclitic pronouns, through which the Indo-European verbal inflection is believed to have arisen, be presented as particularly precious?¹⁸⁰ (translation)

This was the accepted explanation of the verbal endings among many linguists during the nineteenth century after Bopp's (1816; 1820) first treatments of what was later called *agglutination*. However, around the turn of the century it came to be rejected (see e.g. Jespersen, 1922 [1949]; Meillet, 1911). Tegnér was writing several

Pronomen for sig, blevet en almindelig Passivform for all Personer og med fuldstændig og reen Passivbetydning" (Madvig, 1857: 21-22, different styles original)

¹⁷⁹ The Danish publications (Madvig, 1856; 1857) were much later translated into German (Madvig, 1856-1857 [1971]).

¹⁸⁰ Original: "Hvarför skall då t. ex. den fördunkling af efterhängda pronomen, genom hvilken den indo-europeiska verbal-böjningen anses hafva uppkommit, framställas såsom varande särskildt prisvärd?"

years before it was rejected by many, at a time when other scholars as we have seen above firmly believed that personal endings stemmed from pronouns.

Like Meillet and Rask, Tegnér mentions the origin of the passive forms. First of all the Swedish passive, which he notes is derived from *-sik*, the older form for *sig* (third person reflexive pronoun), which has become reduced to *-s* (1880: 53), a weakening which he later explains as being due to the speaker's urge to speak faster (1880: 53-54), cf. Gabelentz ease of production and ease of perception. Similar changes can be seen in Russian and Portuguese according to him, and he believed that most grammatical endings when traced back to their origins could be proven to have arisen in a similar **phonetic** manner:

Almost everywhere, where one can trace the origin of an inflectional ending, it proves to have arisen in the same purely phonetic way.¹⁸¹ (Tegnér, 1880:53) (translation)

Much more than this is not said about this form of change by Tegnér. He makes it clear that he believes that forms can be joined together and the way he speaks of flectional languages makes it clear that he views them as deriving from autonomous parts, which have merged together into one single whole:

In the flectional languages, as far as they are flectional, the merger [literally 'melting together'] of the inflectional element and the stem is complete, so that they cannot be separated from one another. But instead of calling this merger 'organic' – an expression which we are used to associate with the additional concept of something of a higher standing – one would be as justified here in using the name 'amalgamation', 'muddying' or something similar.¹⁸² (Tegnér, 1880:49) (translation)

We can compare this to Bopp's notion of monosyllabic roots, although Tegnér usually refrains from speaking of roots and does not say anything about the autonomous words being monosyllabic.¹⁸³

¹⁸¹ Original: "Nästan öfveralt, där man till sitt ursprung kan följa en böjningsändelse, visar det sig att den uppkommit på samma rent fonetiska väg." (1880:53)

¹⁸² Original: "I de flekterande språken, för så vidt de äro flekterande, är sammansmältningen mellan böjningselementet och stammen fullständig, så att de ej kunna utsöndras från hvarandra. Men istället för att kalla denna sammansmältning 'organisk' – ett uttryck vid hvilket vi äro vana att fästa bibegreppet af något högre stående – kunde man med lika skäl här använda namnet 'amalgamering', 'hopgrumling' eller något dylikt." (1880:49)

¹⁸³ On at least one occasion however, he does say something about a 'stamord', roughly 'stemword' (1880: 119). If a language was built up only of this kind of words Tegnér believes it would make it impossible to learn the language. This is because the human memory would not be able to cope, having to learn the meaning for each single word from start instead of being able to derive it from the parts of the word in some cases at least (Tegnér, 1880: 118-119).

Tegnér also discusses semantic changes, both in relation to this combination of autonomous words into one word and for its own sake. He speaks of how the affixed pronouns become more obscure (“fördunklingen af efterhängda pronomen” (1880: 49)). Like many others after him, including Meillet (1912) and many others treating grammaticalisation, Tegnér speaks of the development of negations and he notes the semantic change of French *rien* from *rem* ‘thing’ (1880:125). It started with the French negation *ne ... rien*, where *rien* had the meaning ‘something’, but developed the meaning of ‘nothing’ (Tegnér, 1880: 125). Similarly, he mentions the German *nicht* and English *not/naught* (1880: 68), and the Danish *ikke uden* ‘not without’ > *kun* ‘only’ which he compares to French *ne... que* ‘only’ (1880: 63-64).

Tegnér noted that many of the Indo-European languages appear to have developed from synthetic languages with many endings into languages using more and more periphrastic forms, as exemplified by many of the perfects we find (e.g. French, German, English perfects (1880: 49-50)), and the development of genitive expressions through prepositions rather than endings (1880: 50).

Notably, he also mentions an example which has recently been discussed by Norde (2002) as an example of exaptation. In Swedish, many fruitnames have started to take an *-on* ending, as Tegnér says, by analogy with some fruit names that already had an *-on* ending (1880: 75). This is discussed by Norde as the development of a ‘berrymaking suffix’ out of a former plural ending (Norde, 2002). And as we shall see below (section 6.5.3) similar examples were also discussed by Elias Wessén (1889-1981).

To sum up, Tegnér appears to have some sense of there being a process whereby new endings can develop from previously autonomous words – in other words close to the default definition of grammaticalisation (in the second sense mentioned in the introduction (5.0.1)). However, he does not use any of the more common terms for this such as *agglutination* or *grammaticalisation*. Occasionally, he speaks of *sammansmältning* ‘lit. melting together (noun)’ (1880: 49), a term which he may have taken over from German scholars who sometimes talked of *Verschmelzung* (see e.g. Brugmann 5.1.8), and also *amalgamation* (1880: 49). *Sammansmältning* was a term he seems to have used to refer to what has usually been called *agglutination*, the joining of two words into one where the morpheme

boundary between them is still visible.¹⁸⁴ He also recognised that there may be moves from non-lexical items such as plural endings into e.g. derivational morphemes, something now sometimes seen as exaptation. However, the change which brings about new grammatical items was seen as primarily proceeding from the lexicon.

5.5.4 Adolf (Gotthard) Noreen (1854-1925)

Throughout his life, Adolf Noreen (1854-1925) was very productive and published quite a lot in both Swedish and in German (some of his writings were published posthumously). Therefore I have not had chance to look at everything he wrote, but will concentrate on three of his publications which I thought might be of particular interest in this context since if he did have a concept of grammaticalisation, it seemed likely to me that it would appear in at least one of these texts. The publications I will look at are *Vårt Språk – Nysvensk Grammatik i Utförlig Framställning* ('Our Language – A Comprehensive Presentation of Modern Swedish Grammar') vol 7, part 4 – morphology (1906); *Geschichte der nordischen Sprachen* (1913a, 3rd ed.); and *De Nordiska Språken* (1913b, 3rd ed.).

In *Vårt Språk* VII: 4 Noreen treats word formation, a process which is today usually seen as different to grammaticalisation, but which also lies very close to grammaticalisation in some ways and it is possible that (at least some forms of) grammaticalisation could be seen as a particular type of word formation by some linguists. Especially, if we also include the process of creating new derivational morphemes in grammaticalisation.

Noreen (1906: 20-21) mentions that occasionally a word can be called *sammansatt ord*, *sammansättning* or *kompositum*, in other words, a compound by Swedish or Latin terminology. These words / compounds can be split into parts which can be used autonomously. However, most of them can also be divided into two groups, weak or strong compounds. The strong compounds can be called "*konglutination*" ('conglutination') and the weak "*agglutination*" ('agglutination'). This is important to note here since Noreen here uses the term *agglutination*, which

¹⁸⁴ It is not absolutely clear whether he sees this as a diachronic process or if it is only an abstract way of explaining the structure of words and their inflection in flecational languages.

at this time had long been used for both *agglutinative languages* and *agglutination theory*.

In '*conglutinated*' compounds the words would usually have to be changed both phonetically and in their order of appearance to get an ordinary word combination. Sometimes one of the parts is an affix, then he calls the word a *sammanväxning / kombination* (i.e. something that has grown into one, or a combination). But sometimes the parts can still be used autonomously and then he calls it a *sammanfogning / konglomeration* (i.e. something that is only joined, or a conglomeration).

Weak compounds or agglutinations are compounds where the parts of the word remain more or less the same as when used autonomously (Noreen, 1906: 22). The term *agglutination* thereby seems to include one of the first stages which one might recognise in an increased bonding situation through, e.g. grammaticalisation. Noreen also claims that *agglutination* can either occur through *sammanställning / juxtaposition* (when words are just put next to one another without actually being joined into one word), or *gruppord / komplex*, which he explains as when a combination could be seen as a word combination, but one does not really want to see it as that (Noreen, 1906: 22-24).

There are two types of autonomous morphemes, according to Noreen: words and word combinations (*ordfogning*), and all of the above types interestingly belong to *words* rather than to word combinations. A *compound*, on the other hand, or *construction* ('konstruktion') as it is also called, is defined as "an autonomous morpheme, consisting of several words, of which each and every one for the reflection is understood as an independent moment with its own special meaning, but which however at the same time are held together by a common semantic task; the latter as opposed to a mere juxtaposition of words" (Noreen, 1906: 34-35, in translation).^{185, 186} This group can even include such vague combinations as idioms.

In *Geschichte der nordischen Sprachen* (1913a) Noreen has a whole chapter on the *Geschichte der Flexionsformen* ('history of the flexional forms'). One of the

¹⁸⁵ Original: "ett självständigt morfem, som består af flera ord, af hvilka hvart och ett för reflexionen framstår såsom ett särskildt moment med hvar sin speciella betydelse, men som dock samtidigt alla sammanhållas af en gemensam semologisk uppgift; detta sednare i motsats till en blott ordsamling" (Noreen, 1906: 34-35)

¹⁸⁶ It appears that Noreen is trying to distinguish between what in a translation of one of Žirmunskij's publications has been called *compounds* and *word combinations* (cf. Žirmunskij, 1961; 1966 [1961]) (cf. chapter 3).

things he mentions is that it was quite common at one time to cliticise the personal pronouns to the verbs, which is also what he (and many others, as we have seen above) believes has given us the Scandinavian passive *-s* (Noreen, 1913a: 178-180, 219).

There are also some pronouns that have changed because they have merged with other elements and have then demorphologised from a modern point of view, although Noreen of course does not use this term. Noreen gives this process no particular name. He only notes that certain forms are derived from two previously autonomous words. One example of this is the Old Swedish *þōn*, which he derives from *þau* and the article *en* (i.e. the word that became the definite affix / article) (1913a: 185, 190). He calls this a “*Verschmelzung*”, so the two words are believed to have (metaphorically) melted into one (1913a: 185), common among German scholars during the nineteenth century, and as we have seen above it seems likely that Scandinavian scholars may have adopted it from their German colleagues.

He also mentions how the longer Scandinavian demonstrative pronouns (Sw. *detta, denna, dessa*) developed in Common Scandinavian, through compounding (“*Zusammensetzung*”). The demonstratives equivalent to the German *der, die, das* and the particles *-si-*, **-sē*, **-sa*, were joined into *sarsi* (Nom. Masc. Sg.), *þesse* (West-Scandinavian Nom. Masc. Sg.), *susi/þpis* (Nom. Fem. Sg.) *þita* (Nom. / Acc. Neut. Sg.), etc. , which we can compare to the German pronouns *dieser, diese, dieses*, etc (1913a: 187-188). In this context he uses the term *Verschmelzung* again, but this time to mean that two new demonstratives of different origin have blended to form one paradigm (Noreen, 1913a: 188):

Aber keine von beiden scheint alle Kasus herausgebildet zu haben, sondern sie sind – wenigstens in der Literatur – in einem Paradigma vereinigt worden, wo die Mehrzahl der Formen der *-si*-Bildung hören. Diese Verschmelzung zweier ursprünglich verschiedenen Bildungen hat aber veranlaßt, daß viele Formen auf *-si* durch Kontamination Nebenformen auf *-sa* bekommen haben, gleichwie umgekehrt Formen auf *-a*¹⁸⁷ solche auf *-i* neben sich haben.¹⁸⁸

¹⁸⁷ Diacritic missing.

¹⁸⁸ Translation: But neither of the two seem to have developed all cases, but rather, at least in literature, they unite in one paradigm, with the majority of the forms belonging to the *-si*-formation. This *Verschmelzung* of two originally different formations has however led to many forms in *-si* having subforms in *-sa* through contamination, as conversely forms in *-a* have subforms in *-i*.

It seems *Verschmelzung* was a metaphorical term which could be used for more than one thing. Therefore, it seems unlikely to have been seen as a term. However, in one of its senses it also appears to have been calqued into Swedish, appears to indicate that it was beginning to be seen as a proper term.

Other developments treated by Noreen include *ne wæit ek > nekkuat* ‘something’, a change which seems to qualify as grammaticalisation (1913a: 192). The development in West Scandinavian of the negation *ne... enge* ‘none’, where *en-* was first the only inflected part, then it stopped being inflected, and instead inflections were affixed to *-ge* (1913a: 193). *Beide* ‘both’ is also noted to stem from two words *bai* ‘both’ and *þai* ‘they’ (1913a: 194).

One of the most common examples of grammaticalisation in Scandinavian languages, however, is the passive form, which is also mentioned by Noreen (1913a). He claims that it was formed through the syncope of a dative or accusative reflexive pronoun (depending on the variety of Scandinavian) and added to the (active) verb (1913a: 219). Notably, the same pronoun was used in both numbers and all persons (1913a: 219), which as I noted above was also mentioned by Rask (5.5.1). As I mentioned in 5.5.1, today we might see this as indicating a later stage of grammaticalisation, and the result of extension and possibly obligatorification. However, Noreen does not discuss this at all, he only notes it in a ‘matter-of-fact’ kind of way. Furthermore, Noreen, like Rask, claims that the oldest West-Scandinavian sources occasionally show the first person pronoun, possibly indicating an earlier stage of grammaticalisation from our present day point of view, when more variation was possible.

Ein neues Medio-Passiv, das den nordischen Sprachen spezifisch ist, wird in der Vikerzeit (wenn nicht früher, was aus Mangel an älteren Belegen nicht zu entscheiden ist) dadurch gebildet, daß an die aktive Form das Pron. reflexivum (in synkopierter Gestalt) tritt, entweder als Dativ [...] oder – ohne wesentlich verschiedene Bedeutung – als Accusativ [...]. Hierbei ist zu merken, daß *-ss* (aus **siR*), *-sk* (aus *sik*) nicht nur in der 3. Sg. und Pl., sondern als generelles Reflexivpronomen für alle Personen gebraucht wird; jedoch kommt noch in der ältesten wn. Literatur allgemein *-mk* (aus *mik*), seltener *-m* (aus **-mR*, **-miR*) in der 1. Sg. [...] vor.¹⁸⁹ (Noreen, 1913a: 219)

¹⁸⁹ Translation: A new medio-passive, which is specific to the Nordic [/Scandinavian] languages, was developed during the Viking age (if not earlier, which is not possible to determine due to the change in the oldest extant samples), that the reflexive pronoun (in syncope form) is joined to the active form [of the verb], either in the dative [...] or – without particularly different meaning, in the accusative [...]. In relation to this one can observe, that *-ss* (from **siR*), *-sk* (from *sik*) was not only used in the third singular and plural, but as a general reflexive pronoun for all persons, though in the

As I said above, this was a change that had already been noted by Rask (1993 [1818]: 132, 146, 171, 256-258) in the early nineteenth century and discussed by him in comparison with other Indo-European passives. Tegnér similarly noted that there were parallels between the Scandinavian passive and the Russian and Portuguese reflexive verb forms. The change is also treated by Noreen in *De nordiska språken* (1913b) where he calls it “*vidfogning*” (‘joining together’ (noun)) (1913b: 8-14).

Another important example in the grammaticalisation literature has been the definite article. Noreen remarks that the Scandinavian article was originally a demonstrative pronoun, which in Old Norse came to be added before adjectives and after nouns. He also notes that the article merged with the noun (“*verscholzen*”), which led to certain changes both in the article and in the nouns (1913a: 224-227).

Having treated the definite article / suffix, it was of course not a long step to take to look at the indefinite article. But only very briefly does Noreen observe that the number *einn* (West-Scandinavian), *én* (East-Scandinavian) developed into an indefinite article in Scandinavian languages (1913a: 227). Still it is a clear example of something developing from a concrete number to bear the meaning of indefiniteness, and a definite example of grammaticalisation, and also quite a common example today.

It is important to note that Noreen was not only aware of the fact that new endings can sometimes appear through the affixation, or agglutination, of one word to another, but also that sometimes a new ending could appear in a rather roundabout manner. He noted that the singular of the West-Scandinavian preterite was actually formed by analogy (this is what he calls it) with the first and third persons singular present tense. Even though the *-t* there was actually an ending and not part of the stem, this was adopted by the second person preterite indicative of some verbs and it then spread to other verbs. Later this ending spread to other verbs (1913a: 228-229).

Summing up, we can say that Noreen (at least in the three books that have been studied for this thesis) did not say much about the origin of grammatical forms. The most he said about them was in his German book on the history of the Scandinavian languages (Noreen, 1913a). There he treated several examples which could today be classified as grammaticalisation. However, he does not treat them under one heading,

oldest West-Nordic literature general *-mk* (from *mik*), more rarely *-m* (from aus **-mR*, **-miR*) in the first singular is also still used.

nor does he label their development with one particular term. Whether or not he had a clear concept of something similar to grammaticalisation is unclear. However, it does seem quite unlikely. Nevertheless, the fact is that he did note that certain endings stemmed from autonomous words (cf. sense (2) of grammaticalisation in section 5.0.1). He also noted that inflectional morphemes sometimes arise through what he called *analogy*. Furthermore, it appears as though his common use of words meaning literally that something melted into one, shows that he was making use of the so-called *agglutination theory* (even though the term was not always used in association with agglutination theory in his work). Notably, in this he included both agglutination that leads to new inflectional morphemes and agglutination of words into compounds where neither part gains a more grammatical function.

5.6 Summary

It is clear that during the nineteenth century the agglutination theory took form and spread across the Western world. Firstly, it seems to have been accepted by many scholars but towards the end of the century it came to be questioned more and more.

There was no accepted terminology for grammaticalisation during the nineteenth century, nor for agglutination. However, the term *agglutination* and a German metaphorical term *Verschmelzung* became commonly used to mean the merger of two words or the fixation of one word onto another.

Occasionally, scholars during the nineteenth century discussed the development of lexical words into more grammatical words and particles, but there was not usually any connection made between this and the development of new affixes. Humboldt appears to have seen some form of connection between them, possibly in the form of a continuum. Similarly, at the turn of the century, we see a discussion of something which resembles our modern cline of grammaticalisation / grammaticality appearing in Sweet's work. Although it seems quite possible that he got this from someone else, e.g. Humboldt.

It is important to note that even at this time, many scholars noted the semantic and phonetic aspects involved in the making of new affixes. In Rask's and Noreen's

discussions of the Scandinavian passive we also see comments reminding us of expansion and obligatorification in the more recent grammaticalisation literature.

Some scholars during the nineteenth century presented clearly unidirectional views and claimed that all inflectional and derivational morphemes must have originated in content words. However, occasionally some scholars admitted that it was possible that other linguistic elements may have developed a function, e.g. a sound change, such as umlaut and ablaut – but naturally this was not then called agglutination and unfortunately hardly anyone discussed the similarities and possible connections between the different means of gaining new grammatical markers. Notably, towards the end of the century, scholars started to debate whether agglutination theory was right or whether perhaps pronouns could have arisen from original endings, and whether endings on one part of speech could have been developed into endings that could be used on another part of speech. This was when the alternatives to agglutination theory called *adaptation theory* and *evolution theory* arose, theories which clearly contradicted what is now commonly called the *unidirectionality hypothesis*.

There is no big difference between scholars from the different countries during this time. In all five regions: Germany, France, Britain, the United States of America and Scandinavia, there were scholars who discussed something similar to grammaticalisation. Usually this was mainly agglutination theory, although it was not always called that. However, considering the minor role Britain and Scandinavia has played in the work on grammaticalisation in the late twentieth century it is interesting to see that both of these regions were rather active during the nineteenth century.

6. COINING, FORGETTING AND REVIVING GRAMMATICALISATION (1900-1970)

6.0 Introduction

At least since Lehmann's brief introductory treatment of the history of grammaticalisation in his book *Thoughts on Grammaticalization* (Lehmann, 1982 [1995]) and his other articles in the 1980s (1985a; 1987; 1989), it has been assumed that *grammaticalisation* was coined by Antoine Meillet in 1912. Another work which has not received as much attention as Lehmann's in the field of grammaticalisation, but which mentioned Meillet's presumed role as coiner of the term, even earlier, was a paper by Vincent (1980).

However, Meillet did not treat grammaticalisation much more than in passing. He mentioned it once in a paper in a journal which appears to have been aimed at a wide academic audience (Meillet, 1912), *Scientia*. Furthermore, in a review, Meillet (1911) mentioned one change that might have been classed as something similar to grammaticalisation, namely the development of personal endings on verbs, but without going into any details and without using the term *grammaticalisation*. In 1915 the term appears again in a paper that was published in the *Annuaire* of the École Pratique des Hautes Études (Meillet, 1915-1916 [1921]).

When Meillet mentions this concept or process, the texts usually have a ring of trying to make scholars familiar with some of the ideas of historical linguistics, or

even popularism. It feels as though he is not attempting to introduce a new technical term, and definitely not a new concept. So it seems **if** he introduced the term *grammaticalisation* it was done accidentally, when trying to give a clear picture of this phenomenon to non-linguists or undergraduates. This is a rather natural way for terms to be created, because they have a clarity that technical terms that were ‘designed’ to be technical terms do not generally have.¹⁹⁰

Was Meillet in any way an instigator in the study of grammaticalisation? Or was he simply part of a more general movement? It is a long time before we start to come across the term *grammaticalisation* in English literature. There is the odd mention in the 1960s and the OED lists uses of the verb *grammaticalize* also from the 1930s and 1950s. John Orr’s (1885-1966) translation (and revision) of Iorgu Jordan’s (1888-?) *Introduction to Romance Linguistics* (Jordan, 1937) is noted to include the term. Notably, this in a translation (!) of a discussion of French sentence structure in interrogative sentences – where Jordan claims that inversion has become grammaticalised. This agrees with Meillet’s (1912) suggestion that word order may grammaticalise, and may have been influenced by Meillet’s use of the term and discussion of the concept. This appears particularly likely seeing as the discussion concerns Romance linguistics, and is a translation from a Romance language (Romanian).

The second citation of *grammaticalize* in the OED is from 1961, when it is used in a discussion of the difference between analytical and synthetic languages in the Czech journal *Brno Studies* (Vachek, 1961: 10). Josef Vachek (1909-1996)¹⁹¹ notes that word order is more grammaticalised in analytic languages (Vachek, 1961: 10). Interestingly, the author is not listed by the OED and further we should note that this is a non-British/American publication and the author is neither British nor American, however the article was written in English. It is notable that once again *grammaticalise* is used in a discussion of word order, a use of the term that has been frowned upon by more recent grammaticalisationists such as Hopper and Traugott (1993). Prototypical (narrow) grammaticalisation according to Hopper and Traugott (1993: 50) should be unidirectional and word order is not.

The last reference is from John Lyons’s *Introduction to Theoretical Linguistics* (Lyons, 1968: 438) where he similarly to Roman Jakobson (Jakobson, 1971 [1959])

¹⁹⁰ Thanks to Andrew Linn for discussing the creation of terminology with me.

¹⁹¹ <http://www.mlp.cz/cz/offline/perlie/v/2012136.htm>

in a discussion of Franz Boas's work *Language* (Boas, 1938), claims that some "notions" may be grammaticalised in one language and lexicalised in others. This is a sense of grammaticalisation / grammaticalise which is quite different to that which is discussed in this thesis and therefore it does not need to be discussed any further. Especially seeing as we have already treated Jakobson's article briefly above (section 3.3.1).

All of the above citations from the OED Online concern the term *grammaticalise*, rather than *grammaticalisation*. However, the OED also lists two uses of *grammaticalization*, both of which occur before the 1970s. In 1955 the *Archivum Linguisticum* is noted to have included a claim that "[t]he affectivity of reprise has been weakened by grammaticalization" (Priestley, 1955: 28).¹⁹² Ten years later *Language* is to have included a review that entailed a statement regarding "[t]he creation of oppositions through grammaticalization" (1965) (Cardona, 1965: 107). Neither of these two citations make it quite clear how the term *grammaticalisation* is being used. But a look at the two sources shows that what is meant is similar to what we mean by the term today. However, we should also note that one deals with French and the other with Indo-European, since this may be of importance.

Leonard Priestley (1955) uses the terms *grammaticalized* / *grammaticalization* several times and not only on the page that the OED refers to (Oxford English Dictionary, 1989h). We should first try to make it clear what Priestley means by *reprise*:

Reprise is formed by the juxtaposition of a nominal sentence and a verbal sentence. In the verbal sentence, the nominal sentence is represented by a pronoun. Between the two, a pause, of varying length, is often the sign of the grammatical split implied in the alternative terms 'dislocation' and 'segmentation'. When the pause is very marked, we might describe reprise as a juxtaposition of two 'phrases monorèmes, e.g. 'Pauvres gens! Je les plains.' When the pause is less marked, one might describe reprise as a 'phrase dirème', with nominal and verbal element, e.g. 'Cette lettre, je ne l'ai jamais reçue'. (Priestley, 1955: 1)

As I said above, Priestley appears to use *grammaticalisation* in more or less the same way as we do now. For instance, when reprise (as described in the quote above) takes over from inversion in what Priestley calls an "interrogative formula", he says that it has grammaticalised (1955: 12). He also notes that the extended use of reprise

¹⁹² The author is not mentioned by the OED.

leads to a change in the forms which can occur in the nominal part. At first only definite forms can occur there, but as it grammaticalised also indefinite forms appear (1955: 12-13). Moreover, he notes that the extended use of reprise, through its grammaticalisation as an interrogative marker, means that it becomes less expressive (1955: 21, 28). And he claims that reprise is both an “affective device” and “a grammatical formula” (1955: 28).

The review in *Language*, that is mentioned in the OED’s coverage of *grammaticalise* / *grammaticalisation*, was written by a linguist active in the States (George Cardona) and it is a review of a Spanish book on Indo-European. In other words, it deals with the area which has been noted to have carried on discussing grammaticalisation after Meillet’s work (cf. Lehmann, 1982 [1995]), namely Indo-European studies. It mentions that the writer of the book reviewed, Francisco Rodríguez Adrados, discussed how grammaticalisation could lead to new oppositions in language, e.g. the distinction between present and preterite. *Grammaticalisation*, although not defined in the review, seems to mean more or less the same as what we now mean by it. Instead of being given a definition one is referred to the definitions of grammaticalisation and other terms in the book being reviewed. Cardona also says that there is an interesting discussion of this phenomenon but he does not go into it anymore due to lack of space (Cardona, 1965: 107 fn.). Notably, Cardona also mentions a process which he calls *infection*. Through this he says that a feminine marker has appeared from part of a linguistic item, *g^{wo}nā ‘woman’ is said not to have contained a feminine marker *-ā at first, but this “came to be considered an indicator of feminine gender and acquired this value by infection” (Cardona, 1965: 107). There is some resemblance between this and exaptation, if what Cardona means is that -ā was part of the word all along, or of some other feminine words, and only later was interpreted as a feminine marker.

It is clear from these entries in the OED that *grammaticalise* / *grammaticalisation* were used in English after Meillet’s use of the term in 1912, and before the revival of the term in the 1970s. However, only three of the recorded uses are by British scholars and they all relate to the French language! The rest of the entries are by non-British scholars.

Other relatively early uses of the term *grammaticalisation* or references to a similar process, from the 1960s, include Benveniste (1968), Kuryłowicz (1965

[1975]), Žirmunskij (1961; 1966 [1961]) as mentioned in section 3.3.1. It is also clear that the use of the term in many of the OED entries is closer to Meillet's suggested broad use of *grammaticalisation*, than to the use of the term since the 1970s. It includes word order changes, and in fact the examples in those entries centre around changes in word order in a sense, or at least around ways of expressing grammar, in general. This is a major difference between this early usage of the term in English and the later use of it, after the so-called revival.

The main question is, does this present a true picture of the situation in Britain around this time? And since the references in the OED were primarily non-British or related to other European languages, it seems possible that most linguists in Britain would not have been aware of this term. I have found no indications that this was not so, but a more indepth study of work, both language specific and in general linguistics in Britain would be needed to say for sure what the state of affairs was in the early twentieth century. Similar studies would also be needed for other countries.

In Scandinavia the situation appears to have been slightly different to that in Britain. The term *grammaticalisation* (*grammatikalisering*, *Grammatikalisierung*) is used fairly early on, with some links to German scholarship. But the references to Meillet, without use of the term are even earlier, as we shall see below (see section on Birger Bjerre 6.4.5.).

6.1 The Presumed Coiner of the Term *Grammaticalisation* – (Paul Jules) Antoine Meillet (1866-1936)

As far as I can see, it was Antoine Meillet (1912) who coined the term 'grammaticalization' and first applied it to the concept for which it is still used today. (Lehmann, 1982 [1995]: 1)

This is one of the first mentions of Meillet's role in the history of grammaticalisation, and probably the most well known, due to the wide circulation of Lehmann's book. The only mention of Meillet coining the term *grammaticalisation* before Lehmann (1982 [1995]) which I have found is in a paper by Vincent (1980: 56), as I mentioned above. Vincent's paper is also included in Lehmann's (1982 [1995]) bibliography, however there is no reference to it in the discussion of the first use of the term – so I presume he must have missed Vincent reference to Meillet. Vincent explicitly

mentions Meillet's famous paper in his section on grammaticalisation and also cites his definition:

We begin, therefore, with a descriptive and (partial) theoretical characterization of this venerable concept – at least as old as Meillet's classic paper 'L'évolution des formes grammaticales' (1912), from which we take the definition of grammaticalization as 'le passage d'un mot autonome au rôle d'élément grammatical' (p. 131), and the example of Latin *passus* 'step' yielding French *pas*. The process which seems to be operative here is a kind of semantic 'bleaching' whereby *passus* loses its independent semantic content and acquires its new and more general meaning from the syntactic environment *ne.....* in which it occurs with increasing frequency. (Vincent, 1980: 56)

Meillet's first known work on grammaticalisation was the article he published in *Scientia* in 1912 (Meillet, 1912). He published it in what by some has been called a popular science journal (e.g. Haspelmath p.c.), which today would probably be interpreted as a sign that it was a generally accepted theory that he was just attempting to bring to the general public. However, at the time that need not have been the case. Scientific journals were then still quite scarce and although Meillet was the secretary of the Société de Linguistique de Paris, which seems to have meant that he was also the editor of their publications (the *Mémoires* and the *Bulletin*), that does not mean that he could or wanted to, publish all of his work there. In addition, we should note that the philosopher and historiographer of science Claudio Pogliano (1953-) (1997) does not choose to call *Scientia* a popular science journal, but instead claims that the journal was an attempt to do something about the specialisation that was taking place at the time. There appears to have been a feeling that people were becoming too specialised in their own specific fields.

What Meillet says about grammaticalisation in the paper from 1912 is quite well known. Lehmann (1982 [1995]: 4) has accused him of not defining the term he presumably introduced, but he does give a brief description of it as:

... le passage d'un mot autonome au rôle d'élément grammatical.¹⁹³ (Meillet, 1912; 1921: 131)

Although this does not occur in direct conjunction with the term *grammaticalisation*.

Meillet (1912) comments somewhat on the history of the concept. He claims that it is something that has been less popular in the last 40 years, but that it carried

¹⁹³ Translation: the passing of an autonomous word into the role of a grammatical element

on being studied during those years and that it was becoming more popular again a few years into the twentieth century. Unfortunately, he mentions no names, and nor does he relate his concept explicitly to agglutination theory, or any other term. Therefore, we do not know for certain if it was agglutination theory that he was referring to as the ‘predecessor’ of what he called *grammaticalisation*, the phenomenon that had been somewhat less studied in the preceding 40 years. But it seems like the only possibility. We do not know which parallels and which dissimilarities (if any) he could see between agglutination theory and grammaticalisation.

It is also interesting that if this was something that had been less studied in the 40 years preceding Meillet’s paper (1912), that takes us back to 1872. This does not have to mean that grammaticalisation was hardly ever discussed during these forty years, however, it could be that it was under attack and that it was still mentioned by people who wanted to defend it.

An important issue in grammaticalisation studies of today is the directionality of the change, as we have seen in chapter 3. Meillet says nothing about the directionality of the change he discusses. Meillet’s definition of grammaticalisation does not say explicitly that there can be no movement in the opposite direction, i.e. from functional > lexical, or submorphemic > grammatical. Nor does he state this explicitly elsewhere in his few articles which deal with grammaticalisation (1912; 1915-1916 [1921]; 1921). The only direction he mentions could be said to be from an independent word to a grammatical element, but we must remember that he also mentions word order. He suggests that word order can also be called on for grammatical functions.¹⁹⁴

He does not refer to any changes that do not go from lexical > grammatical and therefore later scholars have often interpreted his work as implying unidirectionality (cf. Lindström, 2002). The fact that he only explicitly deals with changes from independent word to grammatical item, does **not** mean that movements in other directions would not have been seen as grammaticalisation by him. Even less does it imply that he thought that there could **never** be any changes in other directions. It only shows us that either he had not thought of any counterexamples, or he saw no need to mention any.

¹⁹⁴ The question is: would all word order changes be grammaticalisation according to him?

Most grammaticalisationists know of another paper that Meillet wrote on grammaticalisation. In 1915 he published a paper on the history and development of conjunctions. This paper is of course more specific than the former where he basically gave a brief overview of this concept of change. It was also published in a different type of publication, *Annuaire de l'École pratique des Hautes Études*, in the section for history and philology.¹⁹⁵

... le trait commun à tous ces développements consiste en ce que, par l'effet de la répétition qui en a atténué progressivement la valeur expressive et en a fait oublier la signification propre, l'élément qui figure à la jonction de deux phrases tend à devenir un simple outil grammatical: il se « grammaticalise » pour ainsi dire. Le sens initial de l'élément devient chose à peu près négligeable et sans conséquence pour le développement ultérieur.¹⁹⁶ (Meillet, 1915-1916 [1921]: 169)

The examples that occur in Meillet's papers, apart from the conjunctions that he discusses in his second article, are, e.g., future auxiliaries, negations, German *hiu tagu* 'this day' > *hiutu* > *heute* 'today'. The last of these is an example that has been questioned quite frequently by later grammaticalisationists (cf. Hopper and Traugott, 1993; Lindström, forth.; Wischer, 2000). He also suggests that word order might be a matter of grammaticalisation, another issue of debate among recent grammaticalisationists (e.g. Hopper and Traugott 1993).

A few years later Meillet also discussed another example of grammaticalisation briefly in the *Bulletin de Société de Linguistique de Paris* (Meillet, 1926 [1951]). The example was of *aujourd'hui* 'today,' which of course is similar to the German example of the formation of *heute*. They both include mergers of two words or more, including a word for 'day' which today is not usually recognised by anyone except an etymologist. This shows that Meillet had not completely stepped away from the topic as it may have seemed because it is only the two papers from 1912 and 1915 that are usually mentioned in the recent grammaticalisation literature. However, Meillet does not mention the term *grammaticalisation* in the section on *aujourd'hui*,

¹⁹⁵ This was some kind of yearbook for *L'École pratique des Hautes Études* in Paris. The *Annuaire* printed annual reports of the lectures at the *L'École pratique*, teaching programmes, the status of the theses that were worked on at the *L'École pratique* but also some articles. (For more information see: www.ephe.sorbonne.fr/presentph.htm).

¹⁹⁶ Translation: the trait common to all of these developments is that, through the repetition thereof, a meaningfulness has decreased progressively and thereby the true meaning has been forgotten, the element that appears at the joining of two phrases tends to develop into a simple grammatical tool: it 'grammaticalises' so to speak. The original sense of the element becomes something more or less negligible and without consequence for the final development.

instead he only discusses it as a frozen form with parallels in many other Indo-European languages, something which might be an indication that grammaticalisation was not really seen as a term by Meillet.

Meillet had also commented on at least one of the ideas of agglutination theory previous to the two papers by him that have become familiar to everyone who works on grammaticalisation. In a review of Stephen Langdon's *A Sumerian Grammar and Chrestomathy with a Vocabulary of the Principal Roots in Sumerian and a List of the Most Important Syllabic and Vowel Transcriptions* (Langdon, 1911), he (Meillet, 1911) writes:

Je me bornerai à lui conseiller la prudence quand il touche au domaine indo-européen: **presque aucun linguiste s'occupant de langues indo-européennes n'oserait affirmer**, comme il le p. 126, **que le *-mi du type gr. φημι soit un ancien pronom personnel.**¹⁹⁷ (Meillet, 1911: 143, emphasis mine)

Otto Jespersen (1922 [1949]) quotes part of the last statement that Meillet makes in this review (this part is reproduced in bold), in an attempt to show that most linguists at the time when he was writing had stepped away from the earlier agglutination theory. The belief that personal endings on the verbs could be derived from personal pronouns was a rather old idea, mentioned in Bopp's early treatments of agglutination theory and possibly going back to Condillac at least (cf. Lehmann, 1982 [1995]: 3). This had hung on as the accepted truth for a long time and was repeated many a time in rather general books on linguistics, cf. e.g. Whitney (1867 [1973]: 75). But in the latter part of the nineteenth century it had started to be questioned.

The reader may feel that Meillet thinks that Langdon has presented an old-fashioned view of language in relation to the personal verb endings. However, his views are not absolutely clear in this extremely short review – one page in total, where Meillet also says: “Je ne puis malheureusement émettre sur le travail de M. L. aucune opinion”¹⁹⁸ (1911: 143). The sense that comes across is that this is one thing that he can comment on, and presumably he can do so because this is something that

¹⁹⁷ Translation (Natalia Slaska, PhD Student, University of Sheffield) I will limit myself to advising him to be cautious when he deals with the Indo-European topic:.... And the part in bold was quoted in translation by Jespersen (1922: 384) as: “Scarcely any linguist who has studied Aryan languages would venture to affirm that *-mi of the type Gr. *fēmi* is an old personal pronoun” (Jespersen 1922:384).

¹⁹⁸ Translation: Unfortunately I can utter no opinion of M. Langdon's work

was very obvious to him as he read this book and something which appears completely wrong and perhaps old-fashioned to him.

Meillet was obviously aware of the importance of the phonological and semantic changes involved in grammaticalisation. He spoke of the bleaching (“*affaiblissement*”, lit. weakening) of both the pronunciation and the meaning of the words involved in grammaticalisation (Meillet 1912 [1921]:139), something of which we can see clear parallels in both recent treatments of grammaticalisation. Even the terminology is similar to more recent terminology – *affaiblissement* can be translated as *fading*, *weakening* and nowadays one usually speaks of *bleaching*, *weakening* or *attrition*. Today the term *bleaching* is usually reserved for the semantic weakening that usually accompanies grammaticalisation, whereas phonological changes tend to be referred to as *weakening* or *attrition*.

6.1.1 Grammaticalisation 1912-1970

Grammaticalisation did not become a standard term internationally after Meillet’s publications. Instead it appears to have gone mostly unnoticed until people started consulting the publications of collected papers by Meillet that were published approximately ten years later, and not even then did it gain widespread international recognition and usage. As noted by Lehmann (1982 [1995]: 7) and Hopper and Traugott (1993: 24) the term and concept appear to have carried on being used in Indo-European Studies, where scholars were still interested in historical changes, while the rest of linguistics went into a largely synchronic era. The term was also used occasionally by others but with other meanings.

It seems that the term *grammaticalisation* was only used occasionally during the period between the writings by Meillet and the 1970s. This is interesting since Meillet had also noted that it was a concept that had been less studied in the 40 years before his own work on the subject (Meillet, 1912; 1921: 133). But the new interest in the concept that he appears to have seen (“On commence maintenant à s’y attacher de nouveau”¹⁹⁹ (Meillet, 1912; 1921: 133)) also appears to have got halted fairly soon. The entries we find in the OED (cf. section 6.0) and the other uses that have been noted between Meillet’s writings and the 1970s (cf. discussions on lexicalisation and grammaticalisation in chapter 3), are sometimes of

¹⁹⁹ Translation: One is currently starting to concern oneself with this once again.

grammaticalisation in a meaning that may have been a bit closer to Meillet's than to its usage since the 1970s in that word order for instance is clearly included. Sometimes, however, they come close to neither Meillet nor Givón, uses which are more closely linked to the different ways of realising expressions in language – through the grammar or the lexicon. This should also make it clear that *grammaticalisation* is a term that different people have coined to mean different things, but it is also a term that people have borrowed from one another and still ended up using differently (cf. Lindström, forth.).

6.2 Britain

6.2.1 Arthur Waley (1889-1966)

Arthur Waley (1889-1966) was an orientalist in Britain, whom I found a reference to in a treatment of grammaticalisation by Anju Saxena (1995) (see also section 7.2). This is the only paper (Waley and Armbruster, 1934) by him I have so far been able to find. It was clearly written by Arthur Waley himself, however two authors are listed on the paper, Waley and Charles / Carl Hubert Armbruster (1874-1957), the latter probably being mentioned because Armbruster let Waley cite part of his forthcoming Nubian grammar in the paper.²⁰⁰

The short paper discusses the change from a lexical verb meaning *to speak* to an auxiliary, in different languages, for instance, a development of so called quotative uses.

It looks, indeed, as though all three words for 'to say', 'to speak' were capable of functioning simply as verbal auxiliaries. Such a usage would be hard to explain did it not exist in numerous living languages. (Waley and Armbruster, 1934: 573-574)

Waley believes that the uses of *to speak* as an auxiliary may have originated in an onomatopoeic use, such as saying “the kettle says phizz”²⁰¹ when it is boiling. But he

²⁰⁰ The only Nubian grammar by Armbruster that I have found is one that was published posthumously in 1960 (Armbruster, 1960).

²⁰¹ Waley's example.

also lists the various uses which Armbruster has noted, most of which seem to involve tendencies (Waley and Armbruster, 1934: 574-575).

Waley's brief treatment of the development of this possible auxiliary is interesting in that it was written at a time when grammaticalisation might have started to lose some ground, quite a few years after Meillet's paper (1912). Furthermore, it was written during the period when it has been recognised by Lehmann (1982 [1995]: 6) that grammaticalisation may have lived on, though only marginally, in what we might call Comparative Linguistics. The paper is also of some importance because it treats the development of auxiliaries and not grammatical inflections, a part of grammaticalisation studies which was to become more important towards the end of the twentieth century than it had been in the studies of language change in the previous century when *agglutination theory* with a focus on affixes had been the 'in-thing'. Worth noting is also the fact that Waley makes no attempt to classify what type of change this is, and so no term is used either.

6.2.2 Thomas Burrow (1909-1986)

In his treatment of the Sanskrit language, Thomas Burrow (1909-1986) mentions both *adaptation* (cf. *exaptation*, in the meaning of the adaptation of suffixes into new functions) and changes which could be classed as *agglutination*, although I have not actually seen him use that term.

Burrow rejects the old hypothesis that the personal endings of verbs should stem from personal pronouns, and says that there certainly has not been enough comparative work done on this to be able to prove such a theory, even though it has been suggested fairly often (Burrow, 1973: 317-318). He opts instead for the *adaptation hypothesis*:

By some process of adaptation, the course of which it is not now possible to follow, certain nominal formations became associated with particular persons and number, and at least a fair proportion of the existing personal terminations came into being in this way. (Burrow, 1973: 318-319)

This however does not mean that Burrow dismisses the possibility that words can be agglutinated and become affixes. Quite frequently he discusses particles which have been added onto verbs, for instance. This is also how he explains the

Sanskrit *augment*, the vowel- (or diphthongal) prefix which is added to a verb to form the past tense. He writes: “The augment seems in origin to have been a separate word, namely a particle *é* meaning ‘there, then’ which came to be compounded with the verb” (Burrow, 1973: 304). In a clearly non-unidirectional view (if we compare it to the current proclaimers of the unidirectionality hypothesis) Burrow is open to the option that grammatical inflections may stem from different sources, both from lexical items and from other suffixes, for instance.

6.2.3 John Lyons (1932-)

I include John Lyons (1932-) in this chapter, since he is one of the few whose use of the term *grammaticalisation* has made it into the Oxford English Dictionary (Oxford English Dictionary, 1989h). However, his use of the term in his book *Introduction to Theoretical Linguistics* (Lyons, 1968), does not coincide with what we now usually use the term for. However, it is useful to show that the term had been used in English, in Britain, before the huge increase in its usage, but not necessarily in the same way as it is now used. Lyons discusses *grammaticalisation* similarly to Jakobson’s discussion of Boas’s work *Language* (Jakobson, 1971 [1959]), where Jakobson says that Boas claimed that some “notions” may be grammaticalised in one language and lexicalised in others (cf. Boas, 1938, and chapter 3).

6.2.4 Christopher John Elinger Ball (1935-)

In a rather brief paper from 1968, C. J. E. (Christopher John Elinger) Ball writes about the weak preterite in Germanic languages (Ball, 1968). Ball mentions that the most common theory of the origin of the dental preterite is what he calls *composition theory*, clearly an alternative term for *agglutination theory*. He however indicates that this theory may have been less popular, or half-forgotten about for a while, in saying that it “has recently been revived by Wisniewski” (Ball, 1968: 186).

It is quite clear that Ball himself would not actually want to support the theory of agglutination or composition theory:

In my view it is infinitely preferable to take what is, after all, the simplest and most obvious course, and straightforwardly to derive the dental of the preterite form from the dental of the past participle, as Begemann did as long ago as 1873. (Ball, 1968: 186)

However, he says that this alternative view, although a rather obvious alternative in his eyes, has often been overlooked (Ball, 1968: 186). Still it is clear that the theory he is referring to is something like adaptation theory which we have seen received some popularity in the late nineteenth century.

Unfortunately, this is the only publication that I have found by Ball where he treats anything close to grammaticalisation or even language change. There is therefore not much more that I can say about his theories, except that it seems as though he may have picked up on the idea of agglutination theory from Roswitha Wisniewski (1926-) (Wisniewski, 1963), and that it could therefore be correct to say that it had been on a low flame for a while. In addition, we should stress that Ball is **not** one of the linguists who has tried to derive (almost) all inflectional and derivational morphemes from autonomous words.

For the alternative theory he mentions (Georg Emil) Wilhelm Begemann, who is said to have thought of this alternative as early as 1873. Begemann in fact wrote on the subject of the weak preterite more than once, but the publication referred to by Ball must be *Das schwache Präteritum der germanischen Sprachen* (Begemann, 1873). Another publication, a supplement to the first, came out the year after (Begemann, 1874).

Linguists	Grammaticalisation?	Unidirectional?
Arthur Waley	grammaticalisation	?
Thomas Burrow	grammaticalisation (?) (adaptation, compounded)	No. (adaptation)
John Lyons	-- (grammaticalisation)	?
C. J. E. Ball	agglutination (composition theory) adaptation (cf. exaptation)	No (adaptation)

Table 6.2.1: British linguists' views on the development of grammatical elements (1900-1970).

Linguist	Examples
Arthur Waley	'to say' > auxiliary verb
Thomas Burrow	nominal formations > personal endings particle é 'there, then' > Sanskrit augment (PAST)
John Lyons	(grammaticalised vs lexicalised notions)
C. J. E. Ball	Germanic weak preterite < (<i>did</i> or) the past participle

Table 6.2.2: Examples used by British linguists between 1900-1970.

6.3 The United States

So far I have not managed to find very many American scholars who discussed something similar to grammaticalisation during this period. There are probably others, however the fact that they are hard to find speaks for itself. This is probably at least partly due to the force of structuralism and the emphasis on synchronic linguistics in the United States for the first half of the twentieth century.

6.3.1 Edward Sapir (1884-1939)

Edward Sapir (1884-1939) has been noted for his ideas on typology, and there are signs in his work of something that at least resembles the **result** of a process of grammaticalisation. However, he does not deal very much with the process itself. He was more interested in how different languages have categorised the world around them and how this is reflected unconsciously in their language. Although, he also has a passage where he talks briefly about what we would call *grammaticalisation*, but he does not call it anything in particular. He mentions that prepositions can develop out of adverbs, that the French future has developed through “**coalescence of originally independent words**” (Sapir, 1921: 118, emphasis mine), where he uses the same word as Jespersen (1922 [1949]) (see section 6.4.1), viz. *coalescence*. He also considers the possible origins of some Latin case endings which he sees as bleached forms that have later gained a syntactic function (Sapir, 1921: 118-120).

It is interesting that Sapir published *Language* (1921) at nearly the same time as Jespersen wrote his *Language* (1922 [1949]), and the two were also reviewed together with Joseph Vendryes (1875-1960) *Langage* by Meillet (1922). Since Meillet (1922) did not really note Jespersen’s interest in what he himself had called *grammaticalisation* a few years previously, it is not surprising that he does not comment on anything similar in Sapir who says much less on this issue. However, it may be of importance that Meillet does not note the difference in the extent of the treatment of the origin of grammatical elements (or morphological elements) in Sapir’s, Vendryes’s and Jespersen’s new books on language and linguistics.

Notably, Sapir (1921) seems to see derivations as going through the same kind of changes as inflectional morphemes. Jespersen (1922 [1949]) also seemed to see strong parallels between inflectional and derivational morphemes. It is possible that this was different to Meillet's view, since we do not really see any mention in his work of derivational morphemes. Or Meillet may have felt that it was obvious that these kinds of changes should be dealt with since it was general knowledge that they occurred.

6.3.2 Leonard Bloomfield (1887-1949)

Leonard Bloomfield (1887-1949), the famous American behaviourist and one of the first American structuralists, does not say very much about language change in his book *Language* (1935 [1969]). However, considering the focus on synchronic studies in structuralism, this is hardly surprising. But like Saussure, for instance, he at least touches on analogy and where affixes come from.

Interestingly, Bloomfield notes, as Bybee (1985), that there is a difference in how close to the stem derivational morphemes and inflectional morphemes are affixed (1935 [1969]: 222). He also calls attention to how difficult it sometimes is to draw a line between what a compound is and what a word plus a derivational affix is:

In extreme cases, of course, the form may be so unlike the independent word that we may hesitate between calling it a compound-member or an affix: a form like *fortnight* [...] lies on the border between compound and simple word. (Bloomfield, 1935 [1969]: 229)

This reminds us of the increased bonding and the phonological attrition that is often seen as part of grammaticalisation, whether the development of derivational morphemes is included in this process or not. Similarly, Bloomfield (1935 [1969]: 222) also notes that certain compounds tend to get frozen (in my words), so that the order of the words in the compound can no longer be changed: e.g. *bread-and-butter*. However, he recognises that there are also certain phrases in which the order has been fixed and these should not count as compounds.

Bloomfield does not really seem to have a sense of grammaticalisation. However, he briefly mentions how **rare** it is for “a relatively independent form” to be reduced “to affixal status” (1935 [1969]: 414).

Compound-members are occasionally reduced, by sound-change, to suffixes; thus, the suffix *-ly* (*manly*) is a weakened form of *like*, and the suffix *-dom* (*kingdom*) of the word *doom*. This happens especially when the independent word goes out of use, as in the case of *-hood* (*childhood*), which is a relic of an Old English word [ha:d] ‘person, rank.’ (Bloomfield, 1935 [1969]: 414-415)

Bloomfield sees this as primarily a phonetic change, and it is interesting how rare he believes that this is. Note also that he is once again speaking of derivational morphemes and not inflectional ones, although *-ly* could possibly be seen as somewhere in between the two classes. He goes on to reiterate how unusual it is for two words to merge:

Merging of two words into one is excessively rare; the best-known instance is the origin of the future tense-forms in the Romance languages from phrases of infinitive plus ‘have’ [...] This development must have taken place under very unusual conditions; above all, we must remember that Latin and Romance have a complicated set of verb-inflections which served as a model for one-word tense-forms. (Bloomfield, 1935 [1969]: 415)

Bloomfield has certainly not been affected by Bopp’s theory that the personal endings on verbs could be derived from free words. He also seems somewhat unaware (although highly unlikely considering his educational background) of the number of inflections that had also previously existed in Germanic languages, if he thinks that Romance languages are so very special in having a complicated and rich inflectional system.

In addition, Bloomfield discusses a few examples of changes which we would now class as reanalysis, but which he does not call anything at all, although he treats them in the chapter on analogic change:

In the later Old English period, final [n] after an unstressed vowel was lost, except in sandhi before a vowel. [...] The sandhi [n] was generalised in a few cases as a word-initial. Old English *efeta* [...] ‘lizard’ appears in Middle English as *ewte* and *newte*, whence modern *newt*. [...] Similarly, *eke-name* ‘supplementary name’ gave rise to a by-form with *n-*, modern *nickname*; [...] On the other hand, an initial [n] was in some forms treated as a sandhi [n]. Thus, Old English *nafogar*, literally ‘nave-lance,’ Middle English *navegar*, has been replaced by *auger*; Old English [næ:dre] gives Middle English *naddere* and *addere*, whence modern *adder*; Old French *naperon*, borrowed as *napron*, has been replaced by *apron*. (Bloomfield, 1935 [1969]: 419)

Another example which might qualify as analogy and reanalysis today is his mention of *ein Trunk Wasser* [NOM / ACC] instead of *ein Trunk Wassers* [GEN], which he believes has arisen by analogy with *ein Trunk Milch* [NOM / ACC / GEN] (Bloomfield, 1935 [1969]: 420).

There is one more interesting example of analogy in *Language*. Bloomfield claims that we can sometimes identify a syntactic change by finding a pattern that could have been used to create the current use of a word or phrase analogically. His example is the use of *like* as a conjunction, which would be seen as grammaticalisation today:

From the sixteenth century on, we find English subordinate clauses introduced by the word *like*. We can picture the innovation in this way:

<i>to do better than Judith</i>	:	<i>to do better than Judith did</i>
= <i>to do like Judith</i>	:	<i>x,</i>

where the outcome is the construction *to do like Judith did*. (Bloomfield, 1935 [1969]: 407)

There are some changes which Bloomfield (1935 [1969]: 420) believes one cannot explain through analogy, even though they resemble analogy. He claims that these changes, “*adaptive new-formations*”, work without a model.

<i>actorine</i> (Pattern: <i>Paul: Pauline</i>)	
<i>chorine</i> (Pattern: <i>actorine</i> – but <i>actor</i> – <i>actorine: chorus</i> – <i>x</i> (* <i>chorusine</i>))	

Chorine is noted to be different to *actorine* both in how it is formed and in what it means which makes it seem unlikely that it was formed by analogy.

6.3.3 Joseph Harold Greenberg (1915-2001)

Joseph H[arold] Greenberg (1915-2001) is perhaps most known for his work on language classification and typology. He is seen by many as the founder of modern work on typology. Like many of the people who were to become most important to the development of grammaticalisation studies during the 1970s Greenberg did much work in Africa and on African languages.

In a study of markedness from 1966, he touches on the links between phonologically marked items in the language and grammatical markers. He notes that a marked sound can on some occasions be called on to mark a grammatical category, as in the case of German umlaut (Greenberg, 1966: 69), this we can compare to what is now called *exaptation*. He also comments on the importance that frequency plays in the development of grammar.

Thus in phonology, diachronic process explains frequency, while in grammar, frequency explains diachronic process. Frequency not included in *la langue* definitionally is in fact an ever present and powe[r]ful factor in the evolution of grammatical categories and thus helps in explaining the types of synchronic states actually found. (Greenberg, 1966: 69)

This appears to indicate that Greenberg is open to changes not only from the lexicon into the grammar, but also that other linguistic elements can be called on to mark grammatical relations.

But within grammaticalisation studies it is really only one of Greenberg's articles that is well known (viz. Greenberg, 1991). This has already been discussed in chapter 3, but we can just note that in that paper Greenberg discusses grammaticalisation, lexicalisation and something which he calls *regrammaticalisation*. Similarly to the article from 1966 this paper also makes it clear that Greenberg hardly thinks of grammaticalisation as a strictly unidirectional process.

6.3.4 Dwight Le Merton Bolinger (1907-1992)

Dwight L. Bolinger (1907-1992) has been seen as one of the linguists who founded American functionalism. Under those circumstances it also seems quite natural that as we shall see below he and Talmy Givón had some interconnections, Givón admitting to having been influenced by Bolinger (Givón, 1979a).

In a publication from 1968 Bolinger can be found to use the term *grammaticizing*, in citation marks. He uses this notably before grammaticalisation gets going again during the 1970s, and it is also worth noting that he uses it in a discussion of word order developments:

When the Old English inflectional endings gave way, their functions were largely taken over by preposition and word order.²⁰² Instead of determining the nominative case by an ending, we now determine it by its relative position [...] There are drawbacks: when word order is laden as heavily as it is in English, sometimes it must lay part of the burden down and simply hope that some other means of distinction will pick it up – perhaps ‘what makes sense’ as against ‘what makes nonsense’ in the general situation. [...]

The real trouble that English brought upon itself with this ‘**grammaticizing**’ of word order is difficult to appreciate if we continue to think of the order of Latin and Old English as ‘free.’ It was not really free, but it expressed a part of the message that was secondary to who does what to whom and when, though still important. (Bolinger, 1968: 118-119, emphasis mine.)

This is not the only time that Bolinger discusses something similar to what we now call *grammaticalisation* in this publication from 1968. It is just the only time that he uses a term similar to ours. Although, he also speaks of *agglutination*, using that term. This is the process through which he claims that languages can “build inflections”:

An uninflected language can build inflections through **agglutination** – merely adding elements together; there is not much difference after all between *amo, amas, amat* and *Ilove, youlove, heloves*, if we want to think of the pronoun and its verb as a unit. An inflected language can lose its inflections. But whether inflection and lack of inflection are in some sort of historic alteration is beyond our ken. (Bolinger, 1968: 131, italics + bold original, emphasis in bold only mine.)

Bolinger does not provide the reader with any attested examples of this type of change, apart from *amo, amas, amat*, which he does not give any original forms for. He also stresses that we do not know if there is a tendency to alternate between inflections and no inflections. It is interesting that he chooses to illustrate agglutination with the example of verbs with distinct personal endings and pronoun + verb, seeing as one of the first examples of agglutination was exactly that. However, as noted above (e.g. section 5.6) the idea that Indo-European personal endings on verbs stemmed from pronouns had been dismissed by most people by the beginning of the twentieth century.

²⁰² Reference in Bolinger to Mustanoja (1960: 68).

Linguists	Grammaticalisation?	Examples
Edward Sapir	Yes. No term.	adverb > preposition French future developed through “coalescence of originally independent words” Latin case forms – bleached compared to the origins.
Leonard Bloomfield	Touches on where affixes come from, but does not have a sense of grammaticalisation. Occasionally something like grammaticalisation is seen as <i>analogy</i> by him.	<i>like</i> > <i>-ly</i> , <i>doom</i> > <i>-dom</i> (primarily a phonetic change) Romance future <i>like</i> > conjunction (‘analogy’)
Joseph H. Greenberg	grammaticalisation regrammaticalisation	marked sound (e.g. umlaut) > a sign for a grammatical category
Dwight Bolinger	<i>grammaticizing</i> <i>agglutination</i> – used to “build inflections”	word order (‘grammaticizing’)

Table 6.3.1: American linguists’ views on the development of grammatical elements (1900-1970).

6.4 Scandinavia

6.4.1 Otto Jespersen (1860-1943)

Meillet’s article which has been said to have introduced the term *grammaticalisation* was reprinted in 1921, one year before Otto Jespersen’s (1860-1943) *Language: Its Nature, Development and Origin* (1922 [1949]) was published.²⁰³ In brief histories of grammaticalisation it has sometimes been noted that Jespersen discussed a process similar to grammaticalisation under the name of *Coalescence Theory* in this book (e.g. Heine et al., 1991a: 7; Lehmann, 1982 [1995]: 3 fn.), and it is essential to note that he does so without any mention of Meillet’s 1912 article and Meillet’s (presumably) newly coined term *grammaticalisation*. The only terms that Jespersen mentions are *coalescence theory*, *agglutination theory*, and *secretion* the last of which may remind some of us of the process which has sometimes been called *exaptation*. However, this was not the first time that Jespersen tackled this subject, as he also discussed *agglutination theory* in a section of his book *Progress in Language* (1894 [1993]).

²⁰³ This section is based on Lindström (2003 (forth)).

Jespersen (1922 [1949]) dismisses *agglutination*, partly because he does not think it covers the whole process he has in mind, but also because of its association with agglutinative languages (1922 [1949]: 375). Instead, Jespersen chooses to speak of a process of *coalescence*. However, he is not quite happy with this new term either, since he has noticed that similar results, in other words new formatives, may be achieved through a process which could be seen as moving in the opposite direction or at least in directions different to those normally recognised as agglutination (or coalescence).

Jespersen was Meillet's senior by a few years, and he grew up in Denmark, which by that time had produced more than one well known linguist, as most present-day linguists are aware. Today he is perhaps most known for his interest in grammatical systems, sociolinguistics, language acquisition and language teaching, and for his work on the English language. He has also been recognised as one of the few who dared to tackle the topic of the origin of language, after it had been banned by the newly founded French *Société de linguistique de Paris* (cf. Morpurgo-Davies, 1998: 2), and he was also interested in artificial languages.

Jespersen was fascinated by language and grammar and wrote extensively on what he himself may have called the 'philosophy of grammar', in accordance with the title of one of his most well known works (Jespersen, 1924 [1992]). He was also interested in linguistic variation and the effect that social circumstances can have on one's language. It seems quite natural under these circumstances that he should also have been interested in something which resembles what we today call *grammaticalisation*, which makes an appearance in at least two of his works: *Progress in Language – with special reference to English* (1894 [1993]), and *Language: its nature, development and origin* (1922 [1949]).

In *Language: its nature, development and origin* (1922 [1949]) Jespersen devotes a chapter to the "origin of grammatical elements" (1922 [1949]: 367-395). This is a chapter of utmost value to us today, since it not only tells us about Jespersen's own views of the subject matter, but also attempts to provide the reader with a summary, commentary and critique of the kind of theories that were prevalent at the time. And it gives us an idea of what had been popular, and what had been accepted, as well as disputed, in the recently preceding decades.

It is interesting that Jespersen (1922 [1949]) shows familiarity with at least one of Meillet's writings (viz. Meillet, 1911). Although there is no mention of Meillet's

paper from 1912, and Jespersen neither mentions nor uses the term *grammaticalisation* (Jespersen, 1922, 1894 [1993]).²⁰⁴ This is of importance in judging the influence and importance of Meillet's article at the time. But also in judging the recognition of grammaticalisation as a concept in the late nineteenth and early twentieth centuries. However, it is important to note that it was only in the year directly preceding the publication of this work by Jespersen that Meillet's article became more widely available, having first been published in the general science journal *Scientia*. Clearly, it seems as though Jespersen was not familiar with Meillet's (1912) paper. He had also discussed this topic earlier than Meillet, in his review and criticism of *agglutination theory* in 1894, so it was not a new field to him in 1922.

The fact that Meillet reviewed Jespersen's *Language* 1922, is interesting, but unfortunately Meillet does not comment at all on his, Vendryes and Jespersen's mutual interests in the evolution or origin of grammar or the fact that Sapir also mentions something similar, but much more briefly. Vendryes, like Jespersen, has a whole chapter on the issue of morphological changes, which he like Meillet himself treats as being of two kinds: analogical and what we would call *grammaticalisation*, although Vendryes uses no term at all. Vendryes even has a reference to Meillet (1912) in a footnote attached to the heading of the chapter (Vendryes, 1921: 184). This is the first reference we find to this article. This could perhaps be an indication that Meillet's own interest in this field was dwindling. But it could also be a sign that it was simply a common area of research at the time, and that he did not think it was terribly original, or he might have thought that it was quite obvious that such a section should be included. And although Jespersen appears to have introduced some new original ideas, they do not seem to have been taken up by the research community.

Meillet does observe in passing that Jespersen discusses "l'origine des éléments morphologiques"²⁰⁵ (Meillet, 1922: 3), something which he does not note that the other two authors had done. However, notably he does not appear to see Jespersen's discussion as one of the origin of **grammatical elements**, but rather only of **morphological elements** – which goes well with agglutination theory or

²⁰⁴ This latter point was important to check since it bears on the history of the term *grammaticalisation* and whether Meillet was first to use it.

²⁰⁵ Translation: the origin of morphological elements

coalescence theory, and reasonably well also with what Jespersen calls the process of secretion which we shall discuss later. The fact that Meillet saw a continuity of the nineteenth century work in his own work on grammaticalisation makes it particularly interesting that he does not really pick up on this similarity between Jespersen’s work and the earlier work on agglutination theory. Especially if that is what he saw as the predecessor of grammaticalisation, and we note that Jespersen clearly referred to agglutination theory.

The most important question for us is quite naturally, whether Meillet saw any difference between his own work on the evolution of ‘grammatical’ elements and Jespersen’s work on the origin of ‘morphological’ elements. A first thought would be that Jespersen might have included derivational morphemes, whereas Meillet might not have. However, this is not quite clear. Jespersen certainly thinks that grammatical inflectional morphemes and derivational morphemes can sometimes develop in similar ways, whereas Meillet does not discuss any derivational morphemes (cf. table 6.4.1). It therefore seems quite possible that there is an important difference here between Meillet’s and Jespersen’s work, and this could be why Meillet only recognises a treatment of the origin of morphological elements in Jespersen’s book (1922 [1949]). However, the fact is that both Jespersen and Meillet considered their work to be a continuation of the nineteenth century work on *agglutination theory*.

Meillet’s examples of Grammaticalisation	Jespersen’s Examples of Coalescence (C) and Secretion (S)
Romance future	Romance Future (C)
IndoEuropean negation	Scandinavian passive (C)
<i>Hiu tagu > hiutu > heute</i>	Suffixed definite article (e.g. Scandinavian) (C)
(word order changes)	German <i>-en, -er</i> plural (S)
Conjunctions	English <i>-en, -s</i> plural (S)
	<i>my – mine</i> (S)

Table 6.4.1: Meillet’s and Jespersen’s examples of grammaticalisation and coalescence and secretion respectively.

In addition to Jespersen’s discussion of his own views on the origin of grammatical elements, he gives an overview of the history of such ideas. What Jespersen calls *the old theory* is the idea that flecnal languages (where the formal elements have merged with the roots) had developed from a previously isolating language, where there were only roots. This was a process believed to have proceeded through a stage of agglutination, where some formal items had developed, but were still independent from the roots (1922 [1949]: 367). Although this was not

accepted by every linguist in the nineteenth century, it was certainly a common view of the way that language had evolved (cf. chapter 5 and Lindström (2003)).

Jespersen chooses to use William Dwight Whitney's (1827-1894) work as an example in his criticism of the old root theory, giving as an example the following extract from Whitney (*Life and Growth of Language* (1875)):

The firm foundation of the theory of roots lies in its logical necessity as an inference from the doctrine of the historical growth of grammatical apparatus (Whitney, 1875: 200, cited in Jespersen 1922 [1949]: 367)

There is an interesting difference between Jespersen's own views of the evolution of language and those of Whitney. Whitney was more traditional in his views and saw language as progressing on a continuum or cline from isolating to agglutinating to flectional. Whitney criticises Ernest Renan (1823-1892) for choosing to see linguistic evolution as something like the opposite, namely from synthetic to analytic, a view which Jespersen appears close to agreeing with Renan on. Although Jespersen does not think that the stages can simply be reversed, it is more complicated than that (Jespersen, 1894 [1993]: 126-127; Whitney, 1867 [1973]: 177, 284-286):

On every point our investigation has led us to scepticism with regard to the system of the old school of philology. [...] It could not do simply to reverse the order of the three stages of evolution, and say that flexion is the oldest stage, from which language tends through an agglutinative stage towards complete isolation; for flexion, agglutination, and isolation do not include all possible structural types of speech, nor do these words with sufficient definiteness characterise the successive stages of those languages whose history is comparatively best known. [...] THE EVOLUTION OF LANGUAGE SHOWS A PROGRESSIVE TENDENCY FROM INSEPARABLE IRREGULAR CONGLOMERATIONS TO FREELY AND REGULARLY COMBINABLE SHORT ELEMENTS. (Jespersen, 1894 [1993]: 126-127, emphasis original)

However, it is clear that Jespersen also recognises moves from analytic to synthetic. Although he and Renan think the original language was synthetic, since our oldest attested forms and reconstructions indicate longer words and more grammatical inflections.

There may be an interesting contrast between the present formulations of grammaticalisation and the ideas of an original state of roots that Jespersen discusses, but which he does not agree with. Jespersen (1922 [1949]: 373-374) observes that

linguists borrowed from Sanskrit grammarians a sense that the first roots were verbal only and mainly general and abstract. This seems to go against current views of grammaticalisation, where there is rather wide agreement that there is usually a movement from concrete to abstract. Unless this is just a different way of saying that it is the most general, basic words that tend to grammaticalise and in other words not the more specific words. If so this coincides very well with current work on grammaticalisation. As an example, it is more likely that *go* would develop a grammatical function than that *stroll* would.

Agglutination theory is the ‘next step’ in Jespersen’s history of the theories of the origin of grammatical elements, although it more or less just gives a name to part of the theory discussed at the beginning.

According to the received theory [...] some of the roots became gradually attached to other roots and lost their independence, so as to become finally formatives fused with the root. This theory, generally called the agglutination theory, contains a good deal of truth ... (Jespersen, 1922 [1949]: 375)

The earliest works that Jespersen mentions which are to have supported agglutination theory, apart from Bopp, are from the early 1870s: *Oriental and Linguistic Studies* (Whitney, 1873-1874), *Life and Growth of Language* (Whitney, 1875) and *Kleine philosophische Schriften* (Madvig, 1875) (cf. Madvig, 1856).

Jespersen sees agglutination theory as containing some good and valuable ideas, however he has some problems with this view, which he attempts to remove by noting three “provisos”:

1. Flecional languages can never have been “wholly agglutinative” during a “definite period.”
 2. Formatives must have originated in real words and not in roots.²⁰⁶
 3. This is not the only process which can lead to new formatives (cf. Meillet 1912).
- (Jespersen, 1922 [1949]: 375)

He also claims that agglutination “may be called the rectilinear process, but by the side of that we have also more circuitous courses, which are no less important in the life of languages for being less obvious” (Jespersen, 1922 [1949]: 375). In other words, Jespersen’s views hardly seem unidirectional.

²⁰⁶ cf. grammaticalisation today where the idea is also lexical item, not root > grammatical element

The way Jespersen sees it, agglutination is only an intermediate stage in the evolution of grammatical formatives. He therefore objects to the term *agglutination theory* and would rather call it *coalescence theory* (Jespersen, 1922 [1949]: 375-376). He also claims that the most important aspect of this development is not the agglutination, but the loss of independence through semantic bleaching and phonetic attrition. This may have been expressed in different words by him, but what he says clearly resembles these two important aspects noted in recent studies of grammaticalisation:

What is really the most important part of the process is the degree in which one of the components loses its independence, phonetically and semantically.

As 'agglutination' is thus only one intermediate stage in a continuous process, it would be better to have another name for the whole theory of the origin of formatives than 'the agglutination theory,' and I propose therefore to use the term 'coalescence theory.' The usual name also fixes the attention too exclusively on the so-called agglutinative languages [...] (Jespersen, 1922 [1949]: 375-376)

The process discussed by Jespersen is in other words a process which involves agglutination, loss of semantic and phonetic independence just like grammaticalisation is often said to involve: phonological attrition, semantic bleaching and affixation (cf. Lehmann, 1982 [1995]). All three of which are important, but it is the loss of independent meaning and form that is of most importance to Jespersen. In other words, Jespersen includes the whole continuum of the creation of new grammatical formatives including the creation of independent grammatical formatives and not only the creation of new affixes, as would seem to be the case with *agglutination* in a literal sense, just as we would today.

We should also note that coalescence, which is rather similar to *agglutination*, does not only include the formation of grammatical formatives, as e.g. independent grammatical items and grammatical inflectional morphemes, but also the development of new derivational morphemes.

There can be no doubt, therefore, that some at any rate of our suffixes and prefixes go back to independent words which have been more or less weakened to become derivative formatives. But does the same hold good with those endings which we are accustomed to term flexional endings? The answer certainly must be in the affirmative – with regard to *some* endings. (Jespersen, 1922 [1949]: 377)

We should note that Jespersen does not see this as the only way in which new endings may be formed. He is critical of the idea that **all** prefixes and suffixes, all grammatical formatives, should stem from independent words and he comes back to this question a few times in his chapter on the origin of grammatical forms. In modern terms this would be seen as a clear rejection of a strong hypothesis of unidirectionality (cf. chapter 3).

Jespersen (1894 [1993]; 1922 [1949]) gives some examples of flexional endings that have originated in independent words, such as the Scandinavian passive, the Romance future and the suffixed definite article, e.g. in Scandinavian languages. However, he questions whether there are any more examples than the ones he mentions:

As to real flexional endings traceable to words, their number is even comparatively smaller than that of derivative suffixes; the three or four instances named above are everywhere appealed to, but are there so many more than these? And are they numerous enough to justify so general an assertion? My impression is that the basis for the induction is very far from sufficient. (Jespersen, 1922 [1949]: 379, cf also 1894 [1993]: 66-67)

This should be compared to Hopper and Traugott (1993: 128-129) who conclude quite the opposite. Since we know of no cases, according to them, where grammatical forms have arisen “full-fledged”, we ought to assume that all endings stem from independent words.

The examples have been used by many others also, repeated many a time, as Jespersen notes (Jespersen, 1894 [1993]: 66; 1922 [1949]: 377, 379). (But he does not mention any independent grammaticalised words at all, for instance, he does not mention the French negation *ne... pas* as so many others have done.)

However, believing that grammatical formatives could arise in other ways than from lexical items was not Jespersen’s only reason for dismissing the unidirectional agglutination theory. As noted above, he also did not believe that language originated in an analytic isolating root stage. Therefore, all formative elements could not be assumed to have developed from roots or even words, since in his eyes there were formative elements in the original form of language which he believed was a synthetic kind of language. Notably, he also claims that most linguists have stopped spending time on agglutination theory:

The impression left on us by all these cases is that many of the earlier explanations by agglutination have proved unsatisfactory, and that linguists are nowadays inclined either to leave the forms entirely unexplained or else to admit less rectilinear developments, in which we see the speakers of the old languages groping tentatively after means of expression and finding them only by devious and circuitous courses. (Jespersen, 1922 [1949]: 384)

As was noted above, Jespersen recognises that there are other processes apart from coalescence or agglutination that can give us new grammatical formatives. These, he claims, are difficult to classify, but one of the processes “is important and distinctive enough to have its own name” and he decides to call it “*secretion*”.

By secretion I understand the phenomenon that one integral portion of a word comes to acquire a grammatical signification which it had not at first, and is then felt as something added to the word itself. Secretion thus is a consequence of a ‘metanalysis’ ... (Jespersen, 1922 [1949]: 384)

Note that the term *metanalysis* occurs here inside citation marks, which indicates that Jespersen did not expect his audience to be familiar with the term. Also *secretion* appears to have been a new term and it is of course quite clear that this is a term that he just coined. He also claims that *metanalysis* is a new coinage:

We now come to the phenomenon for which I have ventured to coin the term ‘metanalysis,’ by which I mean that words of word groups are by a new generation analyzed differently from the analysis of a former age. [...] *A naddre* (the ME. form for OE. *an nædre*) thus became *an adder*, *a napron* became *an apron*, *an nauger*: *an auger*, *a numpire*: *an umpire*; and in psychologically the same way *an ewte* (older for *evete*, OE. *efete*) became *a newt*: metanalysis accordingly sometimes shortens and sometimes lengthens a word. (Jespersen, 1922 [1949]: 173)

From this we see that Jespersen recognised a particular type of what we might call *reanalysis*. A form of reanalysis that can be involved in the creation of new grammatical elements as well as in the new form of some lexical items. However he never speaks of anything like metanalysis or reanalysis in connection with agglutination or coalescence, which in our words would also often involve reanalysis. A slightly different kind of reanalysis. He also speaks of this kind of process, and in fact what we would be more likely to call *metanalysis* today, in relation to suffix extension (e.g. *lait-ier* > *lai(t)-tier*). Although, then the process is not referred to by any term at all (Jespersen, 1922 [1949]: 386-387) and he cannot

class it as metanalysis since he has restricted this to changes within word groups and not within words.

Looking at a few of Jespersen's examples it becomes clear that this also has clear parallels in the contemporary grammaticalisation framework. His examples include *-en* and *-s* plurals in English, as well as *-en* and *-er* plurals in German; the development of a distinction between *my-mine*, first based on phonetic environments and later acquiring a grammatical function (Jespersen, 1922 [1949]: 384-385). Jespersen's *secretion* clearly resembles what we might call *exaptation*, or *regrammaticalisation* today (cf. Greenberg, 1991; Lass, 2000; Wegener, 2002).

But Jespersen appears to see secretion as being a parallel process to what he calls *coalescence*, whereas many linguists today would claim that exaptation is a process distinct from grammaticalisation, and therefore not a counterexample to the unidirectional movement of grammaticalisation. Both processes, as well as others, contribute to the development of grammatical items, and secretion is therefore an indication that coalescence is not quite wide enough as a category for the processes that lead to the formation of grammatical elements. Therefore he believes that linguists who have claimed that grammatical endings develop through agglutination, have been at least partly wrong.

In a firm dismissal of the present strong hypothesis of unidirectionality, Jespersen states:

I have no doubt that the vast majority of our formatives, such as suffixes and flexional endings, have arisen in this way through transference of some part, which at first was unmeaning in itself, from one word to another in which it had originally no business, and then to another and another, taking as it were a certain colouring from the words in which it is found, and gradually acquiring a more or less independent signification or function of its own. (Jespersen, 1922 [1949]: 391)

What is this other than the recycling of junk that Lass (1990) referred to as *exaptation*? And notably, this gives secretion together with analogy greater prominence and spread than coalescence.

6.4.2 Erik Wellander (1884-1977)

Erik Wellander (1884-1977) was someone who cared deeply for the Swedish language (Molde, 1977) and he was professor of German at the University of

Stockholm (Hovdhaugen et al., 2000: 440). First and foremost he was interested in semantics and language use. In one of his studies of the development of meaning, or the ‘mechanism’ of the development of meaning, as he called it (“Betydelseutvecklingens Mekanik” (1968)), he speaks of grammaticalisation. But he also makes it clear that this is not the first time that he speaks on the subject. He had also treated it in a paper that was published in 1964 (Wellander, 1964), but which had been presented to Nyfilologiska Sällskapet (the Neophilological Society) in Stockholm, in the autumn of 1930 under the title “Till frågan om de grammatiska formernas uppkomst” (= ‘on the question of the development of grammatical forms’).

It is interesting that Wellander in both papers (1964; 1968) uses the term *grammatikalisering* / *Grammatikalisierung* (= *grammaticalisation*). And perhaps even more interesting is the fact that he claims that Humboldt has shown us the way in this field of research in his thesis from 1821 (“Über das Entstehen der grammatischen Formen” (Humboldt, 1825 [1905])).²⁰⁷

In this matter Wilhelm von Humboldt has shown us the way in his thesis ‘Über das Entstehen der grammatischen Formen’ from 1821, but the road has been rarely travelled. I made an attempt in a lecture for the Nyfilologiska Sällskapet in Stockholm during the autumn of 1930, ‘Till frågan om de grammatiska formernas uppkomst’, eventually published in *Studia Neophilologica* Vol. XXXVI (1964). Here I would like to try to develop and illustrate further the thoughts that were there implied.²⁰⁸ (Wellander, 1968:105) (translation)

Wellander also claims that Humboldt used the term *grammaticalisation*, or rather presumably *Grammatikalisierung* (1968: 141).

The construction *ha* [‘have’]+perfect participle has a very interesting history, which goes far back in time. The development of the concrete meaning-bearing verb into a temporal auxiliary verb is a mysterious example of **what Humboldt called grammaticalisation** of content words – [...] ²⁰⁹(Wellander, 1968:141) (emphasis mine) (translation)

²⁰⁷ He refers to it as being from 1821, however the only publication I have found it from 1825 (repr. 1905) and there it says that it was read to the Academie der Wissenschaften in 1822.

²⁰⁸ Original: “Här har Wilhelm von Humboldt visat vägen i sin avhandling ‘Über das Entstehen der grammatischen Formen’ av år 1821, men den vägen är föga trampad. Ett försök gjorde jag i ett föredrag i Nyfilologiska Sällskapet i Stockholm på hösten 1930, ‘Till frågan om de grammatiska formernas uppkomst’, omsider tryckt i *Studia Neophilologica* Vol. XXXVI (1964) Här vill jag söka att ytterligare utveckla och belysa de tankegångar som där antytts.” (Wellander, 1968: 105)

²⁰⁹ Original: “Konstruktionen *ha*+perfektparticip har en högeligen intressant historia, som går långt tillbaka i tiden. Det konkret betydelsebärande verbets utveckling till temporalt hjälpverb är ett gåtfullt

But this should have been in the thesis from 1821, since Wellander mentions no other publication by Humboldt, and there is no use of the term in that thesis. Nor do there seem to be any other terms in the thesis for a similar process.

Both of Wellander's papers which, according to him, are meant to deal with grammaticalisation, treat not only some rather clear cases of grammaticalisation but also some cases which may not as clearly belong to the class of grammaticalisation in the eyes of modern linguists. It is clear that what he is most fascinated by is semantic change, and so all sorts of changes where a verb takes on a new meaning when used together with a particle, for instance, come to be seen as examples of this type of change. However, it is sometimes quite uncertain whether they could be classed as grammaticalisation today. Perhaps fossilisation would be closer. Still it should be noted that in the fact that he notes that there are words which have lost all or most of their material meaning and become grammatical signs, he does appear to have a notion similar to what we would now call *grammaticalisation* (cf. Wellander, 1964: 127). Notably, he claims (1964: 128-129) that the general belief regarding how grammatical forms arise still seems to be more or less what Humboldt himself proposed, namely that: "gewisse sachbezeichnende Wörter verlieren allmählich ihre Bedeutung und werden zu nur formbezeichnenden Sprachelementen, zu reinen Ausdrücken für grammatische Verhältnisse."²¹⁰

It may be interesting to note the title of Wellander's first paper (1964) – "Zur Frage über das Entstehen der grammatischen Formen" and to compare this to Humboldt's title "Über das Entstehen der grammatischen Formen" which is almost identical; Wellander's title is just a bit longer. We can also compare them both to Meillet's (1912) title "L'Évolution des Formes Grammaticales". All very similar and they all make it clear that there must have been a rather long-standing continuous fascination with the origin of grammatical forms.

It is clear, especially in his paper from 1964, but also in the paper from 1968, that Wellander had been impressed by Humboldt's thesis (1825 [1905]). He refers to it quite often in both works and in the first paper (1964) he cites what would count as grammatical forms in Humboldt's eyes:

exempel på vad Humboldt kallade grammatikalisering av sakbetecknande ord – [...]" (1968:141, emphasis mine.)

²¹⁰ Translation: certain content words generally lose their meaning and become merely form-denoting linguistic elements, pure expressions of grammatical relations.

Die grammatische Wörter sind solche, ‘die allgemein gar keinen Gegenstand, sondern bloss ein Verhältniss, und zwar ein grammatisches, bezeichnen’ (S. 292). Als Beispiele für grammatische Wörter werden Präpositionen, Konjunktionen (S. 303) und Hilfsverben (S. 304) angeführt. ‘Wirkliche’, ‘ächte’, ‘wahre’ grammatische Zeichen sind – ausser der Wortstellung – die verschiedenen Modifikationen eines Wortes, die man mit der Bezeichnung Flexion zusammenzufassen pflegt: bedeutsame Silben, bedeutungslose Buchstaben oder Silben, Umwandlung der Vokale, Veränderung der Konsonanten, Silbenwiederholung (S. 295).²¹¹ (Wellander, 1964: 127)

What is interesting here is of course that he mentions word order, as a true grammatical sign, comparable to inflections. Word order was also seen by Meillet (1912) as something which could grammaticalise, but in recent grammaticalisation studies it has sometimes been rejected. Hopper and Traugott (1993: 50) opine that since it is not unidirectional it cannot be seen as grammaticalisation in the narrower sense (lexical item > marker of grammatical relations), which is the sense they are focusing on and which they see as prototypical. Although they admit that word order changes can enable grammaticalisation in the narrower sense to take place and they can also be an outcome of such changes (1993: 50).

Wellander also cites Humboldt as having said that all languages to him definitely seem to have originated in agglutination (“ ‘Denn’, sagt Humboldt, ‘ich kann die Überzeugung nicht verlassen, dass doch alle Sprachen hauptsächlich von Anfügung ausgegangen sind’ (S. 301).” 1964:127).²¹² Humboldt is also cited as having mentioned sound and meaning changes, as well as changes in bonding or loss of autonomy, which words go through in this process (1964: 128).

Wellander (1964:128-129, 148-149) notes that new grammatical forms usually develop through the loss of meaning in content words. Although he also mentions that it is possible that linguistic elements without any meaning acquire morphological functions (cf. Jespersen, 1922 [1949], secretion; Lass, 1990, exaptation), as in the example *Lämmer* ‘lambs’ (an umlaut plural of German *Lamm* ‘lamb’) according to Wellander.

²¹¹ Translation: The grammatical words are such ‘which in general indicate no resistance at all, but only a relation, namely a grammatical [relation]’ (p. 292). As examples of grammatical words prepositions, conjunctions (p. 303) and auxiliary verbs (p. 304) are noted. ‘Real’, ‘genuine’, ‘true’ grammatical signs are – apart from word order – the different modifications of a word which one usually summarises with the term flexion; meaningful syllables, meaningless letters or syllables, shifts of the vowels, changes of the consonants, repetition of syllables (p. 295).

²¹² Translation: ‘Then’, says Humboldt, ‘I cannot give up my conviction that really all languages have primarily developed through agglutination.’

Unfortunately, Wellander appears happy to use examples more than definitions. Like many people working on grammaticalisation after him, he felt that concrete examples were to be preferred to long explanations (Wellander, 1968: 105). This also means that he does not really explain what he means by *grammaticalisation* / *Grammatikalisierung*. For instance, he has several examples of the ‘mechanism of semantic development’, but are they all grammaticalisation in his eyes and if so why? Some of his examples would definitely count as grammaticalisation, such as the development of the numeral *en* ‘one’ both into a Swedish indefinite article and into a use where it can appear in the plural, e.g. *Vad är ni för ena?* ‘lit. What are you [plural] for ones?, i.e. Who are you lot then? (a bit rude) (Wellander, 1968: 105-107). He claims that all that remains of the number *en* ‘one’ in this usage is a grammatical sign, in the terms of Humboldt (Wellander, 1968) (Wellander, 1968: 107). Wellander (1968: 107) also notes that although this is a new usage that *en* has acquired, the old usage as a number, a pronoun and an article also still remains. In other words he noticed what we would now call a *split* (Heine and Reh, 1984) or *divergence* (Hopper, 1991) and what during the nineteenth century was often called *isolation* (see chapter 5).

Wellander’s (1964:130) examples include Swedish *hos* (‘at (somebody’s place)’) which is a preposition derived from the noun *hus* ‘house’, and he questions how French *chez* and the adverbial derivative *–ment*, developed – two examples which are rather common in the recent grammaticalisation literature.

Other examples are more problematic. He mentions *kvinnlig riksdagsman* (‘lit. female parliament-man’, i.e. a female member of parliament) where he notes that *man* cannot possibly mean ‘a man’, instead he sees it as a new derivational ending (1968: 108). This could still today be seen as an example of grammaticalisation, at least by some. It would depend on how the linguist looked upon the creation of new derivational morphemes. But would we see *provår* ‘lit. test year’ developing from that meaning to the meaning of a test period as in *provår på en termin* (‘lit. test year for one semester/term’) as grammaticalisation or only a semantically bleached compound or a fossilised / frozen expression (cf. Wellander, 1968: 109)? Another example is, *timme* ‘hour’ which in educational terms means a lesson (usually less than an hour) (cf. Wellander, 1968: 109). Is that grammaticalisation? *Knyta upp* lit. ‘to knot up’, meaning ‘to unknot’, *duka av* ‘lit. set the table off’, meaning ‘to clear the table after a meal’, are other examples, and there are many more examples of this

kind. Wellander also gives the example of Eng. *grandson* and *great grandson* which certainly has nothing to do with size, but has been created on analogy with *grandfather-grandmother*, *great grandfather* – *great grandmother* (1964: 132-133), which he sees as a form of word formation.

Word formation is nowadays usually seen as distinct from grammaticalisation, but it is not clear how Wellander saw the two. Did he class metaphorical uses of a verb where they have come to have an extended meaning or the opposite meaning with the help of a particle, and metaphorical extensions of adjectival modifiers extended through analogy, as the same kind of change as nouns turning into prepositions (Wellander, 1964: 132-133; 1968: 109-116)? It is difficult to say, but sometimes it seems as though he sees all of these as grammaticalisation.

Wellander (1964: 137-138) actually says at one point that the change when one in German starts to say *losbinden* ‘tie loose’ by analogy with *festbinden* ‘tie up’, is not a gradual change, but a sudden creation. According to him, this is analogical word formation. This would definitely mean that it would not be seen as grammaticalisation in current research, since for one thing grammaticalisation is always gradual. But does Wellander see this kind of changes as the same kind of change as that which is involved when new grammatical forms develop?

He also mentions that some phrases become used more and more, and therefore become frozen phrases which then can be modified through analogy and turn into something quite different from the original syntactical and semantic phrase (1964: 138-139). And he explicitly says that to be fully grammaticalised, a verb (and presumably also other parts of speech) only **needs to be used often** in new combinations of a similar kind.

Wie aus obigem hervorgeht, fehlt nicht viel, dass das zweite Glied einer neu gebildeten Zusammensetzung zu einem blossen morphologischen Hilfsmittel herabsinkt, zu einer ‘grammatischen Form’, um mit Humboldt zu reden.

Damit das Verb als völlig ‘grammatikalisiert’ betrachtet werden kann, braucht es nur fleissig in neuen Zusammensetzungen ähnlicher Art verwendet zu werden. Ist eine lebhaft analogische Neubildung von ähnlichen Komposita am Werke, wird nämlich einerseits die Sachbedeutung immer mehr abgeschwächt, andererseits tritt die formale Funktion immer deutlicher zutage.²¹³ (Wellander, 1964: 138)

²¹³ Translation: As appears from the above, not much is missing, that the second part of a newly created compound sinks into merely a morphological auxiliary, to a ‘grammatical form’, to use Humboldt’s term.

In Wellander (1968: 127-128) there is also some mention of the increased usage of prepositions instead of case inflections, and what this has meant for the prepositions, how, for instance, *på* ‘on’ has lost much of its concrete locative meaning in many of its uses and now only fulfils a grammatical role. Wellander (1968:128, 131) sees at least part of this process as being caused by analogy, and he also talks of “mechanical analogy” (“mekanisk analogi”, “mechanischer Analogie” (1964: 146; 1968: 131)) which he claims leads to the meaning fading away (cf. *semantic bleaching*). Analogy is often mentioned in connection with grammaticalisation. Meillet (1912) saw analogy as one of two processes in language that caused grammatical change, the other being grammaticalisation. Instead Hopper and Traugott (1993) have seen analogy as a process that is at work in the later stages of grammaticalisation, after a construction has been reanalysed and starts to spread to new uses.

Wellander (1968: 132-134) also finds it important to note when the original meaning is one that can no longer be the correct one in the context. Several times he remarks that once we start to get into the habit of using an expression it tends to expand its frame of usage, and through this its meaning is bleached (“bleknar” see e.g. (1968: 134)) more and more.

One of Wellander’s more straightforward examples of grammaticalisation (in our eyes at least) is one which Meillet (1912) also used, namely the periphrastic perfect in some Indo-European languages. However, to Wellander this is a slightly mystical example:

The construction *ha* [‘have’] + perfect participle has a highly interesting history, which goes back far. The development of the concrete meaningful verb into a temporal auxiliary verb is a mysterious example of what Humboldt called grammaticalisation of a word which denotes things – [...] ²¹⁴ (Wellander, 1968: 141, cf. 1964: 144) (translation).

For a verb to be seen as totally ‘grammaticalised’, it only needs to be used frequently in new compounds of the same kind. If a lively analogical creation of similar compounds is at work, then on the one hand the concrete meaning becomes weaker and weaker, and on the other hand, the formal function becomes clearer.

²¹⁴Original: “Konstruktionen *ha*+perfektparticip har en högälgigen intressant historia, som går långt tillbaka i tiden. Det konkret betydelsebärande verbets utveckling till temporalt hjälpverb är ett gåtfullt exempel på vad Humboldt kallade grammatikaliserings av sakbetecknade ord – [...]”

This shows the development of new means of expressing tense and aspect, a rather typical example, which has been treated several times (Bybee and Dahl, 1989; cf. Bybee et al., 1994; Dahl, 1985, etc.).

It is important in this context to note that Wellander (1968:146-147) treats the development of the verb *ta* 'take'. He claims that Östergren²¹⁵ (p. 8) calls *ta* an auxiliary, using the following examples: "*Men så tog människan och gick in vid teatern [...] Ta och spring ner efter litet grädde åt mig!*"²¹⁶ Wellander questions what the meaning of *ta* is in this type of phrase. He believes that it seems as though it in some cases only serves to make the length of the utterance seem more "polite" ("artig längd" = polite length). But the more it is used the more it loses some of its concrete meaning and becomes more and more subordinated grammatically and semantically according to Wellander (1968: 147). The reason I found it important to pick up on the discussion of *ta* is that it has also been discussed more recently by Lena Ekberg (1993a, see also chapter 7). She treated this much more extensively, but without any reference to Wellander or Östergren.

Wellander is definitely primarily interested in the semantic changes that constructions and elements can undergo and how this can lead to still further new usages, in a gradual expansive process (cf. e.g. Wellander, 1968: 191). He is happy to speak of it as the development of new grammatical devices on some occasions, and as *grammaticalisation*, a term which he claims to have adopted from Humboldt. Still whether all of his examples are meant as examples of new grammatical devices is not clear. Some are examples of new derivational morphemes, new bleached prepositions, aspect and tense markers. Although as I have shown above he also has examples of verbs which with a particle mean the opposite of what they originally meant and it seems doubtful whether that should be seen as the same type of change. And did the meaning of the verb really change, or is it the particle that expresses the difference in meaning as compared to the original verb?

From a historical point of view it is interesting to see that Wellander (1964: 129) says that this part of semantic (lit. meaning) research is often left to the side:

²¹⁵ Unfortunately it is not clear who this is, nor if there is a specific publication that he has in mind. However it seems likely that he is referring to (Karl) Olof Östergren (1874-1963). Östergren was a linguist, a reader in the Swedish language at Uppsala University from 1905 and professor from 1936. Östergren is primarily famous for his work within the area of stylistics (www.ne.se).

²¹⁶ Translation: (1) Lit. But then took the human being and went in by the theatre [...], meaning 'But then the person decided to join the theatre.' (2) Lit. Take and run down after some cream for me! Meaning 'Go and get some cream for me!'

Charakteristisch für die neuere Bedeutungsforschung ist, dass diese Seite des Problems gern beiseite gelassen wird. Die Entwicklung des Formensystems ist, wie leicht einzusehen, erheblich schwieriger klarzulegen als gewisse andere Bedeutungsveränderungen, aber für das Verständnis der Sprache als solcher ist gerade die Klarlegung jener Entwicklung von entscheidendem Belang.²¹⁷

Notable here are two things, (1) that he sees this as part of the study of meaning, and (2) that he claims that it has not been studied much recently. He also claims that there has not really been any progress in this field since Humboldt's times (1964: 129), and in the later paper he says it is a field of research that has been little explored (1968: 105).

Let us round off this section with Wellander's own summary of how grammatical forms arise:

Die Entstehung einer 'grammatischen Form' liesse sich also schematisch ungefähr folgendermassen darstellen:

Ein Wort mit Sachbedeutung wird gewohnheitsmässig in einer syntaktischen Verbindung von einem gewissen Typus verwendet, wo die verschiedenen Wörter in semasiologischer Hinsicht zu einer Einheit verschmelzen. Die syntaktische Zusammengehörigkeit ist ursprünglich die durch die Sachbedeutung der verschiedenen zur Verbindung gehörenden Wörter bedingte, aber der Ausdruckstypus wird durch fleissigen Gebrauch mechanisiert, automatisiert.

Nach diesem Ausdruckstypus als Muster werden nun in rein mechanischer Analogie neue Verbindungen gebildet, wo aufgrund des Inhalts anderer zur Verbindung gehörenden Wörter die Sachbedeutung des Worts nur zum Teil oder gar nicht mehr vergegenwärtigt werden kann. Die syntaktische Struktur ist hier schon vom Anfang an die des mechanisierten Ausdruckstypus; die ursprüngliche syntaktische Zusammengehörigkeit ist wegen des Inhalts der einzelnen Wörter nicht mehr möglich.

Damit sind auch die semasiologischen Voraussetzungen für eine weitere Grammatikalisierung des Wortes geschaffen. Inwiefern eine solche wirklich zustandekommt, hängt davon ab, ob das Wort in seiner neuen rein formalen Bedeutung sich im morphologischen System als nützlich erweist, ob es für sich eine bestimmte Funktion erringen kann. Ist dies der Fall, so folgen möglicherweise auch Verlust des Akzents, Reduktion des Wortkörpers u. dgl. (Wellander, 1964: 147-148)²¹⁸

²¹⁷ Translation: Characteristic of more recent semantic research is that this side of the problem is commonly left to one side. The development of the form system is, as is easily seen, significantly more difficult to elucidate than certain other semantic changes, but for the understanding of language as such the exposition of exactly this development is of significant importance.

²¹⁸ Translation: The development of a 'grammatical form' can therefore be schematically described along the following lines:

A word with concrete meaning is normally used in a particular type of syntactic relation where, from a semantic point of view, the different words melt into one unit. Originally, the shared

6.4.3 Elias (Gustaf Adolf) Wessén (1889-1981)

Elias Wessén (1889-1981) never uses the term *grammaticalisation* in the works which I have been able to consult (*Svensk Språkhistoria I-III* (1941; 1943; 1956; 1958; 1962; 1965 [1992]; 1968 [1995]; 1971 [1992]), *De nordiska språken* (1939; 1975 [1992])). However he does treat something which reminds us forcibly of the examples that used to be given of agglutination. He also has examples where we would talk of reanalysis today, where he notes that change has occurred in the meaning, or changes as to whether both parts of a word or only one of them is inflected, see examples below.

One of the classic examples in Scandinavian works on grammaticalisation, agglutination, as we have seen in chapter 5, is the passive or reflexive forms. Wessén (1941: 27, 87; 1956: 160, 164; 1962: 36, 122; 1965 [1992]: 173, 177; 1968 [1995]: 42, 134) also mentions that the passive/reflexive verb forms ending in *-s* have developed through the joining of the active verb to a reflexive pronoun in the dative or the accusative. He says that the pronouns and the verb “melted together” (“*sammansmälte*”), in other words merged with one another, both “prosodically and semologically (=semantically)²¹⁹” (“*prosodiskt och semologiskt*”). This appears to be similar to saying that there is some phonological attrition and semantic bleaching involved.

The expression *sammansmältning* is the same as that which was used by Noreen earlier (5.5.4), and which I have noted has an exact parallel in the German *Verschmelzung* (chapter 5). It is quite common that Wessén speaks of words melting together, and this appears to be as close as we get to a term for agglutination in his

syntactic relation is the one determined by the specific meaning of the different words belonging to the connection, but the type of expression is mechanised and automated through frequent use.

Following the pattern of this type of expression, new connections are formed through purely mechanical analogy where, because of the content of other words belonging to the relation, the specific meaning of words can only partly, if at all, be recalled. This means, right from the start, that the syntactic structure is that of the mechanised type of expression; due to content of the individual words the original syntactic relation is no longer possible.

This is how the semantic premisses for further grammaticalisation of the words are also created. To what extent this is realised depends on whether the word in its new purely formal meaning is proven useful in the morphological system, whether it can take on a specific function. If this is the case then loss of accent, reduction of word forms and such like may follow.

²¹⁹ Semantics in the meaning ‘the study of meaning’ has in the Swedish tradition sometimes been referred to as *semologi* (Wellander, 1968: 76).

works, since it always refers to two words cliticising and becoming more closely joined to one another in the form of one word.

One of the other examples of this that he mentions is the development of the Swedish word *båda* ‘both’ (Wessén, 1962: 103; 1968 [1995]: 113-114).²²⁰ This developed through the compounding of **ba-* (which originally had the same meaning as *båda* has now) and the pronoun *de* ‘they’. First, both parts were still inflected, but after some time it was only the second part that could take inflections. Today we might see this as indicating a reanalysis and a closer merger, and possibly also demorphologisation, or univerbation and decategorisation (cf. e.g. Hopper and Traugott, 1993). What Wessén says is that the two are “firmly joined” (“fast förenats”), indicating that there has been what we would call an ‘increase in the bonding’ between the two words. Another additional reanalysis is noted in the fact that “[s]ince *-s* was added to the genitive [...], the form *bäggia*, *bägge* without any ending [...] has started to be used as the base form [...]” (translation).^{221, 222} This shows that he has noted that people started to analyse the forms differently (subconsciously of course) - in other words a form of reanalysis in our eyes.

Wessén (1956: 53; 1962: 107; 1968 [1995]: 118) also mentions that the emphatic demonstrative pronoun *denne*, *detta*, *dess*, developed through the affixation of the particles *-si*, *-a* to the different forms of the demonstrative pronoun *sa*, *su*, *pat* (literally he speaks of the particle as having been added, “lagts till”) (cf. 5.5.4).²²³ It is interesting that since this demonstrative has merged with the particle,²²⁴ it has sometimes been reinforced by *här* ‘here’. And Wessén (1956: 54; 1965 [1992]: 59) says that because the stress was then usually on the adverb *här*, the pronoun was reduced in that combination and the new form *den/det här* ‘this’ appeared. This is a clear sign that Wessén has noted that phonetic reduction may sometimes accompany certain changes in meaning.

Still on the issue of demonstratives, it is well known today that demonstrative pronouns can develop into definite articles, and this was also remarked on by Wessén

²²⁰ The origin of this form was not mentioned in the first edition (Wessén 1941: 74).

²²¹ Original: “Sedan *-s* tillagts till genitiven [...], har den ändelselösa formen *bäggia*, *bägge* [...] börjat brukas som grundform [...]”

²²² *bäggia*, *bägge* were the old genitive forms: Nominative *baþi(r)*, *baþa(r)*, *baþi(n)/bäþi*; Accusative *baþa*, *baþa(r)*, *baþi(n)/bäþi*; Genitive *bäggia* (*þera*); Dative *baþum*. (Wessén 1968 [1995]: 113).

²²³ The forms are also listed in the first edition of the first volume, but the origin of these pronouns is not discussed (Wessén 1941: 76).

²²⁴ In (1965 [1992]: 92) he says that they had “sammansmält”. And he claims that they now form a “kompositum”, i.e. a ‘composite’.

(1941: 76-77; 1962: 107; 1968 [1995]: 118-119). He noted that the demonstratives *sa, su, þat* were occasionally used more or less as a preposed definite article, which he compares to the German definite article in *der Mann* ‘the man’. But the Scandinavian languages eventually developed a suffixed article instead which came from a different demonstrative pronoun, *hinn, hin, hit* (cf. also Wessén, 1939: 11; 1975 [1992]: 31). Also here he speaks of forms that have “melted together” (“*sammansmält*”), i.e. they merged into one word. In the first uses of this affixed article both the noun and the article were still inflected for case and number (as already mentioned in chapter 5), but this eventually stopped and instead only the article at the end was inflected, possibly indicating both what we would call *deategorialisation* and *increased bonding* today. However, Wessén does not discuss this change any further or try to categorise its type.

Furthermore, there are other pronouns that Wessén notes were originally compounds. But instead of looking at them all, we will move on to negations, which are of importance because they have been examples in the grammaticalisation literature. Wessén (1941: 79; 1962: 110; 1968 [1995]: 122) mentions the ON word *ängin* ‘nobody’ which he derives from **äinn-gi* ‘lit. someone/anyone-not’, where the second part is the negative particle. The first part derives from the numeral **äinn*, used indefinitely with the meaning of ‘someone’. But in a footnote Wessén tells us that *-gi* was not originally a negative particle. It was generalising or reinforcing, and the negation was expressed by the particle *ne* as most readers will recognise from the history of other Indo-European languages. But because this particle was unstressed it eventually disappeared and the ending *-gi* came to be seen as negative. At least that is the story according to Wessén (1941, 1962, 1968). The story is told slightly differently in Wessén (1956: 87; 1965 [1992]: 94) where he says that *-gi* came to be used **because** the negation was not emphatic and needed to be reinforced, which might however just be a different way of saying the same thing. We can compare this to *pas* in *ne... pas* which was also used as reinforcement originally. This sounds like a rather typical case of grammaticalisation, where the changes are always going on in a renewing spiral of change. As already mentioned, it also has some parallels to the French negations *ne... pas, ne... rien*, where especially in the first case (*ne... pas*) the negative particle *ne* has started to be dropped more and more in colloquial speech, and instead *pas* has been left to carry the meaning of the negation.

Some brief remarks in Wessén's writings can be compared to comments on phonological attrition in the recent grammaticalisation literature. For instance, Wessén discusses how verbs tend to be pronounced differently if they develop into auxiliaries, e.g. the verb *lata* 'sound' (Present Day Sw. *låta*) has a long *a* sound which is shortened when used as an auxiliary. He also notes that the reason for this is that as an auxiliary the verb is usually unstressed (Wessén, 1941: 49, 83; 1962: 64, 115; 1968 [1995]: 72, 127).

Wessén (1943: 12-13, 44-45; 1971 [1992]: 27-28, 68) has a brief discussion of derivational morphemes as a means of word formation, and he notes that many of the old derivations at some point in the history of the language "were reduced" or may even have disappeared through syncope, which led to a need for new derivational morphemes (cf *renewal*). According to Wessén, the new derivational morphemes appeared through the weakening of the last part of compounds until they became derivational endings, e.g. *tro-lig* (cf. Germ. *-lich*, Eng. *-ly*), *lång-sam* (cf Eng. *-some*), *vän-skap* (cf. Germ. *-schaft*, Eng. *-ship*), but also through borrowing.

In other cases new derivational means have developed when the second part of compounds has lost its autonomous meaning and through a mechanical usage become only a suffix. The original meaning has been bleached or disappeared completely. The starting point for such a development could have been a single word but also a group of words. On the border to being derivational endings are probably *-lös* in *sorglös*, *rådlös*, *hjärtlös* [...], *-full* in *tankfull*, *livfull* [...] ²²⁵ (Wessén, 1971 [1992]: 68) (translation) (cf. Wessén, 1943: 44)

Notable in the citation above is the fact that he talks about a mechanical usage of compounds leading to the development of the last part into a suffix, and that he speaks of the bleaching or disappearance of the original meaning of this part. But it is also important to note that all of his examples here are of derivational morphemes, and not inflectional endings.

There is no in depth treatment of the origin of inflectional endings in general in Wessén's *Svensk Språkhistoria* (= History of the Swedish language) (1941; 1943; 1956; 1958; 1962; 1965 [1992]; 1968 [1995]; 1971 [1992]) or in *De Nordiska Språken* (=The Nordic / Scandinavian Languages) (1939; 1975 [1992]). However, he

²²⁵ Original: "I andra fall har nya avledningsmedel uppkommit därigenom, att efterleden i sammansättningar har förlorat sin självständiga betydelse och genom ett mekaniskt bruk sjunkit ned till ett rent suffix. Den ursprungliga betydelsen har avbleknat eller helt försvunnit. Utgångspunkten för en sådan utveckling kan ha varit ett enda ord eller också en grupp av ord. På gränsen till avledningsändelse står väl *-lös* i *sorglös*, *rådlös*, *hjärtlös* [...], *-full* i *tankfull*, *livfull* [...]"

does mention that content words can develop into purely formal linguistic elements, and gives examples of some conjunctions which have arisen in this way, and it is likely that inflections would also have been seen as originating in this way (Wessén, 1965 [1992]: 275). Wessén (1975 [1992]: 19) also admits that at the time only a minute amount was known about the origin of inflectional endings.²²⁶ However, he claims that it seems most likely that they have developed out of particles and other short words which have been cliticised to the stem and then merged with this (“*sammansmält*”). In this way he illustrates what we would call a cline from particles / short words to clitics to suffixes, which we can compare to Hopper and Traugott's (1993) cline of grammaticality, for instance. He also says that some suffixes are likely to have been postpositions before, and others were probably more meaningless and were only added for “deictic or emphatic meaning”.

But Wessén (1943: 59; 1971 [1992]: 68) also realises that sometimes what happens is not that two words are joined and that one is weakened, but that sounds belonging to the stem of the word are separated from the stem and become seen as part of the derivational morpheme. After which, this new derivation is sometimes used in other words.

Norde (2001a: 245-246; 2002: 53-55) has mentioned that Swedish has developed a special, usually pejorative, derivation *-er*. Wessén likewise remarks on the fact that *-er* was originally a strong nominative masculine ending, but developed various new uses, one of which was the possibility of forming pejoratives, such as *slarver* (someone who is always messy and unorganised), *spjuver* (someone who makes jokes, teases and tricks you²²⁷). This kind of change can be compared to what Lass (1990) has called *exaptation*. Another example which has been mentioned by Norde (2002: 55-56) as an example of exaptation in Swedish is the so called berry suffix *-on*. This was also mentioned by Wellander (cf 6.4.2) and it is mentioned by Wessén (1943: 26; 1971 [1992]: 45-46) who claims that its origin is not quite clear. But since most of these words are mainly used in the plural Wessén believes it is likely that the use somehow originated in the plural forms of some neuter *n*-stems, and that it spread in the use of a derivational ending.²²⁸ This meant that some fruit

²²⁶ This is not mentioned in the first edition of *De Nordiska Språken* (Wessén 1939).

²²⁷ Nowadays this is no longer pejorative but is used e.g. of little children who are a bit naughty, but it is used sort of teasingly usually accompanied by a smile.

²²⁸ The idea that it could be seen as a derivational ending is not included in the first edition of the second volume (Wessén 1943).

and berry names which had not taken the ending added it by analogy with the other fruit and berry names. Originally the ending was only used in the plural, but it spread to become used both in the singular and the plural.

Another interesting change in the history of Swedish is the development of the pronoun *ni*, even though this is not an example of grammaticalisation, but rather only of reanalysis. According to Wessén this pronoun started to make an appearance around the middle of the seventeenth century. It was first only used in the plural but later developed into a form of polite address in the singular, which however disappeared during the 1960s. The form of the pronoun developed through what we might today call *reanalysis*, or *metanalysis*. Similarly Wessén (1962: 197; 1968 [1995]: 219) says that it was a change in the syllable boundary that happened when the verb and the pronoun were inverted, e.g.: *veten-I* > *veten-ni* > *vet-ni*, *vissten-I* > *vissten-ni* > *visste-ni*.²²⁹

Wessén does not only discuss the development of new affixes. In his discussion of changes of part of speech, he (Wessén 1943: 11; 1971 [1992]: 26) mentions that many of the Swedish conjunctions are derived from adverbs (*då* ‘then’, *sedan* ‘later’, *eller* ‘or’, *och* ‘and’), others are derived from pronouns (*ty* ‘because’, possibly *att* ‘that’ (cf. also 1956: 255; 1965 [1992]: 274-275, where he claims that this is the most likely origin of the conjunction ‘att’) or prepositions (*för* ‘because’, *efter* ‘after’). However, he does not go into **how** these changes occur, and does not make it clear whether there is a difference between these kinds of changes of part of speech, and conversions which we would nowadays usually simply see as a form of word formation which is most common in English. He does discuss *conversion* (Wessén, 1971 [1992]: 26), using that term also, and explains it as a free change of class. He claims that this type of change is highly familiar from English, so it seems as though he sees a difference between conversion and other changes of part of speech (such as those which can appear as part of the change which we now call *grammaticalisation*). However, in the first edition of the same volume of *Svensk Språkhistoria*, the section about *conversion* is missing (Wessén, 1943).

The issue of where prepositions come from is also brought up in Wessén (1956: 78; 1965 [1992]: 84), where he tells us that the *til* ‘to’, for instance, developed from a word meaning ‘goal’ which still lives on in German *Ziel*. Wessén (1956: 78;

²²⁹ I have not been able to find anything about this in the first edition of the first volume (Wessén 1941).

1965 [1992]: 84) noted that prepositions had become more and more popular during the Old Swedish period, at the same time as the case endings became less and less frequent. And he says a few words in general about where they could come from:

Such [new prepositions] have arisen from adverbs or adverbial expressions (case forms or prepositional expressions). **The adverbial meaning and use has always preceded the use as a purely formal word.** Several of our prepositions have developed from nominal forms.²³⁰ (Wessén, 1965 [1992]: 84, emphasis mine) (translation) (cf. 1956:78)

In some cases the preposition was originally the main word and the noun was its modifier [...]. In other cases both words probably modified the verb separately: only eventually did they become more closely connected to one another and melted together into one unified expression, where one of the parts has been markedly bleached: *a mote, gen, genum, bak, bland*. One can in our oldest texts often get a good look into the process, through which an adverbial expression loses its full meaning and sinks to become a grammatical formal word.²³¹ (Wessén, 1965 [1992]:86-87, italics mine) (translation) (cf. 1956:80)

Since we can clearly see in the older documents in Scandinavian languages, as well as many other Indo-European languages, that they at one time had more case inflections and that they then lost many of these and started to use prepositions instead, the origin of prepositions is highly likely to fascinate linguists.

In *De Nordiska Språken* (1975 [1992]: 17) Wessén discusses another one of the most common examples of grammaticalisation, namely the French future. He explains that this is an example of a periphrastic (“perifras”) tense form which has merged together (“sammanmält”) into a compound (“sammansättning”), and later continued its development into a “simplex” – a simple form. In the words of present-day grammaticalisationists it has gone through *univerbation*.²³² Moreover, he does not stop at explaining the French future in this way, but claims that both the Latin

²³⁰ Original: “Sådana [nya prepositioner] har uppstått av adverb eller adverbiella uttryck (kasusformer eller prepositionsuttryck). **Den adverbiella betydelsen och användningen har alltid gått före bruket som rent formord.** Åtskilliga av våra prepositioner har framgått ur substantiviska former.” (emphasis mine)

²³¹ Original: “I vissa fall har prepositionen ursprungligen varit huvudord och substantivet dess bestämning [...]. I andra fall har de båda orden troligen vart för sig varit bestämning till verbet: först efterhand trädde de i närmare förbindelse med varandra och smälte samman till ett enhetligt uttryck, där det ena ledet har fått en starkt avbleknad betydelse: *a mote, gen, genum, bak, bland*. Man kan i våra äldsta texter ofta få en god inblick i det förlopp, varigenom ett adverbiellt uttryck förlorar sin fulla betydelse och sjunker ned till ett grammatiskt formord.”

²³² This is not mentioned in the first edition of *De Nordiska Språken* (Wessén 1939).

future (*amabo*) and the Latin imperfect (*amabam*) are compounds which consist partly of the verb for ‘to be’, Latin *fui* (Wessén, 1975 [1992]: 17).²³³

Wessén also inquires into the origin of the weak preterite ending in Germanic languages (1975 [1992]: 17-18).²³⁴ He believes that this ending stems from a periphrastic usage of the predecessor to the English verb *do*, German verb *tun*, which is to have existed in Proto-Germanic times.

One would suppose then that ON. 1st sg. **kallō-ðeðo* ‘I did calling’ > **kallōðō*, from which Icel. *kallaða*, 1st plural **kallō-ðēðum* (got. **kallodedum*) > **kallōðum*, [...]. Proto-Germanic **kallō-*, **vali-*, **domi-*, [...] (to the verbs *kalla* [‘call’], *välja* [‘choose’], *döma* [‘judge’], [...]) is a compositional form, to which the auxiliary verb has joined itself through cliticisation. The more the two parts melted into one unit, the more the second part lost of its meaning from the auxiliary verbs and the more it felt like a flexional element.

As far as the question of details is concerned the Germanic dental preterite offers many difficult problems, where different opinions stand sharply against one another.²³⁵ (Wessén, 1975 [1992]: 18)²³⁶ (translation TL)

To sum up, we can say that Wessén believed that derivational morphemes, inflectional morphemes and free grammatical words could all develop out of lexical items. However, there were also other ways of creating new endings in his eyes. They could be formed through the addition to an affix of sounds which were originally part of the stem. In other words a change in the morphemic boundary. New derivational morphemes could also appear through the extension of an existing ending into a new meaning and new words. Wessén does not really use any term for this or for a concept similar to grammaticalisation, although *sammansmältning* appears regularly for a process similar to agglutination and univerbation. Something which speaks for it being seen as a term is the fact that the same expression was used for these phenomena in German linguistics during the nineteenth century. Notably, however, *sammansmältning* is used not only for cases which we would today

²³³ This is not mentioned in the first edition of *De Nordiska Språken* (Wessén 1939).

²³⁴ This is not mentioned in the first edition of *De Nordiska Språken* (Wessén 1939).

²³⁵ Original: “Man skulle då förmoda urn. 1 sg. **kallō-ðeðo* ‘jag gjorde kallande’ > **kallōðō*, varav isl. *kallaða*, 1 pl. **kallō-ðēðum* (got. **kallodedum*) > **kallōðum*, [...]. Urgermanskt **kallō-*, **vali-*, **domi-*, [...] (till verben *kalla*, *välja*, *döma*, [...]) är en kompositionsform, vartill hjälpverbet enklitiskt har anslutit sig. Ju mer de båda beståndsdelarna sammansmälte till en enhet, desto mer förlorade andra leden sin betydelse av hjälpverben och kändes som ett flexiviskt element.

I fråga om detaljer erbjuder det germanska dentalpreteritum många svåra problem, där olika åsikter står skarpt emot varandra.”

²³⁶ This is not mentioned in the first edition of *De Nordiska Språken* (Wessén 1939).

recognise as cases of grammaticalisation, but also for cases of word formation, such as what Wessén calls ‘unreal’ / ‘false’ compounds where the first part is a case form usually a genitive (Icel. *landa-merki*, *sólar-hiti*). This, he says, was originally a “juxtaposition”, but the two words merged into one unit (Wessén, 1975 [1992]: 22-23).²³⁷

The discussions of this topic are scattered throughout Wessén’s publications and not too easy to find. Still, they are most definitely there, including some of the common grammaticalisation examples such as the Scandinavian passive, the definite article and the French future.

6.4.4 Alf Sommerfelt (1892-1965)

Alf Sommerfelt (1892-1965) was the first Scandinavian professor in general linguistics. He had received his education abroad, primarily in Paris (Hovdhaugen, 2000: 330-331), but came back to Scandinavia (Norway) to work. In a paper from 1921 Sommerfelt discusses Meillet’s article about grammaticalisation (1921 [1962]: 15-16). He notes that Meillet discusses the simplification of the nominal case systems in many Indo-European languages, but also the tendency to substitute periphrastic forms for the former simple forms (Sommerfelt, 1921 [1962]: 16). According to Sommerfelt it is partly usage that makes language change:

C’est à cause de l’usure, par exemple, que les désinences se vident de leur sens, ce qui détermine la création d’autres procédés plus expressifs. Et c’est par suite de l’usure qu’un mot peut perdre son autonomie et être réduit à devenir un outil grammatical (voir Meillet, *op. cit.* [1912 [1921]], p. 130 et suiv.)²³⁸ (Sommerfelt, 1921 [1962]: 16-17)

It is interesting to see that Sommerfelt here also talks of loss of autonomy and reduction into a grammatical element similar to Meillet.

Meillet had much influence on Sommerfelt who was deeply impressed by him (Sommerfelt, 1937 [1962]). He was one of his teachers in Paris and it was Meillet’s special type of Neogrammarian ideas that Sommerfelt made use of in his work on

²³⁷ This is not mentioned in the first edition of *De Nordiska Språken* (Wessén 1939).

²³⁸ Translation: It is because of usage, for instance, that the inflectional morphemes are emptied of their meaning, that which determines the creation of other more expressive processes. And it is therefore through usage that a word can lose its autonomy and become reduced into a grammatical tool (see Meillet, *op. cit.* [1912 [1921]], p. 130f.)

historical linguistics. But he was not only interested in historical linguistics, he was also the one who introduced structural linguistics in Norway (Hovdhaugen et al., 2000: 330), although with few followers.²³⁹

In one of Sommerfelt's other publications we can also read the odd comment on language changes that we would now call grammaticalisation. For instance, he notes that the definite article which exists in some European languages often arose from a demonstrative pronoun ("et påpekende pronomen") (Sommerfelt, 1948: 78). So, clearly he considered both the development of grammatical affixes and function words, and it seems as though he might have seen them as part of the same process.

6.4.5 (Karl) Birger Bjerre (1902-1993)

Birger Bjerre (1902-1993) published a major work on Swedish conjunctions in two volumes (1935; 1938). In the first volume we find references to Meillet's paper on the grammaticalisation of conjunctions (1915-1916 [1921]), and also to some other works which Meillet published, e.g. the volume where the 1915 paper was reprinted which also includes the 1912 paper which has been seen as having introduced the term *grammaticalisation* into linguistics (Meillet, 1921). But there is no direct reference to the 1912 paper, and Bjerre does not use the term *grammaticalisation*, instead he speaks of the conjunctions as being "mechanicised" (see e.g. Bjerre, 1935: 13).

As an example of how we are sometimes forced to renew our language, something which Bjerre believes has also happened in the history of the conjunctions, he mentions the French negations (Bjerre, 1935: 16), a common example of grammaticalisation. He also speaks of the bleaching which in his eyes leads to a need for the conjunctions to renew themselves.²⁴⁰ In addition, he mentions that linguists have noted that due to the high frequency with which these words are used, there may be phonetic reductions involved in this need for a new form (1935: 17), a conclusion which he later relates to Meillet's paper on conjunctions (1915-1916 [1921]) (Bjerre, 1935: 19).

But where do conjunctions come from? Bjerre (1935: 20) says that they can be of several different origins: "pronouns, adverbs, prepositions and ancient particles",

²³⁹ Thank you to Andrew Linn for telling me more about the reception of Sommerfelt's introduction of structural linguistics in Norway.

²⁴⁰ "betydelsen förbleknande" = the bleaching of meaning (1935: 17)

they can also be “nominal caseforms” or certain “verbal forms”. However, the most common of the conjunctions usually cannot be related to their original meaning by most of the language speakers, because they have proceeded so far in their “mechanisation as a grammatical form element”²⁴¹ (1935: 20). This term, *mechanisation*, certainly refers to a process which is part of grammaticalisation. However, it does not appear to mean quite the same as *grammaticalisation* in the meaning of either (1) development of new grammatical means of expression, or (2) the development of a lexical element into a grammatical element. “Mechanisation” seems closer to “conventionalisation”. A word can conventionalise as a grammatical element, but it could also be conventionalised as something else, e.g. as part of a set phrase. Bjerre’s explanation of what this process is exactly, reads as follows:

In a narrow sense, mechanisation means that every single word or expression, which is often used, will lose its freshness and fullness of expression precisely through its usage and develop into an abstract linguistic sign with bleached meaning.²⁴² (Bjerre, 1935: 84) (translation)

This citation mentions the bleaching of the element due to its frequency of use, and that it tends to develop into a more abstract linguistic item. This latter point was long an accepted ‘fact’ among grammaticalisationists, but it has been questioned recently in connection to grammaticalisation (e.g. by Meyerhoff and Niedzielski, 1998), who are not certain whether grammaticalisation always involves a move from concrete to abstract. A few pages later Bjerre goes on to say that:

... the mechanisation process, which from this point of view means that the metaphorical inner linguistic form thanks to the frequent use of the word in similar contexts is no longer necessary as an association link, instead, conversely, sound complex and meaning are eventually directly connected, in other words that the metaphorical inner linguistic form itself has come to be the actual meaning. When for instance the German conjunctions *während*, *weil* in modern times probably are no longer associated with the verb *währen*, and the noun *Weile* respectively, then the bleaching of the metaphorical inner linguistic form means not merely a change of meaning but also a change of part of speech, since the metaphorical inner linguistic form which eventually can only be reproduced historically, led to the change of participle or noun

²⁴¹ Original: “... mekanisering till grammatiska formalelement...”

²⁴² Original: “I inskränkt mening innebär mekaniseringen, att varje enstaka ord eller uttryck, som ofta brukas, just genom bruket förlorar sin omedelbara friskhet och uttrycksfullhet och övergår till ett abstrakt språkligt tecken med förbleknad betydelse.”

into the part of speech of conjunctions (Funke, *Innere Sprachform* p. 71).²⁴³
(Bjerre, 1935: 85-86) (translation)

Still, when Bjerre mentions the *mechanisation* of new prepositions, it suddenly sounds as though *mechanisation* is the same as grammaticalisation. He says that the word has often been used together with different nouns, and it has been bleached so that only its relational sense remains, and this means that it has been *mechanised* (Bjerre, 1935: 87). However, this could still be read with a meaning of conventionalisation. Some of the examples of prepositions that Bjerre mentions are Fr. *chez* which comes from a word for ‘house’, just like the Swedish preposition *hos* ‘at (someone’s place)’, which he also mentions. Like Wessén he also brings up Swedish *till* ‘to’ which derives from a word for ‘goal’ (1935: 87).

Bjerre claims that this change, whereby a linguistic element becomes a grammatical element is gradual, an important characteristic of what we now call *grammaticalisation*. He also points to the fact that often the final stage of the process where the element would have a purely syntactic relational meaning, is in fact never reached (1935: 87-88). Similarly, grammaticalisationists today often speak of grammaticalisation as proceeding along a cline or continuum, but they claim that it need not run the full course, instead it often stops somewhere along that cline.

The conjunctions Sw. *att*, Eng. *that* are noted to have been derived from the demonstrative pronoun which acted as object of the clause now preceding the subordinate clause (Bjerre, 1935: 70). Interestingly, Bjerre also notes that temporal meanings of words often stem from the locative, or space related, meanings (1935:103), similar comments are often seen in the grammaticalisation literature (cf. e.g. Heine et al., 1991a). However, he also observes that they do not have to be derived from locative meanings but can in fact be just as original as the locative senses (1935: 107), thereby clearly not subscribing to a strong unidirectionality hypothesis of the sense development of prepositions (cf. chapter 3).

²⁴³ Original: “... mekaniseringsprocessen, vilken från denna synpunkt innebär, att den figurliga inre språkformen tack vare ordets flitiga bruk i likartat sammanhang icke längre varit nödvändig som associationslänk utan tvärtom ljudkomplex och betydelse så småningom förknippats direkt, m.a.o. att den figurliga inre språkformen själv kommit att utgöra den egentliga betydelsen. Då exempelvis de tyska konjunktionerna *während*, *weil* i modern tid sannolikt ej längre associeras med verbet *währen*, resp. subst. *Weile*, innebär förbleknandet av den figurliga inre språkformen icke blott en betydelseövergång utan även en ordklassförskjutning, i det att den figurliga inre språkformen, vilken sedemera kan reproduceras blott på historisk väg, förmedlat överglidningen av particip eller substantiv till ordklassen konjunktioner (Funke, *Innere Sprachform* s. 71).”

Bjerre's process of *mechanisation* has many parallels to grammaticalisation – it is associated with highly frequent usage, semantic bleaching, phonological reduction, change of part of speech and the words tend to become more abstract. However, the way he uses the term it seems closer to a sense of conventionalisation. This is however also **involved** in grammaticalisation, but since it could cover expressions which become idiomatic and fossilised which would not normally classify as grammaticalisation, it is not identical to what we would call *grammaticalisation*. It therefore seems more as though he has pinned down and explained a process **related** to grammaticalisation, rather than grammaticalisation itself.

Linguists	Grammaticalisation?	Examples
Otto Jespersen	<i>coalescence</i> (<i>agglutination</i>) <i>secretion</i>	Romance future (coalescence) Scandinavian passive (coalescence) German <i>-en, -er</i> plural (secretion)
Erik Wellander	<i>grammaticalisation</i>	verb + particle = new meaning <i>Lämmer</i> <i>hus</i> 'house' > <i>hos</i> 'at (somebody's place)' Fr. <i>-ment</i> <i>kvinnlig riksdagsman</i> 'female MP'
Elias Wessén	<i>sammansmältning</i> 'the process of melting into one' <i>conversion</i>	Scandinavian passive / reflexive <i>-s</i> < <i>sik / ser</i> (refl. pronoun) <i>*ba-</i> 'both' + <i>de</i> 'they' > <i>båda</i> 'both' demonstrative pronoun > definite article particles / short words > inflections nominative inflection > pejorative derivational suffix – <i>er</i> adverbs > conjunctions (<i>conversion?</i>) <i>till</i> 'to' < Noun meaning 'goal' French future
Alf Sommerfelt	no term	demonstrative pronoun > definite article
Birger Bjerre	<i>mekanisering</i> 'mechanisation'	conjunctions < pronouns / adverbs / prepositions / particles / nouns / verbs <i>hos</i> 'at (someone's place)' < <i>hus</i> 'house'

Table 6.4.2: Scandinavian linguists' views on the origin of grammatical forms (1900-1970).

6.5 Scholars from Other Regions

6.5.1 Jerzy Kuryłowicz (1895-1978)

Since Jerzy Kuryłowicz's (1895-1978) definition of grammaticalisation is often cited in grammaticalisation studies, it is important that we take a look at this Polish scholar. His definition has also often been used to show that the unidirectionality hypothesis is in fact no hypothesis, but instead it is intrinsic to grammaticalisation.

Kuryłowicz claims that grammaticalisation is:

... the increase of the range of a morpheme advancing from a lexical to a grammatical or from a less grammatical to a more grammatical status" (1965 [1975]: 52).²⁴⁴

His definition clearly only mentions one direction, i.e. movements from lexical to grammatical and from less grammatical to more grammatical. However, the latter part is only unidirectional from a functional perspective and could be interpreted more widely. It could be understood to allow changes from submorphemes to grammatical elements, for instance.²⁴⁵ In addition, a definition of this kind is of course also reliant on the definer's understanding of grammatical and lexical – but also on the reader's understanding of the same.

Kuryłowicz (1965 [1975]) actually discusses a reverse process as compared to grammaticalisation, a process which he calls *lexicalisation*. This might be seen as proof that he did not believe that grammaticalisation was unidirectional, however before we can be sure that is the case we need to be clear about what a counterexample to unidirectionality would be. As we saw above (3.4.2) different linguists have had quite different ideas of what degrammaticalisation as the opposite direction to grammaticalisation would have to involve.

Kuryłowicz clearly says that there is a process called *grammaticalisation* and there is a process which goes in the other direction, called *lexicalisation*. It seems as though he may therefore have viewed lexicalisation as distinct from the process of grammaticalisation, and therefore it is still possible that he saw **grammaticalisation**

²⁴⁴ cf. Campbell 1991 note 1.

²⁴⁵ cf. Campbell 1991 note 1.

as unidirectional. However, he clearly did not believe that processes in the other direction were impossible.

The examples of lexicalisation in Kuryłowicz's paper (1965 [1975]) include inflections becoming derivations – which would be a move from more grammatical to less grammatical,²⁴⁶ and possibly also from grammar into the lexicon depending on where we think that derivations and inflections should be categorised. There are also different examples, which involve freezing of certain constructions and could therefore be seen as additions to the lexicon, e.g. *be*-perfects. If we compare these examples to the examples of grammaticalisation that he uses, we find ordinary perfects, futures, and case forms – periphrastic forms which have moved towards becoming merged into one word.

It is a fact then that Kuryłowicz does not **explicitly** say that grammaticalisation could not involve part of a morpheme, for instance, gaining a grammatical function. Instead it is possible that such an example could come under less grammatical > more grammatical in his definition.²⁴⁷ And in general, such a view would be classified as counter-unidirectionality nowadays.

6.5.2 Viktor Maksimovich Žirmunskij (1891-1971)

Another scholar who has occasionally been mentioned in histories of grammaticalisation is Viktor Maksimovich Žirmunskij (1891-1971). Žirmunskij (1966 [1961]) uses the term *grammaticalisation* (in translation, but the Russian term *grammatisaziya* appears to be a similar formation (1961)). His examples include perfects and futures in German, French and Russian, which would be classed as grammaticalisation today.

Žirmunskij never explicitly says that the opposite direction is not possible, movements from functional to lexical are also not mentioned. However, there is also nothing to indicate that he believes that all functional elements should have sprung from lexical items. His paper primarily contains descriptions of combinations of words that become compounds, or combinations of words that become set expressions. Some of these might be classified as lexicalisation, rather than as grammaticalisation as Žirmunskij suggests for some kinds of changes. But clearly

²⁴⁶ At least according to some, although Heine appears to see it as the opposite.

²⁴⁷ As Campbell 1991 also noted in his discussion of the particles *es*, *ep* in Estonian.

Žirmunskij does not see these two processes as opposites, but rather as parallel processes (cf. Lindström, forth), and the differences between his sense of grammaticalisation and Lehmann's sense, for instance (seeing as Lehmann, 1982 [1995] appears to have been the one who brought attention to this paper), are probably quite a lot bigger than presumed by Lehmann.

6.5.3 *Émile Benveniste (1902-1976)*

Émile Benveniste (1902-1976) was a student under Meillet, who followed in Meillet's foot steps and became interested in both historical and general linguistics. Brief histories on grammaticalisation sometimes mention that Benveniste touches on grammaticalisation in at least one of his articles. However, he does not use the term *grammaticalisation* (Benveniste 1968), instead, Benveniste speaks of *mutation* (Fr. *transformation*) and *auxiliation* (Fr. *auxiliation*) (1968, 1974). He defines mutation as:

... a diachronic process observable in linguistic categories by distinguishing two types of mutations, inherently different, with different causes and effects in the evolution of languages: ... (Benveniste, 1968: 85)

... procès diachronique étudié dans les catégories linguistiques, en distinguant deux espèces de transformation, différentes par leur nature, qui ont dans le développement des langues des causes et des effets distincts: ... (Benveniste, 1974: 126)

The two types of mutation he distinguishes are *innovating mutations* and *conservative mutations*. The first “result[s] from the loss or emergence of formal classes, processes which thus modify the total stock of available categories.” (1968: 85).²⁴⁸ The second type, *conservative mutations*, “serve to replace a morphemic category by a periphrastic category with the same function” (1968: 86).²⁴⁹

The second type of change that he speaks of which has also been related to grammaticalisation, is auxiliation.

The mutations of special interest to us in this context are those which are both productive of and realized by a new class of signs, to be known as signs of

²⁴⁸ Original: “produites par la disparition ou par l'apparition de classes formelles, modifiant ainsi l'effectif des catégories vivantes.” (Benveniste, 1974: 126)

²⁴⁹ Original: “consistent à remplacer une catégorie morphémique par une catégorie périphrastique dans la même fonction, ...” (Benveniste, 1974: 127)

auxiliation. To illustrate this process of “auxiliation” we may select the periphrastic development of two verbal categories, the perfectum and the future, in the Romance domain. [...]

The formal characteristic of this mutation is its operation through the rise of a “syntagm,” which stands as its essential condition, whatever the further course taken by this syntagm (kept separate in the perfectum, welded into a unit in the future). The auxiliation syntagm may be defined as the alliance of an inflected auxiliary with an uninflected element, the “auxiliate.” To these two components we must add a third, which consists in the coalescence of the two, a combination productive of a new shape, distinct from either component, and a new function as well. (Benveniste, 1968: 86)²⁵⁰

With regards to auxiliation, Benveniste (1968: 93, 1974: 135) also notes that the verb that is reduced is generally a “semantically very broad”²⁵¹ verb, which should be compared to the more recent comments about it being the more general verbs (i.e. *go* rather than *walk, wander, stroll*) that tend to be grammaticalised as auxiliaries.

His examples of mutation include some of the prototypical examples of grammaticalisation – for instance, the Romance perfect and future, the definite article.

Innovating mutations	Conservative mutations
partial / complete loss of gender	morphological (synthetic) comparative > adverb + adjective
reduction of number distinctions	case endings > preposition + noun
reduction of “systems of nominal classes”	Romance perfect (<i>auxiliation</i>)
development of the definite article	Romance future (<i>auxiliation</i>)
development of new types of adverbs: Eng. <i>-ly</i> , Fr. <i>-ment</i>	Greek future (<i>auxiliation</i>)
	Sogdian (East Iranian dialect) future (<i>auxiliation</i>)

Table 6.5.1: Benveniste’s examples of innovating and conservative mutations

²⁵⁰ Original: “Les transformations que nous considérons plus spécialement sont celles qui à la fois produisent une nouvelle classe de signes, qu’on pourrait appeler les signes d’auxiliation, et qui sont réalisées corrélativement par ces formes d’auxiliation.

Nous envisageons spécialement ce processus d’auxiliation dans le développement périphrastique de deux catégories verbales, le parfait et le futur, sur le domaine roman. [...]

La caractéristique formelle de cette transformation est donc qu’elle s’opère par la création d’un syntagme, qui en est la condition fondamentale, quel que soit le sort ultérieur de ce syntagme (maintenu distinct dans le parfait, soudé en une unité dans le futur).

Le syntagme d’auxiliation peut être décrit comme l’association d’un auxiliaire fléchi et d’un auxilié non-fléchi. A ces deux éléments nous en ajoutons un troisième, qui réside dans la combinaison des deux, condition produisant une forme nouvelle.” (Benveniste, 1974: 127-128)

²⁵¹ Original: “sens très générale” (Benveniste, 1974: 135)

Clearly both mutation and auxiliation resembles grammaticalisation. However, neither is identical to what we would call *grammaticalisation*. Benveniste's concept of mutation is wider. It includes the reduction of certain categories in a language, e.g. number distinctions or gender, and this does not seem like something we would call *grammaticalisation*. Mutation appears to focus on the whole language system and the fact that when something changes in a language the whole system is affected – in true Saussurean style. The grammaticalisation is really only one possible part of this change, e.g. auxiliation which is a type of conservative mutation, according to Benveniste. Auxiliation is a change from synthetic to analytic, and maybe also back to synthetic. But since this focuses only on auxiliary verbs it obviously does not cover our whole concept of grammaticalisation.

6.6 Summary and Conclusions

This chapter has shown how ideas of the development of grammatical elements were occasionally discussed in the early twentieth century. However, from an international perspective there do not seem to have been that many works that treated grammaticalisation or agglutination. So, it seems that the topic may have experienced a slight decrease in interest during this period. I have however found the odd person who has looked at similar things, such as Waley who had noted the grammaticalisation of auxiliaries, Ball who discussed the development of the Germanic preterite and the revival of the old theory for this, and also Burrow who recognised that both agglutination and adaptation had taken place in Sanskrit, although he does not seem to have used the term *agglutination*.

Interestingly, I have found more people in Scandinavia who showed an interest in this area of research at this time, than elsewhere. This is quite possibly due to a stronger continued interest in comparative historical linguistics in Scandinavia (cf. Hovdhaugen et al., 2000: 306):

... until 1965, most Nordic linguists worked within the framework of Neogrammarian historical-comparative linguistics. The Nordic scholars, their way of thinking, and their approaches to linguistic research were, for the most part, little influenced by the advent of the new structuralist approach or other theories [...]. Most of these innovations did not affect mainstream Nordic linguistics with full force until after 1965.

Still, Scandinavian linguists were also much involved in the development of linguistics in the first half of the twentieth century according to Hovdhaugen et al. (2000: 304-305).

It could be that because it is a smaller research community the distinction between modern language, classical languages and linguistics took longer to develop than in the States, for instance. The first professor of general linguistics in Scandinavia was Sommerfelt in Oslo (Hovdhaugen et al., 2000: 306-307, 310), long before the other universities in Scandinavia²⁵² were to follow:

Oslo	1931
Trondheim	1964
(Helsinki	1966)
Stockholm	1967
Lund	1969
Odense	1969
Umeå	1969
Copenhagen	1976
(Turku	1978)
Gothenburg	1983
Bergen	1983
Tromsø	1984
(Joensuu	1991)
(Hovdhaugen et al., 2000: 307)	

The state of linguistics as opposed to modern languages and classical languages, may have been similar in Britain. We know for instance that John Rupert Firth (1890-1960)²⁵³ was chosen for the first chair in general linguistics in 1944 (see e.g. Allen, 2002: 18; Leech, 2002: 156), which suggests that the progress of general linguistics in Britain might have shown some similarities to Scandinavia at the time. Perhaps then we could expect more work from within the historical-comparative framework, where grammaticalisation could be attested more. What we see is that the uses of the terms *grammaticalise* and *grammaticalisation* have been found primarily in work by linguists in Britain who concentrated on French, or Romance languages. Unfortunately, I have not had the chance to go through that much of the work that was done on modern languages and classical languages in Britain during

²⁵² In the list below I also include the Finnish universities of Helsinki, Turku and Joensuu since Finland has very close connections to Scandinavia.

²⁵³ The reason that J. R. Firth has not been treated above is that I have not been able to find any publications by him that treat anything close to grammaticalisation. (A detailed look at his unpublished work could however prove that he did have such a concept after all.)

this time, but have only skimmed all forms of linguistics by looking in the *Transactions of the Philological Society*. It seems the Philological Society should be rather representative since it was the focus for philological and linguistic research in Britain for a long time. The Philological Society was most popular between the 1840s and the 1870s, however its popularity then appears to have increased again between the 1930s and 1960s (Fiona Marshall (PhD Student, University of Sheffield), p.c.).²⁵⁴

The United States are rather unrepresented in this chapter. This may be partly due to where I have looked for pieces on grammaticalisation in American writings. However, as I said above (6.3), it is also likely to be at least partly due to the synchronic focus in linguistics in the United States for most of this period.

A comparison of the terminology that was used shows that the most common ‘term’ in older Swedish works on something similar to grammaticalisation appears to be *sammansmältning*. This is used by Tegnér in the nineteenth century, Wessén in the early to late mid-twentieth century, and it also occurs sometimes in more modern work on grammaticalisation, e.g. Ekberg (1993a, cf. chapter 7). It seems that this term was probably due to German influence, since *Verschmelzung* was used in Germany during the nineteenth and early twentieth centuries (see chapter 5).

Another term used in Scandinavian writings is Bjerre’s term *mekanisering* ‘mechanisation’, which as a concept seems wider than both agglutination and grammaticalisation in that it could easily include all forms of conventionalisation or routinisation of language. Furthermore, such a reading also appears to fit most of his examples, even though some of them are also clearly examples of grammaticalisation.

There was only one of the Scandinavian linguists mentioned who used the term *grammaticalisation* (in its Swedish form *grammatikalisering*, and its German form *Grammatikalisierung*) and that was Wellander. From what he says it would seem that he got this term from Humboldt, which however seems doubtful on closer inspection since in the only text by Humboldt that he refers to, Humboldt makes no use of it. But Wellander clearly relies a lot on Humboldt’s work and he has adopted the concept of a process whereby autonomous words develop into grammatical devices from Humboldt.

²⁵⁴ Ms Fiona Marshall has written her MA thesis about the Philological Society during the 19th century and is now researching British linguistic societies also for her PhD.

In Britain the early twentieth century shows a usage of terms such as *composition*, whereas linguists in the States hardly touch on subjects like grammaticalisation at that time. They also do not appear to have been using any specific term for the concept we now usually refer to as *grammaticalisation*.

As I have mentioned above, the term *grammaticalise / grammaticalisation* has been listed in the OED with reference to a few publications between the 1930s and the 1960s, however it seems that most of these actually carry a somewhat different meaning of *grammaticalisation* than what most linguists would usually have today. However, in a few cases the sense seems quite close to Meillet's sense, in that they notice the grammaticalisation of word order. In others they might be closest to conventionalisation, in others still they are most like a description of a language type.

This chapter and the previous chapter (chapter 5) has shown that there clearly was a concept close to grammaticalisation in Britain, (the United States), Germany and Scandinavia before the 1970s, however the term *grammaticalisation* was long not used in the same sense as today. Instead there were many other terms floating around for various aspects of grammaticalisation, mainly for the processes of cliticisation and agglutination, but also for conventionalisation which may be involved in grammaticalisation. So far I have not found any terms for the whole phenomenon before *grammaticalisation* became more popular again at the end of the twentieth century.

The only two linguists discussed in this chapter who definitely refer to Meillet's work are Jespersen (very briefly) and Bjerre. Meillet did not actually write much on the subject of grammaticalisation himself, and he did not find it worth commenting on Jespersen's or Vendryes's treatments of grammaticalisation in his review (Meillet, 1922). Perhaps it was simply a matter of general knowledge, which everyone was well aware of and considered in their work, but did not usually pay any special attention to? Perhaps they saw no need to scream and shout about this concept and phenomenon. Jespersen claims that he has observed that linguists at the time when he was writing (1922) had more or less stopped talking of agglutination theory, or were at least more careful in their claims of where endings had come from.

It is, of course, fascinating that Jespersen (esp. 1922) who treats grammaticalisation in more depth than Meillet ever did and also in English rather than in French, has not become more widely known to linguists interested in grammaticalisation. This has not even been picked up in the recent upsurge in

interest in grammaticalisation and in the history of grammaticalisation, apart from very briefly by Lehmann (1982 [1995]) and Heine et al (1991a) as noted above. But the main reason for this is likely to be the fact that Meillet happened to use the same term as that which came to be used again after the revival in the 1970s. This term had been used occasionally in the interim, however not particularly often and it does not seem as though the term or the concept was borrowed from anyone when it was (re-)introduced in the 1970s by Givón and others.

Meillet (1912) seemed to claim that grammaticalisation was taking off again, but ten years later it seems that Jespersen was correct in the claim that people were moving away from a unidirectional view, or tended to leave the subject of where grammatical formatives originate unexplored. There are not many texts that appear after this on the topic of the development of new grammatical elements, before the 'revival' in the 1970s. It is not until the late 1960s and more and more since the 1970s, that this starts to become a popular subject yet again. As yet I have hardly found any treatments of grammaticalisation between the early 1920s and the presumed 'revival' of the topic in the 1970s. But there are a few.

As I mentioned in section 6.5, there are some publications from the late 1960s, e.g. by Jerzy Kuryłowicz, who uses the term *grammaticalization*, by Émile Benveniste who speaks of *mutation* and *auxiliation* but clearly has a concept of grammaticalisation similar to ours. *Mutation* appears closer to our concept of grammaticalisation, however *auxiliation* is also grammaticalisation, only a specific form of grammaticalisation (cf. Hopper and Traugott, 1993: 25 for a somewhat different view). Viktor Maksimovic Žirmunskij wrote a paper in Russian in the early 1960s that was translated into English a few years later. In the translation (Žirmunskij, 1966 [1961]) the term *grammaticalisation* is used, and he appears to have had a concept similar to grammaticalisation.

There were a few others who mentioned grammaticalisation fairly early, as noted by Lehmann (1982 [1995]), Hopper and Traugott (1993) and the Oxford English Dictionary (online), e.g. Carl Meinhof (1936), Calvert Watkins (1962 (publ. 1964)) (1964), Roman Jakobson (1971 [1959]), John Lyons (1968), John Orr / Jorgu Jordan (1937), Josef Vachek (1961), Leonard Priestley (1955) and George Cardona (1965). But sometimes the term was used with a meaning different from that which the term normally has today (see further chapter 3 and section 6.0).

7. THE REVIVAL? (1970 – 2002)

7.1 The Corner Stones in Modern Grammaticalisation Theory

As I have mentioned above the 1970s have come to be seen as a period of revival for grammaticalisation studies (cf. Bybee et al., 1994; Lehmann, 1982 [1995]). In this chapter I shall take a closer look at what happened in the last decades of the twentieth century. I shall discuss where the concept and term *grammaticalisation* came from when they first started to reemerge, and how the concept has developed since then in the three main regions that I am primarily interested in (Britain, Scandinavia and the United States) briefly touching on the situation in Germany. This will also lead up to some conclusions hopefully on what grammaticalisation is today.

The year 1971 – “Today’s morphology is yesterday’s syntax” – has sometimes been seen as the year when grammaticalisation as a concept was awoken after a period of hibernation (cf. Hopper and Traugott, 1993: 25; Lehmann, 1982 [1995]: 6-8). Lehmann (1982 [1995]) and Hopper and Traugott (1993: 24-25) have recognised that grammaticalisation seems to have carried on being studied among the linguists studying the history of Indo-European languages and comparative linguistics between 1912 and the 1970s. But in 1971 Talmy Givón (1936-) brought it out of its deep sleep and made use of it again, without showing any knowledge of the history

of this concept (cf. Lehmann, 1982 [1995]). However, he did not use the term *grammaticalisation* then. In fact, he did not call it anything at all.

The term *grammaticalisation* seems to have ‘come back’ (if it ever went away that is) in the mid- to late 1970s. But was it borrowed from Meillet? Or was it borrowed from the Indo-European scholars who had carried on his tradition (e.g. Jerzy Kuryłowicz (1895-1978))? Or was it coined again?

It is clear that the popularity of grammaticalisation has had its ups and downs, but I believe that it is possible that vague concepts similar to grammaticalisation may have been around since we realised that languages change and develop. I believe that *grammaticalisation* (in the 1970s) was not so much borrowed from Meillet and/or his followers, as reinvented or independently coined. This is a conclusion which comes fairly naturally if we consider the existence of terms such as *lexicalisation*, and later also *syntacticisation* and *morphologisation*.

It seems to me that the excellent century-old pedigree of *grammaticalisation* was actually invented in the early 1980s when the history of this ‘new’ process in typology, diachronic functional grammar and African linguistics was first studied by Christian Lehmann (1982 [1995]). Although I admit that Lehmann was not the first to suggest that Meillet had coined the term, this had been done at least once before, since we find such an assertion in a paper by Vincent two years earlier (Vincent, 1980).

Hopper (1996: 219) discusses the renewed interest in grammaticalisation in the 1970s. He puts this down, partly, to the “growing interest in pragmatics and discourse”, but also “the interest in language universals and the exploration of ‘naturalness’ in language conceived in functionalist terms” – this was after all the time when some linguists took a first affirmative step away from the formalist, generativist linguistics that had predominated since the middle of the century and the new American school of functionalism took form. It was also the time when semantics, pragmatics, and discourse came to the fore.

Hopper relates all of this to Greenberg’s work on typology starting in the 1950s (Hopper, 1996: 219), and the interests that arose of combining Greenberg’s work on typology with some observations of the Prague School of linguistics (Hopper, 1996: 219). Naturally, he also notes the importance of Givón:

An important figure in the development and popularization of the idea that grammar was a product of change and that its forms could be attributed to discourse functions was Talmy Givón. (Hopper, 1996: 220)

Givón's famous statement that "today's morphology is yesterday's syntax", is seen by Hopper (1996) as something that became a slogan for functional linguistics. Lehmann (1982 [1995]), on the other hand, points to the fact that Carleton T. Hodge (1917-) (1970) had already used a similar phrase: "one man's morphology was an earlier man's syntax" (Hodge, 1970: 3) the year before. But Givón makes no reference to that paper nor to any other paper by Hodge, instead he says that he is paraphrasing "an old master" (Givón, 1971a: 413). In a footnote it becomes clear that the "old master" that he is referring to is Lao Tse (604 BC?):

This trip was inspired in part by an analect variously ascribed to Confucius but most likely emanating from the greatest of all time trippers, Lao Tse, who is reported to have remarked, on the occasion of being informed that Chinese was an isolating language: 'Weep not, my children, for today's syntax is tomorrow's morphology'. (Givón, 1971a: 413)

Still, it is true that Hodge also wrote a brief paper on grammaticalisation and that he used the similar phrase "one man's morphology was an earlier man's syntax". In a brief paper (Hodge, 1970), in the older version of *Language Sciences* from Bloomington, Hodge wrote on the history of the study of the development of grammatical items. He is not known to have written anything else on grammaticalisation, however the fact that he worked on less known African and Asian languages, e.g. Hausa, Mandigo, does seem to indicate a possibility that the concept was at least included in some of his other work, since work on African languages and typology appears to have been among the areas where grammaticalisation has flourished. But of course the fact that he worked on African languages makes it all the more interesting that Givón has no reference to this paper by him.

In the paper Hodge notes that many scholars (since Bopp) have believed that morphology stems from earlier syntactic constructions (1970: 1-2). This is an idea which Hodge notes can also be seen in Tooke and others before Bopp, however "Bopp was the major figure in promulgating the idea that inflections arose from the compounding of roots and 'auxiliaries' (Robins 1968: 173-174 [(Robins, 1968)])" (Hodge, 1970: 2). And Hodge, like many others before as he said, asserted that it

seemed that there was a form of linguistic cycle at work whereby syntax becomes morphology and is then replaced by syntax and becomes morphology again, which is replaced by syntactic structures, and so on. Jespersen is however seen as someone who argued against this view, since he believed that all the historical records show a move from “heavily morphologic to the predominantly syntactic” (Hodge, 1970: 2).

Hodge (1970: 3) follows the history of the ideas about the development of morphology out of syntax from Bopp, through Brugmann (1892; 1895), Hermann Hirt (1865-1936) (1904-1905; 1927b; 1927a; 1934) and he notes that others continued the ideas after that. Apart from that, the paper also briefly treats the history of Hittite and the history of Egyptian.

Notably, Hodge does **not** use the term *grammaticalisation* and he has no reference to Meillet, therefore there can be no link through this to Meillet’s use of *grammaticalisation* even if Givón did read the paper.

Givón’s importance did not only come through the 1971-paper and slogan. It is also clear that the fact that he has been so important to functionalism meant that he would have an effect also on grammaticalisation studies. Grammaticalisation has long had a clear link to functionalism, and it is only recently that the generative / formalist school has become more interested in this phenomenon.

In addition, Givón held a summer course at the Linguistics Institute later in the 1970s, still before grammaticalisation really took off (something which appears to have happened step by step in the late 1970s and in the 1980s and more quickly in the 1990s). Givón himself is certain that quite a few of the famous names in grammaticalisation attended this course (Givón p.c. 2002), and Hopper (1996) also mentions this course in his remarks on grammaticalisation:

In his course at the Linguistics Institute in 1976 and in his book [*On Understanding Grammar* (Givón, 1979a)] Givón illustrated the discourse motivation of such linguistic parameters as reference, tense-aspect, word order, and patterns of negation. He spoke of the “syntacticization” of grammaticalized constructions out of autonomous elements and identified ([1979a]:223-31) a series of functional poles that were conducive to either a loose, unstructured or a tighter, grammaticalized formation [...] (Hopper, 1996: 220-221)

On Understanding Grammar (Givón, 1979) also clearly had an important role in the spreading the concept of grammaticalisation. According to Hopper (1996: 221) Givón can be seen as the person who popularised the notion of grammaticalisation.

Based in typology, Givón noted paths that tended to occur in languages when lexical items develop into grammatical elements. Like others before him, during the nineteenth and early twentieth centuries, he noticed that semantic “bleaching” was involved in this process (cf. Givón, 1979a). Like others, Hopper says that this was an old idea, “although,” quite correctly he points out, “it is not clear where Givón found it since Meillet, Gabelentz, and Kuryłowicz do not appear in his bibliography” (Hopper, 1996: 221). In a similar manner, Lehmann (1982 [1995]: 6) notes that Givón seems badly informed regarding the past of the concept of grammaticalisation. Still, Lehmann (1982 [1995]) and Hopper (1996) both assume, that Givón borrowed this notion from someone else, dismissing without a word the possibility that he may have come up with it himself, even if it did exist in earlier work by other linguists. However, Hopper at least emphasises that the fact that something exists does not mean that it was read:

While it is indisputable that many of these ideas were around in the 1970s, they were scattered and often unpublished, and this book as well as Givón’s course on diachronic syntax at the 1976 Linguistics Institute were notable events in the blend of typology, grammaticalization, and discourse linguistics that has characterized much of linguistics since the 1970s. (Hopper, 1996: 221)

According to Hopper there was a distinct form of grammaticalisation studies developing at the University of Cologne during the late seventies, in the so-called *Unityp* project. There grammaticalisation is said to have developed as an empirical tool and as a “perspective on typology”. This group of linguists noted certain “principles” involved in grammaticalisation, which Hopper tries to provide a list of in his article (Hopper, 1996: 222-224). He also distinguishes a second direction that the linguists in Cologne who were involved in the development / revival of grammaticalisation (among others Bernd Heine and Christian Lehmann) have been interested in. This direction has tried to use the theory of grammaticalisation as a “**framework** for grammatical description” (Hopper, 1996: 224, emphasis mine). This framework has made use of what has sometimes been referred to as panchronic analyses and grammars, in other words it has attempted to combine synchronic and diachronic facts and ideas (cf. Hopper, 1996: 224).

Linguists in this group have tried to catalogue changes, which items that are most likely to grammaticalise and which paths they are likely to follow and the

'head' of the group is clearly Bernd Heine. As part of their attempt to generalise and catalogue language change, they have also presented the idea that there are two models for the creation of prepositions – “the body-part model” and the “landscape model” (Hopper, 1996: 224). Even though there are differences between the work of the Unityp group and the American functionalists, we can see strong links between their work. Both groups have been interested in paths and universals, typology and African languages and there is certainly a need to look closer at how the two groups influenced one another, but unfortunately I shall have to leave that for a later date.

Many grammaticalisation researchers have been interested in trying to find more **general parameters** that may have motivated the kinds of changes that are often classed as grammaticalisation (Hopper, 1996: 226). Concrete > abstract has been a popular metaphor of study, and the metaphor of bleaching was introduced already in the nineteenth century (Hopper, 1996: 226). But Hopper seems to think that bleaching is no longer part of grammaticalisation studies, which I am not quite sure is true. I believe it still plays quite an important role, perhaps partly because the people who first start doing work on grammaticalisation still start by referring to what we may call the set texts, e.g. Heine and Reh (1984), Lehmann (1982 [1995]) and Hopper and Traugott (1993). These are texts which are now to some extent outdated, for instance, in the case of the often mentioned *bleaching* which has recently been questioned (e.g. Meyerhoff and Niedzielski, 1998); but also Lehmann's idea of scope decrease has been criticised by Tabor and Traugott (1998) who believe that there is often scope **increase** in grammaticalisation; and Lehmann's (1982 [1995]) notion of obligatorification has also been questioned, e.g. by Hopper (1991) and Hopper and Traugott (2003).

Hopper and Traugott (1993) is of course the most modern and up to date of the three texts mentioned and they do not accept semantic bleaching as a definite characteristic of grammaticalisation and the fact that a new edition of this core text has now appeared should make the situation better still (Hopper and Traugott, 2003). Following Traugott's own work they emphasise that the opposite, namely (pragmatic) strengthening, tends to play a more important role, at least in the early stages of grammaticalisation (cf. Hopper, 1996: 227). Hopper seems quite right to claim that the matter of semantic change in grammaticalisation was widely debated in the 1980s and 1990s (1996: 227), and it does seem as though this is less of an issue at the beginning of the 21st century. Quite a few of the participants at the *New*

Reflections on Grammaticalization 2 conference in 2002 commented on the lack of discussion and presentations of the semantic aspects of grammaticalisation.

Coming back to Givón's role, Lehmann (1982 [1995]: 7-8) indicates that Givón can be placed in a typological framework. He says that “[s]witching back, [...] to the conception of evolutive typology, we find this revived in the two articles by Carleton T. Hodge [(1970)] and Talmy Givón [(1971a)].” The so-called evolutive typology is one of the two areas that Lehmann recognises as areas of development for grammaticalisation, the other being Indo-European historical linguistics. Lehmann seems quite right to say that the concept (notably he here chooses to speak of “agglutination theory”) “does not [...] regain its former popularity until Hodge 1970 and Givón 1971 [(1971a)]” (Lehmann, 1982 [1995]: 6). But we may want to question whether what happened in the early 1970s was a revival of agglutination theory, which he decides to mention here again, or was this something else? Perhaps there lies a difference partly in the fact that during the 1970s it was typology and universals that were the main interest of the scholars who worked on grammaticalisation, whereas during the nineteenth century linguists concentrated on historical descriptions and comparisons of Indo-European languages in particular. There certainly was a difference in what agglutination theorists and 1970-grammaticalisationists focused on – development of forms or development of grammatical markers, including inflections, function words and possibly word order.

As mentioned above, Lehmann (1982 [1995]) also notes the fact that Givón does not seem aware of the history of this concept. It is true that he does not appear to have been aware of the fact that such a concept had been studied before, and this makes it a bit paradoxical to say that the concept was ‘revived’.

Givón came from the typical 1960s formalist background. He took a big step when he started to look into the history of language, and the function of language structures, a step that was not always approved of by the scholars around him (Givón p.c. 2002). That he did not know the history of the concept was mainly due to the fact that in that field of formal linguistics the concept had never existed and linguists are after all usually primarily taught about the history of the main school of linguistics (which at the time was formal Chomskyan linguistics). Even now, how much is the general linguistics undergraduate taught about the history of linguistics – unless he/she **opts** to do a course in the history of linguistics (where this is an option that is!)? Usually introductions to linguistics teach the student a bit about Saussure,

Bloomfield, Sapir, Whorf – mainly twentieth century linguists, who were primarily interested in the synchronic aspects of language, and a bit about the Neogrammarians and the beginning of historical and comparative linguistics.

There was not much in the typical early twentieth century linguistics that bore any resemblance to grammaticalisation. Linguists had been concentrating on structure, synchrony and practical implications, leaving history (diachronic linguistics), evolutive typology and philosophy of language to the past. And as Hopper and Traugott (1993: 24-25) note, grammaticalisation (as a term they say, but I dare say, also as a concept) was “consistently overlooked in the textbooks of synchronic and historical linguistics of the period,” the period being the time after Meillet’s use of the term. They also note that this period of “amnesia” extends to the present, since still in the 1990s some textbooks on historical linguistics do not include the term. (Fortunately, the situation has now improved (cf. Hopper and Traugott, 2003: 25).) As I noted, nineteenth-century linguistics is usually introduced to undergraduates in linguistics, however not in any depth and I dare say that most undergraduates go away only with a knowledge of there having been comparisons, reconstructions of a proto-language, discussions about sound changes and possibly some idea of debates around analogy during the nineteenth century.

Nowadays linguists who are interested in history are perhaps more common; and function, typology and diachronic linguistics have become parts of linguistics of today and cannot be shoved to the back of the shelf and forgotten about. But still, linguists today are not often interested in (or perhaps rather, do not often have the time to be) looking very far into the past for other linguists with similar ideas. And even if they attempt to consider some quite early writings which may include something of interest, they stay very close to their own focus of interest in general – be that discourse, determiners, or genitives; Swedish, English or African languages.

Givón might have done the same, he might have looked at writings from the past concerned with his own field of interest – African language typology and universals. But there was not much in this area to find in the eighteenth, nineteenth or early twentieth centuries. In addition, linguists, like all scholars, like to feel that they are making progress. And they usually conduct their experiments based primarily on what is considered general knowledge at that time. So did Givón, and it seems he did not spend much time looking for a past for his ideas. Givón also admits that he did not tend to read that much of the early literature (Givón p.c. 2002).

An important issue in grammaticalisation studies today is the unidirectionality hypothesis (chapter 3). Meillet never explicitly said that grammaticalisation can only move in one direction, but he did only discuss one direction (cf. Lindström, 2002). In 1999, Haspelmath attempted to find the first statements about grammaticalisation being a unidirectional process (Haspelmath, 1999). The first explicit mention of this that he could find was in one of Givón's papers from the mid-1970s:

One may offhand argue that an opposite process than the one outlined above, i.e., a process of prepositions becoming semantically enriched until they turn into verbs, **is at least in theory possible**. For this I find **only rare, sporadic examples**, such as in English 'up' as in 'to up the ante', or 'off' as in 'off with his head', or 'away' as in 'away with the rascal'. **There are a number of reasons why such a process should be extremely rare**. To begin with, when a verb loses much of its semantic contents and becomes a case marker, in due time it also loses much of its phonological material, becomes a bound affix and eventually gets completely eroded into zero. It is thus unlikely that a more crucial portion of the information contents of the utterance – i.e., the semantic contents of a verb – will be entrusted to such a reduced morpheme. Further, while the process of change through depletion is a predictable change in language, its opposite – enrichment or addition – is not. The argument here is rather parallel to the uni-directionality of transformations of deletion in syntax. It also closely parallels arguments in phonology, concerning the relative feasibility of strengthening/addition vs weakening/deletion of, say, consonants. (Givón, 1975: 96, underline original, emphasis (bold) mine.)

But although Givón has been recognised as one of the linguists who believes in unidirectionality, and he also claims that his views are still unidirectional (Givón p.c. 2002), his statements here prove that he does not hold the very strongest view whereby all means of expressing grammar through form changes derive from the lexicon – he only claims that the opposite, the “reverse” should be “extremely rare”.

Interestingly, it is in the article from 1975 where Givón explicitly speaks of some form of unidirectionality, that we also find the term *grammaticization* appearing for the first time in one of his writings (Givón, 1975: 49). This is also the first use of *grammaticisation* or *grammaticalisation* that I have come across in the 1970s apart from in writings by Indo-Europeanists such as Kuryłowicz (1965 [1975]) and Anttila (1972).

Interestingly, Givón himself (p.c. 2002) claims that he has not used the term *grammaticization* because he does not like it, so maybe it was a misprint, maybe it should have said *grammaticalization*. However, we should note that Bolinger also

used this term in the 1960s (cf. section 6.3.4). *Grammaticalisation* also is not the only term Givón uses for the concept. In the same paper he also calls it “lexical re-analysis”:

The second diachronic process involves two mutually-linked changes which, in combination, affect the lexico-syntactic typology of the language in rather profound fashion: The lexical re-analysis (or ‘grammaticalization’) of verbs as prepositional case markers, and the correlated change from serializing to a non-serializing VP typology. (Givón, 1975: 49)

This is some indication that the term *grammatic(al)isation* was new and had not yet been approved of by the research community. We now need to know whether Givón means the same by *grammaticisation* as what we mean by *grammaticalisation*, and we will return to that issue shortly.

Of course we are interested in what terminology Givón may have used before he started using the term *grammaticisation* / *grammaticalisation*. However, in his early work, Givón did not use a term for grammaticalisation. He had then only just realised that there is often (or always) a relation between morphological forms and syntactic structures – a first step to realising that there may be a development from one to the other. In 1971, he says:

... the higher verb [cause] [...] receives no independent lexicalization and kill lexicalizes [cause-die]. [...] The arrow [...] will [...] be interpreted as both a ‘synchronic’ T-rule of lexicalization and a ‘diachronic’ rule by which ‘cause’ became a bound ‘derivational’ morpheme.
(Givón, 1971a: 410 - the words in square brackets are original.)

In a paper from 1977 (Givón, 1977), Givón finally uses the term *grammaticalisation*, or actually only as a verb, *grammaticalized*. In this paper he speaks of a form of grammaticalisation which Meillet would have recognised, but which later grammaticalisationists, such as Hopper and Traugott (1993), have frequently dismissed, namely the grammaticalisation of word order (Givón, 1977: 181):

Somewhere in the middle of this typological continuum one finds Israeli Hebrew, in which the frequency of VS syntax in both grammar and text is on the wane (as compared to Spanish), and **in a number of environments SV syntax has already grammaticalized**. Finally, close to the other end of the continuum one finds **English, in which SV syntax has grammaticalized almost completely in most environments** and is the prevalent order in text,

though a few ‘relic’ environments, such as existentials, ‘surprise subject’ and a number of other ‘frozen’ attestations of VS syntax still remain. (Givón, 1977: 181, emphasis mine.)

It is clear that word order changes are a form of grammaticalisation in Givón’s eyes, just like they were to Bolinger and to Meillet. He also notes possible parallels between the grammaticalisation of word order and loss of verb-subject agreement (1977: 186). For instance, he suggests that subject-agreement may mean that grammaticalisation of word order is slower to happen (1977: 246).

In terms of text frequency at acquisition time, then, a viable subject-agreement paradigm must surely act as a retardant in the drift along our continuum toward grammaticalized SV syntax. (Givón, 1977: 246)

In 1979, Givón discusses grammaticalisation together with *syntacticisation* and *morphologisation*. This has led Hopper and Traugott (1993; 2003) to claim that Givón preferred the latter two terms to *grammaticalisation*:

... Givón’s book *On Understanding Grammar* (1979) was a highly influential, if slightly idiosyncratic, summing up of the decade’s thought on these matters. It firmly placed all linguistic phenomena in the framework of “syntactization” and “morphologization” (terms which Givón preferred to “grammaticalization”), and emphasized the essential function dependency of linguistic rules and categories. (Hopper and Traugott, 1993: 29, cf. 2003: 29)

I can see that some truth may lie in their claim regarding his preference, however it should be made clear that Givón uses all three terms, and with different meanings. It seems as though he sees syntacticisation and morphologisation as two steps in the overall process of grammaticalisation, “parts of the same process” (1979a: 220). So if we are to discuss the whole cycle of change we have to speak of *grammaticalisation*.

If language constantly takes discourse structure and condenses it – via syntacticization – into syntactic structure, one would presumably expect human languages to become increasingly syntacticized over time. In fact, this is not the case. Rather, syntactic structure in time *erodes*, via processes of *morphologization* and *lexicalization*. (Givón, 1979a: 208-209, italics original)

While in the past I have looked at these processes in a narrower context, dealing with the rise of morphology per se, it seems to me now that it may be more revealing to **treat syntacticization and the rise of grammatical morphology as two mutually dependent parts of the same process**. Via syntacticization the language loses message transparency while it gains

processing speed. The concomitant rise of morphology offsets the losses by adding *coding* to the construction, thus facilitating the emergent mode of automatic processing. (Givón, 1979a: 220, emphasis mine)

Was there then anyone who influenced Givón to start using one of the terms *grammaticalisation* / *grammaticisation*? Notably, in a paper from 1974 Givón (Givón, 1974) referred to Lord (1973). Lord, as mentioned above, has been slightly criticised for using the term *reanalysis* in some of her later work (Lord, 1976) to mean grammaticalisation (see Heine and Reh, 1984). It is of course possible that she also used this term in 1973²⁵⁵ and this also appears to have been the term that Givón first used. Interestingly *reanalysis* seems to have been popular in formalist circles for some time, with some differences of meaning associated with the difference in terminology, e.g in the work of Lightfoot. Could this be illustrative of a formalist – functionalist divide perhaps? The term *grammaticalisation* has now also crept into formalist discussion, but does that mean that the formalist concept of this kind of change has also come closer to the functionalist concept of grammaticalisation? (Unfortunately, I have not had the chance to include a discussion of the new book on grammaticalisation from a formalist perspective written by Roberts and Roussou (2003)).

In a paper from 1973 (submitted in 1972), Givón (1973) refers to Raimo Anttila in private communication in relation to some Finnish data. Raimo Anttila included grammaticalisation, as term and concept, in his introduction to historical and comparative linguistics which was published in 1972. It therefore seems possible that he may have mentioned this term and concept to Givón. However, Givón gives no indication of this, and claims that he does not remember having heard of the term before he started using it himself (Givón p.c. 2002), although considering his connections to Bolinger and Bolinger's use of *grammaticization* in 1969, that seems unlikely.

Furthermore, the 1973-paper (Givón, 1973) also includes a reference to Benveniste (1968), a paper which certainly deals with the **concept** of grammaticalisation, but where the term is replaced with *mutation* and *auxiliation*, even though Benveniste was one of Meillet's students. There is no clear link to a particular source in his writings where Givón may have got the term

²⁵⁵ Unfortunately I have not yet been able to consult this paper, since I have not managed to get hold of it so far.

grammaticalisation (or *grammaticisation*) from. However, there is one person who Givón had a lot of contact with and who he has acknowledged that he was influenced by (Givón, 1979a: xiv), someone who also used the term *grammaticisation*, namely Dwight Bolinger (1907-1992) (6.3.4):

What I know about language owes much to many people. [...] There are three people that I have long considered beacons of integrity and common sense in linguistics, a field rife with fads, factionalism, and fraticide: Dwight Bolinger, for teaching that language could be understood in the context of communication; Joseph Greenberg, for refusing to consider language without languages; and Kenneth Pike, for insisting that language was inevitably embedded in cognition, culture, and man's construction of his universe. (Givón, 1979a: xiv)

Bolinger (1968) talks of the “ ‘grammaticizing’ of word order” (1968: 119), notably in citation marks which probably indicates that this is a new term. The fact that he says ‘grammaticising’ rather than ‘grammaticalising’ of course seems to link up quite well with the fact that the first attested form in Givón is also a form of the term *grammaticisation* rather than *grammaticalisation*. And the fact that Givón also wrote about the grammaticalisation of word order could be another important indication of a connection between him and Bolinger on the issue of grammaticalisation.

How much like our notion of grammaticalisation is Givón's concept? Like Meillet, Givón (1975: 82) recognises that grammaticalisation involves what has often been called *semantic bleaching*, which he calls “depletion of some semantic material” (underline original). It seems quite clear in his early writings that what he was first interested in was the semantic structure of languages, and semantic change. He also spoke of semantic reanalysis before he started discussing syntactic reanalysis (cf. Givón, 1971a; 1971b). However, he recognises that there may be enrichment of the meaning of another word in the construction affected by the reanalysis, that as some words lose meaning others may gain new meanings (Givón, 1975: 94, underline original):

At this point it should be noted that the re-analysis under consideration involves two opposite processes:

- (a) As discussed above, the depletion of much (though not all) of the semantic contents of a verb in the series (or all verbs except one), so that it becomes ‘a preposition’;
- (b) The parallel enrichment of the semantic contents of the remaining verb, so that it becomes – both semantically and syntactically – more complex.

The overall semantic complexity of the entire construction remains virtually unchanged [...]

The first time I noticed the actual term *semantic bleaching* in Givón's work was in his book from 1979 (Givón, 1979a: 232, 265), and by then it had already been used by Lord (1976), a publication which Givón is likely to have been familiar with:

The change can be described, as a 'bleaching' process in which verbs lose meaning and syntactic properties, remaining as grammatical morphemes marking relationships. (Lord, 1976: 189)

The fact that Givón speaks of *bleaching*, similar to the term *affaiblissement* 'weakening, fading' which Meillet had used in the early 1910s, could suggest a link between them that had not previously been there. However, I have found no references in Givón's work from the 1970s to Antoine Meillet, or any of his followers who used exactly the same terminology as him, such as the Indo-Europeanist Kuryłowicz. Although, as I said above there is a reference to Benveniste's treatment of *mutation* and *auxiliation*. Maybe it is also worth noting also that the term *semantic bleaching* is presented in italics, but this seems to simply be the means of emphasis used in the book. However, why does he use italics for semantic bleaching more than once, even in a footnote once, something he does not usually do with other terms. Why would semantic bleaching need such emphasis?

Givón also speaks of "phonological attrition", the term that was later to become used in most texts on grammaticalisation when the phonetic changes that grammaticalisation often entails are discussed (Givón, 1979a: 232, 245).

If we wish to compare Givón's work to later work on grammaticalisation, we can also see that he acknowledges that the verbs that grammaticalise as prepositions tend to decategorise (in later terminology, Givón uses no term), something which had clearly also been recognised by others by then:

Morphological criteria – One of the first things that may happen to erstwhile serial verbs, as suggested in Li and Thompson (1973[...]) [(Li and Thompson, 1973)] and Pike (1970 [(Pike, 1970).]) is loss of ability to take normal verb affixes [...] (Givón, 1975: 82, underline original)

Givón may have been criticised for not knowing about the past of grammaticalisation theory. However, as is clear from some of the quotations above, where we find references to work on processes such as grammaticalisation done by

other people around the same time as himself. Givón was not unaware of works by other people with similar ideas. Lord, for instance, is noted to have discussed the derivation of prepositions from verbs (Lord, 1973), an example which Givón also mentions and which can be seen as an indication that his concept of grammaticalisation then was probably much the same as it is to us now (cf. Givón, 1975: 49). There are also references to Li and Thompson, and Pike which may be of interest to us in studying the history of grammaticalisation.

Probably partly due to Greenberg's influence on him (and the role of this form of ideas at the time), Givón claims that he thinks he has found a universal tendency. A tendency "for a language lacking many prepositions to begin a serial pattern which, eventually, may result in the re-analysis of many verbs as prepositions" (1975: 57). Givón also noticed that there were grammaticalisation 'channels'. He says that he has noticed "[a]n amazing consistency as to what verbs may give rise to what prepositions," in other words he has noted that there is a tendency for verb X in different languages to move into meaning Y as a preposition, and, he had also noticed that verbs such as 'go' often develop into future markers (cf. Givón, 1973; 1974; 1975).

The main examples of grammaticalisation from 1975 are verbs that go through a stage in a serial verb construction and later end up as prepositions, clearly examples that are influenced by the main languages that were studied by the linguists who developed ideas about grammaticalisation in the 1970s (viz. Chinese and some Bantu languages). But there are also examples of nouns that develop into case markers (Givón, 1975: 50) and verbs that develop into auxiliaries and later become tense-aspect markers (Givón, 1973; 1974; 1975).

Givón's statement regarding case markers in Kpelle is an example of a relatively strong unidirectionality hypothesis, since he claims that **all** case markers stem from nouns (Givón, 1975: 50). However, as we have noted above, his ideas are not quite as unidirectional as they may seem at first, because he does admit that there is at least a theoretical possibility for a movement in the opposite direction.

Givón also attempts to explain **why** changes such as these would occur, putting it down to speed and ease, etc., in other words general principles of human communication:

... it is a communicative mode which arises – diachronically, ontogenetically, and most likely also phylogenetically – as a result of grammaticalization-syntacticization of the pragmatic mode of discourse. The process of syntacticization is itself **motivated by a number of communicative factors** pertaining to the immediate situation in which communication takes place, the **degree of time pressure, the degree of preplanning, the amount of shared presuppositional background** etc. (Givón, 1979a: 268, emphasis mine.)

[T]he principles which governed the documented diachronic change were run-of-the-mill universal principle[s] which govern human communication wherever it takes place [...] They are principles such as ‘ease of delivery,’ ‘maximal differentiation,’ ‘ease of perception-processing,’ ‘reduction of ambiguity,’ ‘maximization of clarity of the code-meaning correlation’ or ‘creative elaboration.’ (Givón, 1979a: 268)

This reminds me of Meillet’s words about what motivates grammaticalisation. He claimed that what made grammatical forms appear, and what made them ‘fade / weaken’ both semantically and phonetically in his words (“affaiblissement de la prononciation, de la signification concrète des mots et de la valeur expressive des mots et des groupes de mots”²⁵⁶) was the need to speak with force and the need to be expressive – in other words communicative needs (Meillet, 1912; 1921: 139). It must be admitted that this is an important parallel between Meillet and Givón.

Interestingly, at one point Givón uses an example that reminds us more than usual of Bopp’s treatment of what came to be called *Agglutination Theory* during the nineteenth century. He mentions that grammatical agreement arises through reanalysis “of an anaphoric pronoun into a (normally verb-bound) agreement morpheme”, at the same time as topics are reanalysed as subjects (Givón, 1979a: 209). This is a claim that also reappears in some of his later writings (Givón, 2001: 400). As we have seen in chapter 5 in particular, the idea that personal verb endings in the Indo-European languages should be derived from pronouns was a popular hypothesis in the nineteenth century but it was dismissed around the turn of the last century, by Meillet (1911), Jespersen (1922 [1949]) and many others, in particular those who subscribed to e.g. *adaptation theory* such as Sayce (1884). However, since then this view appears to have reemerged among some historical linguists and typologists (cf. e.g. Givón, 2001: 400).²⁵⁷

²⁵⁶ Translation: “weakening / fading of the pronunciation, of the concrete meaning of the words and of the expressive value of words and groups of words”

²⁵⁷ I have tried to find out more about its status among historical linguists and Indo-Europeanists today by posting a query on the mailing list HISTLING and by talking to Dr Christiane Schaefer who

Another type of change that Givón discusses is verb serialisation, which is noted to lead to a requirement of more case marking. Since only two arguments can be marked through position, when two verbs move closer through syntacticisation of a former paratactic construction, one verb is left with more arguments than before and the other begins to act as a grammatical marker (Givón, 1979a: 221):

The original, paratactic, loosely concatenated expression has no need for case marking, since each nominal argument is sufficiently identified, in terms of its case-function, by the verb to which it is paired in a small atomic clause. When the serial chain is reanalyzed as a single sentence with one complex verb, the problem of case marking then arises. This is so because the verb can mark only two arguments – topic-agent and object – positionally. But the syntacticization added more object arguments, and they need to be marked with respect to their case-function. Hence the ‘extra’ verbs, which in a sense already had been functioning, in part, to mark the case-role of their paired arguments, now assume this case marking function as their major *raison d’être*. (Givón, 1979a: 221)

Serial verbs are shown to lead to a need for more case markers, auxiliary verbs on the other hand are shown to develop into tense-mood-aspect markers, although both develop from originally concatenated clauses (Givón, 1979a: 221-222):

Diachronically, the process thus involves the condensation-syntacticization of two loosely concatenated clauses, with the second one exhibiting subject anaphora under coreference (by either *zero*, an anaphoric pronoun or subject-agreement pronoun on the verb) into a single clause under a single intonation contour. Invariably, the ‘main’ verb becomes morphologized, most commonly as a tense-aspect-modal marker, while the second verb – semantically much more specific or ‘weighty’ – remains the sole verb of the syntacticized construction. Once again, then, syntacticization and the rise of grammatical morphology seem to go hand in hand.” (1979:222)

This type of change was also discussed by Meillet (1912) in his comments regarding the rise of the Romance future and the Indo-European perfect forms. It is a widely recognised form of grammaticalisation which has also received much attention in more recent times (cf. Bybee and Dahl, 1989; Bybee et al., 1994; Dahl, 1985).

Givón (1979a) also notices a fact of language change such as grammaticalisation that has later been noted by Hopper (1991), viz. *persistence*, where a previous stage is reflected in the restrictions on the later usage of a form or structure. However, Givón speaks of **structure**, of parataxis and hypotaxis, whereas

teaches Comparative Indo-European Studies at Uppsala University. Unfortunately, I have still not managed to find out exactly what the accepted view is today.

Hopper is **more** concerned with the persistence of meaning in individual words. However, in general Hopper is also interested in the organisation of discourse. What Givón says on this matter is that the “syntactic constraints” that used to apply to the construction when it was paratactic tend to be reflected in the later state of hypotaxis (Givón, 1979a: 261).

Apart from discussing changes such as the origin of case markers and tense-mood-aspect markers; phenomena such as reanalysis, semantic bleaching, phonological attrition and persistence, etc., Givón (1979a: 264) observes that verbs can lead to prepositions and from there on to conjunctions. This is of course related to his claims regarding the relation between parataxis and hypotaxis. The origin and renewal of conjunctions (clearly related to the parataxis, hypotaxis issue) is also a form of grammaticalisation that Meillet discussed in his second well-known paper on grammaticalisation (Meillet, 1915-1916 [1921]). It is also a common example in the current grammaticalisation literature. But there are no clear links between Meillet and Givón, as far as I can tell, apart from a possible transmission of the concept through Benveniste (1968). I will now proceed to have a look at what others have had to say on grammaticalisation since the 1970s and where their ideas appear to have come from.

One of the most active and well-known linguists in the field of grammaticalisation is the American scholar Elizabeth Closs Traugott. Her interest in grammaticalisation stems from her interest in semantic change and it seems to have been in the early 1980s that she started writing more on the subject. She does not seem sure herself exactly when she first learnt about grammaticalisation, although she is sure that although she did not attend Givón’s summer school which included much discussion of this phenomenon, she did attend some of his lectures at conferences (Traugott, p.c. 2002). Her contacts with Meillet appear to have been earlier than those with Givón though. She claims that she has a copy of Meillet’s *Grammaire Comparative* (Meillet, 1925) which she has had since before 1967 (Traugott, p.c. 2002). However, according to my findings this book by Meillet does not contain any treatment of grammaticalisation. But it could be that she had also read other things by him around this time.

Traugott has co-authored the most famous textbook on the subject (Hopper and Traugott, 1993; 2003), edited books on grammaticalisation (Traugott and Heine, 1991b), presented and published numerous papers on the topic (e.g. Tabor and

Traugott, 1998; Traugott, 1982; 1985; 1986a; 1988; 1995 [1997]; 1997; 1999b; 1999a; 2000b; 2000a; 2001; forth.; 1991) and she has also written a lexicon entry describing what it is (Traugott, 1994). Apart from working to introduce others to the subject of grammaticalisation, her primary interests have always been in semantics and pragmatics and this is also where her own contributions to grammaticalisation theory have been made.

Apart from hers and Hopper's introduction, she has perhaps become most known among grammaticalisationists for her papers on subjectification, which are strongly based in a pragmatic and semantic perspective. In the early 1980s she presented the idea that there was a unidirectional cline from propositional > textual > expressive meaning (Traugott, 1982). What this paper wanted to show was that although grammaticalisation might involve semantic losses, it also involves gains in more abstract senses and also in "pragmatic meanings" (Traugott, 2003b: 633). Later in the 1980s she reformulated her hypothesis "as three tendencies", of which one was called "subjectification", meaning "the development of a grammatically identifiable expression of speaker belief or speaker attitude to what is said" (cited in Traugott, 1995: 32; Traugott, 2003b: 633-634). In one of her latest papers she admits that *textual* in the discussion from the 1980s has been misinterpreted and she has now included that only in brackets, as an optional part of the cline: propositional > (textual) > expressive meaning (2003b: 633). Although, apart from that she believes that the hypothesis has largely been proven correct (Traugott, 2003b: 633). Notably, however Traugott stresses the fact that subjectification cannot **only** occur in grammaticalisation (2003b: 634).

Another contribution from Traugott has dealt with the parameters that Lehmann (1982 [1995]) set up for grammaticalisation, some of which she has questioned. Together with Tabor she has attempted to prove that scope does not always decrease in grammaticalisation, but rather it **increases** (Tabor and Traugott, 1998; cf. Traugott, 2003b: 638-643). She also claims that "clause-internal adversative and manner adverb > sentence adverb > clause-external adverb with discourse marker function" violates bonding (Traugott, 2003b: 642), but we must note that her presumed counterexample is not an example of bonding within a phrase like *instead of* or *anyway*, but rather concerned with the strength of the connection between clauses. Similarly, Hopper and Traugott (2003: 31-32) note that others of Lehmann's parameters need to be questioned or at least redefined: namely, semantic

bleaching and obligatorification and so they have been by some (e.g. Hopper (1991), Hopper and Traugott (1993: 68, 87-88, 2003: 94) and Laury (1996, 1997, both cited in Juvonen, 2000).

Having started out as one of the people who quite rigidly claimed that grammaticalisation was unidirectional, even though she and Hopper already in the early 1990s admitted that this was an unsolved issue (Hopper and Traugott, 1993), recently, Traugott has become more accepting of the idea that there may be counterexamples to the unidirectionality hypothesis. With Tabor (Tabor and Traugott, 1998) she argued that unidirectionality had to be tested more before it could be accepted as an empirical hypothesis, and in her latest writings she quite openly accepts that there are some counterexamples (2001; 2003b; 2003a; *forth.*). The way she now sees it, since change is not only cognitive but also social “no change is likely to be exceptionless” (Traugott, 2003a: 124), but on the whole she still believes that grammaticalisation is unidirectional (Hopper and Traugott, 2003; Traugott, 2003b).

Traugott has also refined her definition of grammaticalisation. She now sees it as:

The process whereby lexical material in highly constrained pragmatic and morphosyntactic contexts is assigned grammatical function, and once grammatical, is assigned increasingly grammatical, operator-like function. (Traugott, 2003b: 645)

What is more, although she has broadened her view of grammaticalisation by emphasising that we must consider constructions rather than isolated lexical items, she claims that both as a “type of change” (cf. phenomenon (Hopper and Traugott, 1993)) and as an “approach” (cf. framework (Hopper and Traugott, 1993)) grammaticalisation still remains distinct from “morphosyntactic change” and “change” respectively, which seems justified.

Paul John Hopper, with whom Traugott co-authored the famous textbook on grammaticalisation, is most known for his work on discourse, and his and Traugott’s introduction to grammaticalisation (Hopper and Traugott, 1993). He has however also written a lexicon entry on the subject of grammaticalisation (Hopper, 1992), a historical review of the field (Hopper, 1996), and he has also co-edited a book on grammaticalisation (Giacalone Ramat and Hopper, 1998). Similarly to Lehmann, Hopper has also discussed the various characteristics of grammaticalisation such as

what he calls *layering*, *persistence* and *divergence* (Hopper, 1991) and he has written many more articles that touch on the subject. One of Hopper's most famous ideas overall and in connection to grammaticalisation is probably that of *emergent grammar*, first presented in a paper in the late 1980s (Hopper, 1987), where he claims that there is no grammar only grammaticalisation! But he is also very often cited by other linguists in relation to his notions of *persistence*, *divergence*, *layering* etc which he presented in the early 1990s (Hopper, 1991).

Hopper started out as an Indo-Europeanist in the 1960s, which meant that he came across grammaticalisation in the works of Kuryłowicz and Watkins (Hopper, p.c. 2003). He then saw grammaticalisation as “a supplement to other methods” such as the comparative method and internal reconstruction, but during the late 1970s it began to take on a different form to him. Teaching at the 1976 LSA Institute Hopper spoke to Givón, Li, Bybee, Thompson, Greenberg and others and he began to see that grammaticalisation could also be used in synchronic studies (Hopper, p.c. 2003). Among his first publications to include the term *grammaticisation* (the version of the term he has tended to prefer in his own writings) was a paper on aspect where he spoke of “grammaticized meaning” (Hopper, 1982: 10). However, what he means by this is not absolutely clear, although it seems to mean something close to conventionalised:

By ‘additive’ meanings I shall intend those wholly or partially grammaticized meaning [sic.] of an aspect which are extensions of the discourse function. To the extent that an additive meaning can be accounted for by the *possibility* of a subsequent event, even though no such event is explicit in this discourse, part of the discourse function adheres to the form as a part of its meaning. (Hopper, 1982: 10)

But considering his note a few pages later that: “[i]n Malay, where the event sequencing function has remained to a large extent ungrammaticized, presumably because it was never concentrated in a single morphological or syntactic form” (Hopper, 1982: 16), it seems he must mean adopted into the grammar as in being restricted by grammatical rules. We should also note in passing that Hopper refers both to Bolinger's second edition of his *Aspects of Language* (cf. Bolinger, 1968) and Givón's *On Understanding Grammar* (Givón, 1979a), which both use *grammaticise* and *grammaticalise* respectively, although Hopper does not refer explicitly to the use of these terms in the two publications.

In his rather extreme article (Hopper, 1987) in defense of functional grammar, and in defense of discourse, Hopper suggested that grammar was always emergent in the sense that it changed with every utterance in discourse, just as much as it contributed to discourse. However, he claimed that this meant that it was not a prerequisite for discourse, but I would say that the fact that it changed through discourse does not mean that it was not a prerequisite, and he himself also said that it was both cause and effect (Hopper, 1987). But Hopper thought that since grammar always changed, one could claim that there was no grammar in a static sense and therefore there was in fact only grammaticalisation:

Because grammar is always emergent but never present, it could be said that it never exists as such, but is always coming into being. There is, in other words, no ‘grammar’ but only ‘grammaticization’ – movements toward structure which are often characterizable in typical ways. [...] The point again is that any decision to limit the domain of grammar to just those phenomena which are relatively fixed and stable seems arbitrary. (Hopper, 1987 online, section 2)

In this paper on Emergent Grammar, like in the paper from 1982, Hopper uses the term *grammaticisation*, however he also uses the term *grammaticalisation* once: “identifying recurrent strategies for building discourses – strategies which have intra-linguistic or inter-linguistic generality (or both) and which move toward grammaticalization along parallel lines.” There does not seem to be any difference implied between the two terms, so probably the one use of *grammaticalisation* is only a slip of the pen.

Yet another famous American linguist who has been very active in the development of grammaticalisation studies is Joan Bybee. She has concentrated perhaps primarily on the development of tense, mood and aspect (morphology), topics on which she has published both on her own and together with other scholars (Bybee, 1985; 1990; 1994; Bybee and Dahl, 1989; Bybee et al., 1991; Bybee et al., 1994). Bybee herself believes that she may have picked up on the subject of grammaticalisation from Theo Vennemann (1937-) during her years as a graduate student at UCLA (1970-1973). Although she believes that the concept was only implicit in Vennemann’s work. She also appears to have been in contact with Givón during this time, however she believes that her ideas on grammaticalisation probably were not so much influenced by him as by Vennemann seeing as Givón in fact was in Africa a lot at the time (Bybee, p.c. 2002). Later in the 1970s she did however sit

in on Givón's course on typology at the summer institute, where she says that "the types of changes that occur in [... grammaticalisation] were appealed to consistently" (Bybee, p.c. 2002).

According to her own account Bybee first applied the concept of grammaticalisation in her book *Morphology*²⁵⁸ (Bybee, 1985), but without using the term. However, she correctly claims that she did use the term "*grammaticization*" in a paper she wrote together with Pagliuca at the same time (Bybee and Pagliuca, 1985):

In this paper we propose a characterization of the notion of semantic generalization in grammatical meaning for certain aspectual and modality categories. We show that certain correlations between the degree of formal **grammaticization** and semantic generalization can be found in a large sample of unrelated languages. Finally, we outline briefly a theory of semantic change that accounts for **the development of lexical material into grammatical material** [...] (Bybee and Pagliuca, 1985: 59, emphasis mine)

Bybee and Pagliuca (1985) mainly treat the semantic development that takes place in the grammaticalisation of modal and aspectual markers. However, they also touch on the parallels between semantic bleaching, phonological attrition, frequency and fusion (e.g. 1985: 76). In addition, they state early on that grammatical markers develop from lexical items and a few times they note that grammaticalisation in various ways is unidirectional (Bybee and Pagliuca, 1985). They mention two scholars who treated the connection between semantic weakening and grammaticalisation before, viz. Givón (1973) and Fleischman (1982).

Like Hopper, Bybee has generally chosen to use the term, *grammaticization*. The choice of this term as opposed to the more common one, is presented in hers and Perkins and Pagliuca's monograph on the evolution of grammar (1994) where they state that:

Since the recent revival of interest in grammaticization in the early 1970s, two terms – grammaticalization and grammaticization – have been used, usually interchangeably. When we began the current work in 1983, both terms were in use and we settled on the shorter, more elegant of the two: grammaticization. Since that time the longer term has appeared in print more frequently than the shorter one. We nonetheless adhere to our original choice,

²⁵⁸ Written during the academic year 1983-1984 (Bybee, 2002, p.c.).

without, however, feeling that an issue needs to me made of this choice between two perfectly adequate terms. (Bybee et al., 1994: 4 fn.)

Most of Bybee's work has had a much more typological flavour to it than Traugott's, for instance, and in that sense her work could be said to be closer to Givón's. Her views on grammaticalisation have always been of a unidirectional kind, where grammaticalisation is seen as moving only from lexical to grammatical and from less grammatical to more grammatical. This primarily counts for the semantic development involved in grammaticalisation, but it can also be seen in grammatical and phonological change (Bybee et al., 1994: 13). However, she has also noted that there have been some counterexamples. For instance, together with Pagliuca and Perkins she has introduced the example of the Irish pronoun stemming from a person suffix *-muid* into the corpus of counterexamples to the unidirectionality of grammaticalisation (Bybee et al., 1994).

Only one example of affixed material that has become free has come to our attention, and in this Irish case there is strong paradigmatic pressure for the reanalysis of person/number suffix as a free pronoun. (Bybee et al., 1994: 13)

In a couple of papers she has also noted that even a zero marker can come to express grammatical meaning in that it is opposed to an explicit marker (Bybee, 1990; 1994).

There is one point where Bybee was perhaps earlier than many other grammaticalisationists. Already in the mid-1990s she, Perkins and Pagliuca noted that they thought it was wrong to say that "one source concept" could give us more than one "grammatical category", as Heine et al. (1991a: 338) were observed to think (Bybee et al., 1994: 11). Instead they believed that **constructions** grammaticalised. This has been more and more recognised recently and in one of the latest handbooks on historical linguistics (Joseph and Janda, 2003) both Traugott and Bybee, stress that constructions, and not linguistic items, grammaticalise (Bybee, 2003; Traugott, 2003b).

Bybee has for some time now been interested in the importance of frequency in change. In one of her most recent writings she tries to show the effect that repetition can have on grammaticalisation. There she shows how repetition promotes semantic bleaching and then generalisation and thereafter further semantic bleaching, but also phonological reduction (Bybee, 2003). She also argues that this is a way of explaining the tendency for grammaticalised items to retain older features of the

morphosyntax of the language, since frequent items tend to retain these features (cf. *persistence*) longer than the less frequent ones on which analogy works more often (Bybee, 2003).

Apart from these four American scholars, German scholars have also played an important role in the development of grammaticalisation studies since the 1970s. In Hopper's (1996) article on the history of grammaticalisation, he notes that there is something close to a German school of grammaticalisation studies:

In Europe, a group of linguists at the University of Cologne, working originally under the auspices of Hansjakob Seiler's Unityp (Universals and Typology) project, were developing a distinctive approach to grammaticalization both as an empirical tool in linguistic description and as a perspective on typology. (Hopper, 1996: 222)

This makes the importance of German linguists in the recent history of grammaticalisation quite clear. Unfortunately, I will not have chance to discuss their contributions in any detail here, but I would like to at the very least give a quick review of the work of the three main grammaticalisationists from Germany, Bernd Heine, Christian Lehmann and Martin Haspelmath. All three are often referred to in studies of grammaticalisation.

Bernd Heine's (1939-) work which explicitly treats grammaticalisation started in the early 1980s (see Heine and Reh, 1984).²⁵⁹ Heine also says, in personal communication, that he first came across the concept around this time and primarily through Givón's work (Heine, p.c. 2003). But from the first book he published on the topic, Heine also proclaimed that aspects (at least) of a similar concept had also been discussed by Antoine Meillet:

Grammaticalization is an evolutionary continuum. An attempt at segmenting it into discrete units must remain arbitrary to some extent. This applies in particular to the processes we propose in the present chapter, which are meant to serve as a means of segmenting this continuum. Although most of these processes appear to be 'definable' in some way or other, it is hardly possible to trace clear-cut boundaries between them. Meillet (1948: 135) touches on this problem when he discusses the transition from words that he refers to as *mots principaux* to *mots accessoires*. [...] But in spite of the problem as to where a *mot principal* ends and a *mot accessoire* starts, Meillet maintains that it is necessary to delimit the two (1948: 135). [...] Our position is similar to Meillet's. (Heine and Reh, 1984: 15-16)

²⁵⁹ This was an extended version of a working paper published by Mechthild Reh and Bernd Heine in 1982 (Heine, 2003, p.c.).

However, as far as Heine's knowledge of and influence by Meillet's work on the issue of the term *grammaticalisation* is concerned, Heine himself is careful and says that he is not sure whether he got the term from Meillet or from his American colleagues (Heine, p.c. 2003).

It seems that the notion of grammaticalisation is clearly something that Heine has adopted more from Givón (e.g. 1971a; 1971b; 1975; 1976; 1979a; 1981), than from Meillet, and maybe there is also some influence from Lehmann's (1982; cf. 1982 [1995]) book on thoughts on grammaticalisation which he also refers to. However, it seems perhaps more likely that Lehmann and Heine were both influenced by Givón and developed the concept of grammaticalisation around the same time, seeing as they appear to have worked together. Apart from references to Givón and Lehmann, Heine and Reh's book from 1984 also contains references to Lord (1973), not to mention a reference to the difference between Heine and Reh's own use of the term *grammaticalisation* and the use which Henry Max Hoenigswald (1915-) and others had made of the term:

Note, however, that 'grammaticalization' is used here in a wider sense than with most other authors. For example, Hoenigswald (1963: 34 [(Hoenigswald, 1963; 1964: 44)]) describes it as 'the emptying of lexically meaningful morphs (compound members, etc.) and their transformation into 'function' elements', then this refers to only one, functional, aspect of grammaticalization. (Heine and Reh, 1984: 15 fn.)

Hoenigswald's (1964: 44) definition of grammaticalisation, a term he uses in citation marks, does seem very much like Meillet's and Kurylowicz's definitions, or indeed most definitions of grammaticalisation in the last decades.

The trend from so-called synthetic to so-called analytic structure may be observable in certain areas, but so is the opposite trend, sometimes even in the same language family or area. It is probably only because the Indo-European idea of progress (or degeneration) from inflection to construction, from morphology to syntax, from bound to free had become a cliché, that the equally typical notion of **'grammaticalization' – the emptying of lexically meaningful morphs (compound members, etc.) and their transformation into 'function' elements** – was not presented as a counteraction, although at least in a minor way it has served to build up forms that look like new inflections (e.g., the Romance adverbs in *-mente*, from *mente* 'with (such and such) a mind' [...]) (Hoenigswald, 1964: 44, emphasis mine)

This definition is probably seen as narrower than Heine and Reh's definition since they do not focus on the semantic aspects which they appear to think that Hoenigswald does:

With the term 'grammaticalization' we refer essentially to an evolution whereby linguistic units lose in semantic complexity, pragmatic significance, syntactic freedom, and phonetic substance, respectively. (Heine and Reh, 1984: 15)

Heine's work has always been within the field of African languages and typology, a field which could also be said to link him very clearly to Givón's work. However, he has also shown a strong sense of wanting to learn about what past scholars have done on similar topics, and already in the early 1980s his and Reh's publication contained a reference to Gabelentz (1891) and one to Meillet. But whether these references were found by Heine and Reh or if they picked it up from Lehmann who had then written his book on grammaticalisation, which included a chapter on its history, is not clear.

Much of Heine's work has been concerned with finding patterns and universals in languages, in the manner that Hopper (1996) says this 'Cologne school of grammaticalisation' is known for. He has also published a dictionary of grammaticalisation chains / clines / pathways, whatever we wish to call the common paths of change that languages tend to take (Heine and Kuteva, 2002). This work on patterns is inspired by Heine's interest in the mind and how language relates to cognitive processes, an interest which has been particularly prominent in some of his publications which have also treated grammaticalisation (1993a; 1994; 1997a; 1991b; 1991a).

The German linguist Christian Lehmann (1948-) has made two major contributions to grammaticalisation theory. In his publications in the early 1980s (1982; 1985a) he set up a number of parameters for the study of grammaticalisation which have been used ever since: attrition, bonding, scope decrease, obligatorification, etc. Some of these have in recent years been questioned as I have mentioned above, but many still basically stand and are still referred to. The other contribution he made early in the 1980s was that he introduced a historical perspective on the **theory** and **concept** of grammaticalisation, rather than only on languages. In the first chapter of his book (Lehmann 1982; 1982 [1995]) he asserted that Meillet may have been the first to use the term *grammaticalisation*, but also that

the concept was related to agglutination theory and that it had been studied for a long time, although at times its popularity had wavered. This appears to be one of the first times that Meillet's possible role in the history of grammaticalisation studies was mentioned, although a couple of years before then Nigel Vincent had already cited Meillet's definition of grammaticalisation (1980). Vincent's paper is also listed among Lehmann's references, however Lehmann does not refer to it when he notices that Meillet may have been the first to use the term.

Recently, Lehmann has not been one of the most active linguists in the field of grammaticalisation. However, he still publishes the occasional paper and one of the issues that he has kept returning to is the opposition, or relation rather, between grammaticalisation and lexicalisation (1989; 2002) as the reader may already have had some indication of in the previous chapters of this thesis. He now emphasises that grammaticalisation and lexicalisation are not to be seen as opposites, but rather as parallel changes – an idea which has partly been brought about by an increased interest in constructions, and a realisation that constructions rather than single linguistic items grammaticalise (Lehmann, 2002). This is a recent trend in grammaticalisation studies which I have noted that we can also see in Bybee (2003) and Traugott (2003b).

The unidirectionality debate has been largely sidestepped by Lehmann, it seems. An interesting point is that Lehmann may have coined the term *degrammaticalisation* (1982; 1982 [1995]), an act in itself which according to some sent linguists searching for counterexamples of unidirectionality!²⁶⁰ Thereby he could be said to have started the whole unidirectionality debate.

Returning to Vincent, whom I mentioned briefly above, Vincent has not written that much on the topic of grammaticalisation. Still, his interest has occasionally made itself known, most recently in the discussions around the unidirectionality hypothesis. Vincent, in a sense, took part in this debate already in 1993-1995 when he presented and published a paper on the topic of exaptation and regrammaticalisation (see further in chapter 3) (Vincent, 1995). However, lately he has become more active in the discussion of the impossibility of counterexamples in work which he has done together with his colleagues Kersti Börjars and Thórhallur Eythórsson (Börjars et al., 2002; 2003).

²⁶⁰ This was mentioned at the New Reflections on Grammaticalization 2 conference in Amsterdam.

Another linguist who was quite soon to join the unidirectionality debates, siding with a unidirectional view of grammaticalisation much like Vincent, was the German scholar Martin Haspelmath. Special about Haspelmath, in comparison to the two other German linguists mentioned above (i.e. Heine and Lehmann), is that he was at least partly introduced to the concept by Joan L. Bybee during his time as a student of hers at Buffalo (Haspelmath, 1990: 63). This influence can also be seen in that he uses the term *grammaticisation* (1989?; 1990) in his first papers on the topic. However, he later changed to the more common term *grammaticalisation* (1999; 2002; forth). Nevertheless, his interest in typology also shows a clear link to the other two German linguists discussed above.

Haspelmath launched into discussions of grammaticalisation in the late 1980s, with thoughts first around specific paths of grammaticalisation (purposive > infinitive in 1989?; passive morphology in 1990) and then he got involved in the discussion of some of the basics of grammaticalisation – how it relates to reanalysis (1998), and why it is unidirectional (1999) and the latter issue in particular seems to be one that he is still eagerly pursuing (2002), which is of course only natural in that it is perhaps one of the biggest issues among grammaticalisationists at present.

A Scandinavian linguist who was also rather early to discuss grammaticalisation was Östen Dahl (1945-). He published a book on tense and aspect (Dahl, 1985) the same year as Bybee published her book *Morphology* (Bybee, 1985) which discussed similar topics and both of these books touched on the concept of grammaticalisation. Although Dahl is known to have done some work on grammaticalisation and to have a big interest in this field – mainly fed by his typological interests, he has not actually written that much on grammaticalisation. In his book on tense and aspect he does not really treat grammaticalisation, but rather just looks at various means of expressing aspect and tense in different languages (Dahl, 1985). However, Dahl mentions grammaticalisation a couple of times, with references to the British linguist Bernard Comrie (1947-) (Dahl, 1985), whom we will return to shortly. Dahl also speaks of the difference between grammaticalisation and lexicalisation briefly, but unfortunately without making clear what the difference between them is:

What I am suggesting here appears to be consonant with Comrie's view (Comrie, forthcoming [1985]) that the difference between 'grammaticalization' and 'lexicalization' 'can be understood in terms of the

interaction of two parameters: that of obligatory expression, and that of morphological boundness'. (Dahl, 1985: 23)

However, it seems he means to say that the difference is that grammaticalisation means obligatorification and lexicalisation means that something becomes morphologically bound, possibly something similar to what is occasionally called *univerbation*. He also mentioned that according to Comrie, tense can be seen as “grammaticalized location in time” (cited in Dahl, 1985: 23, from Comrie 1985).

Dahl has inspired quite a few people working on grammaticalisation, partly through his 1985 book, which discussed some of the typological varieties of tense and aspect forms. This book also partly lay as a foundation for a paper published by Dahl and Bybee (1989) where they compared the findings from their two books (Bybee, 1985; Dahl, 1985), both of which had treated similar aspects of language. In this paper they explicitly discuss grammaticalisation, as opposed to Dahl's brief mention of it in 1985.

The only sources in Dahl (1985) that definitely treated some form of grammaticalisation, were Comrie's books on tense and aspect (1976; 1985) where Comrie certainly talked about something that was called *grammaticalisation*. Although exactly what he meant can only be arrived at through reading Comrie's books. When we take a closer look at Comrie's books we can see that he uses the term in both books.

Comrie was born and educated in Britain, and received his PhD from Cambridge in 1972 in linguistics. Between 1978 and 1998 Comrie worked in the United States holding a position in California and he is now Director of the Max Planck Institute for Evolutionary Anthropology in Leipzig, Germany (Comrie, online C.V.).

He has defined grammaticalisation as when a language has a grammatical category that expresses a certain type of reference, e.g. time reference. Grammaticalised is opposed to lexicalised in this case, in the sense of having lexical items ('words') for something more or less (Comrie, 1976 [1981]: 6; 1985: 10). This sense of grammaticalised and lexicalised is the same as in Jakobson (1971 [1959]) when discussing Boas, and in Lyons (1968), and although it is the same term as the one which is usually used for the concept I am discussing in this thesis, it is clearly also different from that concept in its more typological and synchronic character. However, even though Comrie does not mean the same by *grammaticalisation* /

grammaticalised as I do in this thesis he has a sense of the fact that diachronically tense may derive from aspect and mood markers. He also says that tense markers can give rise to aspect and mood markers (Comrie, 1985: 12), which we could see as grammaticalisation in the sense of grammatical elements becoming more(?) grammatical, or at least becoming other grammatical elements. However, notably he also claims that “there are hardly any good attestations of grammatical tense marking deriving from lexical items that express time location” (Comrie, 1985: 10), something which would have been a very obvious case of grammaticalisation in our sense of the word.

Interestingly, Dahl (1985) also includes references to work by Hopper (Hopper, 1982; Hopper and Thompson, 1980) and Li, Thompson and Thompson (1982) who are known to have worked on the discourse and typology perspectives of grammaticalisation respectively. Although, whether grammaticalisation was treated in the texts he refers to is not clear. Naturally, Dahl also has references to his own work but this does not seem to involve grammaticalisation, nor does his reference to an article by Koptjevskaja[-Tamm], even though we will see below that she has also mentioned grammaticalisation in some of her later work.

In Bybee and Dahl (1989) the two authors speak of “*grammaticization*” in a very default manner of lexical items developing functional uses:

... grams develop out of lexical material by a gradual generalization of meaning which is paralleled by a gradual reduction in form and fusion with the head [...]. Perfect and progressive are less grammaticized, less general meanings, and thus show less grammaticization of form. Past, perfective and imperfective are more abstract and general grammatical meanings, and thus show more grammaticization of form. (Bybee and Dahl, 1989:56)

Like in many other papers on grammaticalisation Bybee and Dahl (1989) also discuss the different criteria and aspects of grammaticalisation. They look at how items tend to become more fixed, obligatory and sometimes become affixed to the main word in the phrase. They also note the relation between the use of certain grammatical morphemes and their former lexical meaning (1989: 93). For instance, the use of a future meaning may depend on whether it comes from a verb that expressed desire or obligation. This bears some resemblance to the notion of *persistence* which was later introduced by Hopper (1991). Furthermore, they (Bybee and Dahl, 1989: 94, 96) discuss how some languages have grammaticalised items

from several different sources which exist in parallel, but are used in slightly different ways – similar to the sense of *layering* in Hopper (1991).

Bybee and Dahl's (1989) perspective is primarily typological and they attempt to make statements regarding universal tendencies. In relation to grammaticalisation they have noted that “the mechanisms by which grammaticization is implemented [are] the same across languages, but also the actual semantic material that is molded by this process appears to be very similar across languages” (Bybee and Dahl, 1989: 96).

There are some big names in current grammaticalisation studies which have not yet been mentioned, linguists who have only started making their voices heard on these issues more recently and in particular on issues such as the unidirectionality hypothesis of grammaticalisation and the questions of whether grammaticalisation is a theory, a phenomenon or an epiphenomenon. Two of the most well-known linguists in the debates concerning unidirectionality and the epiphenomenon issue are Richard D. Janda and Brian D. Joseph. Both gave plenary lectures at the *New Reflections on Grammaticalization 2* conference. Both also published articles in a well-known issue of *Language Sciences* which was devoted to grammaticalisation and more specifically to the issues of whether grammaticalisation is unidirectional and whether it is an epiphenomenon (*Language Sciences*, vol. 23:2-3).

Janda has become particularly known among grammaticalisationists in later years when he has written about the unidirectionality hypothesis. He has tested and pulled the hypothesis and tried to show that it **could** be a tautology, depending on how one defines it and how one defines grammaticalisation (Janda, 2001; 2002). Janda has also brought up the role which hypercorrection plays in language change and therefore in grammaticalisation, and he has pointed to the fact that interestingly Meillet also showed an interest in hypercorrection in some of his work (2001; 2002).

Joseph is perhaps primarily known to grammaticalisationists as one of the people who has argued that grammaticalisation is in fact an epiphenomenon (2002; 2001). This is an issue which, as we have touched on before, has haunted grammaticalisation studies since around the turn of the century. It still has not been resolved and probably never will be. It seems there will always be some who claim it is an epiphenomenon, whereas most grammaticalisationists would say it is not. However, grammaticalisationists also would not spend that much time on an issue which to them is of little interest.

Another rather well-known grammaticalisationist from the United States who also appeared at the *New Reflections on Grammaticalization 2 (NRG 2)* conference was Jurgen Klausenburger. Klausenburger has worked a lot on the history of French, and he has proposed a treatment of this history which combines markedness theory and grammaticalisation (Klausenburger, 2000), but he has also discussed how grammaticalisation can be explained. Using Keller's invisible hand hypothesis he has tried to show what it could be that motivates languages to change in the manner which we usually refer to as grammaticalisation (Klausenburger, 1998). His paper at the NRG 2 conference treated the affix or clitic status of certain elements in French (Klausenburger, 2002).

Before we proceed to look at some of the other linguists who have had an interest in grammaticalisation, let us have a brief look at two of the most important linguists in the rather recently developed formalist angle on grammaticalisation. One of the first to tackle grammaticalisation from a generative perspective was Ian Roberts, who is now perhaps one of the most well known British scholar to have worked on grammaticalisation in recent years (Roberts, 1993; Roberts and Roussou, 1999). Together with Roussou Roberts is also the first to have published a longer treatment of grammaticalisation from a formalist perspective (2003).

For a long time generative linguists largely ignored the ideas of grammaticalisation that were circling around among functionalists, a natural result since generativists have not been as concerned with the history of language (cf. Newmeyer, 1998: 225, 292). Another contributing factor seems to have been their emphasis on every learner 'creating' their own grammar from scratch with no knowledge of the past. However, since Roberts entered this discussion, other generativists have followed (e.g. van Kemenade, 1999), although naturally not all agree with Roberts's views on the matter.

Like other generativists, Roberts and the Greek linguist Anna Roussou believe that language change is connected with a resetting of parameters, which occurs when learners happen to develop a grammatical system that differs from the system that their models / teachers have (Roberts and Roussou, 1999: 1020). What Roberts (and Roussou) tries to do, in combining grammaticalisation and generativism, is to show that something that used to move within the structure of the sentence, becomes reanalysed as having the position it used to move to all along, and thereby the structure is simplified.

... we would like to argue that grammaticalization is connected to the loss of movement whereby a lexical item that previously realized two syntactic positions (features) now realizes only (the higher) one (categorical reanalysis). In other words, we get grammaticalization when we have a change from the F^*_{move} to the F^*_{merge} option. If the change is from F^*_{move} to F , then we still have loss of movement, but no categorical reanalysis, hence no grammaticalization... (Roberts and Roussou, 1999: 1022)

This means that by applying a ‘least-effort strategy’ (cf. Newmeyer, 1998: 292-293) Roberts can also explain the reanalysis, and grammaticalisation, that takes place. Newmeyer however questions whether such a strategy could really explain why this type of reanalysis happens, since he believes that it is clear that this does not explain all instances of grammaticalisation and it is questionable whether this is ever enough to explain a single one (Newmeyer, 1998: 293-294).

Another generativist who has become known for his work on changes which he has usually called *reanalysis*, but has now occasionally started to refer to as *grammaticalisation*, is David W. Lightfoot (1979; 1991; 1999; 2003). Lightfoot did his undergraduate degree in Classical studies at King’s College, University of London in the early to mid-1960s. But then he went on to the University of Michigan to do a PhD in linguistics, which he completed in 1971 (David W. Lightfoot, homepage). Lightfoot has long claimed that grammar changes during acquisition, as other formalists would also claim. In addition, he has claimed that changes which are often referred to as *grammaticalisation* by grammaticalisationists, could be better **explained** as reanalysis caused by, what he now at least calls, “local causes”, changes that have already occurred (Lightfoot, 2003). This does sound slightly circular, much like a chicken and egg problem, but still to some extent he has a point in that if we want to explain the changes that occur we should look at the contexts in which they occur and perhaps not directly try to argue for a principle of language change. Lightfoot himself also recognises that the fact that we do not know “why the linguistic environment should have changed in the first place” is a bit of a problem:

Environmental changes are often due to what I have called chance factors, effects of borrowing, changes in the frequency of forms, stylistic innovations, which spread through a community and, where we are lucky, are documented by variation studies. Changes of this type need not reflect changes in grammars. But with a theory of language acquisition which defines the range of theoretical choices available to the child and specifies how the child may take those choices, one can predict that a child will converge on a certain

grammar when exposed to certain environmental elements. This is where prediction is possible, in principle (Lightfoot, 2003: 121)

Reanalysis then is caused by differences in the Primary Linguistic Data (PLD) and leads to changes in the parameter settings, and thereby changes in the grammar for the individual speaker (e.g. Lightfoot, 2003: 110). Lightfoot dislikes the talk of historical principles and in that sense he also dismisses grammaticalisation. However, he recognises that grammaticalisation is a “real phenomenon” in the sense that it is a “semantic tendency for an item with a full lexical meaning to be bleached over time and to come to be used as a grammatical function” (2003: 106). But he does not believe that it is a “general, unidirectional or explanatory force” (2003: 106). And there can be no universal tendency to grammaticalise since then there would be no counterexamples moving in the opposite direction, towards free lexical forms, and Lightfoot believes that it has been sufficiently proven that there have been such changes (2003: 106). He even goes as far as to claim that, in his eyes “there are no principles of history; history is an epiphenomena and time is immaterial” (2003: 121).

Exactly what Lightfoot means by the quote above could be discussed at length. However, as I understand it he means that things simply happen and it is only as we look back that we can try to see patterns and that is when we start to think that we can see principles at work causing things to happen. There are no real principles at work. History as such therefore also is not a phenomenon, but only something that we can see when we look back and try to clarify exactly what happened.

Another linguist who has tried to work on grammaticalisation from a more formal perspective is Elly van Gelderen. She has written papers on grammaticalisation in Old English (e.g. van Gelderen, 2000) and her paper at the NRG 2 conference dealt with the change when a phrase becomes reanalysed as a functional head (van Gelderen, 2002).

7.2 Lesser Known Linguists Working on Grammaticalisation

We shall now turn to some of the other names of grammaticalisation theory and primarily the less known linguists who have worked in the field, which will serve to give us some idea of how popular this concept is in the different regions I have concentrated on in this study. Below I will primarily try to give some idea of what kind of work, and how much work that has been done on grammaticalisation in Britain, the United States and Scandinavia.

As may have been gathered from the sections above Britain has not played a very prominent role in grammaticalisation studies, however every now and then a British scholar has made his/her voice heard on the matter. The most well-known of these cases have been discussions of grammaticalisation from what can be called a formalist or generativist perspective, rather than the more common functionalist and often typological viewpoints. Some other rather well-known British commentaries on grammaticalisation have concerned the so called unidirectionality hypothesis, e.g. Vincent's paper on regrammaticalisation / exaptation (Vincent, 1995). And more recent papers by Vincent and his colleagues in Manchester (Börjars, Eythórsson and Vincent, 2002, 2003).

With the recent increase in the interest in grammaticalisation studies Britain has also attained its fair share of linguists working in this field. In Edinburgh two scholars, at least, are working on the grammaticalisation of discourse strategies, Miriam Meyerhoff and Isabelle Buchstaller. Meyerhoff was until a couple of years ago working in the States, and Buchstaller, who is originally from Germany, was her PhD student there and moved with her to Edinburgh. Their main area of interest has been the development of quotative constructions, where Meyerhoff has primarily looked at a small language (Bislama) on an island close to New Zealand where she is originally from (see e.g. Meyerhoff and Niedzielski, 1998), and Buchstaller is writing her PhD thesis about the development of *like* and *be going to* in American and British English (see e.g. Buchstaller, forth).

Both of these linguists have clearly been strongly influenced by the discourse school and in Buchstaller's largely synchronic work one can also see an interest for

cognitive grammar which she has partly made use of in her network structure representations of varied uses of *like* and *be going to*. Both of them also believe that grammaticalisation is not unidirectional.

Jim Miller, also in Edinburgh, has shown an interest in aspects of grammaticalisation similar to those discussed by Bybee and Dahl (Bybee, 1985; Bybee and Dahl, 1989; Dahl, 1985) in that his main interest lies in the expression of aspect. On this he also presented a paper at the *New Reflections on Grammaticalization 2* conference (Miller, 2002).

7.2.1 *Ways of Appreciating the Popularity of Grammaticalisation*

As one way of trying to appreciate how much work that is being done on grammaticalisation in Britain, Scandinavia and the United States I would like to have a closer look at the list of participants from the *New Reflections on Grammaticalization 2* (NRG2) conference in the Netherlands in 2002. Although we cannot be sure of how representative the sample of linguists who attended the conference actually was, it is interesting to see if the three regions I have looked at were represented seeing as some of the big names in recent debates around grammaticalisation were there and the first conference in this NRG-series had become quite well known by the time the second one took place.²⁶¹, ²⁶²

There were quite a few people from Britain who attended and presented papers, Gregory D. S. Anderson (2002), Debra Ziegeler (2002) and John Payne (2002), Payne has published on similar topics before, for instance, in a paper on *suffixaufnahme* (Payne, 1995). Other linguists active in Britain who presented papers were Svetlana Kurtes (2002), Jacqueline Visconti (2002), and Sheila Watts (2002). And as already mentioned, Miller (2002) also presented a paper, as did Thorhallur

²⁶¹ There were quite a few famous linguists who have worked grammaticalisation that attended NRG1. Participants from the United States were: Marlyse Baptista, Philippe Bourdin, Joan Bybee, Wallace Chafe, Marianna Chodorowska-Pilch, Genevieve Escure, Talmy Givón, Jennifer Hayes, Paul Hopper, Minju Kim, Jurgen Klausenburger, Jordan Lachler, Hyo Sang Lee, Douglas Lightfoot, Carol Lord, James MacFarlane, Marianne Mithun, K. Aaron Smith, Sung-Ock Sohn, Soteria Svorou, Liang Tao, Rena Torres Cacoullos, Elly van Gelderen.

Participants from Britain were: Guy Deutscher, Diana Lewis.

Participants from Scandinavia were: Karin Aijmer, Gisle Andersen, Lena Ekberg, Christer Platzack, Anna-Brita Stenstrom, Ljuba Veselinova.

²⁶² NRG 3 is planned to take place in Santiago de Compostela in 2005 – see <http://www.usc.es/ia303/Gamma3/NRG3.htm>.

Eythorson who presented a paper he had co-written with Kersti Börjars and Nigel Vincent (Börjars et al., 2002).

Most of the linguists who are well known in the field of grammaticalisation come from either the United States or from Germany. But how many lesser known linguists are there in the United States, for instance, who are working on grammaticalisation? Among the linguists active in the States who participated in the conference were Bridget Drinka²⁶³, Mirjam Fried (2002), Hyo Sang-Lee (2002), Doug Lightfoot (2002), Eve Ng (2002), Jung-ran Park (2002), Reijirou Shibasaki (2002), Christopher M. Stevens (2002), Soteria Svorou (2002), Jennifer Ann van Vorst (2002), and Kendra J. Willson (2002).

There were also some Scandinavian scholars who presented papers on aspects of grammaticalisation, for instance Bettina Jobin (2002), Marika Lagervall (Lagervall and Propst, 2002), Henrik Rosenkvist (2002), Hans-Olav Enger (2002), Jan Terje Faarlund (2002a), Jens Nørgård-Sørensen (2002), Lene Schøsler (2002). However, half of them have not yet published on the topic.

Bettina Jobin, is doing her PhD at the German department at Stockholm University on *Genus-grammatische und semantische Aspekte von Personenbezeichnungen im Deutschen und Schwedischen*. Within this study grammaticalisation has played quite an important role, for instance, she presented a paper at NRG2 on the “grammaticalization of a derivational suffix”, with a comparison of German *-in* and Swedish *-inna, -ska* (2002). Her references included both Scandinavian and non-Scandinavian sources on grammaticalisation, e.g. Dahl (1991), Haspelmath (1999), Lehmann (1982 [1995]; 1991).

Marika Lagervall is doing a PhD at the Department of Swedish language at Göteborg (Gothenburg) University, on the topic of modal verbs in Swedish. Unfortunately, she has not yet published anything on grammaticalisation, but she also presented a paper in Amsterdam, together with Ron Propst, about changes concerning Scandinavian modals (Lagervall and Propst, 2002). Their references include Bybee et al (1994) and, Lehmann (1982 [1995]), two of the most common sources on grammaticalisation. The paper also includes some references to auxiliary verbs, both Scandinavian and non-Scandinavian, which perhaps may involve some discussion of something similar to grammaticalisation.

²⁶³ Drinka did not present a paper.

Henrik Rosenkvist, a PhD student at the Department of Scandinavian languages at Lund University, presented a paper on the Swedish conditional *hvar* (Rosenkvist, 2002), the development of which is one of the things he is studying for his PhD. His references included Hopper and Traugott (1993), Roberts and Roussou (1999) and Tabor and Traugott (1998) which are all important texts in grammaticalisation studies. It is notable that he includes one of the early texts on the formal approach to grammaticalisation (Roberts and Roussou, 1999). However, seeing as he works in a formal paradigm it is not very surprising.

Since Rosenkvist is looking at Swedish conditionals and conjunctions it must also have been natural for him to look at Bjerre's work on conjunctions (1935), which he also includes in his list of references. As we saw above (section 6.4.5), Bjerre was aware of some of Meillet's work on grammaticalisation. He also discussed a process of *mechanisation* which Rosenkvist (p.c.) believes bears some resemblance to grammaticalisation, and which I noted above seems to relate most closely to a part of grammaticalisation, which is sometimes referred to as *conventionalisation* or *routinisation*.

The references in the works of all three of these PhD students show a knowledge of both Scandinavian and non-Scandinavian linguistic sources, but they all clearly rely more on non-Scandinavian sources when it comes to their discussion of grammaticalisation. Only one of them (Rosenkvist 2002) shows any knowledge of early twentieth-century Swedish sources on grammaticalisation.

7.2.2 Grammaticalisation Studies in Scandinavia – How has the Concept Fared in Smaller Research Communities?

Compared to Britain it seems slightly easier to find work on grammaticalisation in Scandinavia where this field has really started to flourish in the last few years. When Elsie Wijk-Andersson (1939-1997) published a paper on grammaticalisation in 1997, with examples from Swedish (Wijk-Andersson, 1997), she relied heavily on Hopper and Traugott (1993). The references to earlier Swedish contributions to the study of the rise of constructions to express grammatical relations were not many at all. Some were however included in the list of references, but they seemed to disappear in the actual paper. These included Karl Gustav Ljunggren (1906-1967) (1936), Wellander

(1964; 1968), Elias Wessén (1968 [1995])²⁶⁴, and there were also some references to early German writings on similar topics: Gabelentz (1891), Humboldt (1891) and a French reference to Meillet (1912).

This paper made me wonder whether there were any earlier Scandinavian traditions in this area of linguistics. The few references seemed to indicate that there had been some interest at least, but it was hard to say to what extent even the scholars referred to had shown an interest in the origin of grammatical forms. As I found out, and as we have seen in the last two chapters (chapters 5 and 6) there had been some work on issues similar to grammaticalisation, and some have treated what could be seen as part of grammaticalisation.

Perhaps the first scholar in Scandinavia to start talking about grammaticalisation in what we could call the ‘post-revival’ period, starting in the 1980s, was Dahl. He also made a name for himself not only in Scandinavia but also internationally and he is therefore referred to relatively often in studies of grammaticalisation, at least when these concern tense and aspect, which were his main concern (cf. section 7.2).

It is however, during the 1990s that grammaticalisation really starts to become popular in the world and so also in Scandinavia. Although the interest does not seem to have been immense, there appears to have been close to a paper a year at least published in Sweden on the topic and some also in the other countries, but I have found few publications by Scandinavian scholars internationally.²⁶⁵ The only Scandinavian grammaticalisation studies that seem to have been published abroad, as far as I know, apart from Dahl’s are studies by Anju Saxena (e.g. (1995) in *STUF*), Mats Eriksson ((1995) in *Language Sciences*), Lena Ekberg ((1993b) in *The Belgian Journal of Linguistics*), Maria Koptjevskaja-Tamm ((1997) in *Rivista di Linguistica*) and Jan Terje Faarlund (2002a, 2003b) in a monography from John Benjamins and in a paper in the proceedings from International Conference on Historical Linguistics (ICHL) 2001.²⁶⁶

²⁶⁴ Although Wijk-Andersson refers to the original 8th edition and not the reprint and she also says that it is from 1969, however the 8th edition appears to have been published in 1968.

²⁶⁵ And since most of the publications were in Scandinavian languages and published in Scandinavian journals, even if people got hold of the paper they probably would have had some problems understanding it.

²⁶⁶ I have checked the MLA bibliographical database for all of the people mentioned and there only Saxena, Dahl and Eriksson appeared to have published internationally on the topic of grammaticalisation.

Another Swede who has recently worked a bit on grammaticalisation, and more specifically on the question of whether grammaticalisation is unidirectional or not, whether degrammaticalisation exists, is Kersti Börjars. However she works in Manchester, England, and not in Sweden and has been working there for many years and has never actually worked for a longer period at a Swedish university until recently when she spent some time at Gothenburg University (2002). She has however kept some connections with the Scandinavian research community, for instance she served as president of the Nordic Association of Linguistics for a period.

Interestingly, linguists in Denmark have recently (2001-) set up what they call the grammaticalisation network (*Grammatikaliserings netværket*) and they appear to be very interested in the subject, even though there does not seem to have been that many publications on grammaticalisation by Danish scholars so far. However, publications now seem to be starting to appear, a couple of years after the network was initiated. The network is for instance now preparing a joint volume edited by Lars Heltoft and Lene Schøsler. Some work has also appeared before by e.g. Heltoft and Schøsler.

Interestingly, Norway has also started to publish more on grammaticalisation recently, with Jan Terje Faarlund being one of the people who seems to be encouraging people to publish on this subject in Norway. Recently he edited a volume called *Språk i Endring* (Faarlund, 2003a), where most of the papers treated various aspects of grammaticalisation in Norwegian and partly also in the other Scandinavian languages.

There has also been at least one conference on grammaticalisation arranged in Sweden. This conference focused on grammaticalisation research with the help of corpora and was held at Växjö University in April 2001. Corpus research is another branch where we find a scholar who is active in Sweden who has shown an interest in grammaticalisation and also attracted some attention internationally, viz. Merja Kytö (1953-) who is a Finnish linguist active at the English Department at Uppsala University. Together with Matti Rissanen and Kirsi Heikkonen she edited a volume called *Grammaticalization at Work: Studies of Long-term Developments in English* (Rissanen et al., 1997). However, she does not appear to have published anything else on grammaticalisation and also in the volume she co-edited there is nothing that she wrote herself. Although, she is currently preparing an article together with

Romaine on the English construction *like to* which is likely to include some comments on grammaticalisation (Kytö p.c.).

Let us now take a closer look at some of the linguists who have worked on grammaticalisation in recent years but who have not become all that well known. Wijk-Andersson (1997), who was already mentioned briefly above, gives a good and informative Swedish introduction to grammaticalisation, which she defines as:

Grammaticalisation means that a lexical unit becomes a grammatical unit, or in other words that an autonomous content word changes into a particle or an affix.²⁶⁷ (Wijk-Andersson, 1997: 19) (translation)

In addition Wijk-Andersson (1997) also sees fit to mention something about the history of the concept. Like many others she sees the term as stemming from Meillet (1912), but the concept is seen as older, as others have also noted (Harris and Campbell, 1995; Hopper and Traugott, 1993; Lehmann, 1982 [1995], etc.). She mentions that Humboldt discussed a similar topic in 1825.²⁶⁸ Humboldt discusses something similar to grammaticalisation in relation to the development of languages through different stages – much like those often called isolating, agglutinating, flexional (cf. Humboldt, 1825 [1905]; Wijk-Andersson, 1997). Wijk-Andersson (1997: 20) also mentions Gabelentz's thoughts about two competing tendencies that result in a cyclic phenomenon of grammaticalisation, as they have been by Hopper and Traugott (1993) and Lehmann (1982 [1995]), etc.

In addition, as mentioned above, Wijk-Andersson (1997) discusses the work on grammaticalisation by one earlier Swedish linguist, namely Erik Wellander (cf. 6.4.2). She interprets Wellander's view of grammaticalisation as referring to something that "starts with lexical words losing their concrete content" (1997: 21).²⁶⁹ And in a brief treatment of how the Germanic noun **lika* has developed into a suffix in the following stages, **-lika > lik(er) -lig*, she refers to another Swedish linguist, Walter (Alvar) Åkerlund (1902-) (1929), who it therefore seems possible may also have had a sense of something similar to grammaticalisation (Wijk-Andersson, 1997: 22).

²⁶⁷ Original: Grammatikaliserings innebär att en lexikal enhet övergår till att bli en grammatisk enhet, eller med andra ord att ett självständigt innehållsord blir till en partikel eller ett affix. (Wijk-Andersson, 1997: 19)

²⁶⁸ However, unlike Wellander (see section 6.4.2) she does not mention that he is to have called it *grammaticalisation / Grammatikalisierung*.

²⁶⁹ Original: "börjar med att lexikala ord förlorar sitt sakinhåll" (1997: 21)

Like Hopper and Traugott (1993) Wijk-Andersson (1997) discusses grammaticalisation in terms of reanalysis, decategorialisation, analogy, clines, divergence, renewal, semantic weakening, phonetic reduction, persistence, metaphor, metonymy (incl. subjectification), gradual etc. The difference is that she primarily gives Swedish examples and she also attempts to think of Swedish terms for all the concepts. The whole purpose of the paper. In fact it seems to be an introduction to this phenomenon for Swedish linguists who may have overlooked it. In order to make it both easier to grasp and more interesting Wijk-Andersson provides Swedish examples. It also serves the Swedish research community in the sense that it provides us with Swedish terminology for the various aspects of grammaticalisation.

Apart from the mention of Wellander it is rather unclear how the Swedish linguists that Wijk-Andersson (1997) mentions from before 1980 looked upon the changes which we would now see as grammaticalisation, whether or not they had a similar concept. Her references to them consist of citations of their examples, which she chooses to use to illustrate various processes that have been seen as part of grammaticalisation. In other words, she certainly does not rely on other Swedish linguists in her sense of grammaticalisation and study thereof, for this she relies on Hopper and Traugott (1993).

However, the 1997 paper was not the first time that Wijk-Andersson discussed grammaticalisation. She also mentioned this term and concept rather briefly in her thesis (Wijk-Andersson, 1991), although only to say that Sw. *bara* ‘only’ could be seen as an example of what Meillet called *grammaticalisation*. She gives no reference to any specific publication by Meillet, and she does not say where she got this information from, although she goes on to say that Haskå (1988) and Lehti-Eklund (1988; 1990)²⁷⁰ have also used this term and concept in their descriptions of Swedish examples of language change.

In her thesis, Wijk-Andersson (1991: 187, 192) also uses clines similar to those used in the grammaticalisation literature. Although, she does not explicitly say that they are illustrations of grammaticalisation, using that term. Instead, they are simply used to show the continuum and development of the use of Sw. *bara* ‘only’ from a fuller meaning to something purely functional, and from a marked part of speech to a functional particle.

²⁷⁰ It is not absolutely clear whether she refers to both publications or just one of them.

Wijk-Andersson (1991) discusses grammaticalisation briefly without any reference to the biggest names in grammaticalisation theory (apart from Meillet), but instead with reference only to two scholars who have worked on the Swedish language and presented their work in Sweden (Haskå and Lehti-Eklund), and one international scholar who has mentioned grammaticalisation with regards to Chinese, but whom she refers to not in relation to grammaticalisation but only elsewhere (Shi, 1989). It therefore seems like she did not first learn about grammaticalisation from the American linguists. Instead she seems to have learnt about it first from Swedish and Finnish linguists (Lehti-Eklund works in Finland). However, the fact that she a few years later picked up the topic of grammaticalisation again, and more thoroughly this time, indicates that she felt that Swedish linguists were not really familiar with the concept and she then used American resources primarily to clarify the notion to her contemporaries in Sweden.

Lena Ekberg's (1954-) (1993b; 1993a) papers on the metaphorical and grammaticalised uses of the Swedish verb *ta* 'take' discusses grammaticalisation in a sense which to many may in some of the examples come closer to lexicalised or fossilised. Ekberg (1993a: 105) also uses the term *lexicalised* herself in the meaning of syntactically and semantically frozen, which she sees as a parallel process to grammaticalisation. For instance, the bold part in, *Hon hade äntligen **kommit till insikt om** hans svek* ('She had finally realised that he had betrayed her', the highlighted part literally means 'come to insight of') is seen as both lexicalisation and grammaticalisation (Ekberg, 1993a: 105). This is an example of a specific *aktionsart* marked by verbs and prepositions, a phenomenon which she also claims to have treated in Ekberg (1989). So Ekberg believes that lexicalisation can occur together with grammaticalisation (see e.g. 1993a: 126, 127 fn.), and she thinks that some of her examples show both lexicalisation and grammaticalisation. The reason she has for seeing them as grammaticalisation is that verbs like *komma* 'come' and *ta* 'take' can develop a use where they specify *aktionsart* (see also Ekberg, 1989). Admittedly, such a change should count as grammaticalisation.

It is of course true that the development of means of expressing *aktionsart* could be seen as examples of grammaticalisation, but in some of Ekberg's examples it may be difficult to see why they should express *aktionsart*. However, Ekberg (1993a: 114) also states that her examples are meant to show that grammatical and metaphoric senses of a word can be very close to one another and difficult to draw a

sharp dividing line between and this may explain one's hesitation. Her examples include (where G=grammatical, and M = metaphoric):

ta ett beslut (M) lit. 'take a decision', i.e. make a decision
ta någon med hem (M) lit. 'take someone with home', i.e. 'take someone home with you'
ta bussen (M) (lit.) 'take the bus'
ta beslut (G) lit. 'take decisions', i.e. make decisions
ta och gifta sig (G) lit. 'take and marry oneself', i.e. 'go and get married'
ta emot ett råd (M) lit. 'take against/towards INDEF.ART. advice', i.e. 'accept advice'
det tog honom tio minuter (M) (lit.) 'it took him ten minutes'
bussen tar 75 passagerare (M) (lit.) 'the bus takes 75 passengers'
ta råd (G) (lit.) 'take advice'
ta det förnuftigt (G) (lit.) 'take it sensibly'
(Ekberg, 1993a: 114)

Of these, I have problems seeing the grammaticalisation in *ta beslut*, *ta råd*, *ta det förnuftigt*, which are similar to some of Wellander's examples of semantic changes (see section 6.4.2) and I also am not sure whether they should be classed as grammaticalisation in the sense of (1) or (2) in section 1.1, if they were to be classed as grammaticalisation. Still, looking at Ekberg's definition of grammaticalisation her understanding of the process seems similar to that of Meillet (1912) and others who have discussed grammaticalisation:

Grammaticalisation of a lexeme means that this develops from a content word into a grammatical element which does not primarily relate lexical content but instead structures such content in different ways (see Talmy 1988 [(Talmy, 1988)]).²⁷¹ (Ekberg, 1993a: 109) (translation)

cf. Meillet (1912):

... le passage d'un mot autonome au rôle d'élément grammatical.²⁷² (Meillet, 1912; 1921: 131)

A few lines in Ekberg (1993a: 109) also explicitly mentions that grammaticalisation involves a change from more autonomy to less autonomy, just as Meillet (1912) said that it involved the development of an autonomous word into a grammatical element. She recognises that the element tends to be restricted syntactically and sometimes even go through affixation.

²⁷¹ Original: "Grammatikaliserings av ett lexem innebär att detta utvecklas från ett innehållsord till ett grammatiskt element som inte i första hand förmedlar lexikalt innehåll utan strukturerar ett sådant innehåll på olika sätt (se Talmy 1988)." (Ekberg, 1993a: 109)

²⁷² Translation: "the passing of an autonomous word into the role of a grammatical element"

Part of the problem in discussing Ekberg's views seems to be what she means by *aktionsart* which is not absolutely clear. *Aktionsart* can be seen either as a lexical class such as stative verbs, for instance, but it can also be seen as the manner in which a verbal action is performed or takes place, close to the meaning of *aspect* and this is possibly the more common understanding of the term in Swedish. The latter meaning also seems to fit quite well with most of her examples.²⁷³

The main aim of the paper by Ekberg (1993a) is to show that the different uses of, for instance, the verb *ta* 'take', are connected in a form of network, a cognitive network or a radial structure as used in cognitive semantics (1993a: 106).²⁷⁴

The references in Ekberg's (1993a) paper are primarily non-Swedish. She refers to several of the well-known figures in discussions of grammaticalisation: Heine and Reh (1984), Lehmann (1982 [1995]; 1985a),²⁷⁵ Romaine and Lange (1991), Sweetser (1988; 1990), Thompson and Mulac (1991), Talmy (1988) and Traugott (1980; 1982; 1986b; 1986a; 1988). However, there are also some Swedish scholars mentioned in the bibliography, such as Anward and Linell (1976) on lexicalisation, some of Ekberg's own earlier work including the one from 1989 which includes a discussion of the use of various verbs to mark what she calls *aktionsart*.²⁷⁶ She also makes reference to Eriksson (1992) which will be discussed below, and one publication by Lehti-Eklund (1990), who I mentioned earlier.

Since Ekberg (1993a) is very interested in the semantic and cognitive aspects of grammaticalisation, it is hardly surprising that she shows much reliance on Traugott's work from the 1980s. For instance, she calls attention to Traugott's discussion of the propositional, textual and expressive 'components' (1993a: 109). Naturally, the two tendencies in grammaticalisation that she chooses to mention first is loss of lexical meaning and that the sense tends to gain in abstractness (1993a: 110), the latter of which is an aspect of grammaticalisation which has been questioned more recently (e.g. Meyerhoff and Niedzielski, 1998).

Furthermore, Ekberg (1993a: 110-111) remarks that Lehmann (1982 [1995]; 1985a) and Heine and Reh (1984) have listed a number of criteria that can be used to

²⁷³ The two terms *aspect* and *aktionsart* are clearly not always used to label distinct phenomena and when they do label distinct phenomena there is very clearly different means of describing these and delimiting them (for further discussion see e.g. Enger, 2001).

²⁷⁴ She bases her analysis on Lakoff (1987) and Langacker (1987; 1991).

²⁷⁵ Ekberg's reference is actually to the original 1982 edition.

²⁷⁶ Ekberg claims that the paper from 1993 is the only paper she has written which explicitly discusses grammaticalisation, although there are some others that come close and may be of interest to grammaticalisationists. (Ekberg p.c.)

judge whether grammaticalisation has occurred. She goes on to give the reader a brief introduction into what those criteria are. She also relates some of Heine and Reh's (1984) criteria to her example *ta* 'take', namely desemanticisation, expansion, cliticisation, and 'sammansmältning'. In addition, she (1993a: 111) notes the fact that Eriksson (1992) has asserted that it can be difficult to use these criteria in cases of grammaticalisation that have not yet run their full course, and that there are certain criteria which are not suitable for the discussion of free grammatical morphemes.

Mats Eriksson (1961-2001) picked up the topic of Swedish *bara* 'only', discussed in Wijk-Andersson's thesis (1991), again in an interesting study in the early 1990s (1992). This study has references to many of the key works on grammaticalisation that are still referred to today: Hopper (1991), Brinton (1988), Heine and Reh (1984), Heine et al (1991a), Lehmann (1982 [1995]; 1985a)²⁷⁷, Traugott (1982; 1986b; 1989) and Traugott and Heine (1991 eds.). However, the paper does not refer to a single study on grammaticalisation from Sweden, apart from a reference to Wijk-Andersson (1991) which includes at least a brief comment on grammaticalisation, as noted by Eriksson (1992: 4), but which should be seen as primarily being mentioned due to its discussion of *bara*. Furthermore, to no one's surprise, one can mention that the English version of Eriksson's paper from 1995 is no different (Eriksson, 1995).

Still, it should be said that these two papers (Eriksson, 1992; 1995) are interesting and make good use of the contemporary ideas regarding grammaticalisation, approaching the topic in a way comparable to the present-day international tradition of grammaticalisation. Eriksson looks at the use of *bara* in the contemporary speech of teenagers and compares it to some issues discussed by e.g. Hopper (1979), viz. the issue of a foregrounding element in language; and Herring (1991), Tannen (1986) and Romaine and Lange (1991) on the issue of the development of quotatives.

Eriksson does not only claim that he can see a process of grammaticalisation in the development of the adverb *bara* 'just / only' into a foregrounding element, but he also claims that the further development, or *desemanticalisation* (sic.), into a quotative goes in the opposite direction to that predicted by Traugott's papers (1982; 1989), thereby providing a possible counterexample to the unidirectionality

²⁷⁷ Eriksson's reference is actually to the 1982 edition of Lehmann's monograph, but I myself have only had access to the 1995 edition.

hypothesis. He (1992: 24-25; 1995: 43) believes that this linguistic item shows a development not only from propositional > textual > interpersonal,²⁷⁸ but also a development from interpersonal (the foregrounding marker) to textual (the quotative).

In both articles (1992; 1995) Eriksson includes a section where he looks at the development of *bara* in relation to certain grammaticalisation criteria which he finds applicable to free grammatical items, e.g. frequency (including obligatorification), desemanticalisation, expansion, (phonetic) reduction, distribution changes (including fixation). He concludes that the item seems to qualify as grammaticalisation according to all of these criteria, but that the phonetic reduction is not definitely related to grammaticalisation in this example since it can also be found in more conventional uses of *bara*.

Eriksson explains the term *grammaticalisation* in a rather typical way:

The term grammaticalization was, according to Traugott and Heine (1991[a]), originally used for the kind of language change phenomena whereby a lexical item developed into a grammatical one. More recently researchers have used the term to refer also to processes that make already grammatical morphemes receive other or more grammatical functions (Lehmann 1985[a]: 303, Heine and Reh 1984: 15).

A strict delimitation of the term is problematic, since it is hard even to decide what is and is not part of the grammar of a language. [...]

[...] Moreover, grammaticalization is a very complex process with different phases: lexical elements develop into free grammatical morphemes, which are receiving new and more grammatical functions and may eventually end up as suffixes tied to lexical elements. Most criteria, however, have relevance only to certain parts of this process (Hopper, 1991: 21). (Eriksson, 1995: 20-21)

However, while being rather typical it is also a very insightful presentation of the definition which emphasises the differences in its use and the problems involved in finding a good definition.

We turn now to another Swedish linguist, Björn Hammarberg, whose main paper on grammaticalisation appears to be one on comparatives (1995). However, recently he published another two papers which were based on L2 Swedish data and dealt with the grammaticalisation of possession, but grammaticalisation in the acquisition of a second language (cf. Hammarberg, 2003), a sense of

²⁷⁸ This is one of the ways in which grammaticalisation has been considered unidirectional, cf. e.g. Traugott (1982, 1989).

grammaticalisation different to that which I am primarily interested in here) (Hammarberg, Håkansson and Martin, 1999; Hammarberg and Koptjevskaja-Tamm, 2002, 2003). His main interest also lies in language acquisition and Swedish as a second language.

Hammarberg's study which appeared in 1995 includes references to many of the most well known publications in the field of grammaticalisation: Heine et al (1991a), Heine and Reh (1984), Hopper and Traugott (1993) and Lehmann (1985a). He only has the odd Swedish reference, only one of which seems likely to include some references to grammaticalisation, namely Roger Källström (1993 [1990]).²⁷⁹

Like Eriksson (1992; 1995), for instance, he also goes through some of the characteristics of grammaticalisation and looks at the Swedish comparative *jämfört med* 'compared to' from all of those different aspects, to see whether it qualifies as grammaticalisation. In addition, he gives a definition of grammaticalisation which is close to the default definition:

Grammaticalisation is the diachronic process in language, through which grammatical morphemes are developed from lexical [morphemes].²⁸⁰ (Hammarberg, 1995:43) (translation)

Apart from this definition, the aspects of grammaticalisation that Hammarberg mentions are that it is a gradual change, that tends to be accompanied by increased frequency, phonological reduction (or attrition as it is usually called in the grammaticalisation literature), that morphological variation tends to disappear, it tends to be reanalysed syntactically and he also claims that grammaticalisation means a change in grammatical category (Hammarberg, 1995: 43-45). However, it is worth considering whether it is correct that grammaticalisation always involves a change in grammatical category or if this is only true when we try to get an overview of the whole change from the beginning to the end – when it may of course be the case that there is more than one change in category. Take the example of *going to* in English. Did the grammaticalisation only occur when it had already been reanalysed as an auxiliary, or had it then already been grammaticalised?

²⁷⁹ I have based my conclusions regarding his grammaticalisation sources on the titles as well as on where references to these texts appear in the text.

²⁸⁰ Original: "Grammatikalisering är den diakroniska process i språket, varigenom grammatiska morfem utvecklas ur lexikala." (Hammarberg 1995: 43)

Hammarberg (1995: 45-46) has also adopted the notion of clines and he speaks not only of Hopper and Traugott's (1993: 7) cline of grammaticality:

content item > grammatical word > clitic > inflectional affix

but also of their cline of categoriality (1993: 103f.) – which once again brings up the change in grammatical category:

major category (>adjective / adverb) > minor category.

Furthermore, to some extent, he (1995: 46) goes into the motivation behind the possible example of grammaticalisation that he discusses (*jämfört med*). Although as in all other more general comments regarding grammaticalisation in his paper he relies heavily on Hopper and Traugott (1993). He also mentions that grammaticalisation has been related to two tendencies in language, which tend to alternate: the need for routines and the need for renewal, both of which he looks at briefly from the point of view of the Swedish comparative *jämfört med*.

Hammarberg also believes that grammaticalisation is unidirectional and can only lead to more grammaticalised structures, and cannot be reversed. However, of course this does not necessarily mean that it has to go from lexical to grammatical, although that appears to be what he means. He recognises that grammaticalisation usually involves a form of routinisation, where the speaker and listener both benefit from trying to use certain phrases and constructions frequently in the same context. He also notes that this may lead to semantic weakening. However, he points out that in the case which he is discussing, grammaticalisation has in fact led to a clarity which may be appreciated in comparison to the use of the Swedish comparative *än* 'than' (1995: 46-47).

Anju Saxena (1959-) did her PhD as one of Givón's students at the University of Oregon. Givón having been seen as one of the people who reintroduced grammaticalisation into linguistics, it is not surprising to see that one of his students should have shown an interest in the same. Saxena has an interest in typology (primarily Tibeto-Burman languages), discourse and more generally in functional linguistics which also could be seen as indicating a high possibility of an interest in grammaticalisation. For many years now she has worked at the Department of Linguistics at Uppsala University in Sweden, however, her interests lie mainly in topics that are primarily discussed in international circles (rather than Scandinavian).

Her main references on grammaticalisation (including references that may include discussions of grammaticalisation even if these are not referred to in Saxena's work) tend to be: Bybee et al (1994), Bybee and Dahl (1989), Dahl (1985; (1994)), Givón (1979a), Heine and Reh (1984), Heine et al (1991a), Hopper and Traugott (1993), Kuryłowicz (1965 [1975])²⁸¹, Lehmann (1987), Lord (1976), Matisoff (1969; 1972; 1976), Sweetser (1988), Traugott and König (1991), Traugott (1982), Traugott and Heine (1991b ed.), Vincent (1995).²⁸²

The earliest reference that I have seen in her work that treats something similar to grammaticalisation is Waley and Armbruster (1934), which has already been discussed in chapter 6, and also perhaps Hu Shih Wen Tsun or Wu Shih-ch'ang²⁸³ (1933) and last but not least Sapir (1921)²⁸⁴ who we know was interested at least in the end result of grammaticalisation. Furthermore, she has a reference in her bibliography to a paper in Meillet (1958) which is a reprint of Meillet (1921), and which includes his famous paper on grammaticalisation (Meillet, 1912). Although, I have seen no reference to this particular paper in her work.

The reference to Wu Shih-ch'ang and one to Hu Shih Wen Tsun appears to stem from Waley and Armbruster (Waley and Armbruster, 1934). However, their reference to Hu Shih Wen Tsun gives no title and Saxena (Saxena, 1995) refers to the title of Wu Shih-ch'ang's work, that Waley and Armbruster mention. Although it is only Hu Shih's treatment of *yen* 'to speak' that is referred to in the paper by Waley and Armbruster. He is said to have claimed that *yen* has three different uses, two kinds of conjunction and a pronominal usage (Waley and Armbruster, 1934: 573).

Saxena also has the odd reference to Swedish linguists, such as to Dahl's work. Dahl, who is rather well known internationally, and whom Saxena has also had some contact with in Sweden (Saxena p.c.). Apart from that the few Swedish references usually do not seem to be works that include views on grammaticalisation.

It is very clear that Saxena holds a unidirectional view of grammaticalisation (see e.g. Saxena, 1995). Her work on grammaticalisation has been on the development of Tibeto-Burman languages, and she has devoted her attention primarily to quotatives, but has also considered spatial and temporal expressions and

²⁸¹ Saxena's reference is to the original edition from 1965.

²⁸² Saxena's reference says Vincent 1993 since the paper was presented in 1993, but she refers to the publication and this came out in 1995.

²⁸³ A misprint in Saxena refers to Hu Shih Wen Tsun as the author but the title is that of a work by Wu Shih-ch'ang.

²⁸⁴ Saxena's reference is to a reprint from 1971 published by Rupert Hart-Davis, London.

verbal morphology, for instance (Saxena, 1995; 1997). She has worked within the frame of typology and historical linguistics, but possibly with slightly more interest in the former, which also shows in her references which tend to be from the functional, typological school.

It is possible that Saxena (1998) should also be seen as treating something bordering on grammaticalisation. In that paper she looks at the relation between spatial and temporal terms in Kinnauri and attempts to relate these meanings within a cognitive linguistic framework. She does not explicitly discuss grammaticalisation in this paper, but in discussing the relation between spatial and temporal words in the language, the notion of grammaticalisation could possibly lie at the back of the minds of both the reader and writer. However, it should be noted that the references in this paper are very few and none of them are distinctly concerned with grammaticalisation. The paper also does not discuss the notion explicitly, and the term is not used a single time.

Another Swedish linguist with an interest in typology and grammaticalisation is Maria Koptjevskaja-Tamm (p.c. 2003) who has worked at Stockholm University since she began doing her PhD in 1981. However, she first studied at Moscow University. She then moved to Sweden, where she got to know Dahl who liked her work and offered her a PhD position (Koptjevskaja-Tamm p.c.).

Koptjevskaja-Tamm has written a few papers which have touched on grammaticalisation since the late 1990s, and as mentioned above she has also published papers together with Hammarberg that treat grammaticalisation during acquisition (Hammarberg, 2003). Most of her papers show an interest in possessives, the Baltic region and typology – more so than they show her interest in grammaticalisation. Notably, she normally does not describe grammaticalisation, or try to explain it, or look at changes based on different criteria which may make them count as grammaticalisation. Instead, she simply uses the terms *grammaticalisation*, *grammaticalised* occasionally in the flowing text, as a description in itself of the particular example that she is looking at (forth. a; forth. b), in other words a typical example of how grammaticalisation can be used as a ‘tool’ in linguistics.

Koptjevskaja-Tamm's references include both Swedish and non-Swedish sources on grammaticalisation, but once again the non-Swedish sources are much more frequent, however interestingly they do not include any of the more common references on grammaticalisation apart from Givón's early treatment (Givón, 1979a).

The references include some of her own work, Koptjevskaja-Tamm (1979a; 2001; forth. a; forth. b), but also Norde (1997), Givón (1979b), Plank (1991), Qvonje (1980), Eksell Harning (1980)²⁸⁵, Payne (1995). There may also be references to something close to grammaticalisation in Torp (1973) and Haugseth (1983). The latter seems likely to include a notion of grammaticalisation since it discusses the development of the Norwegian s-genitive and genitive periphrasis, however there is no explicit reference to such a treatment in either of the publications in the papers by Koptjevskaja-Tamm that I have studied.

On looking at Haugseth's text I have not found anything exactly like grammaticalisation in the sense of a lexical item developing into a grammatical marker. However, Haugseth (1983: 25) does discuss how the Norwegian genitive developed from an inflection into special *s*-ending. She (Haugseth, 1983) also speaks of the the generalisation of certain prepositions in genitive usage, and the use of possessive pronouns as genitive markers. So she speaks of the development of different genitive constructions, but she never mentions the term *grammaticalisation* and she does not discuss a development from something lexical to something grammatical.

Torp (1973: 139) asks whether it could be that the *s*-genitive stems from a possessive pronoun. He reaches the conclusion that the *s*-genitive could have arisen both because the a-stem singular genitive ending was *s*, but also because *his* in Scandinavian languages, i.e. *hans* ends in *s* (Torp, 1973: 140). It seems he may have had a vague sense of something like grammaticalisation, however the paper does not use the term *grammaticalisation* and it is mainly a description of different genitives in Norwegian.

There is also a reference to the third volume of Wessén's *Schwedische Sprachgeschichte* (1970) (Koptjevskaja-Tamm, forth a). As we saw already in section 6.4.3, Wessén seems to have had a notion of agglutination, which resembles some forms of grammaticalisation or rather part of the grammaticalisation process. But her reference to Wessén is not directly related to grammaticalisation, and it seems unlikely she has been influenced by him on this point. She also has many references to Scandinavian publications from the first half of the twentieth century,

²⁸⁵ mentions the change from nouns into prepositions. (Koptjevskaja-Tamm forth. b:83).

which however do not seem to treat grammaticalisation and her references to them concentrate on dialects and linguistic variation.

There is also a reference to an early twentieth century German source, namely Brugmann and Delbrück (1911), a revised second edition of the *Grundriss der vergleichende Grammatik der indogermanischen Sprachen* volume 2.2. Although whether this work includes a notion similar to grammaticalisation, which Koptjevskaja-Tamm might have noted is not certain. Her reference to it only mentions that it deals with the fact that the ablative and genitive forms have become identical in Indo-European languages, so she does not acknowledge any treatment of something similar to grammaticalisation (Koptjevskaja-Tamm, forth b: 93).

There is also a reference to the British scholar, Henry Cecil [Kennedy] Wyld (1870-1945) (1936) and his discussion of the misinterpretation of the English genitive *-s*. However, there does not seem to be a concept similar to grammaticalisation in this publication by Wyld, which is primarily a synchronic description. In other works by Wyld (1906 [1926]; 1927 [1929]) there also does not really seem to be any clear signs of a concept of grammaticalisation, apart from in a book from the 1920s (Wyld, 1927 [1929]) where he mentions that the definite article stems from a demonstrative pronoun.

Koptjevskaja-Tamm does not usually include references to the ‘standard’ works on grammaticalisation such as Hopper and Traugott (1993), Heine et al (1991a), Lehmann (1982 [1995]) etc. In fact, they appear in neither forthcoming a nor b. There is however a reference to a paper by Lehmann (1985b) in forthcoming b, which it seems may also include some references to something similar to grammaticalisation (especially if we consider Koptjevskaja-Tamm’s footnote number 13 in forthcoming b where she claims that the grammatical sources of possessive noun phrases are **also** mentioned in Lehmann 1985b: 92) and there is also a reference to the book on possession by Heine (1997b), which certainly includes comments on grammaticalisation.

In a list of people she has had personal contact with for facts and data from different languages, Anju Saxena and Martin Haspelmath are both mentioned. She claims that she believes that she recalls reading an essay by Haspelmath which treated grammaticalisation and asking him more about this afterwards and she recalls how she wanted to do work on grammaticalisation herself after reading the article (Koptjevskaja-Tamm p.c.).

Yet another scholar in Stockholm who has touched on grammaticalisation is Päivi Juvonen who quite recently earned her doctorate with a thesis on the possible development of a definite article in Finnish (Juvonen, 2000). She claims in her thesis that a development of a definite article from a definite demonstrative determiner (which is what apparently has been suggested for Finnish as for many other languages) counts as a case of grammaticalisation. And of course many others would be quick to confirm that this is true. This is a rather common example of grammaticalisation, and looking at Juvonen's definition of grammaticalisation we see that it is almost identical to Kuryłowicz's (1965 [1975]):

... grammaticalization, the normal process through which lexical morphemes develop into grammatical morphemes, and grammatical morphemes into other, even more abstract, grammatical morphemes. (Juvonen, 2000: abstract)

The main difference is that she simply talks of a development rather than an "increased range" of a morpheme:

... the increase of the range of a morpheme advancing from a lexical to a grammatical or from a less grammatical to a more grammatical status (Kuryłowicz, 1965 [1975]: 52)

However, Juvonen also **claims** to have refined the theory. She says that:

... a refinement of grammaticalization theory, whereby a criterion of obligatory use in specific contexts is proposed as a litmus test of grammaticalization, results in a complex picture of the proposed process of grammaticalization of a definite article in spoken Finnish. (Juvonen, 2000: abstract)

I am not sure how the "criterion of obligatory use" **refines** the theory, since a criterion of obligatorification was part of Lehmann's criteria (1982 [1995]; 1985a). Furthermore, weaknesses with such a criterion have been suggested by Hopper (1991) and Hopper and Traugott (2003), as we have seen above (7.1). Juvonen (2000: 12) herself also remarks that scholars have not been able to agree on one definition of the notion of obligatorification and says that it is therefore somewhat problematic. She also notes that its usefulness in relation to grammaticalisation has been questioned by Laury (1996; 1997), all of which makes one wonder even more how she has refined the theory.

Juvonen's introduction includes a brief outline of what grammaticalisation is, with references to Dahl (forth), Heine et al. (1991a) and Hopper and Traugott (1993). She mentions the most obvious characteristics of grammaticalisation, such as phonetic reduction, loss of independence, unidirectionality, etc. (Juvonen, 2000:11). However, she also mentions that examples such as changes from preposition to noun or verb in English have been seen as examples of counterdirectional changes, but like e.g. Norde (2001a) she notes that those kinds of changes are less common than the typical movement from lexical to grammatical or from less to more grammatical (Juvonen, 2000: 11).

Interestingly, as part of Juvonen's revitalised (?) perspective on grammaticalisation, she observes that written sources have usually been used in the study of grammaticalisation which has led to a focus "on the products or the outcomes of grammatical change" (Juvonen, 2000: 12). She herself, conversely, is more interested in the processes of change, and therefore finds it more interesting to focus on the spoken varieties, where one finds more variation and so can see changes happening and being initiated (Juvonen, 2000: 12).

The references in Juvonen's thesis, as far as grammaticalisation is concerned, are mainly non-Scandinavian, e.g. Bybee (1985; 1994, Bybee and Dahl, 1989), Hopper and Traugott (1993), Greenberg (1978), Diessel (1999), Epstein (1993), Frajzyngier (1997), Givón (1979b; 1995), Heine (1997a), Heine et al (1991a), Hopper and Thompson (1980). However, there are also a couple by Dahl (Bybee and Dahl, 1989; Dahl, forth), and possibly also one by another Swedish linguist, Swedenmark (forth.). In addition, there are also some references to her own work, Juvonen (2000; forth.), Juvonen and Swedenmark (in preparation).²⁸⁶ Furthermore, there are at least a few references to linguists active in Finland and Estonia, such as Laury (1995; 1996; 1997) and Pajusalu (1997; 2000) which probably include comments on grammaticalisation from an Estonian perspective, but which I have not had the chance to consult.²⁸⁷

One of the other Scandinavian linguists who have worked on grammaticalisation is Jan Terje Faarlund, whom I mentioned above. Faarlund is active in Oslo in Norway and he has worked on the grammaticalisation of word order

²⁸⁶ Unfortunately, it does not seem as though Juvonen and Swedenmark (in preparation) and Juvonen (forth.) will be published now (Juvonen p.c.).

²⁸⁷ Since they lie outside the main focus areas of this thesis they will have to be studied at a later date.

(2002b; 2003b), possible counterexamples to the unidirectionality hypothesis (2002a; 2003c), but also the reanalysis and grammaticalisation of infinitives (2003c; forth). His views on grammaticalisation are quite formal. He appears to see grammaticalisation as a form of reanalysis, and reanalysis is seen as a change that occurs during the acquisition of a language (Faarlund, 2003c). He also clearly thinks that there are counterexamples to the unidirectionality hypothesis, such as the Norwegian infinitive marker *at* which he claims has been degrammaticalised. This he shows with examples such as that clausal adverbs can now appear between the *at* and the infinitive in Norwegian (Faarlund, 2003c), e.g. a reversal of Lehmann's bonding criteria.

Faarlund's references show that on the matter of grammaticalisation he has been influenced primarily by international scholars – Elly van Gelderen (1996), Harris and Campbell (1995), and Roberts and Roussou (1999). Clearly he is primarily been interested in the generativistic views on grammaticalisation, and he has been influenced by some linguists who view grammaticalisation as an ephiphenomen, Campbell, for instance.

Hans-Olav Enger is another Norwegian who has become quite active in grammaticalisation issues lately. Like Faarlund he took part in the NRG 2 (Enger, 2002), and he also wrote an article (Enger, 2003) for *Språk i Endring* (Faarlund, 2003a). Enger's articles have been primarily concerned with the development of the Scandinavian passives (Enger, 2000; 2001; 2003). He has tried to illustrate that partly due to the fuzzy dividing line between inflectional and derivational morphemes the Norwegian passive should definitely not be seen as a paradigm example of grammaticalisation (Enger, 2003). If he is right, then this is quite important also from a historical perspective since this is an old example of grammaticalisation which I have seen as an indication that linguists during the nineteenth century had a concept **similar** to our notion of grammaticalisation (cf. chapters 5 and 6).

Enger refines Hopper and Traugott's (1993) cline of grammaticality to include a phase when the item functions as a derivational morpheme before it may go on to become an inflectional morpheme, in order to be able to illustrate the grammaticalisation path that the passive $-s(t)$ has gone through (Enger, 2003: 44):

grammatical word > clitic > derivational morpheme > (inflectional morpheme)

He uses the claim that the passive morpheme has followed this path as **one** argument that this is an unusual type of grammaticalisation, since he believes that Hopper and Traugott does not treat this type of change nor anyone else who has written about grammaticalisation (Enger, 2003: 44-45, 46). It is also important that Enger notes that there are different types of inflectional morphemes, inherent inflectional morphemes and contextual inflectional morphemes (Enger, 2000; 2003: 49-51), a distinction which he adopts from Geert E. Booij (1993). He also claims that a derivational affix cannot become a contextual inflectional morpheme without passing through a stage of being an inherent inflectional morpheme (Enger, 2003: 51-52).

Enger's sources on grammaticalisation are a mixture of international and Scandinavian texts, although they are mainly non-Scandinavian: Bybee (1985), Campbell (2001), Helge Dyvik (1979), Haspelmath (1990; 1993), Heine et al (1991a), Heltoft (1996), Hopper and Traugott (1993), van Kemenade (1999), Lehmann (1982 [1995]), Luraghi (1998) and Nübling (1998).

The Norwegian linguist, Kjell Ivar Vannebo has recently written a paper about a Norwegian equivalent of the *ta* ('take') + *V* construction that Ekberg discussed for Swedish (see above) (Vannebo, 2003). He shows in this paper how Norwegian *ta* has developed a usage as an aspectual marker of initiation, but also that it has an expressive function, a form of emphasis which means that the agent is made to stand out (2003: 261-262). In addition, Vannebo discusses how this construction has changed its meaning but he notes that it is not quite a case of bleaching, and he also notes one can speak of metaphorical extension and reanalysis, as well as phonological reduction (2003: 264-265).

Interestingly, Vannebo goes into some alternative views of the history of this construction which have seen the *og* 'and' as the infinitive marker *at*, which is often pronounced the same way; and others which have seen this as a construction borrowed from Greek (2003: 269-271). However, he concludes that it is not a borrowing nor is it a case of a verb plus an infinitive originally (Vannebo, 2003). He also emphasises that there is no need for old paratactic constructions to disappear, as they give rise to this kind of construction, in other words there may well be splits in the history of a construction. The old construction can carry on at the same time as a new one develops (2003: 273).

The references that Vannebo make use of are both Scandinavian and international, but primarily non-Scandinavian: Abraham (2001), Bybee and Pagliuca (1985), Bybee, Perkins and Pagliuca (1991; 1994), Ekberg (1993b; 1993a), Harris and Campbell (1995), Hopper and Traugott (1993), Li and Thompson (1976), Lord (1982), Ramat (1987), Sweetser (1988), and Traugott and Heine (1991b; 1991c).

One of the most active linguists working on grammaticalisation in Denmark is Lene Schøsler. She is one of the editors who is working on the edited volume of papers that *Grammatikaliseringsnetværket* is preparing (Heltoft and Schøsler, forth). Her own papers on grammaticalisation have been concerned with French (forth; forth.) and with the issue of whether grammaticalisation can be reversed (forth.).

In one of the articles she has written she tries to show that the definite article *le, la* in French is no longer a marker of definiteness, but has developed into a number and gender marker (Schøsler, forth. a). In another article she has argued that the replacement of analytic progressive constructions by synthetic tense constructions is surprising, partly because such a change is contrary to the rules of grammaticalisation (Schøsler, forth. b). These rules of grammaticalisation, she claims, predicts a development from [a full verb of movement + a verb ending in *-ant*] developing into [an auxiliary + V2 *-ant*], after which the auxiliary progresses into being a stylistic marker or a morpheme, and then disappears. But she claims that this example reverted into the analysis as a full verb plus a verb in *-ant* form. However, Schøsler's example of degrammaticalisation does not seem quite right, it shows a form getting less popular, being less generally used. However it does not show a form clearly reversing in its development along the cline of grammaticality in any other way than possibly in how general it is. Rather it is an example of different forms, the simple and the periphrastic means of expressing the progressive, and different senses, interpretation as a full verb and as an auxiliary, existing in parallel. One of the forms (the periphrastic) and one of the senses (the interpretation as an auxiliary) eventually disappear out of use.

It is true that analytic forms often replace synthetic forms, but Schøsler says that the periphrastic progressive forms in French were never obligatory, which could indicate that they were never fully grammaticalised – if we believe that obligatorification is part of grammaticalisation. It certainly seems that the two forms always existed in parallel, which means that just because the periphrastic and not the synthetic form disappears it does not have to be a case of degrammaticalisation, or a

counterexample of grammaticalisation. Even though it may be true that this is a more unusual development in the history of languages, and especially in a language developing typologically in the way that French has done.

Schøsler's work in general, includes a selection of references from Scandinavia, Germany, France and English-speaking countries. Similarly her references on grammaticalisation are from several different countries: Bybee (1985), Bybee, Perkins and Pagliuca (1994), Detges (forth.), Epstein (1995), Greenberg (1991), Heine (1993a), Heine, Claudi and Hünemeyer (1991a), Hopper and Traugott (1993), Meillet (1912), Squartini (1998) and Vincent (1995). But as we can see from this list, Schøsler refers mainly to non-Scandinavian sources.

Yet another Norwegian linguist who has written a few papers which have touched on grammaticalisation in the last two decades is John Ole Askedal (1984b; 1984a; 2000). In the 2000-paper he argues against Norde's view of the Scandinavian *s*-genitive as an example of degrammaticalisation (cf. Norde, 1997; 1998; 2001a; 2001b; 2002). Instead, Askedal argues that since the development of the *s*-genitive took place around the same time as the Scandinavian languages were changing from a synthetic to a more analytic structure, it should be seen as a form of grammaticalisation (2000: 203). He claims that what has actually happened is that as part of the change in the typological structure, the Scandinavian genitive developed from a fused form to a more transparent agglutinating form, which agrees better with the new structure (Askedal, 2000: 205). He finally concludes that the Scandinavian *s*-genitive should definitely be seen neither as degrammaticalisation, nor as regrammaticalisation (Askedal, 2000: 208). He also observes that he has found that both of these terms and concepts are only rarely discussed in the literature – something which was clearly beginning to change already when he wrote this paper.

Askedal's references on the matter of grammaticalisation are all international, non-Scandinavian sources: Greenberg (1991), Hagège (1993), Heine (1993b), Heine, Claudi and Hünemeyer (1991a), Hopper and Traugott (1993), Lehmann (1982 [1995]), Lightfoot (1979), Norde (1997) and Ramat (1992).

Like elsewhere there are linguists in Scandinavia who are interested in grammaticalisation, and they seem to be getting more and more numerous. Just like in other countries this topic has become increasingly popular in the last few years in Scandinavia and there are now also courses specifically on grammaticalisation that BA and MA students can attend at some universities (Cecilia Falk taught a course

specifically on grammaticalisation at Lund University autumn 2002²⁸⁸, Lene Schøsler in Copenhagen, autumn 2002²⁸⁹), conferences explicitly devoted to grammaticalisation have also taken place (*Corpus Research on Grammaticalization in English 'CORGIE(E)'*, Växjö University, 21-22 April 2001²⁹⁰), a grammaticalisation network has been set up in Denmark (*Grammatikaliseringsnetvaerket*) and grammaticalisation even gets mentioned in the national newspapers:

The study shows that the short tag expression [‘] å så [’] is the most common and that the use of this has increased. Maybe this means that the expression is going through grammaticalisation, that is to say that it is losing some aspects of its semantic meaning and acquiring the character of a function word more and more.²⁹¹ (Svenska Dagbladet, Adelswärd, 2003)

The Scandinavian countries have shown an increased interest in grammaticalisation since a formal perspective was introduced and since the unidirectionality debate started. One can note that in comparison to writings on grammaticalisation in other countries, Scandinavian publications on grammaticalisations have usually either included more introductory words on the process than is considered necessary in other countries, or it has been used as a given ‘tool’ without a single word of explanation.

7.3 Summary and Conclusions

This chapter has dealt with some of the treatments of grammaticalisation that have occurred since Givón’s paper that claimed that “Today’s morphology is yesterday’s syntax” (Givón, 1971a: 413). I am aware of the fact that we still need to look more closely at the connections between the beginnings of the use of the term and concept of *grammaticalisation* in the 1970s. But I hope that I have proven that there may be

²⁸⁸ For more information see: <http://www.nordlund.lu.se/Nordwebb/kurser/littlistor/Modullista.pdf>

²⁸⁹ For more information see: http://staff.hum.ku.dk/schoesl/startside_for_%20lingvistisk_emne.htm

²⁹⁰ For more information see: <http://www.hum.vxu.se/arkiv/corgi.html>

²⁹¹ Original: “**Undersökningen visar att** det korta påhängsuttrycket å så är det vanligaste och att användningen av det har ökat. Kanske betyder detta att uttrycket genomgår en grammatikalisering, det vill säga att det håller på att förlora aspekter av sin semantiska innebörd och alltmer anta karaktären av funktionsord.”

reasons to stop to consider the origin of the term *grammaticalisation* and the concept, rather than simply accepting that it must stem from Antoine Meillet, just because he also happened to use the term in a similar sense.

In 1980 Vincent **mentioned** Meillet's first paper on grammaticalisation, and suggested that this was the history of our concept (Vincent, 1980). Then a couple of years later Lehmann (1982 [1995]: 1) **suggested** that Meillet may have coined the term. He said: "As far as I can see, it was Antoine Meillet (1912) who coined the term 'grammaticalization' and first applied it to the concept for which it is still used today" (Lehmann, 1982 [1995]: 1). **This** set the ball rolling – Heine and Reh (1984) did not claim that Meillet had coined the term, but they did refer back to some of his work, namely a reprint of the 1912 paper (Meillet, 1948). A few years later, Heine et al (1991a: 8-9) asserted that "[i]t was Bréal's compatriot, Antoine Meillet, who may be called the founder of modern grammaticalization studies. [...] Meillet not only introduced the term *grammaticalisation* (1912 [1921, TL]: 133), but he also justified the relevance of grammaticalization studies as one of the major activities in the science of language." Hopper and Traugott (1993: 18) claimed that "[t]he term 'grammaticalization' itself was apparently coined by the French linguist Antoine Meillet, an Indo-Europeanist who at one time had been a student of Saussure." A couple of years after that Harris and Campbell (1995: 19) also proved that they had listened to the rumours and drawn the conclusion that the rumours were fact, and that "Meillet (1912:132) introduced the term 'grammaticalization' with the sense of 'the attribution of a grammatical character to a formerly independent word [...].'"

There are many similarities between Meillet's and Givón's discussions of grammaticalisation. They are both interested in the communicative needs that we have and how this can affect the development of our languages. They also both realise that word order can express grammar, and that we can therefore speak of word order as something that grammaticalises – although this is something that people partly moved away from after Givón's first writings on the subject. In addition, they both speak of phonological attrition and semantic weakening. However, I have shown that Givón did not call it "semantic bleaching" in his first works and it seems possible that he may have borrowed the term from Lord when he finally started using it.

This chapter should have made it clear that there are **no** references to Meillet or his followers in Givón's work, apart from one reference to Meillet's student Émile

Benveniste, who did not use the term *grammaticalisation* even though he discussed the concept. Still that Givón may partly have developed the concept based on Benveniste's article is possible. Most of Givón's references are to works on African languages and to works done in the late 1960s and early 1970s in his own field. I have also mentioned above that Bolinger used the term *grammaticization* in the late 1960s. Seeing as Givón admits that Bolinger was important to him in his development as a linguist, this seems significant. Especially when we consider the fact that Givón used this term before he used *grammaticalisation*.

All linguists working on grammaticalisation tend to rely on the latest sources in this field of research and the most well known international sources in the field – the 'must-reads' in grammaticalisation, such as Bybee et al (1994), Heine et al (1991a), Hopper and Traugott (1993), and Lehmann (1982 [1995]). In the Scandinavian publications on grammaticalisation there are hardly ever any references to Scandinavian work in the field of grammaticalisation, except perhaps to some other Scandinavian linguist who has recently presented a study of an example of something that has grammaticalised.

Internationally grammaticalisationists sometimes like to refer to the nice long 'pedigree' of this field of research. There are references to Meillet (1912; 1915-1916 [1921]), Kuryłowicz (1965 [1975]), Žirmunskij (1966 [1961]), Jakobson (1971 [1959]), Gabelentz (1891), Bopp (1820) etc. But in the Scandinavian work on grammaticalisation there are usually no references to a Scandinavian history of work on grammaticalisation or agglutination theory.²⁹² Is this because there were no earlier treatments of grammaticalisation or agglutination by Scandinavian linguists? Or is it because earlier treatments are simply no longer known among the Scandinavian linguists of today? I believe the last alternative is closest to the truth.

Although the linguists are known, this aspect of their work is not dwelt upon. Instead Wessén's work, for instance, is primarily consulted as a textbook series discussing older stages of the Swedish language. As I have shown in this chapter the references to older works on grammaticalisation in Scandinavia are very scarce. There is the odd reference to Wellander (1964; 1968) and one linguist, Rosenkvist, referred to Bjerre (1935). But there were also others who treated examples of mergers of pronouns and verbs, pronouns and particles, and even changes from

²⁹² As noted in chapters above agglutination theory shows some resemblance to grammaticalisation.

nouns to prepositions (see chapters 5 and 6). The most well known internationally was probably Jespersen (1894 [1993]; 1922 [1949]). Another was Tegnér (1880), others still were Noreen and Wessén – two well known Swedish linguists who most Swedish linguists today should have referred to at some point, at least if they are working on Scandinavian languages.

It is probably at least partly because of the need that Scandinavian linguists have of identifying with the international academic circles that they lack knowledge of earlier Scandinavian work on something similar to grammaticalisation. Grammaticalisation is a framework / phenomenon which Scandinavian linguists today have adopted from international writers. Furthermore, it is a topic which also carries on being developed in international linguistic publications.

Having looked in some detail at the lack of Scandinavian sources among Scandinavian linguists who discuss grammaticalisation, let me also note that if we look at the American and British sources, these also tend to take an international view of the history of grammaticalisation. Occasionally we see mentions of the British scholar John Horne Tooke in comments regarding the history of grammaticalisation, and of course the American functionalist Talmy Givón tends to be mentioned. But there are also many American and British scholars who have treated aspects of grammaticalisation that do not get mentioned, such as Henry Sweet, Archibald Henry Sayce and William Dwight Whitney, for instance.

It is clear that grammaticalisation studies have really taken off since the late 1990s. It is since then that publications on grammaticalisation have started to appear more in Britain and in Scandinavia, studies which we could perhaps divide in two groups (1) those that use grammaticalisation in descriptive / typological studies, and (2) those that discuss the recent issues among international grammaticalisationists, such as the unidirectionality hypothesis.

Grammaticalisation ‘came back’ as part of typological studies, and soon developed into a way of understanding discourse strategies and a means of looking closer at certain aspects of semantic and pragmatic change. Recently, the semantic aspects of grammaticalisation have almost ceased to be discussed, whereas the descriptive and the discourse side of grammaticalisation have grown. The increase in the use of grammaticalisation as a descriptive tool in both synchronic and diachronic language studies indicates that grammaticalisation has become a widely known phenomenon – one which linguists now tend to consider as part of the general

knowledge among other linguists. Unfortunately, the fact that grammaticalisation is hardly ever thoroughly defined and often used in slightly different ways, makes this assumption quite problematic. The increase in the use of grammaticalisation in discussions of discourse management and organisation is perhaps partly a sign that the synchronic work on and use of grammaticalisation is becoming more wide spread.

It is clear that one of the most common issues in later years has been the question of whether grammaticalisation is unidirectional or not. Still, the general assumption is still that grammaticalisation **is** unidirectional and that any seeming counterexamples should be seen as distinct changes and if possible they should be dismissed completely as any form of reverse movement as compared to grammaticalisation. An alternative explanation which shows us that it does not appear to counter grammaticalisation in any way should be chosen. But the unidirectionality hypothesis, and so called *degrammaticalisation*, has caused a lot of debate and many of the most recent publications on grammaticalisation have touched on this.

I believe that I can show that unidirectionality has not been explicitly related to grammaticalisation for very long, but that there were similar ideas in the past. I see a problem in the way that some linguists have claimed that the unidirectionality hypothesis is inherent in some older definitions of grammaticalisation, e.g. Kuryłowicz's definition (1965 [1975]: 52). The problem with this being that the authors of these definitions may not have said anything explicit to indicate that they believed that the development of grammatical elements was unidirectional. Instead often these statements appear to be based on the lack of any comments on any processes or examples in other directions. But to take that to mean that they believed that the development of grammatical elements was unidirectional is arguing from, indirect negative evidence – i.e. *argumentum ex silentio* (cf. Lindström, 2002).

In an earlier look at the history of the unidirectionality hypothesis, Haspelmath (1999), has claimed that there seems to have been no explicit mention of grammaticalisation being unidirectional before the 1970s, when he recognises that Givón mentioned this (see Haspelmath, 1999: 1047). (Notably, implicitly he recognises that Kuryłowicz, whose definition of grammaticalisation has often been used to show that unidirectionality is inherent to the definition of grammaticalisation, does not explicitly see grammaticalisation as unidirectional.) However, Givón did

not literally claim that it was impossible to have counterdirectional changes. Rather he said that they were rare:

One may offhand argue that an opposite process to the one outlined above, i.e., a process of prepositions becoming semantically enriched until they turn into verbs, is at least in theory possible. [...] There are a number of reasons why such a process should be extremely rare. (Givón, 1975: 96, emphasis original)

If this is the first explicit mention of unidirectionality in connection to grammaticalisation, note that it is not a strong hypothesis which claims that all grammatical items stem from the lexicon.

Ideas of unidirectionality were present already in the nineteenth century (maybe even before then). These included very strong ideas of unidirectionality, much stronger than one normally finds in work on historical linguistics today. Still, as I have shown in this thesis, there is no explicit sign of unidirectionality in the main treatments of grammaticalisation between 1912 and the 1970s, apart from in Jespersen (1922) that is, where Jespersen strongly dismisses unidirectionality.

Clearly, the fact that Meillet does not explicitly say that counterdirectional changes can be grammaticalisation, does not mean that he did not think that they could. Even less does it mean that he thought that they did not exist at all! As Jespersen's treatment shows, directionality was being discussed and questioned in the early twentieth century. And a look at a few nineteenth century scholars also shows that unidirectionality was a common topic then, so the fact that Meillet does not mention it could indicate that he felt it was general knowledge – but we cannot know for certain.

The unidirectionality hypothesis has been with us for a reasonably long time. In fact, it may have been influencing us through our subconscious since we heard of ideas of language originating in concrete sensuous cries for help, cries out of anger, out of happiness. Moreover, the fact that unidirectionality continues to form a basis for so much of our work, even though we cannot prove its validity, is problematic. Furthermore, Jespersen clearly stated in the 1920s that he did not believe that grammaticalisation (although he spoke of *coalescence*) was a unidirectional change. The sense of grammaticalisation that we have worked with since the 1970s (which resembles Jespersen's broad sense of a grammatical change more than the nineteenth

century views on agglutination), does not have to include unidirectionality, because it is not only a matter of forms merging together now as in the most basic sense of agglutination theory. Weak unidirectionality may of course be seen as true if grammaticalisation is the same as agglutination and coalescence *only*, or lexicon > grammar... In that case the direction is inherent in the definition of the process, and as Janda (2001: 294) says “similar to the fact that [...] the process of walking due north is necessarily unidirectional”, this would be a tautology (cf. Lass 2000).²⁹³

The other hot topic among grammaticalisationists these days is whether grammaticalisation is a ‘real’ phenomenon or only an epiphenomenon. This issue is never treated in the minor articles on grammaticalisation, but only seems to be treated occasionally by the well known figures such as Campbell, Janda, Joseph, and Newmeyer. In fact it is hardly treated by them either, which makes it incredibly difficult to judge whether they are right or wrong. Still, I would like to take a closer look at the epiphenomenon argument, and I think we need to start by reminding ourselves of what an epiphenomenon is:

a. Something that appears in addition; a secondary symptom. (Oxford English Dictionary (online), 1989i)

a secondary phenomenon accompanying another and caused by it (Merriam-Webster OnLine, 2003e)

Linguists who have suggested that grammaticalisation is an epiphenomenon have, in other words, wanted to claim that grammaticalisation is not so much a phenomenon in its own right, as something that comes about through other phenomena. It has been seen as something that depends so much on other phenomena, such as reanalysis, general semantic change, phonological weakening, processes which are clearly independent and can appear without something being an example of grammaticalisation, that it cannot be a phenomenon in its own right. This is a view that has been proposed primarily by linguists with at least one foot in the formalist camp, and most functionalists would not agree with it fully at least. It also makes it rather non-sensical to speak of a unidirectionality hypothesis of grammaticalisation. Since if grammaticalisation is not really a phenomenon as such

²⁹³ This part on the history of unidirectionality is based on Lindström (2002).

but only something that results out of other processes of language change, then how can there be any restrictions on this phenomenon?

Before we go on, we need to consider also what a phenomenon is exactly. The two main online English dictionaries define it as follows:

1a.

In scientific and general use: A thing that appears, or is perceived or observed; an individual fact, occurrence, or change as perceived by any of the senses, or by the mind: applied chiefly to a fact or occurrence, the cause or explanation of which is in question. (Oxford English Dictionary (online), 1989j)

1. an observable fact or event.

2. **a** : an object or aspect known through the senses rather than by thought or intuition [...] **c** : a fact or event of scientific interest susceptible of scientific description and explanation (Merriam-Webster OnLine, 2003f)

Most, if not all, people working on grammaticalisation agree that grammaticalisation involves certain processes. For instance, it often involves semantic and pragmatic changes, phonetic changes, structural changes. But they do not agree on which of these changes that have to occur, and in what way they have to happen, for a change to be classified as grammaticalisation. Still, most of us have an almost intuitive sense of what is grammaticalisation and what is not. It is an “observable fact” (see the citation from Merriam-Webster above) that languages tend to develop new grammatical items from lexical items. It is an “observable fact” that languages sometimes develop other means of expressing grammatical meanings, such as restrictive word orders. We could also say that in many languages we have definitely seen a development from lexical autonomous items into various grammatical items, so it is a “fact or occurrence” (see the OED quotation above) and what some grammaticalisationists are trying to do is to find “the cause or explanation of” this “fact or occurrence”.

In my eyes, there is much that speaks for seeing grammaticalisation as a phenomenon. However, in some ways that makes it too concrete, too much a part of language as such, and is it really part of language? Or is it simply a tool that we work with as linguists? In addition, **what** exactly is the **phenomenon** grammaticalisation, if there is such a thing? Is it the development from lexical items into grammatical

elements? Or is it the development of means of expressing grammatical information in languages, including word order restrictions, intonation, etc.? And what about the changes where part of a word, perhaps even only part of a morpheme, develops a grammatical meaning – is that grammaticalisation? (I am thinking of umlaut developing into a plural marker, for instance.)

My hypothesis would be the following: We have a need in language to develop means of clearly expressing grammatical relations, and this need can be fulfilled in different ways: (1) by using word order, (2) by making use of intonation, (3) by adapting a certain recurring sound alternation for this task (e.g. umlaut for the opposition between singular and plural), (4) by adapting a morpheme that has lost most of its former meaning to a new function (both this and number (3) could possibly be seen as exaptation), or (5) by using a lexical item, or less grammatical item, to mark this grammatical relation. It is true that the last of these, number five, stands out. Most languages show a tendency to make use of lexical items in developing grammatical markers – although not all make use of the other possibilities mentioned. And that makes it important to distinguish this type of change from the other four. Still it is also related to the other four by fulfilling the same function, and I believe we could therefore profit from comparing all five.

Some would call this a teleological view, and teleological bases for hypotheses are not looked upon with a keen eye by scientists. Still, why does language exist? The basic function appears to be communication as a basis for social cooperation. This also means that one of our goals in using language is to use it effectively, to be clear, but also to make it as effortless as possible since it is something we have to do all the time. This I believe is why different ways of making communication more efficient arise – in the way of different types of grammatical markers.

I would like to see a usage where *grammaticalisation* as a term might only apply to (5), but where we also have terms for the other four types of change, terms which stand in a co-hyponymic relation if you like to *grammaticalisation*. And all five changes are seen as part of one type of change that spans over them all, a form of “*supergrammaticalisation*”, which labels the overarching kind of change, which includes all changes that have the function of creating means of expressing grammatical relations.

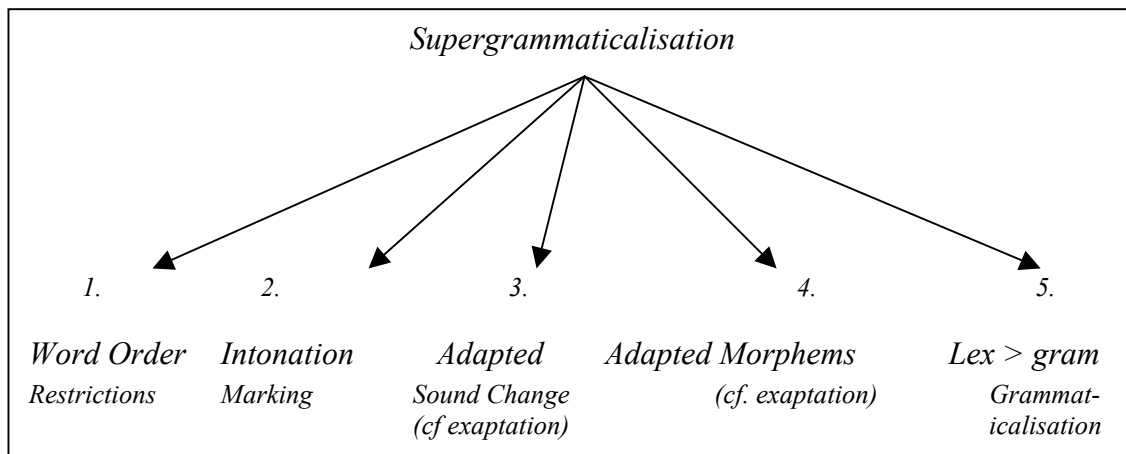


Figure 7.3.1: Grammaticalisation as one type of change under the superposed category of supergrammaticalisation.

PART 4: SUMMARY AND CONCLUSIONS

8. CONCLUSIONS AND SUMMARY

This thesis has explored some of the fuzziness surrounding the concept of grammaticalisation. Furthermore, it has opened the doors to the history of this subject a bit further. It disturbed me that, even though hardly anything has been written on the history of the concept of grammaticalisation, or on the term *grammaticalisation*, many linguists assume that they know who coined the term. No one knows that. This thesis has shown that *grammaticalisation* is a term that has been coined more than once. It has shed light on the fact that there does not **have** to be a connection between the present and the past simply because the same term happens to be used, even if it is used in much the same way.

8.1 What is then Grammaticalisation?

As shown in chapter 3 the term *grammaticalisation* does not mean the same thing to all the linguists who use it. There are such big differences in the way that the term *grammaticalisation* is used that sometimes the same examples, or the same kind of examples can be labelled *grammaticalisation* by person A and *lexicalisation* by person B, even though *lexicalisation* to person A would mean the **opposite** of *grammaticalisation*. At various points in this thesis I have also pointed to the fact that other terms have been used to mean more or less the same thing as the term

grammaticalisation. This led me to call for an increased use of definitions of terminology, in order to avoid misunderstanding each other in the future.

I have also suggested that it would be good if we could rely less on illustrative examples at the cost of definitions. Often in the history of grammaticalisation, the process or phenomenon under investigation has primarily been presented through examples. Coupled with the lack of definitions, this has meant that different linguists have been very likely to understand grammaticalisation in slightly, or more drastically, different ways.

8.2 The Development of Grammaticalisation over Two Centuries

This thesis has not only treated the period when grammaticalisation became more popular again. It is a study of research into the development and origin of grammatical elements and ways of expressing grammatical relations during two centuries.

Grammaticalisation as a concept did not exist at the beginning of the nineteenth century, even though changes which later became specified as grammaticalisation were noted. There was no attempt at any form of generalisation of different types of change. Instead, one often dealt with personal endings in one place, the definite article somewhere else and maybe case endings in yet another section.

During the nineteenth century Bopp's ideas spread and became labelled as *agglutination theory*. Agglutination theory clearly resembles grammaticalisation. However, it starts off as a concept of change through agglutination, composition or compounding, which is both too wide and too narrow to be grammaticalisation. It is too wide because it includes all types of compounding and too narrow since it does not look at the independent grammatical words (conjunctions, prepositions, etc.). It was relatively late that the latter idea was included in the same concept as that which had been discussed as agglutination. This type of change had also been discussed earlier, but without any connection usually being made between it and agglutination. These two aspects of grammaticalisation were two distinct concepts during the nineteenth century. Agglutination was a concept that focused more on form than on

function. The concept of the development of grammatical function words focused on function, and concentrated on the use of independent words as grammatical 'markers'. No link between the two concepts was usually discussed.

Humboldt and Sweet, however, seem to have a concept of the whole cline from a lexical content word to an affix. In addition, Sweet notes that grammatical forms can sometimes arise out of other sources, e.g. from phonological changes, thereby providing us with a link to the recent discussion of the unidirectionality hypothesis and the surrounding concepts of degrammaticalisation and exaptation / regrammaticalisation.

In Meillet's work grammaticalisation is clearly an extended version of agglutination theory. Notably, he says himself that he is discussing a process that was popular during the nineteenth century, and I have concluded that he must have been referring to agglutination theory. I have been intrigued by his claim that the process had been rather less studied in the forty years preceding his own paper from 1912. In fact, we find quite a lot of writings on the origin of grammatical elements between 1870 and 1912, e.g. articles that question agglutination, introductory sections in textbooks of linguistics, etc. But perhaps what had changed around 1870 was that agglutination theory was no longer generally accepted as **the** correct and only way of viewing the origin of grammatical forms. It is at this time that agglutination becomes questioned as I have discussed in chapter 5, and alternatives (*adaptation theory, evolution theory*) arise or are at least talked about more.

During the last decades of the nineteenth century quite frequent discussions appear on the issue of grammatical elements arising from non-lexical sources, often in connection with the term *adaptation*. The writings on adaptation often show a striking resemblance to what nowadays goes under the label of *exaptation, regrammaticalisation* or *functional renewal*. Like exaptation, adaptation involves parts of a linguistic item being given a function, e.g. umlaut and ablaut, or some other linguistic element being given a new function, e.g. a stem formative that becomes a plural inflection. A similar process was also discussed in Jespersen (1922 [1949]) under the term *secretion*. As I have shown in chapter 6, Jespersen includes examples of a stem formative developing into a plural inflection, and phonological changes gaining functional meanings, e.g. English *my* – *mine*.

What is not quite clear, though, is how this process was looked upon in relation to agglutination and the development of grammatical words. Jespersen stresses that a

unidirectional process and other more non-directional processes can result in the same thing. This ratifies classing them as one category with two subcategories. However, others usually do not express themselves as clearly. Some evidently view adaptation theory as an alternative theory that could fully replace agglutination theory.

Many of the early writers only include brief mentions here and there that can be related to grammaticalisation in some way, and they do not see any need to give a unified treatment in a special subsection. This reminds us of how we today often find typological treatments and descriptions of more ‘exotic’ languages containing interspersed comments that something has ‘grammaticalised’, or ‘undergone grammaticalisation’ (cf. chapter 7). Often there is no generalised treatment of the process itself and no unified treatment of all examples that fall into that category – for the simple reason that to them this is just a ‘tool’. Grammaticalisation is not the topic under study in these works, it is not something that needs to be discussed, since the authors assume that it is reasonably clear what it is.

This thesis is an overview of how the concept of grammaticalisation developed. Starting from a recognition of the fact that pronouns often resemble personal endings of verbs, developing into agglutination theory, turning into something more like what we call *grammaticalisation* (including the development of functional affixes and function words), finally resulting in today’s rather heterogeneous concept of grammaticalisation.

Sadly, grammaticalisationists today only tend to speak of one (or two) historical discussions of grammaticalisation, namely a French paper from the early twentieth century by Meillet (Meillet, 1912), and/or his paper from 1915 (Meillet, 1915-1916 [1921]). There are other good, in fact better, historical sources on grammaticalisation we could use. In chapter 6 I have shown that Jespersen’s treatments of grammaticalisation²⁹⁴ are wider and more detailed. He also gives us more of a historical background to this ‘theory’. Furthermore, he wrote in English, which is more widely read by the community of linguists.

It seems as though the main reason that Meillet’s paper has become an important part of grammaticalisation in the 1990s and in the early twenty-first century is that he happened to use a term that linguists in the 1970s also chose to use

²⁹⁴ Partly under the terms *coalescence theory* and *secretion* (Jespersen, 1922 [1949]), partly under no term at all and occasionally making use of the term *agglutination theory* (Jespersen, 1894 [1993]).

for a very similar phenomenon. Of course we should not overlook Meillet's importance to historical linguistics. Still, it is unfortunate that most linguists have not looked any further than Meillet and consequently they have missed the discussion of vital parts of grammaticalisation 'theory' that were published by Jespersen, Sweet, Sayce and others.

8.3 The Grammaticalisation Revival (?)

In the 1970s terms like *lexicalisation*, *morphologisation*, *syntacticisation* were common. Hence a term like *grammaticalisation* must have suggested itself. Furthermore, it took quite a long time before any term started to be used by Givón, for the phenomenon that he had 'rediscovered' in the early 1970s. I have shown that Givón also used terms such as *syntacticisation* and *morphologisation*, although it is misleading to say that he preferred these terms to *grammaticalisation* as Hopper and Traugott (1993) have done. Maybe to some extent he did / does, but he certainly uses all three.

With regard to the terminology, it is important to note once more (cf. ch. 7) that Givón has recognised the influence that Bolinger has had on him in other respects. Bolinger used the term *grammaticisation* in the late 1960s (cf. Bolinger, 1968) and this was the first version of the term that Givón used, before he began using *grammaticalisation*. The possibility that it could have come from Bolinger, is clearly undeniable, although perhaps Givón did not remember having heard someone use it. This effectively means that even though I have claimed that this term could well have been coined several times, it would be interesting to take a closer look at Bolinger's work than I have so far been able to do.

In this thesis, I have focused rather a lot on Givón's work during the 1970s. I am certainly aware of the fact that many others were also involved in the 'revival' of grammaticalisation during that period. I have also noted that Hodge's paper (1970) is more historical than any of Givón's writings on the subject. But, I have explained that the role that Givón has had in functional grammar, in functional linguistics, and the clear link between functionalism and grammaticalisation, not to mention the

famous saying that today's morphology is yesterday's syntax, quoted from Givón (1971a), justifies a focus on Givón.

In the 1970s, grammaticalisation was a subject that was taken up by linguists primarily in the United States. The renewed interest in how we develop grammar appears to have grown out of typology and the research into more 'exotic' languages which Givón and others were involved in. For instance, Givón, Li, Lord, and on the other side of the Atlantic, Heine, were primarily looking at African and Asian languages. Their research into linguistic systems so different from the Indo-European languages, seems to have made them aware of the parallels between lexical content items and words and affixes that fulfilled grammatical, functional roles in language.

8.4 Grammaticalisation in One of the Smaller Corners of the Western World

My comparison of a smaller research community (Scandinavia) to the international discussions of grammaticalisation has shown that concepts similar to grammaticalisation seem to have been studied in Scandinavia since the early nineteenth century. However, it seems it may possibly have been less investigated during the early twentieth century, if we consider Wellander's statement of the rarely trodden path of research (see section 6.4.2). The scattered earlier treatments of processes similar to grammaticalisation are not known to most contemporary Scandinavian linguists. This may be partly due to the fact that the treatments usually appear scattered over the works of a scholar instead of being the main focus of one paper, or of one distinct section of a book. The treatments also often lack detailed discussions, which make them less useful to modern scholars; instead, they usually only note that B appears to stem from A. The lack of references to older Scandinavian treatments of changes such as grammaticalisation is probably also due to the fact that Scandinavian scholars have learnt about grammaticalisation from international sources.

8.5 Grammaticalisation Theory?

In relation to one of the current debates concerning grammaticalisation, we should note that at the turn of the last century, scholars do not appear to have had any reservations about claiming that grammaticalisation was a theory. In fact, it is only in the last few years that many linguists have often frowned at others who have called the ideas around this phenomenon in language *grammaticalisation theory* (cf. 2.4).

So is there a grammaticalisation theory? As I have mentioned in 2.4, according to the OED, a theory is:

A scheme or system of ideas or statements held as an explanation or account of a group of facts or phenomena; a hypothesis that has been confirmed or established by observation or experiment, and is propounded or accepted as accounting for the known facts; a statement of what are held to be the general laws, principles, or causes of something known or observed. (Oxford English Dictionary, (online), 1989: 4a)

I have argued that this is what grammaticalisation is, when it is part of a framework in linguistics. It is “[a] scheme or system of ideas or statements” of how grammatical elements develop and how new grammatical items, or ways of expressing grammatical relations arise. However, I would say that Heine (2003: 584) is right when he says that to grammaticalisationists, whether grammaticalisation is a theory or not is not usually a question. Those who first worked on grammaticalisation (by which I mean to include all changes related to expressing grammatical relations, and both scholars today and in the past) were first and foremost concerned with describing, generalising and understanding more about language. In that sense, we are concerned with hypotheses regarding how we develop more efficient and clearer ways of communicating and based on these we have built a theory. Linguistic data has shown certain tendencies when it comes to how grammar develops and how morphology and syntax changes. Linguists have studied linguistic data in order to see if they can confirm or disconfirm the hypotheses, and, through this, a theory of how means of expressing grammatical relations develop in language has taken form.

In saying this I also wish to make it clear that this is primarily a theory of what I called *supergrammaticalisation* in the last chapter (7.4), although, there are also hypotheses regarding which of the subordinated kinds of supergrammaticalisation are

more common. It would be preferable to spend more time on finding out **why** there seems to be such tendencies, instead of arguing over whether grammaticalisation is a theory or a phenomenon, and whether it is unidirectional!

8.6 Unidirectionality

Too much time has been wasted on arguing over whether grammaticalisation is unidirectional. Especially seeing as previous effort has fallen short of making sure that we understand what other linguists mean in their discussions of grammaticalisation, lexicalisation, exaptation, regrammaticalisation, and functional renewal.

In conclusion, what we need are:

- (1) Meticulous definitions of (super-)grammaticalisation and the related processes when these terms and concepts are used, since usage is still too varied to assume that everyone will know what is meant by any of the terms that are used: e.g. *grammaticalisation*, *reanalysis*, *lexicalisation*.
- (2) A full explanation of the way in which (super-)grammaticalisation, or some sub-categories of (super-)grammaticalisation, might be unidirectional – semantically, phonetically, structurally?

The clear tendency that a development from lexical to grammatical is more common than vice versa, in the languages and periods that we have data from, is fascinating. It is also truly interesting that this seems to be the most common method of developing new means of marking grammatical relations. However, since the representativity of our samples may be questioned (cf. Lass, 2000) and since we clearly have **some** examples where non-lexical parts of language have developed grammatical functions – e.g. word order, phonetic differences (e.g. umlaut as a plural marker in English), etc., we should accept that there are different ways of developing means of expressing grammatical relations. This admittedly seems a teleological view. But, as I have argued in 7.4, the question of teleology can be dismissed given the fact that the most probable motivation is a wish to communicate more efficiently.

8.7 Final Comments

It is my contention that we should spend more time trying to get at the **why**. Why is it that we primarily develop means of expressing grammatical relations from the lexicon? This also implies trying to trace the similarities that may exist between the different sub-categories of supergrammaticalisation that I specified in 7.3. Doing so would give us the opportunity to learn more about language, language change and maybe even about ourselves.

We should also appreciate the value of the history of our concepts. As I have shown in this thesis, jumping to conclusions regarding the history of a term and concept such as grammaticalisation is of little help. Parts of this concept have been discussed for a long time, even if not as one unified concept. Meillet's importance in the history of grammaticalisation is definitely questionable.

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