Kuhnian Incommensurability Between Two Paradigms of Contemporary Linguistics

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Slowly we are learning,
We at least know this much,
That we have to unlearn
Much that we were taught,
And are growing chary
Of emphatic dogmas;
Love like matter is much
Odder than we thought.

From 'Heavy Date' by W.H.Auden

I must review my disbelief in angels.

Brian Patten – Angel Wings

Abstract

This dissertation proposes a theory of reference for the language of scientific theories. This theory of reference looks at the nature of postulation in scientific theories, and shows that mental posits are metaphorical in nature. It is a hybrid of internalist and extensive reference theories. This, allied with the competing epistemological assumptions of competing schools of linguistics, can account for the existence of incommensurability across two paradigms of linguistics.

The relationship between transformational generative grammar and sociolinguistics is vexed. Both claim the same object of study, but with radically different methods and aims. This dissertation shows that the metaphorical nature of the posits used in each leads to incommensurable vocabularies. Thomas Kuhn's notions of paradigms and incommensurability are used to elucidate this relationship.

Chapter one proposes and explains the theory of reference. Chapter two defines the major areas of the thesis. Chapter three explores the history of linguists claiming that a particular area of linguistics instantiates a Kuhnian paradigm, and looks at arguments concerning the possibilities for studying language scientifically. Chapter four explores the epistemological bases of TGG and sociolinguistics, starting from Chomsky's claims to do 'Cartesian linguistics', and concludes that opposing epistemological commitments lead to incommensurability. Chapter five demonstrates the incommensurable concepts and vocabulary items, and shows how my theory of reference can account for that incommensurability, while maintaining a certain amount of the traditional natural science – social science distinction. Because postulation is free and metaphorical, terms borrowed from natural languages into scientific theories can end up with overlapping, but incommensurable, references. Incommensurability is shown to be local and surmountable, through 'language-learning' rather than through 'translation'.

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The writing of this dissertation has coincided with the loss of far too many members of my family. It is dedicated to the memory of my grandmother Eva Smith, my uncle John Gallini, my grandfather Charles Gallini, and my father in law Gerry Ifill.

Chapter One

1.0 Introduction

This thesis proposes a theory of reference for the language of scientific theories, and argues that this theory of reference can explain a number of problems in the history and philosophy of linguistics. Specifically, it argues that co-existing (and, I argue, opposing) forms[1] of linguistics – to be further defined below – can be characterised as 'incommensurable' in Kuhnian terms, and that the problems and misunderstandings engendered by this incommensurability are explicable within the terms of the theory of reference proposed.

This thesis addresses various arguments and inconsistencies in positions held by opposing forms of linguistics. By addressing and solving these problems through the application of a new theory of reference to the language of scientific theories, this thesis aims to advance and in some cases simplify metatheoretical issues in the history and philosophy of linguistics.

The forms of linguistics addressed in this thesis are transformational generative grammar (hereafter TGG) and sociolinguistics. The arguments between these concern whether or not each form of linguistics should rightfully be regarded as a science; whether or not TGG fulfils the criteria for membership as a Kuhnian paradigm; what the aims of linguistics ought to be; and the meaning of key terms as used in each form of linguistics.

The philosophy of Thomas Kuhn (1922-1994) is the thread which connects the parts of this thesis. Kuhn is best known for his 1962 work *The Struc-*

[1] Whether a 'form' of linguistics is a school, a theory, a movement or something else is addressed in chapter two. 'Form' is a usefully neutral term to tide me over until better definitions have been provided.

ture of Scientific Revolutions, which introduced the phrase 'paradigm shift'. However, his later work, which is less well-known and focuses on the philosophy of language, is equally relevant to this thesis, and it is his concept of incommensurability which provides one of the major philosophical bases for my argument.

The thesis concludes by showing that the theory of reference proposed is motivated by a range of problems in the history and philosophy of linguistics, and that such problems can be addressed as misconceptions rather than substantive disagreements.

2.0 Structure of the thesis

The thesis contains five chapters.

In this first chapter I give an overview of the methods of the field. In part three of this chapter I examine how this thesis fits in with linguistics, the history of linguistics, and the history and philosophy of science. In part four of this chapter I introduce the key areas on which my argument is based: transformational generative grammar; sociolinguistics; and Thomas Kuhn's ideas about paradigms, normal and revolutionary science, and incommensurability. In the final part of this chapter I lay out my theory of reference for terms in scientific theories.

Chapter two focuses on defining and elaborating terms which are central to my thesis. First I look in more detail at the works of Thomas Kuhn. I outline his ideas on paradigms and paradigm shifts, and then analyse his views on incommensurability; this requires a comparison of his and Paul Feyerabend's treatment of the same concept. This is followed by an analysis of Kuhn's position on the demarcation of science, and the division between the natural and the social sciences. The second part of chapter two is an examination of arguments against Kuhn's position, including arguments against his conception of the history of science, his supposedly relativist position, and his treatment of incommensurability. Part three of this chapter goes further into the definitions of TGG and sociolinguistics. I look at how they fit into the broader field of linguistics, their relationship to other disciplines, and the nature and practice of their research into language. I also give a treatment of what kind of field they believe themselves to be; Kuhn uses the term 'paradigm' to describe a field of research, but other linguists prefer 'school', 'discipline', or other terms. The final part of chapter two looks at Rationalism and Empiricism from the point of view of linguistics. This is in anticipation of chapter four, which deals with Chomsky's engagement with this issue.

Having laid down the definitions of fundamental terms and concepts in chapter two, chapters three and four look at a set of processes and problems engendered by these terms. The first part of chapter three looks at arguments for and against the idea that language can be studied scientifically, based on two different philosophical approaches to answering this question. The second part of chapter three looks at whether TGG, or any other form of linguistics, can be accurately described as a Kuhnian paradigm, as has been claimed.

Chapter four looks at the interplay between linguistics and early modern epistemology. This is rooted in Chomsky's espousal of Cartesian Rationalism. I look at arguments for and against the alignment of TGG with Descartes, and examine other instances of linguistics appropriating early modern philosophers as epistemological support for their theories.

The problems and processes analysed in chapters three and four demonstrate the causes and manifestations of incommensurability between different approaches to the study of language. This incommensurability is explored in much more detail in chapter five. This chapter shows how different epistemological approaches to language leads to incommensurable concepts of the object of study. At the end of chapter five I show that my theory of reference for term in scientific theories can account for the emergence of this incommensurability.

3.0 Methods and subjects: how does the thesis fit in with the fields of philosophy, linguistics and the history of linguistics?

3.1 History and philosophy of science (including the history *of* HPS)

There is a symbiotic relationship between the history and philosophy of science. All history is more than chronology; in some sense it attempts to explain the past. Philosophy is, in essence, an examination of the mind's interaction with its subject matter. The history of science neatly intersects the two. While it describes the history of a certain aspect of human behaviour, it also gives an epistemological explanation of this behaviour. It cannot do otherwise, since to give an account of what people have done is to give an account of what people (think they) have known, of the entities of which they believe the universe to be composed, of how they have gone about acquiring this knowledge, and of how this acquisition of knowledge was understood, modified or rejected by successive generations. In other words, the history of science is an epistemological history, one in which the historian must be actively engaged. If the historian does not address current epistemological attitudes towards the knowledge under examination, then the history becomes no 'more than anecdote or chronology'

(Kuhn 1962:1). This is why the history of science is never just that, but is in fact the history *and philosophy* of science[2].

So the history of science is an unusual type of history. It is also an unusual type of philosophy. This can be seen in the fact that the phrase 'armchair philosopher' is not necessarily derogatory: logic, philosophy of mind, epistemology and metaphysics can indeed be done from an armchair. Philosophy of science, however, cannot, as it requires a reasonable knowledge of history. Science is, and has always been, a temporally and spatially bounded human activity, usually (but not always) involving the transmission of knowledge among peer groups, and through generations. A philosopher of science who had never heard of Newton or Darwin would, presumably, be lacking vital empirical knowledge. It would not be possible for him or her to give a full account of the nature of scientific knowledge, and how scientists acquire it. So any philosopher of science is, to an extent, a historian, if only of the recent past.

Most of the modern philosophers discussed over the course of my thesis can be said to belong to the analytical tradition of philosophy, such as Karl Popper, Hilary Putnam and Donald Davidson. The primary method used in analytical philosophy can usually be defined as 'conceptual analysis': clarify the concept, then clarify the argument. However, Kuhn's socio-historical approach to philosophy reminds us that there is always an alternative to conceptual analysis when we are attempting to define something. The answer to 'what is science?' could be the kind of careful delineation of the rules of the scientific method that Popper undertook, or we can take a Kuhnian view and baldly state that science is what scientists do. Neither of these is incorrect, but the two answers show two radically different interpretations of what was intended by the question. For example, when Socrates asked 'Who are friends?' in *Lysis* he would have been

[2] See Larvor in Newton-Smith (2000) for a discussion of contrasting attitudes towards the relationship between the history and the philosophy of science.

surprised to be presented with a list of all the friends in Athens, or in the whole world, and yet it is not obvious that this would be an incorrect answer. For this reason, Kuhn's philosophical reading of history seems a radical way of dealing with old problems (although it is not unprecedented: Hegel's discussions of beauty, for example, are strongly historically-oriented, see Houlgate (1998:438-447), and see below for a discussion of Kuhn's influences).

Applying a socio-historical approach to the question of the relationship between linguistics and science has proved extremely fruitful in the case of this thesis. Kuhn's socio-historical approach to philosophy has enabled me to attempt a discussion of the question 'how is language studied?' in a way which does not depend on the traditional conceptual-analytic tools of analytical philosophy. However, where necessary, I have no qualms about using such tools. The section on the definition of 'incommensurability' (chapter two) owes little to historical research and a lot to more standard philosophical practice. On the other hand, chapters three and four on the use and abuse of historical figures in support of various linguists' claims have plenty to do with the sociology of knowledge, the rhetoric of power and the institutionalised history of the discipline of linguistics, and comparatively little to do with the actual philosophical figures under consideration.

Kuhn's work *The Structure of Scientific Revolutions* (1962 – hereafter *SSR*) has been described as 'the most widely read, and most influential, work of philosophy written in English since the second world war' (Rorty 2000:204), and an 'extraordinarily influential—and controversial—book' (Bird in *Stanford*); but naturally his work did not occur in a vacuum. His introduction mentions various influences, such as Whorf,[3] Koyré, Piaget and Quine (1962:vi). None of these provided direct inspiration for his the-

[3] See chapter two for a fuller discussion of Kuhn and linguistic relativity.

ory, however, so much as providing theoretical frameworks such as 'conceptual schemes' which allowed him to develop his theory. Later in the book he acknowledges other influences, such as Polanyi (ibid:44). It has been argued that Kuhn's debt to Polanyi deserved more than one footnote – see Jacobs (2002) for a summary of these debates. It is also common among writers on Foucault and other French philosophers to point out that writers such as Bachelard and Canguilhem developed similar ideas years before Kuhn (see, for example, Gutting 1989:9-55). No one claims that Kuhn plagiarised, but it is clear that Kuhn's work was not an *ex nihilo* masterpiece which created its own genre of philosophy of science.[4]

Kuhn's work did make waves, however, and this was because it flew in the face of most contemporary philosophy of science. The dominant figure before Kuhn was Karl Popper, whose theory of falsification was considered the best definition so far of good scientific practice, and the demarcation line between scientific and unscientific pursuits. Popper's theory of falsification had grown out of the Vienna School's empiricist theories of verification (see chapter two section 1.2 for more on the Vienna School). Popper's theory of the falsification of scientific theories was first disseminated in the 1930s, and so when Kuhn published *SSR* in 1962, he was providing an alternative to a well-established theory.[5]

However, Kuhn's theory is in many ways not a challenge to Popper's falsificationism. Despite the debates between the two (see, for example, Kuhn in Lakatos and Musgrave (eds.) (1970:1-23)), it is possible for them to coexist, as Popper's theory is (broadly) prescriptivist and details what is and is not scientific activity. Kuhn's theory has little to say in terms of demar-

[4] See Bird (2000) chapter one for a description of the philosophical context which gave rise to *SSR*.

[5] Popper's 'falsificationism' has a long and distinguished history of misinterpretation. Lakatos catalogues this in (1970:180-181). He distinguishes three Poppers: $Popper_0$, who never existed, and was a 'dogmatic falsificationist'; $Popper_1$, a 'naïve falsificationist'; and $Popper_2$, a 'sophisticated falsificationist'. According to Lakatos, 'the real Popper consists of $Popper_1$ together with some elements of $Popper_2$.'

cation, instead giving the history of how science has developed (see chapter two for more on demarcation).

The first part of chapter three concerns the debate over how 'scientific' linguistics is, or can be. This debate betrays the fact that the 'philosophy of linguistics', especially with regard to ontological issues, lacks consensus among practising linguists. While linguistics has progressed in empirical and theoretical ways, 'language' as a concept is nebulous. This incongruent situation (that an apparently 'scientific', rigorous discipline has an ill-defined subject matter) forms one of the foundations of my thesis: why should a well-established discipline rest on such controversial philosophical bases? The question is especially interesting because, since Saussure, so much energy has been expended on defining exactly what linguistics is (see chapter 3 section 2.4 for a discussion of Saussure).

Although chapter three examines arguments from within linguistics as to whether the subject constitutes a natural science or not, I do not mean to prejudge the issue by introducing this chapter with an examination of the history and philosophy of science. Kuhn's sociological analysis of the nature of science holds for linguistics as much as for physics and chemistry. Moreover, it applies to astrology or any other epistemological enterprise generally accepted as unscientific. Similarly, chapter four addresses the question of Rationalism and Empiricism, and the history of linguists appropriating one or the other as epistemological ballast for their theories. This does not require me to take a side on the issue, but to examine these debates from a Kuhnian point of view

3.2 History of linguistics

> The observation that one sometimes hears that linguistics is the most scientific of all the humanities and the most humanistic of all the sciences is thus not unfounded. (Bugarski 1976)

If the history of linguistics is to be 'viewed as a repository for more than anecdote or chronology' (Kuhn 1962:1), as noted above, then, like the history of science, it must have epistemological commitments. The above comment by Bugarski suggests, however, that the history of linguistics must position itself vis-à-vis the history of science with respect to the similarities and differences between linguistics and the natural sciences. On the one hand, this naturally assumes that the question of demarcation (see below) is of major importance in the history of linguistics. On the other hand, it suggests that approaches to the history of linguistics can be seen as modifications (or even improvements) to approaches to the history and philosophy of the natural sciences.

As early as 1976, Koerner argued that the historiography of linguistics was 'in jeopardy' because no frame of reference existed for how to properly conduct research into it. His paper claims that

> the scholar engaging in work treating periods or aspects of our linguistic past should be both a historian and a linguist [...] Ideally he should be thoroughly acquainted with the findings of the history of science. (Koerner 1976:688)

He adds that 'no serious attempt has been made up to the present day to place the history of linguistics on a theoretical, if not epistemological basis' (ibid.). Without a theoretical basis, the historian of linguistics is prone to 'adoption of the many *fables convenues* related in the standard histories of linguistics written between 1869 (cf. Benfrey) and 1924 (cf. Jespersen)' (1976:686). Such mistakes lead to 'stories instead of history, distorting previous achievements by presenting them in the light of our present understanding of the nature of linguistics in particular, and of science in general' (ibid.).

Koerner's paper uses a critical reading of Kuhn to help understand the history of science. Although 'I do not think that his [Kuhn's] concepts can be adopted without major revisions as a model for the historian of linguistics' (ibid:689), Koerner uses Kuhn's concept of paradigm-shifts and worldviews (which Koerner modifies to a more general 'climate of opinion') to explain three 'paradigms' – those engendered or created by Schleicher, Saussure and Chomsky. In this way he does not fully endorse Kuhn's philosophy, but he uses it in order to show that

> The historian of linguistic science must not only engage in what, following Kuhn, may be termed 'normal science' and be familiar with the theories put forward by members of our discipline; he also needs a firm grasp of the *res gestae* which may have had a distinct impact on the emergence of a new paradigm in the field. (1976:710)

Koerner's point here is that if the historian of linguistics is to avoid the two mistakes of telling 'stories instead of history, distorting previous achievements by presenting them in the light of our present understanding', then an understanding of the historical context is vital. This is fairly standard historical practice. However, and more to the point, the use of history and philosophy of science can help in analysing what kind of activities scientists engage in.

In the same paper Koerner criticises two histories, by Robins and Aarsleff (both 1967) for not fulfilling these criteria. Robins 'shows awareness of the difficulties involved in presenting an accurate picture of earlier periods, but again the author has made no attempt to provide a firm methodological basis for his account of the history of linguistic ideas' (1976:687). Aarsleff is

> much more satisfactory as it reveals the general atmosphere prevailing in the period under investigation and offers much more detailed factual information. It seems, however, that the author hoped the reader would, through some

kind of osmosis, absorb the method from the thicket of positivistically gleaned historical facts. (ibid)

Robins and Aarsleff are described as representing a form of the history of linguistics which lets analysis and exegesis take second place to the presentation of facts. Whether or not this is fair (in my opinion both Robins and Aarsleff deserve more credit than Koerner affords them) Koerner's point that methodology and epistemology are necessary in the writing of the history of linguistics still holds, if it is to be more than a repository of 'mere anecdote' or '*fables convenues*'.

There is a strand history of linguistics, written by linguists, which represents the opposite to Koerner's view of Robins and Aarsleff letting facts come before exegesis. By this I mean the selection of facts to support a teleological or Whiggish interpretation of history; perhaps the most discussed example of this is Chomsky's *Cartesian Linguistics* (1966a), which Koerner accuses of these sins (1976:685, 689). In chapter four I address this work at length, but for now it should be enough to note that the shortcomings of this type of work *as history* should be obvious.[6]

This thesis fulfils Koerner's criteria. First, in using Kuhn to talk about the present I have less need to understand the past from its own viewpoint, both regarding linguistics and the cultural context. I was trained in Chomskyan linguistics, and so getting into that mindset does not require extensive historical research on my part. On the other hand, I am not a linguist working in that field, and have no professional biases towards it (or indeed, any other form of linguistics).

[6] In addition to these remarks on the purpose and methodology of writing history, there is of course another less-discussed *point* to the history of linguistics, that of discovering and presenting lost or unseen facts or documents. This could perhaps be seen as the purest history, and its value should be obvious. A good example is Joseph (2002) on the meeting between Saussure and Whitney.

Second, my methodological commitments are clear. I largely accept Kuhn's theory – not unquestioningly, but as a broad heuristic. However, I go one further than Koerner in my approach to the interplay between epistemology and the history of linguistics. Rather than merely having a firm epistemological commitment to the history of linguistics, I am using both the history of linguistics and Kuhn's philosophy to make an epistemological point about the nature of language in scientific theories.

In this light, my thesis can be seen as philosophical: whether by conceptual analysis or analysis of primary and secondary works in the field, I hope to arrive at a synthesis of positions which I have found to be contradictory, or at least unreconciled. I rely mainly (but not exclusively) on the major works of major writers in the fields of interest (and, of course, commentaries and analyses in the relevant journals): Kuhn, Popper and Feyerabend in the philosophy of science; Putnam, Kripke, Davidson and Quine (among others) in the philosophy of language; Chomsky and orthodox practitioners of TGG in generative grammar; Labov, Hymes, Gumperz and other major figures in sociolinguistics; various writers including Becher on the sociology of knowledge; Descartes, Locke, et al in the history of philosophy; and so on. Critical and/or historical works which I have returned to repeatedly include the self-explanatory *Sociolinguistic Metatheory* (1994) by Esther Figueroa and *Chomsky: Ideas and Ideals* (1998) by Neil Smith; and *The Linguistics Wars* (1993) by Randy Allen Harris, an account of the development of Chomsky's linguistics, with particular emphasis on the theory of generative semantics of the late 1960s. *Educating Eve* (1997) by Geoffrey Sampson and *From Grammar to Science* (1996) by Victor Yngve[7] feature heavily, providing epistemological and methodological opposition to mainstream linguistic theories.

[7] Geoffrey Sampson is Emeritus Professor of informatics at Sussex University. Victor Yngve is Professor Emeritus of linguistics and psychology at the University of Chicago. See chapter four for an extensive discussion of their positions on philosophical issues relating to the study of language.

Another strategy used in this thesis, which requires comment, is one of perspective and criticism. I criticise Kuhn when necessary, but not often, as the purpose of this thesis is not to critique his work but to use it. Instead, the aim is a critique of the recent and current practices of linguists, and this is where the question of perspective arises. Some linguists have used Kuhn's philosophy to justify the 'scientificity'[8] of their supposed paradigms. Consequently, I devote a considerable amount of space to explaining and evaluating these viewpoints from within the schools of linguistics concerned; that is, I examine how they see themselves, how they read Kuhn, and how they evaluate the scientific (or otherwise) nature of their enterprises (see chapter three). However, in other parts of the thesis I stand 'outside' those disciplines, and attempt to evaluate from a more neutral point of view whether or not their claims stand up to scrutiny, and whether or not a neutral observer would conclude that linguistics (or parts of it) fit the Kuhnian mould, however loosely. The combination of these two perspectives, studying how schools of linguists see themselves and their discipline, but also using the Kuhnian model to explain the relationships and tensions between these schools, gives a rounded picture of the development of at least some aspects of modern linguistics.

4.0 Other preliminary definitions

This section gives preliminary definitions of some key aspects of this thesis: first, the two areas of linguistics which I concentrate on - TGG and sociolinguistics - and second Kuhn's two key ideas, paradigms and incommensurability.

4.1 TGG and sociolinguistics

[8] I didn't invent this rather ugly word - it is in the OED, and is extremely useful for my purposes.

'Linguistics' is a broad term, and I use it in this thesis to refer to the academic study of language in two particular senses. The first, TGG, is the field of study which is generally taken to date from the publication of Noam Chomsky's *Syntactic Structures* in 1957, characterised by syntactic transformations and species-specific linguistic ability, and which has continued to be dominated by his work while undergoing significant revisions. Two of the most notable are the Government and Binding approach of the 1980s, and the Minimalist Program, its current incarnation. The second field of linguistics, sociolinguistics, is defined by Chambers as 'the study of the social uses of language' (2003:2) – an uncontroversial preliminary definition.[9] Chambers goes on to say that sociolinguistics 'encompasses a multitude of possible inquiries', and it is true that sociolinguistics forms a much broader church than TGG. It will become clear over the course of this thesis exactly *why* sociolinguistics is harder to define than TGG. One reason for this heterogeneity of the field is that, while TGG has Chomsky as its founding father, much of sociolinguistics traces its ancestry back to William Labov in the 1960s. However, Labov is only the founding father of one strand of sociolinguistics; other forms look back to Dell Hymes or John Gumperz, and have roots not in dialectological studies but in anthropological or Weberian[10] approaches to the social sciences.

Throughout the thesis, the type of sociolinguistics under discussion is examined within its proper context: first, within Figueroa's (1994) three-way division, and then in the context of more recent approaches and oppositions. In particular, over the last twenty years a division has arisen between 'sociolinguistics', which studies language as it is used in society, and 'sociocultural linguistics', or 'linguistic anthropology', which studies

[9] Chambers' book *Sociolinguistic Theory* is a mid-level textbook on sociolinguistics, focussing on variability. It is frequently alluded to in chapter five.
[10] Max Weber's approach to the social sciences is explored in chapter two. His basic contrast between the natural and the human sciences is described in some depth in Kuhn (2000:216-223).

society through the medium of how people use language. The distinction between these varieties of sociolinguistics will become apparent as the thesis progresses.

Neither TGG nor sociolinguistics are homogenous – it may be the case that fields of study are necessarily fuzzy concepts. However, both are tangibly successful and thriving, represented in universities all over the world, and having their own journals, conferences, textbooks and internal controversies.

4.2 Kuhn's Philosophy

It was noted above that Kuhn's best-known work is *The Structure of Scientific Revolutions* (1962). This described the 'revolutionary' process by which a community of mainstream scientists with a shared understanding of how the world works – a 'paradigm' – turns towards a radically different understanding of the world – a 'paradigm shift'. The change from one paradigm to another is known as a 'scientific revolution', and is engendered by a 'crisis', a period of research when it becomes apparent that the old paradigm has fundamental flaws. The work which scientists do between revolutions is known as 'normal science' (see chapter two section 1.1 for a detailed account of this process). I draw extensively on *SSR*, but also on Kuhn's later work, collected in *The Road Since Structure* (2000), which moved away from the examination of how paradigms are formed and concentrated on issues in the philosophy of language that had arisen from his earlier work. Although the two are complementary rather than contradictory, it will at times be useful to refer to 'early Kuhn' and 'late Kuhn', rather than simply 'Kuhn's philosophy'.

The subject of much of Kuhn's later work – and the key concept of this thesis – is conceptual and semantic incommensurability across scientific paradigms. Incommensurability, like the calculus, was developed independently by two different thinkers at the same time. Thomas Kuhn and Paul Feyerabend both started using the term in the early 1960s to describe situations in which two or more scientific theories could not be compared under any neutral standard because their takes on reality were too different to afford comparison.

This is a very general characterisation of a complex and controversial concept, and one which, quite possibly, neither Kuhn nor Feyerabend would accept in its entirety. There is no definition of incommensurability which covers both philosophers and the subsequent development of the term. Kuhn traces the development of the word from the literal to the metaphorical:

> The hypotenuse of an isosceles right triangle is incommensurable with its side or the circumference of a circle with its radius in the sense that there is no unit of length contained without residue an integral number of times in each member of the pair [...] Applied to the conceptual vocabulary deployed in and around a scientific theory, the term 'incommensurability' functions metaphorically. The phrase 'no common measure' becomes 'no common language.' (1990:35-6)

'No common language' needs clarification, and is potentially misleading. The languages used to describe scientific theories, on either side of a revolution, are supposed to be incommensurable with each other; that is, one cannot express the meaning of the other. However, on closer reading it transpires that Kuhn is only talking about a select group of words used in a technical sense. After a scientific revolution 'dog' still means 'dog', and 'the' still means 'the'. However, for those of us who live in a post-Copernican world, 'planet' means something which could not be expressed in the pre-Copernican, geocentric world. Although they had the word 'planet', it meant something different (it included the moon, but not the earth) (Kuhn 1962:115,128).

This would not matter if the difference was a few isolated words, which could be incorporated wholesale into the new language, in the same way that English frequently incorporates neologisms and foreign words. For example, the referential system of English nouns is *not* incommensurable with the concepts of Japanese cooking, so we can simply learn words like *teriyaki* and *shiitake*. True incommensurability, on the other hand, arises when changes in meaning trigger other changes in meaning. Words do not (generally) have meaning independently from each other; rather, they exist in webs. If you know a word for 'chair' you also know words such as 'stool', 'leg', 'wood' etc. (See Kuhn (2000:48) for an example from French, and a discussion of failures of translation into English.) So the change of meaning of 'planet' is necessarily tied up with a change of meaning of 'sun', 'orbit', etc.

In 'Commensurability, Comparability, Communicability' (in Kuhn 2000), Kuhn talks about the difference between interpretation, translation and language-learning. He also makes it clear that the problem of incommensurability is a problem for the historian of science, not one for scientists themselves. Scientists, immersed in their paradigms, have no cause to study or understand previous paradigms, so the situation does not arise. Historians of science, on the other hand, are concerned with understanding and interpreting texts from a time when the web of meaning was differently connected. Consequently, 'a historian reading an out-of-date scientific text characteristically encounters passages that make no sense' (Kuhn 2000:59), because the words used have changed their meaning with the passing of a scientific revolution. When this occurs, the historian should assume that the author was rational, and attempt to discover through interpretation what meanings attached to those words. For example:

> An important clue to problems in reading Aristotle's physics is provided by the discovery that the term translated 'motion' in his text refers not simply to change of position but to all changes characterized by two end points. (Kuhn 2000:60)

Having realised that 'motion' has a different meaning from that commonly used in modern English, the reader must then look for alternate meanings for related words in order to maintain the coherence of the entire passage. In short, the historian is learning 'a new language'.

Language-learning and interpretation are therefore connected. They are not, on the other hand, connected to translation. Historians of science are not translators, as by definition incommensurability does not allow for translation across paradigms (Kuhn 2000 [1982]:43). According to Kuhn:

> [T]ranslation is something done by a person who knows two languages [...] the translator systematically substitutes words or strings of words in the other language for words of strings of words in the text in such a way as to produce an equivalent text in the other language. What it is to be an "equivalent text" can for the moment remain unspecified. (2000 [1982]:38)

In cases of incommensurability there is no such vocabulary in the target language, because the equivalent concepts do not exist, and so direct translation is impossible.

Practising scientists do not face the problem of speaking two incommensurable languages at the same time, although the schematic nature of Kuhn's presentation of his theory might lead to the impression that they do. When scientific revolutions occur, they are not instantaneous. Old paradigms coexist with the new ones for any length of time (1962:150-1), and so there is no 'moment' when a scientist is forced to abandon their old beliefs and adopt new ones. Indeed, Kuhn emphasises that that older scientists tend to hold on to their beliefs, and the revolution is only truly complete when they retire or die (Kuhn ibid:158). However, Kuhn also notes that 'Just because it is a transition between incommensurables, the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like the gestalt switch, it must occur all at once, (though not necessarily in an instant) or not at all' (ibid:150). This apparent contradiction can be explained. Although the individual scientist may have a moment where he or she accepts the new paradigm, that does not mean that the old vocabulary magically disappears from their head. Instead, the vocabulary takes on a different conceptual role, whereby it describes a theory which is (no longer) held to be true. Moreover, Kuhn is careful to distinguish the individual scientist from the community: 'To speak, as I repeatedly have, of a community's undergoing a gestalt shift is to compress an extended process into an instant, leaving no room for the microprocesses by which the change is achieved' (Kuhn 2000 [1989]:88).

In both chapters two and three I address the problem of whether Kuhn's thought is descriptive, prescriptive, a mixture of the two, or something else. Kuhn's attitude towards incommensurability comes out, in *SSR*, as simply part of the revolutionary description he gives of the history of science. Where 'traditional' history and philosophy of science make revolutions 'invisible' (Kuhn 1962:136-143), Kuhn wanted to show that progress in science was neither smooth nor teleological (ibid:172-3). On top of this, he wanted to show how scientists of past epochs could believe things that now seem impossibly odd to us (Kuhn 2000 [1989]:59).

In order to accomplish this, Kuhn embraced incommensurability as a way of showing how people could believe apparently impossible things. Our current paradigms are incommensurable with previous paradigms; there is 'no common language' into which they can both be translated, and, as a result, the former paradigm seems alien and incomprehensible.

Kuhn claims that after a scientific revolution, we live in a different world (1962:111); this claim is more metaphorical than literal, but can be read as both. Naturally the world *per se* continues to exist, as it always has done, irrespective of the understanding of scientists: 'There is no geographical transplantation; outside the laboratory everyday affairs usually continue as before' (ibid). However, the world *as experienced by scientists* changes, that is to say, the match between the world and scientists' understanding of it. What has changed is the concepts which relate to the ontology of the practice of scientists; and these concepts are linguistic, in the sense that they have names, and are fixed by reference.

Kuhn once again illustrates this with an example from the Copernican revolution:

> Can it conceivably be an accident, for example, that Western astronomers first saw change in the previously immutable heavens during the half-century after Copernicus' new paradigm was first proposed? The Chinese, whose cosmological beliefs did not preclude celestial change, had recorded the appearance of many new stars in the heavens at a much earlier date [...] Late sixteenth-century [western] astronomers repeatedly discovered that comets wandered at will through the space previously reserved for the immutable planets and stars. The very ease and rapidity with which astronomers saw new things when looking at old objects with old instruments may make us wish to say that, after Copernicus, astronomers lived in a different world.[11] (1962:116-7)

This example shows that 'working in a new world' does not end with the gestalt-switch of a scientific revolution. When the change occurs, the scientist's ontology (and accompanying conceptual web) is reshuffled. In this case, the earth was reconfigured as a planet, one of several which revolve

[11] The 'discovery' of comets is slightly more complicated than Kuhn's description suggests. Comets had frequently been seen in the 'immutable heavens' by westerners; two (presumed) examples are the 'Star of Bethlehem' which led the Magi to Jesus' birthplace, and the one depicted on the Bayeux Tapestry. However, these were seen as unpredictable and non-repeatable signals from God or the cosmos, as opposed to regular appearances of moving celestial objects.

around the sun, and the moon was removed from the category 'planet'. However, after making this switch, the scientific community was then in a position to look at the skies through the prism of this new ontology. The ontological and conceptual possibilities for research and discovery were different, and new things could be found which could not have been found before.

The important point here is that incommensurability is more than just a conceptual mismatch. Like everything in Kuhn's work, it is temporally situated, and the way a paradigm unfolds is incommensurable with the previous paradigm – it would be impossible to describe the progress of the new paradigm in the language of the old. For example, an Ancient Greek who took their language seriously would believe atoms to be the smallest possible unit of matter (a-tom – 'uncut', i.e. indivisible). In this case, talking about 'sub-atomic' particles would be literally nonsense, as nothing can be a division of the thing which can't be divided. Moving from the concept to the practice, the act of positing or looking for the Higgs Boson would be nonsensical. The discovery that atoms had component parts was therefore a scientific revolution in the purest Kuhnian sense: the ontological web had to be reconfigured in order to comply with the conceptual changes.

5.0 Theory of reference

5.1 Why have a new theory of reference?

In this section I introduce a theory of reference for newly-coined terms in scientific theories. The main motivation for introducing a separate theory of reference for terms in scientific theories is that the coining of new terms in scientific theories is a different phenomenon from the coining of words in natural language. Throughout this chapter I will introduce detailed evidence and arguments for this position, but for now I merely note that it has considerable intuitive appeal, for the following reasons. Most naming in natural language happened in the prehistoric past, and most ostensive theories of reference which appeal to 'coining', 'dubbing' or 'baptism' as a phenomenon, such as Putnam's (see 1975:215-272) and Kripke's (1980:87-105), acknowledge this fact as a concession to the historical artificiality of such approaches.[12] When dubbing does happen in natural language, when neologisms or jargon are consciously invented, I am quite happy to agree that it happens broadly in the way that Putnam describes. However, when neologisms are coined for the purposes of scientific theories, their application has a quite different nature. First of all, of course, they are not intended for general use, but for the use of a group of specialists who are in a position to understand the term. Second, they often purport to refer to things of dubious ontology; they may or may not refer to things which actually exist in nature, and instead the discovery of such posits is intended to provide evidence for the truth of the theory. Perhaps the most famous of these posits is the Higgs Boson, which is currently being searched for at CERN, but whose existence has never had empirical confirmation. The third reason for the intuitive appeal of a separate theory of reference for scientific terms is that the nature and identity of the posited items are carefully and strictly delineated within the theory being proposed. This contrasts starkly with neologisms in natural language, which often refer to fuzzy concepts rather than natural classes of things.

The theory of reference which I propose is based on a synthesis of 'dubbing' theories and alternative representational theories, dating back to Locke, which state that when we refer to things we refer to our idea of them, not to the things themselves.

[12] These theories of reference are also 'externalist', on the grounds that the meaning of terms lies outside the head of the referrer. See below for further details.

In this chapter I will give evidence for these arguments. I will address theories of reference based on dubbing; and I will use examples both from natural sciences and from linguistics (further arguments and discussion about the relationship between linguistics and natural science will be addressed in chapter 2). However this chapter will show conclusively that my theory of reference accounts for discrepancies between the meaning of terms in natural science and in linguistics, as well as accounting for disagreements across different linguistic disciplines. In this chapter I will also show why I consider that this theory of reference is a natural consequence of Thomas Kuhn's philosophy of language, especially his concept of incommensurability.

5.2 Metaphor and Science; Natural Kinds; Locke

In this section I will address some issues which have led me to propose my theory of reference. I will start with a consideration of metaphor in science, and the possibility of discovering 'natural kinds'.

Theories of metaphor in the philosophy of science can be divided into two camps: the 'standard' empiricist/positivist account, and the anti-realist viewpoint of Kuhn *et al* (see Montuschi (2000:278). Both viewpoints agree that metaphor can be useful if it helps us towards a literal exposition of the facts (Boyd, in Ortony 1979:356-409). One way in which this can happen is by using a metaphor for pedagogical purposes. For example, it has been common teaching practice since Rutherford to compare the structure of an atom to a 'mini solar system' (Boyd 1979:359), with the nucleus as the 'sun' and the electrons as the orbiting 'planets'. This helps learners to visualise the basic structure of an atom, and, at a basic level, the crucial differences between the structure of the solar system and that of an atom are of no consequence (for example, electrons do not *literally* orbit the nucleus). Boyd's empiricist view of science also holds that metaphor can be useful in framing scientific theories, when 'no adequate literal paraphrase is known' (Boyd 1979:360), in contrast to Black (1962:25-47), for whom metaphors merely add 'cognitive content'. According to this theory of metaphor, even though the metaphor does not literally describe the phenomenon at hand, it can be seen as just as precise as the literal description which will one day supersede it. The metaphor does the same work as a literal theory in finding out how nature divides at its joints.

The idea that nature divides at its 'joints' (as Kuhn tends to put it, e.g. (2000:206), is based on the idea that there are such things as 'natural kinds', and that science discovers ever more precise ways of isolating and describing them. The debate over whether or not natural kinds exist, whether we have knowledge of them, and what kinds of things they are, is ancient; Dupré ((2000a) in Newton Smith (2000)) dates it back to Aristotle. Natural kinds are 'those kinds, roughly speaking, that really exist in nature' (2000:311). Of modern philosophers, Putnam (1975:215-271) and Kripke (1980) have both developed theories of reference which take the existence of natural kinds for granted. These theories claim that when we refer to zebras, or water, or any other natural kind, we are literally referring to a kind of thing which is found in nature as a discrete class or set of things. As Kuhn describes this view, we accommodate language to the world (in Ortony1979:418), which is to say that we discover the true nature of natural kinds, and apply our labels more precisely to those kinds.

For Kuhn, however, this is upside down. Kuhn asks whether it might make more sense to talk of accommodating the world to language, or alternatively 'is what we refer to as "the world" perhaps a product of a mutual accommodation between experience and language?' (Kuhn 2000:418, and see also Goodman (1978) and Woolgar (1988) for two other accounts sceptical of natural kinds). This relates to metaphor in that any metaphor used in science will be *de facto* part of our way of looking at the world – it is a tool we have decided to use to describe it. For this reason Kuhn rejects natural kinds, as metaphor and language could have found 'other joints' in nature. This is consistent with Kuhn's view of science as the progress of our instrumental use of nature (see chapter 2). We can get better and better at using nature, but we are not zeroing in on what Kuhn describes as a Kantian *Ding an Sich* ('thing in itself'); this corresponds with the 'essences' of natural kinds, which standard theories of reference and metaphor describe.

I should make clear at this point that I am not using 'metaphor' in a technical sense. In literature and language studies there are a variety of approaches to the understanding of metaphors, their role in language and their exact nature.[13] However, in the field of philosophy of science which is concerned with the role of metaphor in forming or explaining scientific theories, including the papers by Boyd and Kuhn cited above, 'metaphor' is used in the everyday sense, and is surprisingly badly-defined. It merely stands for any statement along the lines of 'X is like a Y', or 'X is a Y' (where X is not usually to be an instance or example of Y).

While the debate between Kuhn and Boyd (*et al*) concerns the role of metaphor in scientific theories, I believe that a successful theory of reference needs to widen the scope of Kuhn's account, and view all of science itself as metaphor for our understanding of the world. This account of metaphor states that all types of theory are a metaphor for the thing in the world which they are trying to describe.

This can be illustrated by comparison with Locke's representational theory of language. Locke observed that words do not refer to things, but to our concepts (or 'ideas') of things[14] (1964 [1690]:259). This theory has several

[13] See Lakoff and Johnson (1980) for a standard cognitive approach to metaphor. Also Davidson (1984 [1978]:245-264) for a 'reductionist' philosophical approach to what metaphors mean.

[14] There are two problems with this. First, did Locke really hold this theory, and second, is it tenable? I am not in a position to prove the first either way. The second, I believe, is

appealing aspects, including solving the problem of negative reference, and explaining how we refer to abstract and non-existent objects in the same way that we refer to concrete ones (ibid:246-249). This theory also implies a certain scepticism about the possibility of direct knowledge of the world, and foreshadows Kant's insistence that we know nothing of the noumena (the *Ding an Sich*), only the phenomena (see Smith and Greene 1940:330-34).

In the same way, science is not a description of the world; rather it is a set of theories which describes our knowledge of the world. This entails a triadic relationship between us, science and the world, just as Locke's theory of reference entails a triadic relationship between us, things and words. It is also in line with Kuhn's relativistic assertion that our scientific theories do not converge towards 'truth', but instead represent our best understanding of the object of enquiry (1962:171-3, and see chapter two 2.3 for further discussion of this point). However, relativism does not imply scepticism. Science and its technological consequences provide evidence that scientific theories describe the best and most successful understanding we have of the world, but this does not entail that they directly depict or describe the world itself.

Realist accounts of natural science hold that, when successful, it interacts with the real world directly; we use objects to inspire and then confirm our scientific theories about them, and these objects have real, physical existence.[15] On the other hand, the social sciences do not deal with real objects but with mental (or behavioural) posits, and one of the strengths of the social sciences is their acknowledgement of this fact along with the es-

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answerable in the affirmative, and such an answer has been given by, among others, Norman Kretzmann, "The Main Thesis of Locke's Semantic Theory" in Tipton (1977:123-140).

[15] Virtually all theories of science are in some sense realist about its objects. See Leplin (2000) in Newton-Smith for a discussion of realist versus instrumentalist accounts of the nature of scientific theories.

pousal of a methodology which acknowledges the *ad hoc* nature of the posits, expecting each researcher to refine and justify them as research proceeds.[16]

For example, when a zoologist refers to 'a zebra', they are literally referring to this thing in the world, in exactly the same way as a visitor to a zoo does (although the account of reference developed later in this chapter also involves a certain amount of scepticism about zebras too). On the other hand, when an economist refers to 'the free market', they are not referring to a physical entity. The 'free market' is a metaphor (or akin to a metaphor, whether this is explicitly stated or not) for collective human behaviour, as defined more or less strictly within the confines of theory at hand. It does not, and does not purport to, refer to a physical object in the world.

Under this 'empiricist' account, then, only theories in social science (and subjects like TGG which also deal with mental posits) are metaphors, as they deal with mental or behavioural posits, while natural science theories are not metaphors in this sense, as they refer literally to things in the world. If natural science terms are not metaphorical then they refer to natural kinds. And if nature does indeed divide at discoverable joints, there is no reason why our descriptions of, for example, how the mind works should not match up with neural processes at some point in the future.

However, this does not accord with Kuhn's analysis of *all* science being our best description of the world, rather than a literal depiction of it. Nor does it fit with Kuhn's (partial) rejection of natural kinds, which I touched on above, and explore in much greater detail in the following section.

[16] See chapter 2 section 2.5 for further discussion of natural and social sciences.

Following Kuhn's theory of metaphor, then, we are directed towards the ultra-relativistic view that all scientific terms, whether in natural or social science, are metaphorical, in the sense that we cannot discover the essence of natural kinds or mental objects; we accommodate the world to our language at the same time as accommodating our language to the world.

Nevertheless, my Kuhnian-based account of metaphor can still argue that social sciences (and TGG) are less 'literal' than natural science. The terms of a theory in natural science refer to our understanding of the world, and this understanding of the world is based on empirical observation. However, this understanding of the world is only provisional, in that it is mediated by the language of the scientific theories used to describe it. That language and the understanding of the world to which it refers is liable to change and, ultimately, revolution, according to the progress of science. Social science, on the other hand, remains two steps away from reality, not just one like natural science. This is because the posits in social science do not refer literally to objects observed empirically in the world, they refer to putative mental or social phenomena which may or may not be empirically confirmed. There is an uncertain ontological connection between the posits and the world, and any social science theory which contains posits begs the question of whether those posits refer or not.

So we can maintain the traditional natural/social science divide under my Kuhnian-based interpretation of the role of metaphor in scientific theories, and this still accords with Kuhn's own account of metaphor and his account of the similarities and differences between the natural and social sciences.

5.3 Metaphor and linguistics

This reading of the metaphorical nature of the material of linguistic theories ties in with Kuhn's analysis of the similarities and differences between the natural and the social sciences. He suggested a hermeneutic base for the natural sciences, just as in social sciences. This 'hermeneutic base' refers to the act of pre-theoretical interpretation which establishes the ontological contents of a discipline, and lays down what kinds of thing may or may not be used in theory formation. The 'hermeneutic base' tends to be overlooked in the philosophy of science in favour of the non-hermeneutic everyday practices of natural science. In contrast, the social sciences acknowledge their hermeneutic base, and this acknowledgement is reinforced by their embrace of hermeneutic everyday practices (see chapter two section 2.5 for details of Kuhn's analysis of social science and hermeneutic interpretation).

If this is right, then the status of TGG becomes difficult to categorise. It is not a natural science with a hermeneutic base, as Kuhn contends all natural sciences are, because of the 'metaphorical' nature of its posits (such as VPs, cyclicity, phases etc). However, it is not a social science because it does not pair an awareness of its hermeneutic base with a hermeneutic methodology (and, more obviously, does not study social practices or situations). According to this Kuhnian analysis, on the other hand, sociolinguistics fits neatly into the social sciences with very little to distinguish it, methodologically speaking.

TGG proceeds along the same path as the social sciences, but tends not to acknowledge the *ad hoc* nature of its posits. I do not just mean the theoretical posits which are genuinely up for argument, and which often fall by the wayside[17]. I also mean the more fundamental operational tools of TGG such as the drawing of syntactic tree-diagrams. Tree diagrams are a rep-

[17] Chambers describes this as 'one of the most bizarre and tragicomic residues of any intellectual tradition' (2003:34). See chapter two, section 2.2 for the full quotation, and others.

resentative tool of a theory of a language, but it is not possible to answer, from a natural-science viewpoint, the question 'What is a tree diagram a diagram of?'. It is not a model or depiction of a brain process, it is a diagram of a theory of language. Even if the mind and the brain are both modular (Chomsky 2000b:117), there is not a one-one correspondence between mental and neural modules waiting to be discovered, as they are different types of module.

As such it is not the case that TGG models neural processes and mental processes in corresponding ways, and that tree diagrams (for example) are a way of depicting our knowledge of linguistic processes. They are, rather, a metaphor for our understanding of linguistic processes, in the same way that a social science posit such as 'identity' is a metaphor for a type of human behaviour, rather than a depiction of or a reference to a thing in the world, as when Bucholtz and Hall describe 'a framework for the analysis of identity as produced in linguistic interaction' (2005:585).[18]

So both TGG and sociolinguistics are metaphors for our understanding of language, and this is especially interesting in the case of TGG, because, I suspect, this conclusion would be much less acceptable to its practitioners than it would to sociolinguists. To repeat the point, if we were ever in a position to literally map language in the brain, the way we can, for example, literally map the orbit of Pluto, it would not have a one-one correspondence with the theories, diagrams and posits of TGG.

This leads us to the conclusion that TGG is epistemically equivalent to social science, but not part of it. By this I mean that, according to the division I made at the end of the last section, there are natural sciences, the language of whose theories is metaphorical of our understanding of the things in the world, and there are social sciences, the language of whose

[18] Chomsky has made repeated defences of studying the mind as a natural object. He specifically addresses the nature of posited mental objects in (2000b:44-5, 104).

theories refers to posits which may or may not coincide with the things in the world. The natural sciences are therefore one 'epistemic step' away from the world, as we interpret our knowledge of the world through a metaphorical language. The social sciences, meanwhile, are two epistemic steps away from the world, as there are both a metaphorical language *and* posits of uncertain ontological status between them.

Under these criteria, sociolinguistics is unquestionably a social science. TGG also falls into this second category, because it deals with posits. However, it is not a social science because its object of study does not lie in the social sphere. It is for this reason that I conclude that it is epistemically equivalent to the social sciences, while not belonging to them.

At the beginning of this section I touched on the Lockean notion of a representative theory of reference. In the next section, I will show how such a theory of reference can be allied to my analysis of metaphor in the language of scientific theories, in order to explain the existence of incommensurability across linguistic disciplines.

5.4 What is the link between metaphor, incommensurability and reference?

In this section I will synthesise the three different strands of the philosophy of language which are central to my theory of reference: metaphor, incommensurability and reference. In the introduction I described Kuhn's notion of incommensurable vocabularies, and his ideas on normal science and paradigms; I also touched on the distinction between natural and social sciences. In this section I introduced Kuhn's account of metaphor, and significantly extended it in order to make it compatible with his radical view of reference and natural kinds, and to explain why the different linguistic disciplines appear to conform to Kuhn's description of immature rather than normal science. I will now show how my account of metaphor can be combined with a hybrid of representative and causative theories of reference in order to explain how incommensurability arises across disciplines which appear to share an object of study.

The answer lies in the theory of 'dubbing' and ostensive reference, as explained by Putnam (1975:215-271), and also Kripke (1980) although, as Kuhn points out, their views are slightly different. I mentioned Putnam and Kripke earlier in this chapter as two philosophers who have developed 'externalist' theories of reference. They hold that part of the meaning of a term is determined by and refers to things in the world, and that when we refer to things we refer to natural kinds as they are constituted in nature; as Putnam summarises his position, 'Cut the pie any way you like, 'meanings' just ain't in the head!' (1975:227). I will concentrate on Putnam's position rather than Kripke's in this section, but in most significant aspects what I say about Putnam also applies to Kripke. Externalist theories of reference are widely held, but by no means universally, and the debate is very much alive. One of Putnam's main opponents, with whom he has conducted a long-running debate about the nature of meaning and reference, is Noam Chomsky, who holds an 'internalist' position with regard to the same questions (see Chomsky (2000b:19-45) for an exposition of Chomsky's view and his arguments against Putnam).

The theory of ostensive reference says that at some imaginary point in the past, someone pointed at a bucket of water and named it 'water'. From then on, whatever had that chemical composition was 'water'. It doesn't matter that they didn't know it was H_2O, just that 'that stuff' was 'water'. Again, see Locke (1964 [1690]:303), who really didn't know what the 'inner essence' of, for example, gold was. Now we at least have a much better idea, knowing that an atom of gold has the atomic number 79. It also does not matter that I do not accept the idea of natural kinds, (but see below), because the ostensive theory of reference is as much about a tradition of naming, passed down through generations, as it is about chemical formulas (or Lockean or Kantian essences). This is how normal language is said to work. It is also how normal science is said to work, because in normal science, empirically observable things are predicted, isolated and named ('dubbed'). Once that dubbing has taken place, the nomenclature spreads through the scientific community at the same time that experiments are repeated to confirm the existence or nature of the newly-dubbed entity. For example, the name 'Brownian motion' could only gain widespread acceptance alongside the widespread acceptance (or rejection) of the phenomenon of Brownian motion.

However, when we are dubbing mental posits, in TGG or in social science, we are one step further away from the unknowable essence of things (if, indeed, such an 'unknowable essence' is really there: from a Kuhnian point of view this is irrelevant). This means that one person can dub one mental posit, and another dub another mental posit. They cannot show each other, and they do not have to tell each other. Crucially, they can come from the same speech community (let's say English for the purposes of this argument). So as theories develop based on these alternate sets of mental posits, there is little reason to expect the webs of meaning (of dubbed mental posits) to match up. Indeed, it would be surprising if they did, as they are taking non-technical English words (like 'language') and applying them to theory-specific mental posits. With no history of ostensive reference for the word or term (e.g. 'linguistic competence'), its theoretical meaning is exactly what it is used to mean within that theory.

This explains also why TGG and sociolinguistics can be incommensurable without being in competition (as Kuhnian paradigms are usually taken to be). Their theoretical languages, while derived from English, have different (but overlapping) putative references.

There remain two things to add to this theory of reference in order to make it work. First, it should be noted that to an extent, Kuhn's theory *entails* a representational theory of ideas, but not necessarily in a Lockean sense (if indeed that was Locke's intention). Kuhn proposed an extreme ontological scepticism, as a result of the relativistic perspective on truth which was a consequence of his history of science. This should not be confused with a scepticism regarding science itself, which, as we have seen, he did not endorse in any sense. His ontological scepticism was a result of the apparent regularity with which scientific posits become redundant. If we stop believing in the ether, then to what does the term 'ether' refer? It can only refer to our concept of it, much like 'Father Christmas' or 'Sherlock Holmes'.

This Lockean theory of representation does not need to stand in opposition to a theory of ostensive reference, as given by Putnam or Kripke. In these theories, there is a chain of reference which leads back to the thing itself, or the initial dubbing. All the Lockean theory does is to replace the original *Ding an Sich* with the original idea, as conceived at the moment of dubbing. It should also be added here that Kuhn addresses Putnam's account of ostensive reference in 'Possible Worlds in History of Science' (2000:58-89), and is extremely critical of Putnam's theory. However, Kuhn's objections do not bear on my use of it. His primary objection is once more that the theory of ostensive reference accounts for our knowledge of natural kinds, a claim which he rejects outright. However, my account relates to the dubbing of entities, and especially mental posits, in scientific theories. His other objections focus on possible worlds themselves, and so are irrelevant to this thesis.

The next thing to note is that the Putnam/Kripke theory can be given a Kuhnian twist. We can attach a proviso to the ostensive theory of reference by adding that all dubbing is provisional, as it occurs in scientific theories. We have seen that a scientific revolution can dispose of, or completely transform, any member or subset of our ontology, so any dubbing can only refer to something as we believe it to exist within our paradigm. When 'water' was first dubbed, its chemical composition was not known. Indeed, it was not known that it had a chemical composition.[19] Our attitudes towards water have changed at least twice since then. First, we discovered that it contains two hydrogen atoms and one oxygen atom. Second, we discovered that 'heavy water' is not quite the same as normal water. All this was before Putnam introduced the hypothetical 'XYZ', the mythical substance which fulfils the role of water on 'Twin Earth':

> Twin Earth is very much like Earth. In fact, apart from the differences we shall specify [...] Twin Earth is *exactly* like Earth.
>
> One of the peculiarities of Twin Earth is that the liquid called 'water' is not H_2O but a different liquid whose chemical formula is very long and complicated. I shall abbreviate this formula simply as XYZ. I shall suppose that XYZ is indistinguishable from water at normal temperatures and pressures. In particular, it tastes like water and it quenches thirst like water. (1975:223)

The existence of XYZ would in theory further complicate the reference, but not change the point: our references to 'water' were only ever provisional, if we take the Putnam/Kripkean line of reasoning. The same, *in extremis*, goes for every other member of our ontology. This explains Kuhn's scepticism towards natural kinds.

This analysis concerns primarily the language of scientific theories. Kuhn's analysis of metaphor, natural kinds and incommensurability deals with the theoretical language of scientific theories, terms which are more or less precisely defined within the confines of a theory, and which also provide the language through which that theory can be articulated. It does

[19] Of course, as far as we know there never was an actual 'dubbing' of water, as there was for some natural kind terms (kangaroos, in both Aboriginal Australian languages and English, and polonium are two examples).

not necessarily apply to ordinary language (as explicitly stated at the beginning of this chapter). Whether or not a Lockean theory of ideas, or a Kripkean chain of reference, is acceptable to philosophers of language at large is irrelevant to the analysis of the language of scientific theories. We would expect that such rarefied language should behave differently from the much less loosely defined natural human languages. Although this theory of reference for scientific language could be useful in a more general theory of reference for ordinary language, it would not be sufficient to account for the ill-defined features of natural language which cause philosophical headaches, such as fuzziness, abstraction and non-referring terms. For example, where we are free in our everyday lives to refer to Father Christmas or the King of France, as Russell showed us, scientific theories do not refer to such things. There is no scientific paradigm which says 'oxygen exists, phlogiston does not'; it merely says 'oxygen exists' and it is impossible to talk about phlogiston within the confines of that paradigm.

We should, therefore, expect different rules to apply to the language of scientific theories, or at least variants on the normal rules. In particular, we should not be surprised to find that reference works differently in scientific theories, because the way things are named is different, and the putative contents of scientific theories are different from those of normal discourse. Science has a different, and in some ways stranger, ontology than everyday life, and consequently a different way of talking about it.

One predictable objection to this account of the language of scientific theories is its rejection of natural kinds. How, it might be asked, can a scientific theory not assume the existence of natural kinds such as water, hydrogen or oxygen? This is indeed counter-intuitive, but not, in my opinion, a big problem for the theory. This extended Kuhnian interpretation is sceptical about our knowledge of natural kinds, but agnostic as to their actual existence. We work with a conception of natural kinds in science, of course, and any natural science assumes that the entities posited in its ontology are extant. However, only time will tell whether those natural kinds are accepted or not after the next scientific revolution, or the one after that. This demonstrates a recurrent aspect of Kuhn's attitude towards science, and the more positive and practical side of his relativism; science is a useful and very successful tool, but it does not, and does not need to, converge on the truth, or any ultimate 'reality'.

So there are several stages to this theory of reference. First, science takes a natural language term and uses it to dub either an observable physical phenomenon (e.g. 'atom', when atoms were actually observed, as opposed to when they were just posits), or a posited theoretical entity ('language', for example). This refers, however, to an idea, not to a thing in the world. We know, from the study of the history of science (and Kuhn's analysis of it) that all scientific posits must remain provisional, as any one of them can (and probably will) be overturned at some point in the future. It may sound paradoxical to say that when we observe something new (an atom for example) that we are naming the idea of it, not the thing itself. But Kuhn's description of the natural sciences shows us that what we are 'seeing' is based on a pre-conditioning about what we expect to see, or think we are seeing. There is no 'observation-neutral' observation.

In this way we can say that all scientific language is metaphorical of our understanding of how the world works, while maintaining the difference between those sciences which deal directly with observable phenomena (the natural sciences) and those which deal with conceptual posits (the social sciences and linguistics).

If the foregoing still seems unnecessarily sceptical of the relationship between language and the world, it can be added here that while Kuhn's philosophy entails some sort of theory of representation with regard to scientific language, it precludes a reductive idealism along Berkeleyan[20] lines. Scientific revolutions not only mean seeing the world in a different way from before, such as seeing change in the heavens, when before they were assumed to be immutable. They also mean looking for and observing things that have not been observed before, because the previous paradigm did not allow for their discovery. There must, therefore, be things 'out there', beyond the scope of our ideas of them – we do not merely discover new 'ideas' after a scientific revolution, or even after a new discovery. Heavy water was not just an idea waiting for us to discover it. Our discovery of deuterium rather led to us naming a new 'idea'. This does not contradict the Kuhnian scepticism about natural kinds. Even if some day 'deuterium' drops out of common scientific discourse, there was still *something* which caused us to dub a new idea with that name. Scepticism about natural kinds does not entail scepticism about the natural world; but, to paraphrase Auden, the world and the language we use to describe it is, like love, much odder than we thought.

5.5 Issues solved by this theory of reference

I mentioned in the beginning of this chapter that there are different motivations for this theory. First of all, it has intuitive appeal: reference in scientific theories perhaps should work differently to reference in normal language. Second, it solves various problems in the history and metatheory of the study of language. Outlining, exploring and explaining these problems is the aim of this thesis. The most significant of these problems, as I have already indicated, is the incommensurability between different types of linguistics. Describing, elucidating and justifying this incommensurability will be the focus of much of this thesis. In chapter two I give a detailed de-

[20] George Berkeley's 'idealism' held that there exists no 'unthinking substance' outside our ideas of what we perceive; in other words, 'to be is to be perceived'. It is often held up as a warning against the dangers of being too philosophically reductive and denying the existence of the real world. See Smith and Greene (1940:1-95).

scription of Kuhn's notion of incommensurability. In chapters three and four I show how incommensurability between different types of linguistics has arisen from opposing philosophical standpoints; and in chapter five I show how this incommensurability manifests itself in the theoretical vocabulary of different types of linguistics.

As well as incommensurability, this theory of reference also helps with other problems thrown up by the analysis of the history of linguistics and its relationship with the philosophy of science. It helps explain why linguists are so prone to accusing each other of being unscientific (chapter three). It can help account for the arguments of the period following the publication of *SSR* over whether or not TGG ought to be seen as a Kuhnian paradigm. And it can shed light on why linguistics has periodic bouts of metatheoretical uncertainty over its epistemological foundations (chapter four) and its identity (chapters two and three).

Chapter two: definitions

The purpose of this chapter is to define more closely some key terms and theories, some of which I have already touched upon in chapter one. This chapter is divided into four parts. The first part describes and analyses Kuhn's theory of scientific paradigms and revolutions, and his theory of incommensurability across scientific paradigms. I looked at both of these briefly in chapter one; this chapter provides a deeper analysis of these two key aspects of his philosophy. It also outlines Kuhn's ideas on the differences between the natural and social sciences, and on the demarcation of science. Part two consists of criticisms of various parts of Kuhn's theories.

Part three is concerned with the institutional makeup of linguistics, and in particular the two types of linguistics which this thesis focuses on. It also addresses questions about how such institutional divisions arise: what is a school of linguistics, or a type of linguistics, or a theory of linguistics; are there any substantive differences between them? Part four describes and defines two philosophical positions, Rationalism and Empiricism, as they have been used both in 'pure' epistemology, and in relation to linguistic theories.

The definitions and delineations of the above terms and positions will then be used in chapter three, which is concerned with developments and arguments in the history of linguistics.

Part 1: Kuhn

1.1 Outline of Kuhn's theory of paradigms

Kuhn's theory, laid out in *The Structure of Scientific Revolutions* (1962), is a sociological account of the history of science, and an examination of what we can learn about science from that account. He says that the mature sciences have followed a similar pattern in their development. This pattern is as follows:

i) Pre-paradigmatic (immature) science, consisting of more or less random fact-gathering, and almost as many rival theories as there are practitioners to account for these facts. (Kuhn 1962:10-22)

ii) First paradigm, which provides a framework attracting all (or nearly all) of the members of the community. This paradigm solves numerous problems and confidently promises to solve more (some of which may not have existed before the creation of that paradigm). (1962:23-34)

iii) Normal (mature) science, in which (nearly) all scientists work within the same framework, trying to solve similar problems, or 'puzzles' as Kuhn tends to call them (1962:35-42). This framework includes 'exemplars', or demonstrations from within that paradigm which teach newcomers how it works while, at the same time, proving its efficacy. The circularity of showing and using theoretical posits in the exemplars accounts for the unlikelihood of a scientist challenging the paradigm, as long as it is fruitful.

iv) Crisis science, where what were once puzzles to be solved start to look like problems for the theory/paradigm itself. (1962:52-76)

v) Revolution, whereby a 'young turk' (or turks)[21] comes up with a new theory which solves the puzzles which have become problems for the old theory. This new theory attracts younger scientists, but mostly not the older ones, who must die out before acceptance of the new theory is complete (1962:92-143). Acceptance of the new theory means a return to 'normal science', as described in step iii) above.

vi) Steps iii), iv) and v) now recur, possibly *ad infinitum*.

It should not need pointing out that this account is heavily schematic; history is contingent on circumstance and never repeats itself down to the last detail. Every scientific revolution takes place under different circumstances, whether political, institutional, religious or personal. For example, Lavoisier's theory of oxygen did not leave him open to threats to his freedom or personal safety in the same way as Galileo's cosmology brought him into conflict with the Catholic church. However, Lavoisier did encounter institutional opposition in the form of senior scientists who had invested their whole careers in the theory of phlogiston, and were therefore unwilling to let it go without a fight. Kuhn's chronology is also schematic (discussed further in 2.2 below). A new 'paradigm' may be decades in the making, and may take centuries to be accepted. Bearing these issues in mind, Kuhn extrapolated the common aspects of each revolution he studied to produce his schema.

The Structure of Scientific Revolutions contains examples from various episodes from the history of science. To begin with, in order to illustrate pre-paradigmatic or immature science, he looks at the study of electricity in the early 18th century:

> During that period there were almost as many views about the nature of electricity as there were important electrical experimenters, men like Hauksbee,

[21] Koerner (1994a) uses this phrase.

> Gray, Desaguliers, Du Fay, Nollett, Watson, Franklin, and others. All their numerous concepts of electricity had something in common – they were partially derived from one or another version of the mechanico-corpuscular philosophy that guided all scientific research of the day [...] Yet [...] their theories had no more than a family resemblance.
>
> One early group of theories [...] regarded attraction and frictional generation as the fundamental electrical phenomena [...] Other "electricians" [...] took attraction and repulsion to be equally elementary manifestations of electricity. (1962:13-14)

The two important points Kuhn makes here about pre-paradigmatic study are that any practitioner can produce their own fundamental theory, and that philosophy plays a significant role in the development of those theories. As we shall see, these are both absent from 'mature' science. Kuhn presents the field of electrical research in the early 18th century as an example of the change from pre-paradigmatic to mature science, as the work of Franklin had a dramatic impact:

> Only through the work of Franklin and his immediate successors did a theory arise that could account with something like equal facility for very nearly all these effects and that therefore could and did provide a subsequent generation of "electricians" with a common paradigm for research. (ibid:15)

So a first paradigm came into being. Franklin and 'his immediate successors' formulated the first paradigm, and the next generation worked with it as normal science. There is no sense in which Kuhn describes the adoption of the paradigm as instant or uncontroversial, a point I elaborate on in 2.2 below.

Moving on to a different field of research, Kuhn holds up Newton's *Principia Mathematica* (1687) as instituting a paradigm in physics (1962:12-13). After Newton, there was no need constantly to re-examine the bases of physics, as the entire community agreed on their validity. Instead, scientists could concentrate on solving the puzzles thrown up by Newton's laws. For Kuhn this constitutes a period of 'normal science'. The as-yet unsolved puzzles were assumed to be solvable within the constraints of the paradigm, and on the whole when puzzles were attempted, they were eventually solved. Unsolved puzzles tended to be seen as challenging rather than problematic (1962:25-28). A puzzle is 'challenging' precisely because it is assumed to have a solution:

> One of the things a scientific community acquires with a paradigm is a criterion for choosing problems that, while the paradigm is taken for granted, can be assumed to have solutions. To a great extent these are the only problems that the community will admit as scientific or encourage its members to undertake. (1962:37)

When it becomes apparent that a puzzle does not have a solution, it becomes problematic rather than challenging; that is to say, there is no point looking for an answer. Instead, the scientific community begins to investigate why their assumptions have led them to ask unanswerable questions, and this self-examination is what Kuhn refers to as 'crisis science'. In relation to the Copernican revolution, Kuhn notes that:

> By the early sixteenth century an increasing number of Europe's best astronomers were recognizing that the astronomical paradigm was failing in application to its own traditional problems. (1962:69)

Towards the end of the nineteenth century it started to become clear that there were serious problems with Newtonian mechanics. Kuhn (1962:72-75) describes how Maxwell's investigations into electromagnetic behaviour (amongst other things) conflicted with the theory of 'ether drag', and this gradually led to a 'crisis' in physics. Where there had once been puzzles to be solved within the framework of Newtonian mechanics, there were now glaring omissions which the theory could not, apparently, solve.

Against this backdrop, it was possible for Einstein's relativity theory to be accepted. This new theory dispensed with many of the tenets of the old, and solved problems which the old one could not. Crucially for Kuhn's account, it was incompatible with the old one. They could not coexist, so physicists had to choose between them. This 'revolution' overturned the world of physics, and laid down a new paradigm, one within which physicists are still working today (1962:98-102).

Kuhn finds this pattern in other episodes of western science, such as the discovery of oxygen by Lavoisier and/or Priestley, and Copernicus' introduction of the heliocentric view of the solar system (1962, especially 66-76).

This is, I hope, an accurate and neutral representation of Kuhn's theory of paradigms and scientific revolutions. It may appear to be relatively simple to express, but this means, as will become apparent, that it is wide open to interpretation and consequently easy to abuse.

1.2 Details of the theory of incommensurability

In chapter one I introduced the notion of incommensurability between competing paradigms, a feature of Kuhn's description of the development of natural sciences which became more significant in his later philosophy. When a discipline undergoes a paradigm shift, the successive paradigms tend to be incommensurable with each other (1962:103). Similarly, the competing paradigms in an immature science are often incommensurable with each other, a situation which needs to be resolved if a unified, mature science is to develop.

In this section I compare and contrast the different interpretations of 'incommensurability' provided by Kuhn and Paul Feyerabend. I then look in detail at two aspects of incommensurability which are particularly relevant to this thesis: the distinction between ontological and methodological incommensurability, and the idea of local incommensurability. This is in preparation for chapter five, where I compare and contrast two co-existing types of linguistics, and surmise to what extent the label 'incommensurable' can be applied to them.

1.2.1 Kuhn and Feyerabend, incommensurability and language

Paul Feyerabend (1924-1984), like Kuhn, wrote philosophy of science from a historical perspective, except that, as we shall see, he disliked philosophy of science. He wrote from a left-wing anarchist point of view, which sought to see the role of science in society in its proper place, and, most importantly, to use science to increase freedom. His works have liberty, rather than theory, at their base, and he saw science as inherently political in this regard. In *Against Method* (1975) he sets out his conception of incommensurability, which was developed simultaneously with, but independently of, Kuhn's conception of it. It is similar to Kuhn's, but there are differences. For example, he presents a comprehensive argument for why rationality, the philosophy of science in general, and rules and regulations governing the practice of scientists, can all go hang. He gives examples from history showing that most rules are flouted at least some of the time, and all that remains is creativity and imagination, along with a certain amount of bloody-minded determination. Feyerabend is entirely positive about science, but, like Kuhn, is keen to place it within a socio-historical context, rather than in the abstract theorising of the Vienna circle and other 'empiricists' whose ideas he is so keen to reject.

Incommensurability also takes in the question of what Feyerabend calls 'rationality' (1975:269-70). For Popper, the refutation of a current theory is the primary goal of scientists, and, once refuted, the over-arching constraint of rationality requires that a theory be consigned to the scrap heap (Popper 1963:33-66). For Feyerabend, the trump-card of rationality is an unnecessary hindrance to the creative activities of scientists. His famous insistence that 'anything goes' includes practices which Popper would regard as irrational. If a scientist sees a theory disconfirmed by an experiment, he or she should be entitled to continue to work with that theory anyway, free from the accusation of irrationality. He cites in favour of this the contention that Copernicus was 'irrational' for pursuing heliocentricism, when all standards of rationality told against it (1975:155).[22] Feyerabend's methodology in this sense, extends to cover which ways of thinking are allowed, or disallowed (if any):

> How is the 'irrationality' of the transition period overcome? It is overcome in the usual way, [...] i.e. by the determined production of nonsense until the material produced is rich[23] enough to permit the rebels to reveal, and everyone else to recognize, new universal principles [...] Madness turns into sanity provided it is sufficiently rich and sufficiently regular to function as the basis of a new world view. And when *that* happens, then we have a new problem: how can the old view be compared with the new view? (1975:270)

This quotation is interesting on several counts. First, the description of the 'transition' is pure Kuhn. The two didn't have disputes, so much as each ploughing an independent furrow, but here similarity turns into synchronicity. Second, the 'determined production of nonsense' echoes many criticisms of TGG made down the years. Harris says that 'Each time Chomsky goes through one of his mini-paradigm shifts, he leaves what

[22] Kuhn (1962:152) provides even more compelling evidence of the creative role of irrationality in the history of science, with the story that, prior to Copernicus, Kepler derived inspiration from an entirely 'irrational' motivation, sun-worship.

[23] Feyerabend does not define exactly what he means by 'rich' in this passage, but it seems to refer to the persuasive content of a theory in terms of its ability to solve problems, as well as its explanatory scope.

Jackendoff terms "disillusioned Kuhnian debris" littering his wake' (1993:260)[24]. Chambers uses a similar metaphor:

> To cite just a few – the affix shift transformation (Chomsky 1957:39-42), the Katz-Postal principle (Chomsky 1965:132), the specified-subject condition (Chomsky 1973), the root clause filter (Chomsky and Lasnik 1977:486), or the antecedent trace chain (Chomsky 1988:116-17). These postulates gather dust with dozens of others in the generativist scrapyard that is surely one of the most bizarre and tragicomic residues of any intellectual tradition. (2003:34)[25]

However, where Jackendoff and Chambers are criticising Chomsky for his theoretical profligacy, for Feyerabend this is a natural consequence of scientific anarchy. If anything goes, then the chances of anything being found increase dramatically.

Feyerabend's attitude towards incommensurability takes two directions. First, he is keen to argue that it is real, against the denials of 'empiricists' such as 'Carnap, Feigl, Hempel, Nagel, and others'[26] (1975:280). The 'empiricist' line is that any language used to formulate a theory is related back to an older 'observation' language', which forms a kind of baseline for comparison between different, and putatively incommensurable, scientific theories. For Feyerabend, this makes as much sense as claiming that, when children learn to speak, they 'start from an innate observation language' (ibid).

He is keen to provide this theoretical basis for the possibility of incommensurability, because it is integral to his thesis that scientific anarchy

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[24] Harris is of course referring to people, not posits, here – posits cannot be described as 'disillusioned'. However, the linguists he is referring to are those whose work has used those posits, and therefore becomes outdated by changes in theory or ontology.

[25] See also Postal (2004:1-12),

[26] Rudolf Carnap, Herbert Feigl, Carl Hempel and Ernst Nagel were just four of the many philosophers more or less involved in the 'Vienna Circle'. The members of this group were broadly empiricists, and the circle was particularly famous for its articulation of logical positivism.

(i.e. the absence of rules in scientific practice) is preferable to a constrictive, rule-based philosophy of science. Proliferation of scientific theories is the professed end of his argument (1975:35-46), that different theories should be allowed and encouraged to coexist, no matter how conflicting they appear to be. Feyerabend (1975:36-7,45) explains how theories tend towards conservatism, (as does Kuhn (1962:35)), and a mixture of imagination and irrationality is required to change them. Incommensurability aids this process:

> Incommensurable theories, then, can be *refuted* by reference to their own respective kinds of experience; i.e. by discovering the *internal contradictions* from which they are suffering. (1975:284)

There is something Panglossian about Feyerabend. He takes a moral stance towards a traditionally amoral subject matter, and shows why humans are both the start and the endpoint of 'facts'; science should be both human and humanitarian. Feyerabend looks back at the glorious mess of history and concludes that in the absence of clearly-defined guidelines, the frailties of human experience have produced more knowledge than the codification of what we should be allowed to know or believe. In other words, theories ain't what they used to be.

Feyerabend really does take science apart, right down to the requirement of rationality. This is why his approach to the philosophy of science is known as 'anarchism' (1975:21), and his motto is 'anything goes' (1975:28, 1978:39-40).

Incommensurability occupies much of Kuhn's later work, from the 1970s and 1980s, a period in which he spent a lot of time either clarifying or backtracking from some of the bolder claims he made in *SSR*. In the case of incommensurability, this meant refocusing the issue on language. By contrast, Feyerabend addresses theoretical incommensurability *by analogy* with language:

> I have much sympathy with the view, formulated clearly and elegantly by Whorff [sic] [...] that languages and the reaction patterns they involve are not merely instruments for *describing* events (facts, states of affairs), but that they are also *shapers* of events (facts, states of affairs), that their 'grammar' contains a cosmology, a comprehensive view of the world, of society, of the situation of man which influences thought, behaviour, perception [...]
>
> I also believe that scientific theories, such as Aristotle's theory of motion, the theory of relativity, the quantum theory, classical and modern cosmology are sufficiently general, sufficiently 'deep' and have developed in sufficiently complex ways to be considered along the same lines as natural languages. (1975:223-4)[27]

For Kuhn, on the other hand, incommensurability of scientific theories *is* a matter of language in a literal sense; it is not analogous to it. Kuhn demonstrates the partial incommensurability of foreign languages, using English and French as an example, in 'Commensurability, Comparability, Communicability' (in Kuhn 2000 [1983]:33-57). Where Feyerabend takes a holistic (and rather under-developed) approach to this linguistic incommensurability, Kuhn discusses in detail how the different words in the lexicon of a language are related in a 'web' of meaning. Rather than addressing grammar, as Feyerabend does, Kuhn then looks at the vocabulary of a scientific theory. This is certainly clearer, as it is not so obvious what the 'grammar' of a scientific theory might entail; whereas a theory certainly has a web of technical words whose meanings depend on each other (Kuhn 2000 [1983]:44).[28]

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[27] See section 2.4 below, on 'Criticisms of Incommensurability', for a fuller discussion of the link between theories of incommensurability and Whorfian theories of linguistic relativity.

[28] Kuhn's description of 'webs of meaning' calls to mind Saussure's view that language 'is a system of signs in which the only essential thing is the union of meanings and sound images, and in which both parts of the sign are psychological' (1974 [1916]:15). However,

For Feyerabend then, incommensurability is something to be searched out and celebrated. For Kuhn, as ever, it is not so simple. Kuhn shies away from value judgements, although he is clearly 'pro-science'. However, he was consistently and famously misinterpreted as claiming that science is irrational, and consequently as a relativist in regard to science (see part 2.3 of this chapter for a further discussion of this). He was not. There is no suggestion in any of his work that he regarded science as anything but the pinnacle of human knowledge, one whose progress could be seen in all of its applications in our daily lives. What he did claim was that what scientists believe today might, and presumably will, be overturned at some point by a new paradigm whose ontology and methodology is incommensurable with our own, and possibly unintelligible to us.

His primary focus as a historian was to show how we have got to where we are, and if incommensurability is a part of that, then we should accept it as a fact of history, and not find it offensive. Some aspects of the history of science may appear irrational, but that is because the history of science is a human history, which has happened in real time. Incommensurability seems to be part of this history. We can therefore contrast Kuhn's more neutral attitude towards the history of science with Feyerabend's, which is largely positive; this again contrasts with Feyerabend's attitude towards modern-day philosophy of science, which is largely negative.

I have provided this comparison of the difference between Kuhn's and Feyerabend's conceptions of incommensurability in order to show that theories of incommensurability can take different forms, of varying degrees of plausibility, and to show that there is no orthodoxy about incommensurability, even to the extent that philosophers dispute what kinds of thing are

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chapter 5 explains in detail exactly what Kuhn's 'webs of meaning' entail, and disproves the idea that his philosophy of language is simply Saussurean-style structuralism.

incommensurable with each other. However, in the rest of this thesis I concentrate on Kuhn's version, unless I state otherwise.

This leads on to the next section. Methodological theory, as distinct from the conceptual schemata I have just been addressing, is paradigm-based just as much as ontology.[29] For this reason, methodology from alternative paradigms can seem incomprehensible, erroneous, or just plain odd.

1.2.2 Ontology and methodology

Feyerabend's writings on incommensurability tend to concentrate on methodology rather than ontology. Kuhn does write about incommensurable methodologies (1962:103), but this tends to be as a natural consequence of incommensurable ontologies, or paradigm shifts. For Kuhn, a paradigm is not just the set of objects postulated within a scientific theory. It is also a social practice, encompassing all the scientists and institutions involved in the paradigm, and the artefacts connected to it, including textbooks and equipment. The exemplars described in the textbooks delimit the nature of the investigative practices of a paradigm, as does the equipment used. In any one paradigm, the availability of a certain piece of equipment will determine whether or not it is used; moreover, paradigms allow or disallow *ab initio* certain pieces of equipment, and this determines, and is determined by, which methodologies are considered permissible and which are not. Accordingly, you cannot build an electron microscope unless you are looking for electrons. Atoms had to be split conceptually before this apparatus could be built; so the construction of the electron microscope was incommensurable with the earlier paradigm, just as much as the activity of using it.

[29] The distinction between ontological and methodological incommensurability is found in the Stanford Encyclopedia of Philosophy http://plato.stanford.edu/entries/thomas-kuhn/.

Exorcism, to give another example, is not allowed in modern science. However good the evidence gets, modern science will not allow exorcism as a methodology, and therefore any instruments which purport to measure or describe exorcism, or provide incantations for it, will also be disallowed. It would take a revolution to re-introduce exorcism into the scientific paradigm. The same goes for astrology charts. Feyerabend (1975:274n) discusses the example of how to determine whether incubi are capable of reproducing or not.

'Exemplars' are also important in Kuhn's description of science. An exemplar is a standard experiment or procedure which is used to demonstrate a theory, and is a vital part of the training of a scientist. It is easy to see how exemplars from different paradigms would be incommensurable: an experiment which demonstrates the existence or nature of phlogiston would not be translatable into the language of one which demonstrates the existence of oxygen.

So there are different types of things – or rather, putative things which are put forward as candidates for scientific study – and there are different ways of studying them. Sometimes these differences are incommensurable. It is tempting to see scientific methodology as simple, and in some ways it is: form a hypothesis; perform an experiment to confirm or disconfirm it; refine, repeat. However, when it comes to equipment and the specific practices of a particular field, especially in the social sciences, it becomes less simple. In chapter four I examine some of the consequences for linguistics of the different types of methodology which are sometimes pressed into service in the study of language.

1.2.3 Local incommensurability

> The claim that two theories are incommensurable is more modest than many of its critics have supposed. (Kuhn 2000:36)

Although I am focusing on incommensurability in some detail, it is worth pointing out that it is quite rare in the grander scheme of things. Most situations which might be candidates for incommensurability are really just a bit different from each other. For example, Darwin's theory of the evolution of life on earth was not incommensurable with the preceding, and competing, Lamarckian view of evolution. Where Lamarck described the inheritance of acquired characteristics, Darwin posited natural selection through random mutation. The two theories coexisted in 'the same world', and their proponents were able to conduct lively and meaningful debates in the same language about which theory was correct (see McKinney (1971) for the differences between Lamarckian and Darwinian evolution).

> To use an expression from the eighties, Kuhn has only asserted 'local incommensurability': only a small group of usually interlinked concepts changes meaning in a revolution. (Hoyningen-Huene (1990:489))[30]

Incommensurability is generally *local*, in that it extends only to that subset of the conceptual scheme, and the related language, which deals directly with the theories under discussion. Creationists and evolutionists can sit down with a coffee and discuss the weather, even though they cannot begin to discuss the beginning of life on earth.[31] Incommensurability in Kuhn's sense does not involve two entirely different world views – it only applies to that part of the world which is actually being studied, and only when it is being studied. As a consequence, a sunset is equally beautiful for a Copernican as it is for a Ptolemaic observer. Incommensurability is also *temporally local*, in that it only applies to networks of words when they

[30] See also Kuhn (2000:36), Irzik and Grünberg (1998:215) and Barker (2001:436).
[31] Intelligent Design is supposed to remedy this lack of communication. The fact that evolutionary scientists utterly reject Intelligent Design, even though it tries to speak to scientists in their own language, is a good indication that the differences are profoundly conceptual.

are used in the context of a particular scientific theory. So a Copernican can remark 'the sun is hot today' without this being incommensurable with a Ptolemaic view of the world, as the word 'sun' is not, in that moment, being used with its technical meaning within a scientific theory.

It is only when these criteria are satisfied that words, concepts or theories can be described as incommensurable. So when a Copernican looks at the sky and says 'I can see three planets and the moon', this makes as much sense to an English-speaking Ptolemaic observer as saying 'there are four people in my family and my sister'; in other words, no sense at all.

In this example, the two will be talking past each other, and no meaningful conversation can occur without some kind of language learning occurring. The Ptolemaic astronomer will need to learn that the Copernican does not include the moon in the set of planets, and instead regards it as belonging to a separate set, satellites of the earth of which it is the only member. In the same way it is conceivable that an English-speaking culture could exist which differed from ours only in that they did not, for some reason, regard sisters as members of the family. The way they talked about families, and the way we do, would therefore be incommensurable.

1.3 Issues of science: Kuhn on natural science, social science and demarcation

This section examines two more issues in the philosophy of science, and Kuhn's position on them. First, the question of the nature of the difference – if any – between the natural and the social sciences; and second, the question of what constitutes a science and what does not. There are two reasons for grouping these together. First, they are both concerned with marking the difference between types of study. Second, they are only of minor importance in Kuhn's writing, although he did pay some attention to them. From the perspective of this thesis, it is not so important to definitively answer the question of whether or not we can label linguistics a natural or social science, or any kind of science at all; I am more concerned with looking at the arguments that have been made about the epistemological foundations of different disciplines, and seeing how different forms of linguistics might fit into this – particularly from a Kuhnian point of view.

1.3.1 Kuhn's analysis of the human-natural science divide

In chapter one I briefly introduced the question of whether linguistics can be seen as a social science rather than a natural science, and what such a division might entail for our view of language. In this section I will examine Kuhn's view of the difference between natural and social sciences, and how that division fits in with the rest of his theory of science.

Kuhn's reading of the divide between the natural and social sciences is consistent with his philosophy of the natural sciences, and belies his claim that he has not thought much about it. In his short paper 'The Natural and Human Sciences' (2000 [1991]), Kuhn sets out his views on the contrast between the human sciences and the natural sciences. The paper is set out as a reply to Taylor (1985),[32] and was delivered at a symposium at which he and Taylor were intended to debate their views on the subject, although Taylor withdrew, leaving the floor to Kuhn. Coming as it does towards the end of Kuhn's life (he died in 1996), it is markedly different from *The Structure of Scientific Revolutions* (1962), both in content and in tone. The content of 'The Natural and Human Sciences' is the basis of this

[32] Charles Taylor's *Interpretation and the Sciences of Man* (1985) compares the study of human action with the study of inanimate objects, and concludes that they are fundamentally different types of object of study, therefore requiring fundamentally different methods of study. As Kuhn points out, this is a fairly standard approach to the study of social sciences. Taylor's work covers epistemology and philosophy of science in relation to a more wide-ranging political philosophy.

section; I need not say much about the tone, except to note that in common with much of his later writing, this essay engages more with other theorists, is more tentative, and in some instances seems either to backtrack from or apologise for the sweeping and schematic nature of *SSR*.

Since Kuhn's views are formulated in response to Taylor's, they must be examined in that context. Accordingly, this section examines Kuhn's disagreement with Taylor by considering the two opposing views of the nature of the natural and human sciences, the possibilities for 'drawing the line' between the two types of science, and the possibility of closing the gap in the future.

I should address one point at the outset. Kuhn quite candidly admits that:

> Then and now, my acquaintance with the social sciences was extremely limited. My present topic – the relation of the natural and human sciences – is not one I have thought a great deal about, nor do I have the background to do so. (2000:217)

Kuhn was not a philosopher of social science. He was a philosopher of natural science who was asked briefly to turn his thoughts towards social science. It might seem more fruitful to ignore his thoughts on the subject which, while coherent, are neither detailed nor generally regarded as significant, in favour of the wide literature which deals with the philosophy of social science.[33] This would be the correct approach if I were trying to establish the place of linguistics within the social sciences, and more specifically, if I were trying to establish that sociolinguistics (for example) fits comfortably into the social science paradigm. However, Kuhn's paper is worth attention because it does neither of the above. While admitting that there are well-established methodological differences between the two sets

[33] See Hollis (1994) for a comprehensive treatment of the philosophy of social science, as well as a substantial bibliography of the major works in the field.

of sciences, he presents an alternative reading of the divide. He describes reading Weber's methodological essays:

> What I found in them thrilled and encouraged me. [Weber was] describing the social sciences in ways that closely paralleled the sort of description I hoped to provide for the physical sciences. [...]
>
> My euphoria was, however, regularly damped by the closing paragraphs of these discussions, which reminded readers that their analyses applied only to the *Geisteswissenschaften*, the social sciences. "*Die Naturwissenschaften*," their authors loudly proclaimed, "*sind ganz anders*" ("The natural sciences are entirely different"). What then followed was a relatively standard, quasi-positivist, empiricist account. (2000 [1991]:216-7)

Kuhn claims that he and Taylor agree that the natural and the human sciences are different, but disagree on what that difference is. For Taylor, human actions have meaning in a way that other objects (for example rock patterns and snow crystals) do not. For this reason, the study of human actions requires *hermeneutic interpretation*,[34] whereas the study of rock patterns requires no interpretation.

> The object of a science of interpretation must be describable in terms of sense and nonsense, coherence and its absence; and must admit of a distinction between meaning and its expression [...]
>
> We can speak of sense or coherence, and of their different embodiments, in connection with such phenomena as gestalts, or patterns in rock formations, or snow crystals, where the notion of expression has no real warrant. What is lacking here is the notion of a subject for whom these meanings are. (1985:2)

[34] Hollis (1994:16) defines hermeneutics as follows: 'Its central proposition is that the social world must be understood from within, rather than explained from without.' See also Rorty (1979:315-356) for a discussion of hermeneutics, which he proposes is 'not "another way of knowing" – "understanding" as opposed to predictive "explanation". It is better seen as another way of coping.'

This seems like a common-sensical notion. The patterns in rock formations do not mean anything – they just are. Human action, on the other hand, is meaningful, from the point of view both of the subject and the observer. Presumably this definition of 'action' discounts things like sneezing. In order to 'explain' human action, interpretation is needed on the part of the explainer – it is to be explained in terms of the meaning that such action might have had for the subject, and in terms of what such meaning could mean both for others and for the observer. Rock patterns do not exhibit such meaning, according to Taylor. Although we can use rock patterns as evidence in our theories about natural history, the patterns themselves do not have 'meaning' in the way that human actions do.

Kuhn disagrees with Taylor on this point. For Kuhn, natural phenomena require hermeneutic interpretation, in that for different people at different times they have different meanings. For example, celestial objects are different for us to what they were for the ancient Greeks: for the Greeks the Moon was a planet, and the Milky Way was in the same class as meteors. For Kuhn, this difference is a difference in hermeneutic interpretation.

> As in the case of equity or negotiation, neither the presentation nor the study of examples [of planets] can begin until the concept of the object to be exemplified or studied is available. And what makes it available, whether in the natural or the social sciences, is a culture, within which it is transmitted by exemplification, sometimes in altered form, from one generation to the next.
>
> I do, in short, really believe some – though by no means all – of the nonsense attributed to me. The heavens of the Greeks were irreducibly different from ours. The nature of the difference is the same as that Taylor so brilliantly describes between the social practices of different cultures. In both cases the difference is rooted in conceptual vocabulary. In neither can it be bridged by description in a brute data, behavioural vocabulary. (2000:220)

So first of all, according to Kuhn's analysis, pre-theoretical 'conceptual vocabulary' plays as strong a role in the natural sciences as it does in the social sciences. Kuhn's major thesis here is that the pre-theoretical bases of natural and social sciences are both hermeneutic. This is to say that to understand the natural science of any period, and to understand the difference between two periods of natural science, requires the same type of interpretation of meaning as the study of two geographically and culturally distinct groups of humans. Looking at and comparing the courtship rituals of the Belgians and the Shona is similar in kind to the comparison of Ptolemaic cosmology with Copernican cosmology.

> My argument has so far been that the natural sciences of any period are grounded in a set of concepts that the current generation of practitioners inherit from their immediate predecessors. That set of concepts is a historical product, embedded in the culture to which current practitioners are initiated by training, and it is accessible to nonmembers only through the hermeneutic techniques by which historians and anthropologists come to understand other modes of thought. (2000:221)

The way scientists learn such pre-theoretical vocabulary is either through being part of that culture, or through 'hermeneutic interpretation', by which Kuhn means the normal interpretive techniques of social scientists studying other cultures, or historians studying the past; scientists use the same technique to learn the pre-theoretical vocabulary of the natural sciences.

However, Kuhn highlights a crucial difference between the natural and social sciences, as the day-to-day activity of natural scientists involves virtually no hermeneutic interpretation, being instead mainly composed of puzzle-solving. This division of a subject into its 'base' (hermeneutic in both cases) and its 'practice of research' (mainly puzzle-solving for the natural sciences, mainly hermeneutic for the social sciences) is the key to Kuhn's thesis. His definition of a science is not the problem-solving activities of the scientists, or the accumulated knowledge of the field, but the sociocultural groupings which stand in a temporal relation to the progression of that knowledge. It is the exclusive nature of these groupings which requires hermeneutic interpretation. For an Andaman Islander to become a western scientist would indeed require hermeneutic reinterpretation, but being a western scientist does not:

> If one adopts the viewpoint I've been describing toward the natural sciences, it is striking that what their practitioners mostly do, given a paradigm or hermeneutic basis, is not ordinarily hermeneutic. Rather, they put to use the paradigm received from their teachers in an endeavour I've spoken of as normal science, an enterprise that attempts to solve puzzles like those of improving and extending the match between theory and experiment at the advancing forefront of the field. (2000:221-222)

On the other hand, doing social science does indeed require hermeneutic interpretation as a matter of course. The shifting nature of the subject leads, in the way social sciences are currently studied, to a constant evaluation of the meaning of the actions being studied:

> The social sciences, on the other hand – at least for scholars like Taylor, for whose view I have the deepest respect – appear to be hermeneutic, interpretive, through and through. Very little of what goes on in them at all resembles the normal puzzle-solving research of the natural sciences. Their aim is, or should be in Taylor's view, to understand behaviour, not to discover the laws, if any, that govern it. That difference has a converse that seems to me equally striking. In the natural sciences the practice of research does occasionally produce new paradigms, new ways of understanding nature, of reading its texts. But the people responsible for those changes were not looking for them. The reinterpretation that resulted from their work was involuntary, often the work of the next generation. The people responsible typically failed to recognize the nature of what they had done. Contrast that pattern with the one normal to Taylor's social sciences. In the latter, new and deeper interpretations are the recognized object of the game. (2000:222)

We have seen that Kuhn sets out to blur the distinction between human actions, the subject of the social sciences, which are held to be meaningful and require interpretation, and rock formations and other objects of the natural sciences, which do not have meaning and do not therefore require interpretation. He does not disagree that human actions 'have meaning', rather that rock formations do not. His example of the heavens is a historical one with many layers, but essentially says that the Greek conception of the heavens *preceded* the study of them. To repeat, placing the Milky Way in the same class of objects as meteors was a hermeneutic act, in the sense that it was an interpretive reading of the night sky. This also applies to rock formations. Rocks have 'meaning' for our society, in that we can only understand them by reference to our current paradigm. This paradigm (I presume) includes things like an understanding of the age of the earth, erosion, tectonic plates and volcanic activity. We tend to call these concepts 'modern science', but Kuhn's thesis is that they are a paradigm just as much as pre-twentieth century geology assumed a much shorter time-scale and no tectonic plates, and before that Noachian Deluvianism or monsters trapped in volcanoes. While the day-to-day activities of modern geologists may consist largely of normal-scientific puzzle-solving activity, the rocks 'have meaning' for us just as much as human actions do, because our study of them is temporally situated within a scientific paradigm; and, crucially, for an outsider to understand this paradigm involves hermeneutic interpretation of the beliefs and actions of the members of the scientific paradigm.

Taylor's analysis of hermeneutic enquiry was not located at this initial classificatory level, however. For him, it is the practice of the human sciences which merits the label 'hermeneutic'. The nature of social scientific investigation changes with its practice, or, as Kuhn would put it, 'new and deeper interpretations are the recognized object of the game.' Humans studying humans recognise that a continuous re-evaluation of the concepts being used to study humans must be attempted in order to make sense of both the researcher and the researched.

The two points of disagreement between Taylor and Kuhn, then, are over the daily practices of a discipline and its cultural/epistemological foundation. For Kuhn, the latter is more important, and the type of daily activities of natural and social scientists, whether puzzle-solving or hermeneutic interpretation, are secondary and likely to change over time. For Taylor, the activities of the two types of scientists are primary, in that they are of fundamentally different types. Taylor does not address the historical bases of the different types of science so much as the nature of their objects of enquiry, but the difference between 'basis' and methodology holds. Rocks, for Taylor, are simply not meaning-bearing entities, whatever the historical or cultural processes which led to modern geology, so the natural sciences require no hermeneutic interpretation; human actions have meaning, so they do.

It is important for Kuhn's argument that phrases such as 'conceptual vocabulary' and 'pre-theoretical vocabulary' apply to the language of scientific theories, not necessarily to natural language learning. I think that Kuhn does not make this point forcefully enough at times, and this can lead to confusion. When he talks about the 'Ancient Greeks', he is talking about Ancient Greek astronomers; when he talks about 'us', he is talking about scientists from within the current scientific paradigm. Non-scientists use the same vocabulary, of course, and the related concepts derive from science when applicable. So virtually all English-speakers believe that the moon revolves around the earth, and the earth around the sun. Similarly they know that Mars, the earth and Venus are all planets, while the moon is not. However, the subject of Kuhn's philosophy of science and of my dissertation is specifically the language of scientific theories, not of natural languages, and I will address this in detail in the next chapter.

Towards the end of 'The Natural and the Human Sciences' Kuhn suggests that there may be some disciplines which are progressing towards 'normal science', by which he means puzzle-solving. It is worth quoting the following passage in full:

> What I'm uncertain about is not whether differences exist [between natural and human sciences], but whether they are principled or merely a consequence of the relative states of development of the two sets of fields. (2000:221)

> [O]ne may still reasonably ask whether they [the human sciences] are restricted to the hermeneutic, to interpretation. Isn't it possible that here and there, over time, an increasing number of specialties will find paradigms that can support normal, puzzle-solving research?

> About the answer to that question, I am totally uncertain. But I shall venture two remarks, pointing in opposite directions. First, I'm aware of no principle that bars the possibility that one or another part of some human science might find a paradigm capable of supporting normal, puzzle-solving research. And the likelihood of that transition's occurring is for me increased by a strong sense of déjà vu. Much of what is ordinarily said to argue the impossibility of puzzle-solving research in the human sciences was said two centuries ago to bar the possibility of a science of chemistry and was repeated a century later to show the impossibility of a science of living things. Very probably the transition I'm suggesting is already under way in some current specialties within the human sciences. My impression is that in parts of economics and psychology, the case might already be made. (2000:222)

However, he adds that this may be impossible in other disciplines, not for theoretical reasons, but because of practical considerations:

> On the other hand, in some major parts of the human sciences there is a strong and well-known argument against the possibility of anything like normal, puzzle-solving research. I earlier insisted that the Greek heavens were different from ours. I should now also insist that the transition between them was relatively sudden, that it resulted from research done on the prior version of the heavens, and that the heavens remained the same while that research was under way. Without that stability, the research responsible for the change could not have occurred. But stability of that sort cannot be expected

> when the unit under study is a social or political system. No lasting base for normal, puzzle-solving science need be available to those who investigate them; hermeneutic reinterpretation may be constantly required. Where that is the case, the line that Charles Taylor seeks between the human and the natural sciences may be firmly in place. I expect that in some areas it may forever remain there. (2000:223)

As ever, rather than dealing in absolutes, Kuhn's definitions are temporally situated. The hermeneutic nature of the practices of its members is an inevitable consequence of the *social* science nature of anthropology, but the fact that at the moment it is the best accumulated body of knowledge we have to describe and compare different societies is indicative of the fact that it is social *science*. The progression of anthropology towards normal science is not a foregone conclusion, but neither is it ruled out on account of the nature of the object of enquiry.

Kuhn's paper is not intended to be the last word on this subject, as he makes clear in the answer to his own question: 'Isn't it possible that here and there, over time, an increasing number of specialties will find paradigms that can support normal, puzzle-solving research? About the answer to that question, I am totally uncertain.' (2000:222) Although the main thrust of his argument emphasises the underlying similarities between social sciences and natural sciences, he does acknowledge the possibility that there may be a practical barrier to social sciences advancing towards normal science. This tentativeness may be unhelpful, but it shows honestly the difference between the suitability of Kuhn's theory in relation to natural sciences, and its interesting uneasiness when applied to other disciplines.

Richard Rorty also takes issue with Taylor on the divide, (albeit with a different text, namely Taylor (1971)):

> The line that Taylor is describing is not the line between the human and the nonhuman but between that portion of the field of inquiry where we feel rather uncertain that we have the right vocabulary at hand and that portion where we feel rather certain that we do [...] In a sufficiently long perspective, man may turn out to be less δεινός[35] than Sophocles thought him, and the elementary forces of nature more so than modern physicalists dream. (1979:352)

Rorty's prediction is consistent with his other views on natural and social sciences – that the division is an artificial one rather than a meaningful one (see 1980:343-356). However, as with many critics of Kuhn, Rorty's analysis only half hits its mark, because it misses the fact that Kuhn's view of the natural-social science divide is primarily a description, not a manifesto. Perhaps 'man may turn out to be less δεινός than Sophocles thought him', but that does not alter the fact that at the moment science proceeds as Kuhn described: natural sciences have a puzzle-solving day-to-day existence, while the social sciences proceed hermeneutically.

Rorty draws parallels between his reanalysis of the situation into normal-abnormal discourse and Kuhn's normal-crisis science divide (1979:320). He claims that this encompasses and by implication renders obsolete the division of disciplines into the 'sciences of man' and the 'sciences of nature' (ibid:321). For Rorty the difference is habit:

> Normal discourse is that which is conducted within an agreed upon set of conventions about what counts as a relevant contribution [...] Abnormal discourse is what happens when someone joins in the discourse who is ignorant of these conventions or who sets them aside. (ibid:320)

The reception of such a person is that 'he is either 'kooky' (if he loses his point) or 'revolutionary' (if he gains it) (ibid:339). For Rorty, anyone who

[35] Although δεινός usually means both 'wonderful' and 'terrible', Rorty appears to be using it to mean 'unknowable', 'unpredictable' or 'strange'.

misunderstands those conventions is engaging in abnormal discourse. For Kuhn, however, the introduction of abnormal discourse is motivated by the need to solve a puzzle which the current paradigm cannot. The difference between 'kooky' and 'revolutionary' is as much in the intent of the speaker as it is in the ear or mind of the listener. It is precisely because natural sciences operate as puzzle-solving day-to-day activities that Kuhn referred any challenge to this as 'crisis'. For Kuhn, those who challenge normal science are not outsiders who don't know the rules of scientific discourse; nor are they unmotivated; they are working (albeit usually younger) paid-up members of the paradigm (1962:144). So, again, Rorty's criticism or refinement of Kuhn's position is not damaging to Kuhn's characterisation of the natural-social science divide.

Another writer who has had an influence on views of the natural science-social science divide is Michel Foucault, who was briefly mentioned in chapter one. He agrees with Rorty that the line between the two is in some sense unimportant (1970:344-5), the important thing being that they are all part of the same episteme which makes such belief possible, although Rorty takes issue with what he feels is Foucault's inconsistent epistemological approach (see Couzens Hoy 1986). The extent to which this applies to Kuhn's concept of incommensurability is discussed below.

Foucault draws clear dividing lines between the two types of study, and says that natural sciences are removed from the matrices of power in a way that social sciences never can be (see Olssen 2006:26, Dreyfus and Rabinow 1982:106-167). However, he draws a tripartite divide between the natural sciences, the three 'empiricities', or quasi-sciences of the modern era, (1970:347), as he terms philology, economics and biology, and the human sciences (literary criticism, sociology and psychology). The empiricities, while having the human as the object of study, do not have 'man' as their object of study; in other words, they too are removed from the matrices of power, and are epistemically more akin to the natural sciences than the human sciences. Writing when generative grammar was very young, Foucault makes a distinction between philology, the empiricity concerned with 'languages' (1970:280-300), and linguistics, the possible science of the next era, concerned with 'language' free from 'man' (ibid:380-2). He holds great hopes for the second, as something which can help kill off the idea of 'man', by instead studying something completely separate (language) (see Sheridan 46-88). 'Linguistics' here seems to be Saussurean *and* Chomskyan linguistics, the two combined into a mega-structuralism (see chapter four for more on Chomsky's Saussurean heritage, and Foucault (1970:xiv) for his attitude towards structuralism), and so for the purposes of unravelling the question of the relationship between different types of linguistics, and why different types of linguistics seem to be different types of study, it begs rather than answers the question.

Foucault is confident about placing linguistics within the realms of normal science. This is partly to do with what he deems to be the object of study. For Foucault, the human sciences study 'man' as an object, while investigating the same thing as the subject; that is to say, social sciences are reflexive modes of study, where the subject and object are the same thing. For him, this incongruity prevents anything like normal scientific study (1970:364-5). However, I think he is over-confident that the object of study in linguistics and philology is so different. Both involve the postulation of temporary and metaphorical mental objects, and, as I explained in the previous chapter, that is the decisive factor which separates the object of study in natural sciences from that in either immature or social sciences, whichever division is being used.

So in relation to Kuhn's analysis of the natural-social science divide, Foucault's approach is different. It places the two types of study on the same footing, within the same episteme, and then draws differences owing to their subject matter. Kuhn, as noted, places them on similar footings, and draws differences according to their methods.

Although Kuhn apparently did not like being championed and misrepresented by Rorty as being anti-science (Tartaglia 2007:178), Rorty's ideas about social and natural science are, to a large extent, in harmony with Kuhn's'. In placing primacy not on the daily activities of a given discipline, but on its historical foundations, Kuhn leaves the door open to a gradual lessening of the divide: 'I'm aware of no principle that bars the possibility that one or another part of some human science might find a paradigm capable of supporting normal, puzzle-solving research' (2000:222). However, he hedges his bets, by saying that in some areas such as politics the division is likely to continue.

1.3.2 Kuhn and demarcation

Here I will return to the question of demarcation, which I have already touched on several times; that is, how to tell whether a given area of study is 'scientific' or not, especially from the point of view of its practitioners and its rivals. Demarcation of science from within linguistics is partly a propagandist issue. That is, the argument does not take the form of asking 'what is science and what is not?', but rather takes the form 'this is why [*my subject/your subject*] is/isn't a science', and this forms the subject of the first half of chapter three.

Kuhn does *not* have demarcation at the forefront of his philosophy, and the most important issue regarding Kuhn and demarcation is that of how others have interpreted and used his philosophy. The following quotation from a philosophy of science textbook underscores a common misinterpretation of Kuhn's theory. This misinterpretation forms one of the cornerstones of chapter 3 and so is worth looking at in detail:

> Some aspects of Kuhn's writings might give the impression that his account of the nature of science is purely a *descriptive* one, that is, that he aims to do

> nothing more than to describe scientific theories or paradigms and the activity of scientists. Were this the case, then Kuhn's account of science would be of little value as a *theory* of science [...]. Unless the descriptive account of science is shaped by some theory, no guidance is offered as to what kinds of activities and products of activities are to be described. In particular, the activities and productions of hack scientists would need to be documented in as much detail as the achievements of an Einstein or a Galileo. (Chalmers 1978:98)

The point here is that Kuhn's theory of science is a *theory* of science. It is not just a description (although it includes this), and it is not a history of anything else. Kuhn does not present a prescriptive account of how to do science; however, his description of the history of science is theoretically structured to address certain questions. These include the historical question 'how has the history of science unfolded?'. However, they also include the theoretical questions 'Why does science unfold in this way?', 'What makes science 'science'?', 'Why is science different?' and 'What is science for, and what does it do?'. In answering these questions, Kuhn provides a theory of science. Kuhn's theory includes an examination of some salient sociological aspects of scientific communities. However, this is not in order to give a social history of science, but to use the social behaviour of scientific groups to provide answers to the above questions about the nature of science.

On the other hand, to repeat the crucial part of Chalmer's argument:

> Some aspects of Kuhn's writings might give the impression that his account of the nature of science is purely a *descriptive* one [...]. Were this the case, then Kuhn's account of science would be of little value as a *theory* of science [...]

If Kuhn's theory is to be a theory, then, it must contain some normative or prescriptivist elements. However, this normative aspect of scientific revolutions, both sociological and theory-based, comes from examining how and why certain scientific developments took place in certain times and places. It is not normative in that it gives a blueprint for how to be scientific. It gives details on what kind of theoretical and sociological elements are present in scientific revolutions, and therefore which ones are likely to be present in future revolutions, but it does not give an exhaustive account of how to make such a revolution come about, or how to turn a pre-paradigmatic, non-scientific subject, into a fully fledged scientific paradigm. In discovering why we study Galileo and Einstein, as opposed to 'hack scientists', we find out what makes science special, and scientific revolutions interesting. However, we cannot hope to find the 'next Galileo', or even the next science, from such observation.

So there is a tension within Kuhn's theory concerning normativity. In one sense, his theory is not normative, in that it does not provide a guide to how to turn a particular, non-scientific field of study into a scientific paradigm, or how to provoke revolution within a given paradigm. In another sense, however, it is normative in that it describes necessary and sufficient theoretical and sociological conditions for scientific revolution and the emergence of first paradigms. If it did not, it would not be a theory, merely a historical description.[36]

On the whole, Kuhn (1962:160-171) has relatively little to say about demarcation.[37] When he does address it, it is with a gnomic question: 'it can only clarify, not solve, our present difficulty [that of demarcation] to recognize that we tend to see as science any field in which progress is marked [...]. Does a field make progress because it is a science, or is it a science

[36] In chapter three I will examine claims that some linguists who have used Kuhn in a prescriptivist way have done so in the first, erroneous, way, in order to validate their field of study, rather than in the second way, which would only apply when their field was unquestionably paradigmatic and had bequeathed its own historical details to the template of scientific revolutions.
[37] This might appear surprising given that this issue had traditionally been seen as central to the philosophy of science (see Popper 1963:33-63, et al).

because it makes progress?' However, Kuhn goes on to shift the emphasis for demarcation away from progress:

> If we doubt, as many do, that non-scientific fields make progress, that cannot be because individual schools make none [...]. The man who argues that philosophy, for example, has made no progress emphasizes that there are still Aristotelians, not that Aristotelianism has failed to progress [...].
>
> With respect to normal science, then, part of the answer to the problem of progress lies simply in the eye of the beholder. Scientific progress is not different in kind from progress in other fields, but the absence at most times of competing schools that question each other's aims and standards makes the progress of a normal-scientific community far easier to see. That, however, *is only part of the answer and by no means the most important part.* (1962:162-3, *my emphasis*)

The implication here is that the absence of competing schools[38] is one of the phenotypical markers of a mature science, as much as the presence of progress, which might normally be seen as being more central to a scientific enterprise. However, Kuhn goes on to describe other aspects of scientific communities, particularly the unintelligibility of their work to the outside world, and the fact that standards of proof are upheld only by other members of the community, not the public or political or other authorities. He also mentions that scientists tend not to read the classics in their fields, and that they learn the trade from up-to-the-minute textbooks (this last point is much further developed in chapter four). As discussed above, another outward characteristic of scientific fields is that most of the work done involves 'puzzle solving'. Any given paradigm throws up puzzles, and it is the job of scientists to pick a puzzle and solve it. For Kuhn, a significant part of the definition of a mature science is that it is a puzzle-solving activity, with the practitioners working on the assumption that their para-

[38] See chapter three section 2 for discussion of the existence of competing schools in modern linguistics.

digm provides a framework in which the puzzles they are working on can be solved (1962:35-42).

The overall conclusion can only be that Kuhn is not terribly interested in the question of demarcation. Despite his assertion that cohesion of the field 'is only part of the answer and by no means the most important part', this cohesion is repeatedly alluded to by Kuhn (1962:160-173), so it seems justifiable to take this as one important condition for a scientific discipline.

Kuhn explains how chemistry and physics are scientific, what is interesting and essential about their scientific status, and what it means for them to be scientific. He gives a working definition of necessary and sufficient conditions for science, based on observation of the history of science. This working definition does not give license to apply Kuhn's theory to any subject, look for fit, and proclaim a science. Kuhn was interested in the internal workings of chemistry and physics because they are the most successful sciences. Although this sounds circular, it is not viciously so. By looking at the history of unquestionably 'scientific' disciplines, Kuhn was able to obtain insights into what 'being scientific' means.

It follows from this that just because any other subject fits Kuhn's pattern, that does not necessarily make it a science. The point really goes the other way. If we could find an unquestionably scientific subject which did *not* fit Kuhn's theory, we would show Kuhn to be (partially) wrong.

The subject of demarcation recurs in chapter three, in relation to claims and counter-claims made by linguists about their own type of linguistics, or about other types. In section one of that chapter I assess claims from linguists that their own discipline is scientific, and instances of linguists claiming that another strand of linguistics is unscientific, either in its methodology or in its ontological (or other philosophical) assumptions. In section 2 I look at examples of linguists claiming that their subject follows the Kuhnian pattern, and at instances of linguists from one area claiming that another strand of linguistics does not follow the Kuhnian pattern. These latter claims tend to come with the implication that in being non-Kuhnian, the discipline under discussion is therefore not scientific.

Part two: arguments against, and misunderstandings of, Kuhn's philosophy

2.1 Paradigms

In the five decades since Kuhn published *SSR*, his notion of scientific paradigms has come under intense scrutiny and criticism. The main point of most of these criticisms can be found in one form or another in either Shapere (1964) or Masterman (1970).

Shapere's criticism of Kuhn's concept of paradigms is that it is too general to be coherently articulated, and therefore of no use. For Shapere, 'anything that allows science to accomplish anything can be a part of (or somehow involved in) a paradigm' (1964:385). Kuhn called Shapere's criticism 'the most thoughtful and thorough negative account of this problem (1974:294n), where by 'this problem' he meant 'the large number of different senses in which the term is used' (ibid.).

Masterman's criticism of Kuhn is similar, but more constructive. She identifies twenty-one different senses in which the word 'paradigm' is used in *SSR*. These range from the very general, such as '(*6*) as a whole tradition, and in some sense a model' or '(*19*) as a general epistemological viewpoint', to the oddly specific, such as '(*13*) as an anomalous pack of cards' or '(*14*) as a machine-tool factory' (1970:61-65). Masterman concedes that 'it is evident that not all these senses "paradigm" are inconsistent with each other: some may even be elucidations of others' (ibid:65). She then divides the twenty-one senses into three types, those which define a 'metaphysical paradigm' those which define a 'sociological paradigm' and those which define an 'artefact or construct paradigm'. The first category includes such things as 'a set of beliefs' and may perhaps be crudely paraphrased as a world-view. The second includes such things as 'a universally-recognised scientific achievement' or 'a set of political institutions'. The third includes such things as 'a textbook or classic work'. In some ways Masterman gives primacy to the third of these, because the main task of scientists doing normal science within a paradigm is puzzle-solving, and 'for any puzzle which is really a puzzle to be solved by using a paradigm, this paradigm must be a construct, an artefact, a system, a tool; together with the manual of instructions for using it successfully and a method of interpretation of what it does (ibid:70).

Masterman credits Kuhn with the observation that scientists do normal science, and this is the reason it is important to define a paradigm precisely:

> That there is normal science – and that it is exactly as Kuhn says it is – is the outstanding, the crashingly obvious fact which confronts and hits any philosophers of science who set out, in practical or technological manner, to do any scientific research. It is because Kuhn – at last – has noticed this central fact about all real science [...] that actual scientists are now, increasingly, reading Kuhn instead of Popper [...] It is thus scientifically urgent, as well as philosophically important, to try to find out what a Kuhnian paradigm is. (1970:60)

It is an interesting point that for Masterman (a scientist, not a philosopher (ibid)), normal science is the crucial aspect of Kuhn's theory. Science is an activity, so the history and philosophy of science should be about what scientists do. However, it should be obvious (if not 'crashingly' so), that scientists have beliefs, communities and tools. Kuhn subsumes all of these under the word 'paradigm', and jokes that if he had written an index for *SSR*, 'its most frequently consulted entry would be : "paradigm, 1-172, passim."' (Kuhn 1974:294). It is clearly a nebulous concept, but it may not be problematically nebulous. Kuhn gave the word 'paradigm' to a very wide concept. What is relevant for my purposes is the characteristics which he attributes to paradigms, especially those such as 'normal science' and revolution. Kuhn himself, in 'Second Thoughts on Paradigms', addressed the various criticisms of the concept, especially those claiming that 'it can be too nearly all things to all people' (ibid:293). Kuhn concedes, at the end of this paper, that 'we shall be able to dispense with the term 'paradigm', though not with the concept that led to its introduction' (ibid:319). In section 3.3 below I will examine why at this point he preferred to partly replace 'paradigm' with 'disciplinary matrix'.

2.2 Synchronic/diachronic diversity

I have already noted that Kuhn's account of scientific paradigms and revolutions is heavily schematic; this should not be a problem as long as it is recognised. One area in which it becomes problematic is in trying to discern the time-frame under which paradigm shifts take place. The schematic account says that at some point during a period of crisis science, a new paradigm is formed which eventually commands the allegiance of all the practitioners in the field, except for some older scientists who become increasingly irrelevant and then die. It has frequently been pointed out that history is rarely this neat. Newmeyer (1996:29) quotes Laudan (1977:137) on this:

> We speak of the Darwinian revolution in nineteenth-century biology, even though it is almost certainly the case that only a small fraction of working biologists in the last half of the nineteenth century were Darwinians. We speak of a Newtonian revolution in early eighteenth-century physics, even though most natural philosophers in the period were not Newtonians.

Laudan provides an interesting solution to this lacuna in Kuhn's account, suggesting that

> A scientific revolution occurs when a research tradition, hitherto unknown to, or ignored by, scientists in a given field reaches a point of development where scientists in the field feel obliged to consider it seriously as a contender for the allegiance of themselves or their colleagues. (ibid.)

Again, this seems a plausible modification to Kuhn's account. When we are dealing with groups of people, their ideas change slowly, partially and inconsistently. Conversely, there are many potentially revolutionary ideas, but it is only when they are taken seriously that any kind of change occurs. There are many people who believe that the earth was created five thousand years ago, but as long geologists feel free to ignore them, then there is no revolutionary impetus to that idea.

Kuhn's analysis of the co-existence of paradigms comes with a crucial caveat. Although 'Old paradigms coexist with the new ones for any length of time' (1962:158), this is indicative of a period of crisis science; it does not constitute 'normal 'science'. In times of normal science, revolutions can be discerned retrospectively; in times of crisis or immature science, competing incommensurable paradigms can coexist.

We saw above (section 1.3.1 of this chapter) that Kuhn gives an idea of the difficulty of untangling the question of the possibility of disciplines making the transition from human to natural science:

> Very probably the transition I'm suggesting is already under way in some current specialties within the human sciences [...] On the other hand, in some major parts of the human sciences there is a strong and well-known argument against the possibility of anything like normal, puzzle-solving research. I earlier insisted that the Greek heavens were different from ours. I should now also insist that the transition between them was relatively sudden, that it re-

sulted from research done on the prior version of the heavens, and that the heavens remained the same while that research was under way.(2000:222-3)

So, for Kuhn, paradigms can form slowly and can be observed doing so, while giving the impression that the change from one paradigm to another can be 'relatively sudden'. Normal science, by definition, does not *normally* allow for competing paradigms (Kuhn 1962(1969):209). It is clear from the chronology of Kuhn's schematisation that as long as there are competing (and incommensurable) paradigms, we must usually refer to this as a period of immature or crisis science. However, it is always contingent on history and individual circumstances; the change from one mature paradigm, to crisis, to revolution, to a new mature paradigm, is in its details different every time.

2.3 Relativism and irrationality

In chapter one I briefly touched on three novel aspects of Kuhn's account of the history of science, which went against the received Popperian falsificationist theory of science (as well as the standard inductivist empirical assumptions of earlier centuries):

- the supposedly uncritical nature of 'normal science'
- the 'irrational' switches from one paradigm to another
- the 'relativist' attitude towards scientific truth (including 'incommensurability')

The first of these flew in the face of Popper's falsificationist theory, which said that the role of the scientist was to test theories critically and accept them only as long as they are unfalsified (Popper 1963:33-66). For Kuhn, the scientist working within a paradigm during times of normal science has no choice but to accept the tenets of the paradigm, and has little desire or incentive to challenge them, for both sociological and intellectual reasons.

The second concerns the reason for switching from one paradigm to another. Kuhn often refers to a standard practice in gestalt psychology, where subjects look at a page covered with coloured dots, and can 'see' the pattern of dots as different things. Where the scientists once saw 'ducks', the story goes, they now see 'rabbits' (1962:111). This is an example of Kuhn inviting undeserved criticism, in this case the charge that scientists act in an essentially irrational way. Although this 'gestalt switch' constitutes a vital part of the revolution, Kuhn makes it clear that a new paradigm solves the salient problems which the old one could not, as well as the problems which the old one could. The change is rational, and the scientists *choose* to see 'rabbits' rather than 'ducks' (1962:77-91).

The analogy between gestalt switch and scientific revolution is useful, but as Barker points out, it is only an analogy:

> Perhaps Kuhn was too successful in explaining his new concept. The idea of a Gestalt switch and the illustrations in terms of duck-rabbit figures were dramatic, and easy to understand, but misleading in crucial respects. To avoid further misunderstandings he dropped references to Gestalt switches and the visual consequences of scientific revolutions. (2001:437)

Much has been made of the supposed 'irrationality' of the behaviour of scientists as Kuhn describes it. He supposedly sees them as little better than sheep[39], slavishly following their paradigm-masters until one scientist with a flash of imagination comes along and leads the younger scientists off to another paradigm, only for the process to be repeated *ad nauseam*. Under this reading scientists are drudges who are actively discouraged from any lateral thinking, questioning of authority or imagination; they are said to

[39] Kuhn (1962:167) responds to this criticism that scientists are 'like the typical character of Orwell's *1984*'.

follow a paradigm with no rationale for choosing that paradigm over any other.

However, an unprejudiced and reasonably close reading of his work shows why this is not the case, and why his work has been so influential. Before I address the question of 'irrationality' in Kuhn's philosophy, I want to show why, *a priori*, it cannot have any force.

Science works, in a technological, functional and predictive way. Whether we use it to land on Mars, to wipe out smallpox, to communicate with people on the other side of the world, or to drop nuclear bombs on Hiroshima and Nagasaki, it works. If it does not work, we have two options. Either revise the theory, try again and make it work in the future, or find out why it does not work. Both of these outcomes fall under any standard description of science. If it does not work, and we decline to make it work or explain why it does not, then it is no longer science. This idea of a technological pay-off, what we might call the 'proof of the pudding is in the eating' criterion, is strangely underrepresented in philosophy of science, although it makes at least a cursory appearance in most wider discussions on the subject (e.g. Harré (1986:37), Popper (1963:111-4), Feyerabend (1975:295-309)). This may be because it is so obvious. Science is demonstrably a powerful way of understanding the world, and the proof of that is our ability to use scientific knowledge to manipulate the world.

Of course some scientific theories are more technologically fruitful than others. But even those which on the face of it have little or no practical application have tangential links with technology. Darwin's theory of natural selection is largely unobservable in practice, in that we can see viruses mutate into new strains, but we cannot watch dinosaurs turn into birds. Nevertheless it is indispensable for modern genetics, and led indirectly to the discovery of DNA. Similarly, if the existence of the Higgs Boson is confirmed by the Large Hadron Collider at CERN, it may not immediately yield new technology, but we can be hopeful that it will in the future. Kuhn never disparages science or scientific methods, and focuses on uncontroversially successful episodes from the history of science, such as Lavoisier's discovery of oxygen and the emergence of the Copernican model of the solar system.

Our scientific mastery of our world is evocatively described by Derek Bickerton:

> If at this moment you look around you, wherever you may happen to be as you read these words, the odds are that most if not all of what you can see has been built, made, or grown by members of our own species. Even if you look out on wilderness, that wilderness survives only because it serves our pleasures, or because the task of subduing it outweighs the profit to be reaped from it – we could subdue it if we chose to. (1990:1)

Bickerton's point here is not triumphalist, or in some way anti-nature. It is merely a fact[40] about humans, what we can do, and what we, as a species, have actually achieved; and one of our major (and species-defining) achievements is science.

I mention this here because Kuhn's approach to science is matter-of-fact. He analyses the history of science from a social and human point of view; that is, he takes into account the foibles, weaknesses and desires of scientists, the constraints placed on them by their historical and social setting, and the consequent actions engendered by these factors which may or may not have been performed consciously. None of this denotes irrationality on the part of the scientists concerned, and the proof is in the pudding. It is well established that the history and philosophy of science must proceed on a 'no miracles' basis (Lipton (2000:191-2), Putnam (1978:18-22)). That is to say that the progress of scientific discovery is a natural facet of hu-

[40] To be precise, it is only a fact (in a non-Kuhnian sense) providing we ignore large stretches of the Antarctic and most of the ocean floor, which are inaccessible to us.

man existence, there is no divinely-given impetus to that progress, and it must therefore be explainable within the normal parameters of human experience and the laws of nature. This is a self-reflexive tenet of the history and philosophy of science. Science does not admit miracles, neither does its philosophy. There is, of course, a difference between a lucky accident and a miracle, and fortune does play a part in the history of science, but that fortune must be explained within the context of a scientist at least being able to recognise that good fortune and to know what to do with it.

From this perspective, then, we cannot accuse Kuhn of attributing irrationality to scientists, or of being somehow dismissive of science, or worse, anti-science. It might have served his cause better if he had made this proviso more forcefully. However, this should not make us leap to the erroneous conclusion that he found systemic irrationality in the history of science, or that he entertained the paradoxical notion that all these centuries of progress and deepening of knowledge were somehow a happy accident.

Kuhn's gives an analogy with Darwinism (ibid:171-2), suggesting that the 'fittest' theory, i.e. that which can solve most puzzles most elegantly or fruitfully, will survive, and the less fit will not. He is specific about what constitutes a stronger or fitter theory: 'Successive stages in that developmental process [the resolution of scientific revolutions] are marked by an increase in articulation and specialization' (ibid:172). Specialisation, the process whereby a paradigm becomes increasingly inaccessible to outsiders, is likely to be more fruitful than non-specialisation. An Aristotelian natural philosopher is less likely to map the human genome than a geneticist. Articulation is an accompaniment to specialisation, in that the more the object of study is narrowed and defined, the more precisely it can be studied.

So there is nothing arbitrary, from Kuhn's point of view, about the progression from one paradigm to the next. He defended his position in a full-length paper, 'Objectivity, Value Judgment, and Theory Choice' (1973), in which he argued that a scientific theory should be accurate, consistent, broad in scope, simple, and fruitful. Tellingly, he says that

> [T]hese five characteristics [...] are all standard criteria for evaluating the adequacy of a theory. If they had not been, I would have devoted far more space to them in my book, for I agree entirely with the traditional view that they place a vital role when scientists must choose between an established view and an upstart competitor. (1973:322)

This is unequivocal – scientists use the standard criteria of rationality in deciding between rival theories, and Kuhn thought this so obvious he barely addressed it in the first edition of *SSR*. However:

> When scientists must choose between competing theories, two men fully committed to the same list of criteria for choice may nevertheless reach different conclusions. Perhaps they interpret simplicity differently or have different convictions about range of fields within which the consistency criterion must be met [...] One can explain [...] why particular men made particular choices at particular times. But for that purpose one must go beyond the list of shared criteria to characteristics of the individuals who make the choice. One must, that is, deal with characteristics which vary from one scientist to another without thereby in the least jeopardizing their adherence to the canons that make science scientific. (ibid:324)

As ever, with Kuhn, to ignore the human is to misunderstand history. There need be no deviation from rationality, and yet the transition from one paradigm to another can be ultimately decided by non-rational criteria.

In the postscript he gives a similar list of what ought to

> enable an uncommitted observer to distinguish the earlier from the more recent theory time after time. Among the most useful would be: accuracy of prediction, particularly use of quantitative prediction; the balance between esoteric and everyday subject matter; and the number of different problems solved. Less useful for this purpose, though also important determinants of scientific life, would be such values as simplicity, scope, and compatibility with other specialities. (1962 (1969 postscript):205-6)

He goes on to say 'That is not a relativist's position, and it displays the sense in which I am a convinced believer in scientific progress' (1962 (1969 postscript):205-6). Whether or not Kuhn ought to be called a relativist or not is in some ways mere nomenclature. He disavows the label in the above passage, but goes on to say 'if the position be relativism, I cannot see that the relativist loses anything needed to account for the nature and development of the sciences' (ibid:207). In other words, employing the label 'relativism' to describe his account of the nature and development of science does not change that account in any way, it just changes the label.

However, although Kuhn rejects the relativist label, there is a clear sense in which it is apt:

> *Relativism: a name given to theories or doctrines that truth, morality, etc., are relative to situations and are not absolute.* (OED)

Kuhn's philosophy of science is relativistic in that it does not ascribe truth to scientific theories:

> It is now time to notice that until the last very few pages the term 'truth' had entered this essay only in a quotation from Francis Bacon [...]. The developmental process described in this essay has been a process of evolution *from* primitive beginnings – a process whose successive stages are characterized by an increasingly detailed understanding of nature. But nothing that has been or will be said makes it a process of evolution *toward* anything. Inevitably that lacuna will have disturbed many readers. We are all deeply accustomed

to seeing science as the one enterprise that draws constantly nearer to some goal set by nature in advance. (1962:170-1)

He goes on to draw a close parallel between his view of the evolution of science and Darwin's view of the evolution of species:

> All of the well-known pre-Darwinian evolutionary theories – those of Lamarck, Chambers, Spencer, and the German *Naturphilosophen* – had taken evolution to be a goal-directed process. The "idea" of man and of the contemporary flora and fauna was thought to have been present from the first creation of life, perhaps in the mind of God. That idea or plan had provided the direction and the guiding force to the entire evolutionary process. Each new stage of evolutionary development was a more perfect realization of a plan that had been present from the start. (ibid:171-2)

Just as scientists had to overcome the shock of not seeing evolution as goal-directed (specifically as seeing humanity as the end-result of evolution), Kuhn suggests that we ought not see science as directed *towards* truth, so much as see it as a passage *away* from ignorance.

> The net result of a sequence of such revolutionary selections, separated by periods of normal research, is the wonderfully adapted set of instruments we call modern scientific knowledge. Successive stages in that developmental process are marked by an increase in articulation and specialization. And the entire process may occur, as we now suppose biological evolution did, without benefit of a set goal, a permanent fixed scientific truth, of which each stage in the development of scientific knowledge is a better exemplar. (ibid:172-3)

Kuhn here actively rejects the notion of 'a permanent fixed scientific truth', in favour of a quasi-Darwinian account of the progression of ideas.[41] Just as evolution is not teleological, neither is science. Scientific theories are as good as they have adapted to be, just like organisms. This goes against what might be termed the common-sense view of science, that if something

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[41] Although in (2000:104) he endorses the existence of '*something* permanent, fixed and stable' (my italics), this does not change the relativist nature of his theory.

is a scientific fact then it is true, and that the job of a scientist is to discover the truth. This bears comparison with the common misconception that evolution somehow 'aimed' at the present, with humans providing some kind of pinnacle, and also the creationist view that God's creation is indeed perfect.

However, Kuhn's relativism is of a specific type. A popular misconception about relativism can be seen in the following passage by Richard Dawkins, who once wrote 'Show me a relativist at thirty thousand feet and I'll show you a hypocrite' (1995:36). However, this was with reference to a different type of relativism, what Dawkins calls 'cultural relativism':

> Airplanes are built according to scientific principles and they work. They stay aloft and they get you to a chosen destination. Airplanes built to tribal or mythological specifications such as the dummy planes of the Cargo cults in jungle clearings or the bees-waxed wings of Icarus don't. (ibid)

Dawkins concedes that his caricature of relativism is a bit of a straw man, and that 'sensible' relativism 'just means that you cannot understand a culture if you try and interpret its beliefs in terms of your own culture' (1995:36n). The strong version of relativism is rarely used with respect to western science (see Williams 2000:84), and of course the example given by Dawkins concerning cargo cults could not be seriously maintained.

Kuhn's relativism towards 'a permanent fixed scientific truth' was one of the most controversial parts of his book. However, his relativism does not hold that other, non-scientific, explanations of the world, whether from other cultures or as alternatives from within western culture (such as creationism or astrology) might be in some way equal to scientific explanation, just because there is no such thing as unchanging truth. Kuhn's philosophy no more admits of astrology than Popper's does. However, it does more to explain why astrology was seen as a successful science in the middle ages than Popper's does, because it takes in the social as well as the theoretical side of the practice of science, and examines the mechanisms which govern communal acceptance of a theory, and communal acceptance of what constitutes progress within a theory.

Again an analogy with Darwinian evolution is informative. The teleological argument sometimes levelled against Darwinism invokes the remarkable good fortune which we experience at being born into the extremely short 'perfect' phase of evolution, instead of one of the more experimental phases along the way. For the Darwinian this is not a problem, as all steps along the way were equally adapted to their environment and able to reproduce. Similarly, given the (possibly infinite) number of world views which we might have been born into, it might seem remarkably lucky that we have been born into the hundred or fewer years when the human race has actually hit upon 'the truth'. From the Kuhnian perspective, however, we have simply been born into one phase of scientific explanation, and, although by definition we are born into the most advanced era of science knowledge, this does not mean that we have been born into the one and only era in which the 'truth' has been discovered. The revolutionary nature of science means that there is a good chance that what we now consider the 'truth' will be reformed or replaced in the future.

The analogy with Darwinian evolution breaks down with regard to the future. A species which is surviving does not actively try to become 'better'. Science, on the other hand, does look for flaws, and tries to improve; a thriving science is one which demonstrates progress. Astrology had to be thrown out of 'normal science' with the advance of astronomy proper. However, this is not to say that astrology was not in some way scientific beforehand. Kuhn's relativism rests on this point. As long as astrology was the best source of knowledge available, it was scientific. It is not scientific today because our current cosmological paradigm is more parsimonious, more fruitful, more predictive, etc. However, we are not entitled to take the leap from that last statement to the statement that the Einsteinian paradigm is 'true', even if we have shown fairly convincingly that astrology was (and still is) 'untrue'.

It is worth elucidating what Kuhn's 'relativism' is not. It does not in any way compare science with non-scientific alternatives. With regard to the question at hand, that of competing paradigms, Kuhn is reasonably straightforward: they ought not to exist in a mature, normal science. Their existence is a sign of crisis science, or, more likely, an immature science. Relativism does not in any way allow a relaxation of scientific standards.[42]

2.4 Criticisms of the concept of incommensurability

There are plenty of criticisms of the concept of incommensurability, on various grounds. Perhaps the most trenchant comes from Donald Davidson, who argues that translation between human languages, and therefore between conceptual schemes, is never impossible in the way that Kuhn describes. This is partly because linguistic 'incommensurability' may be indeterminate from terminological ambiguity:

> So what sounded at first like a thrilling discovery – that truth is relative to a conceptual scheme – has not so far been shown to be anything more than the pedestrian and familiar fact that the truth of a sentence is relative to (among other things) the language to which it belongs. Instead of living in different worlds, Kuhn's scientists may, like those who need Webster's dictionary, be only words apart. ([1974]1984:189)

More importantly to Davidson's argument, anything which can be recognised as a language should be translatable, given enough patience, or 'charity'. Davidson's Principle of Charity

[42] See also Kuhn (2000:99-100) for further development of his stance on 'truth'.

> counsels us quite generally to prefer theories of interpretation that minimize disagreement [...] But minimizing disagreement, or maximizing agreement, is a confused ideal. The aim of interpretation is not agreement but understanding [...] Understanding can be secured only by interpreting in a way that makes for the right sort of agreement. (1984:xvii)

The principle is not given a single definition in Davidson's work, but works on the assumption that agreement can be maximised through interpretation and the assumption that the other person is rational and holds some true belief. Later in this section I will analyse the relationship between what Davidson calls 'charity' and what Kuhn calls 'interpretation'.

Davidson holds that if we can tell that something is language, then it must share some cognitive content with our own:

> We make maximum sense of the words and thoughts of others when we interpret in a way that optimizes agreement (this includes room, as we said, for explicable error, i.e. differences of opinion). Where does this leave the case for conceptual relativism? The answer is, I think, that we must say much the same thing about differences in conceptual scheme as we say about differences in belief: we improve the clarity and bite of declarations of difference, whether of scheme or opinion, by enlarging the basis of shared (translatable) language or of shared opinion. Indeed, no clear line between the cases can be made out. If we choose to translate some alien sentence rejected by its speakers by a sentence to which we are strongly attached on a community basis, we may be tempted to call this a difference in schemes; if we decide to accommodate the evidence in other ways, it may be more natural to speak of a difference of opinion. ([1974]1984:197)

This emphasis on the beliefs of the person whose language we are trying to interpret, rather than their language itself, is for Davidson a way of avoiding unnecessary appeals to such things as 'conceptual schemes' and 'incommensurability'. He says that interpretation can be simplified by assuming rationality, and points out that any apparent failure of translation could be attributed to a difference of belief.

> We must conclude, I think, that the attempt to give a solid meaning to the idea of conceptual relativism, and hence to the idea of a conceptual scheme, fares no better when based on partial failure of translation than when based on total failure. Given the underlying methodology of interpretation, we could not be in a position to judge that others had concepts or beliefs radically different from our own. (ibid)

I think that Davidson's argument misses its mark because for the most part it attacks too vehemently the idea of 'total' incommensurability, which does not allow for any translation between two languages, while not paying enough attention to the idea of 'partial' incommensurability. To emphasise this, earlier in the same paper ([1974]1984:186) he addresses the question of translation between 'Plutonian', 'Saturnian' and earth languages. However, the second type of incommensurability – the partial one – is more what both Kuhn and Feyerabend had in mind, the idea that certain well-defined scientific theories do not share common technical language with their competitors or forebears, thereby making comparison impossible. What Davidson says about charity of interpretation is obviously important (and perhaps the primary job of the historian of science), but Kuhn (2000 (1982):33-58) convincingly addresses this point in some detail.

It could be argued that what is classed as incommensurability is just two sets of very, very different concepts, and that there may be a way of translating *salva veritate* between the two, but this might be very hard to do. In order to address this objection, we must refer back to Kuhn's explanation given in chapter one. The distinction between translation and language-learning is vital. It is possible to learn a new language, of course, but this does not guarantee that translation will be possible. In order to translate perfectly, some language-learning must take place; the vocabulary and concepts of language A must somehow be communicated to a speaker of language B. This will either take the form of introducing new words from language A into language B, or language B inventing new words to represent the concepts which are being introduced from language A. Once these concepts and words have been incorporated into language B, its speakers can now talk to speakers of language A about those concepts. Crucially, however, language B is now different from what it was before this incorporation.

There are parallels to be seen between what Davidson calls 'charity' and what Kuhn calls 'language-learning'. If this is plausible, then there is much less conflict between Davidson and Kuhn. Languages or sets of concepts will remain incommensurable only for as long as their respective speakers or thinkers wilfully keep them that way, with a bloody-minded determination not to understand each other in the slightest.

To illustrate this we might return, once more, to Darwinian evolution and young-earth creationism. To my mind, these are fairly good examples of incommensurable systems, and I give some details for this claim below. However, those of us on either side of the debate fully understand the other without believing them for a second. In this sense, anyone who is aware of the debate holds two incommensurable sets of concepts in their heads; they have learnt both 'languages'. They can think about one and then the other, but what they cannot do is to explain one in the vocabulary of the other.

Many writers have noted the similarity between Kuhn's ideas on incommensurability and the Sapir-Whorf hypothesis, and this is closely related to Davidson's arguments. Davidson's target in the paper mentioned above was 'the very idea of a conceptual scheme', and the idea that people with differing 'conceptual schemes' (and therefore different languages) might have thoughts which are mutually untranslatable. For Davidson the very idea of a conceptual scheme is untenable, and we ought to 'abandon the attempt to make sense of the metaphor of a single space within which each scheme has a position and provides a point of view' ([1974]1984:195). In other words, we cannot make sense of autonomous, and therefore incommensurable, conceptual schemes.

This partially relates to criticisms of the Sapir-Whorf hypothesis, which we saw earlier was heartily endorsed by Feyerabend (see above (1975:223-4)). Here, Feyerabend endorses, but does not give evidence for, the claim that language, at least in part, shapes thought – a claim which, on the surface, looks like a fundamental prerequisite of any incommensurability thesis. Kuhn also cites 'B.L. Whorf's speculations about the effect of language on world view' (1962:vi) as an influence in the preface to *SSR*, although Whorf does not figure prominently in Kuhn's work. Irzik and Grunberg (1998: 213n) say 'Curiously, however, to the best of our knowledge, Whorf's name makes only three brief appearances in the entire corpus of Kuhn's writings: twice in his published works (Kuhn, 1970a, p. vi; 1977, p.258) and once in his unpublished manuscript 'Remarks on Incommensurability and Translation' where he says he is a devoted Whorfian.'

The 'Whorfian' claim began to be called into question following Chomsky's 'Rationalist' turn in the 1950s. Various experiments have been done to determine what, if any, substance there is to the claim, and many, such as Kay and Kempton conclude that the postulation that 'the structure of anyone's native language strongly influences or fully determines the world-view he will acquire as he learns the language' is 'reduced in its consequences' (1984:77).

Interestingly, neither Kuhn's nor Feyerabend's association of scientific theories with natural languages depends on the Sapir-Whorf hypothesis being correct. For Feyerabend, the comparison was an intuitive analogy, one that could be corrected or simply deleted. In Kuhn's case, even if the strong Sapir-Whorf hypothesis, that our particular language shapes the way we see the world, were disconfirmed, his theory that the conceptual contents of different paradigms are incommensurable remains. Even if language has no causal role in concept formation, there is nevertheless an important match between the two. We cannot give a name to something for which we have no concept – although we arguably have concepts without names (Pinker 1994:67-8). Our concepts are interlinked, uncontroversially, and Kuhn makes an intuitively plausible case for the idea that we learn concepts in interrelated groups: 'the child learning "dog" must be shown many different dogs and probably some cats as well' (2000:49). This being the case, there is a certain amount of interdependence between our language and our concepts and this interdependence guarantees the possibility of incommensurability.[43]

So Kuhn's explanation of incommensurability does rest on a weak form of the Sapir-Whorf hypothesis, but this weak form is intuitive and commonsensical. Words and concepts both form webs of meaning, and the two have a strongly correlated match, even if this match is not perfect. On this reading of the Sapir-Whorf hypothesis, the arguments against it which cite language universals as evidence (e.g. Pinker 1994:55-82) are inapplicable. On a deeper level than that which Kuhn is addressing, we may or may not be able to find fundamental patterns of human thought which are not influenced by the vagaries of the variety of human language used by the individual in question. However, we only have words for concepts which we have actually learned: to learn the word 'duck', you need to be able to categorise a duck as a bird (or at least some kind of living thing), and if you live in a land of no ducks, you will have neither the concept nor the word.

Pinker (1994:55-82) quotes several more experiments to this end, and is characteristically less cagey in his conclusions. For Pinker, we all speak

[43] Barker (2001:434) points out that Kuhn's 'non-standard' account of concepts has not been popular in the philosophical world, yet 'at the same time that the philosophical world was first rejecting Kuhn's original work and then ignoring his revisions of it, an enormously influential movement in cognitive psychology and cognitive science was establishing a new consensus on the nature of human conceptual systems that directly supported Wittgenstein's and Kuhn's theories.'

'mentalese',[1] which is to say that, whatever our native language, we all share cognitive and logical/processing functions which are not shaped or influenced by language-specific categorisations. If Pinker is right, we may be forced to the conclusion that incommensurability is not, after all, a valid posit.

However, this negative conclusion rests on the idea that there is a necessary link between incommensurability and the Sapir-Whorf hypothesis. This link can be broken in one of two ways. First, Pinker's position would only put paid to strong incommensurability, the idea that two languages might share no common ground, and therefore not even admit of partial translation. As we saw with Davidson, this is a bit of a straw man – strong incommensurability is irrelevant to Kuhn's and Feyerabend's positions. Second, and more pertinently, even if we do all speak 'mentalese', this does not affect the evolution and history of the particular concepts which individuals happen to have. Put bluntly, if you do not have a concept of something, you do not have a word for it. So even if the way we individuate and relate concepts is the same, this should be no bar to saying that individual sets of concepts, as embodied in individual humans, might be incommensurable with each other. Anyone *might* have happened to have been born in the pre-Copernican world, but I was not, so my belief system might still be incompatible with the belief system of those times.

Local incommensurability is not just a cliché of the history of science, and the above example should go some way to showing why. Incommensurable sets of concepts can exist within the same head, and they do, so it is not all theoretical. Whether or not it has historical or current exemplifications with groups of people who genuinely cannot talk to each other on a given subject, is to an extent irrelevant. For Kuhn's theory to work, incommensurability has to be possible – we need to be able to show that two given sets of concepts cannot be described in each other's terms. This is unconnected to 'charity of interpretation', which asserts, presumably correctly, that humans tend to alter and expand their own vocabularies in order to avoid miscommunication. I have a concept of 'god', but that does not mean that I believe he created the world in six days approximately seven thousand years ago. More than that, I cannot give a coherent account of my (standard scientific) beliefs about the evolution of life on earth which includes the terms 'Noah' and 'Bible' and uses a time-span of five or six thousand years. However, I can read the Bible and 'understand' the alternative explanation given within it.

The parallels between Kuhn and Foucault were addressed above with regard to the difference between social and natural sciences. Another area where they seem to intersect is in regard to Foucault's notion of an 'episteme', which has drawn several comparisons with Kuhn's paradigms (e.g. Dreyfus and Rabinow 1982:160-4). There are clear parallels between the two, but there are also overriding differences. A paradigm can be and is clearly articulated by its members (Kuhn 1962:23-34), whereas an episteme is largely unconscious, and can only really be uncovered by the archaeological historian (1970:xxii and 1972:*passim*). An episteme is much larger-scale, and cannot really be avoided by those who live under it. A paradigm, on the other hand, is specific to the scientific community, and is fuzzy at the edges. The way that one episteme moves onto another and looks incomprehensible compared to what came before is similar to Kuhn's ideas on incommensurability. Similarly, Foucault's analysis of the impossibility of studying human sciences recalls Kuhn's ideas on immature sciences. However, incommensurability is based on specific vocabulary items. Epistemes are not systematically different from each other. Kuhn's notion of paradigms allows us a systematic translation between two incommensurable paradigms, and therefore allows us to analyse different aspects of the process by which avenues of enquiry become sciences. An episteme subsumes all of these things. So while on the surface Foucault might seem to be offering a solution to the problem of incommensurability, a deeper analysis suggests that this is not the case. Historians of linguistics have not used Foucault's notion of episteme nearly so much as they have used Kuhn's notion of paradigms. Presumably this is because the over-arching nature of an episteme includes all the types of linguistics being studied at the same time, rather than being relevant to just one of them, and so the question of whether or not one type of linguistics has a better claim to scientificity than another cannot really be addressed in this framework.

The main point to emerge from this discussion is that Kuhn's concept of 'incommensurability' refers to a very specific set of circumstances, and it is easily misunderstood. If it is misunderstood, it is easy to criticise incommensurability as a grand name for a commonplace phenomenon, as an unnecessarily complicated explanation of uncomplicated situations, or as metatheoretical, relativist, metaphysical nonsense which is antipathetic to the hard-headed scientific endeavours it describes. This last criticism is perhaps the most common. Kuhn and Feyerabend have complicated reputations, in that they are often seen as responsible for the growth of 'anti-science' in the second half of the 20th century, even if that was accidental (Williams 2000:70-85). Kuhn and Feyerabend themselves held science in exceptionally high regard, as I have indicated, but the 'relativism' which they espoused is easily misinterpreted as value-free (in fact it is only value-free in Kuhn, not Feyerabend); if truth is relative and it is in the nature of scientists to be wrong about most things most of the time, then science should not be held as a more worthwhile source of knowledge than anything else. Under this way of thinking, science becomes 'just a theory'. This type of relativism, however, seriously mistakes the nature of Kuhn and Feyerabend's relativism. Neither of them allow for the equation of science with, say, astrology, but they do urge realistic historical understanding of the parallels.

Incommensurability comes into this debate, both on the side of those relativist anti-scientists, and pro-science anti-relativists. For the former group, the 'fact' of incommensurability shows that scientific 'truth' is no more than a top-down imposition of chauvinism on the part of establishment thinkers (again, see Williams (2000:70-85) for a catalogue of such objections – especially of the relativist appropriation of Kuhn). For the latter group, incommensurability is unnecessary, and incoherent. Davidson (1984:184), for example, asked how Kuhn can claim that our paradigm is incommensurable with a former paradigm, and then use our language to describe that paradigm. Instead of the unnecessary layer of conceptual incommensurability, which seems too broad to have any substantive meaning, why not just acknowledge that the range of things which humans have believed and are capable of believing is very, very wide? Exploring this width may be interesting, but should not frighten us into positing metaphysical categories such as incommensurability which only serve to confuse.

I believe that the description given by Kuhn (and also Feyerabend) successfully transcends these objections, as I have outlined above. If incommensurability is constrained and only invoked in carefully specified situations, then it becomes both valid and useful. The criteria for a coherent and justified description of incommensurable concepts are as follows. First, incommensurability is local; it does not usefully apply to entire minds or communities. This is particularly true when describing scientific theories because as often as not they are articulated in the same language, which automatically renders large parts of the theories commensurable. Incommensurability applies to webs of key content words in a theory, and not to irrelevant parts of the language. Tables remain tables after a scientific revolution, walls are still walls, and grammatical features such as articles, auxiliary verbs, prepositions and conjunctions retain their grammatical functions. A Ptolemaic 'the' is the same as a Copernican 'the'. The idea of 'working in a new world' is metaphorical in this sense, as scientists holding incommensurable theories are not necessarily separated by much in either time or space; revolutions can happen fairly quickly, and scientific communities are small. Incommensurability across scientific paradigms is not like the experience of explorers discovering previously uncontacted tribes on remote islands, or Martians, and trying to understand an entirely new natural (or alien) language, mindset and set of customs.

Uncontacted tribes and Martians bring us to the second key criterion for incommensurability. It should be allied to translatability, not to interpretation. When the uncontacted tribe is first contacted, what follows is interpretation rather than translation. Interpretation is allied to language learning, and to incorporation of foreign words or meanings into a language. It is perfectly possible for a modern scientist (or anyone else) to learn the meaning of 'phlogiston', and incorporate it into their vocabulary. We are able to do this because of the interpretive endeavours of historians of science. What we cannot do, however, is translate. 'Phlogiston' cannot be used within a description of a modern theory of chemistry. Learning the meaning of 'phlogiston' means enriching our vocabulary with a foreign term, not translating from our current vocabulary. This is the difference between translation and language learning, or interpretation.

Part 3: definitions of TGG, sociolinguistics and schools

The purpose of this section is to define better the terms 'TGG' and 'sociolinguistics' which, up to now, have been used in broad senses. In chapter one I very briefly introduced them, noting that TGG is centred around the work of Noam Chomsky, and is that it is therefore uncontroversial to refer to it as 'Chomskyan linguistics'[44]. I also noted in that chapter that sociolinguistics is less homogenous than TGG. I will discuss the identity or identities of these different types of linguistics in this section.

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[44] For example, Harris (1993:28-34) uses the term as a heading for a sub-section of a chapter.

In both cases I will examine papers from relevant journals as examples. For TGG I will use a recent issue of *Syntax*, while for sociolinguistics I will use *The Journal of Sociolinguistics*. Both are leading journals in their fields, although neither have monopolies. The issues and papers I have selected are intended to be representative, although of course it would be impossible to find a truly stereotypical issue of a journal in any field.

I will also address the question of nomenclature in this section. Until now I have loosely referred to TGG and sociolinguistics as 'types' or 'forms' of linguistics. However, other writers refer to them as 'schools', 'disciplines', 'theories' and others terms. At least in some cases the particular use of one or other of these terms has is significant in terms of (self-) definition for linguists, and this forms the subject of the final part of this section.

3.1 TGG

Transformational Generative Grammar is, on the surface, easily defined. It is 'transformational' because it explains our linguistic competence in terms of transformations from one level to another level[45]. It is 'generative' because it is a 'system of rules that [...] assigns structural descriptions to sentences' (Chomsky 1965:8). In other words, the rules 'generate' the structural descriptions of the sentences of the given language. This 'system of rules' refers to the knowledge a speaker has of his or her language; it does not refer to the actual physical or neural generation of sentences. Chomsky has noted that 'confusion over this matter has been sufficiently persistent to suggest that a terminological change might be in order. Nevertheless, I think that the term "Generative Grammar" is completely ap-

[45] As ever with Chomsky, the question of exactly what gets transformed has changed over the years. For at least the first half of his career, the key locus of transformations involved Deep and Surface Structure; nowadays, the terms Logical Form (LF) and Phonetic Form (PF) are more likely to be used, although not as replacements or synonyms.. None of this affects the 'transformational' nature of the theory at hand.

propriate' (1965:9). Finally, it is worth noting that the final 'G' in TGG places grammar firmly at the centre of the generative enterprise. In many ways, for Chomsky, knowledge of syntax *is* knowledge of language – phonology, pragmatics and semantics are either subordinate to or dependent on knowledge of syntax (see chapter five section 2.2.2 for a discussion of differing attitudes towards the relationships between these areas of language).

There exist plenty of theories of grammar which stand in opposition to TGG (such as Word Grammar[46] and Systemic Functional Grammar[47] to give just two examples); and plenty of theories of grammar which do not make use of transformations (see Harris (1993:248-252); Postal (2004:4), quoted in chapter three section 1.2; and also Koerner 1983:152). Nor is it necessary for any practising linguist to swear an oath of allegiance to Chomsky or his theories in their entirety – it is perfectly acceptable to criticise some parts and to accept other parts as correct. Nevertheless, it is beyond doubt that Chomsky is the dominant figure in this field, that many linguists are happy to call themselves Chomskyans (Smith 1999:5), and that in the field of syntax (and perhaps in theoretical linguistics) TGG linguists fill a significant number of academic posts and receives a significant amount of funding (although Newmeyer (1996:34-8) thoroughly rejects this idea).

In chapter three section 2.1, I touch on the self-image of Chomskyan (and non- or anti-Chomskyan) linguists: are they a numerical majority? Do they hold positions of power in the institutions where they are employed? Do they face institutional advantage or prejudice from other linguists? In that chapter I also address the question of whether or not TGG can be said to be a Kuhnian-style paradigm. I will leave those questions for now.

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[46] http://www.phon.ucl.ac.uk/home/dick/enc2010/frames/frameset.htm.
[47] http://www.isfla.org/Systemics/.

Instead, in this section I give a more straightforward analysis of what TGG is, and what its practice involves. Essentially, TGG 'attempts to characterize in the most neutral possible terms the knowledge of the language that provides the basis for actual use of language by a speaker-hearer' (Chomsky 1965:9). This has not changed in the near-half century since it was written, and neither have the methods. Linguists have never needed much more than a pen and paper (although the invention of sound recording hugely expanded the possibilities of what data was available for analysis, and computers have radically altered the quantitative analysis of that data). To reiterate, TGG aims to 'characterize knowledge', to discover what people know when they know a language. Where other people hear an instruction, a line of a song or a weather forecast, generative grammarians hear a derivation: some sort of movement from the knowledge of language to its production; a type of grammatical structure, not a token of its production; and a set of rules which govern the production of such structures.

TGG works abstractly; from a completely neutral point of view, the tools of TGG are mental objects, rules and knowledge. What TGG linguists do with these is typically to draw trees showing how sentences are derived from knowledge, and construct rules for these derivations.

In order to get a clearer picture of what these methods involve I will look at *Syntax* (13:3) from September 2010. *Syntax* describes itself on its website as publishing 'a wide range of articles on the syntax of natural languages and closely related fields. The journal promotes work on formal syntactic theory and theoretically-oriented descriptive work on particular languages and comparative grammar'. As if to make its orientation crystal-clear, it advertises itself with praise from Noam Chomsky.

This issue contains three papers. The first, by Evelina Fedorenko and Edward Gibson (183-195), is a study to show that the addition of a third wh-phrase to object-initial multiple wh-questions does not increase acceptability. This paper does three things. It corroborates recent research to this effect; it supports Chomsky's position that there is a subject/object asymmetry in multiple-wh-questions; and it argues that the use of quantitative data in TGG produces more reliable findings, especially regarding complex intuitions.

This paper uses methodology that is standard in TGG. Its central artefact is a questionnaire with 28 sets of sentences; the subjects, who were 'paid for their participation and were naive as to the purposes of the study' were asked to select the more acceptable alternatives in each case. The subjects looked at the sample scenarios and were asked to choose a suitable sentence from a pair to describe that scenario. They were also asked to grade a set of sentences 'on a scale from 1 (not at all natural) to 7 (very natural)'. From the results of this survey, principles were formed about the relationship between sentence structure, wh- embedding, and acceptability. This study is based on the assumptions that native speaker subjects will have graded intuitions about the acceptability of sentences, and that they can intuitively decide which of a pair of sentences is more 'correct'. Notice that the scenarios were constructed by the linguists, and there was no suggestion that real-time utterance data need be used.

The second text, 'The Amharic Definite Marker and the Syntax–Morphology Interface' by Ruth Kramer, is equally standard TGG practice. A longer piece, this analyses evidence from Amharic. It is not made clear where the Amharic came from (the author thanks her informants, but no further information is given). This is unremarkable for TGG (and, of course, it would be seen as ridiculous in sociolinguistics).

This paper has two stated aims. First, to describe the distribution of the definite article in Amharic, and second, to use this as evidence that 'at the first stage of PF (before Vocabulary Insertion/Linearization), the operations that occur (Lowering, Feature Copying, etc.) are not restricted by phase impenetrability.' In other words, the distribution of definite marking in Amharic requires that 'phase impenetrability', (which states that phases which have already been spelled out are not vulnerable to subsequent morphological and other processes), needs to be tweaked to allow for *some* operations to occur after spellout.

This paper uses standard TGG terminology (phase, spellout, PF, LF, linearization, etc.) and standard TGG assumptions. These include the idea that data from one language can be used as evidence for the structure of universal grammar; that it is possible to make theories about the behavior of mental posits; and that those theories will describe a system which is ordered and accessible.

The third paper, 'On Labeling: Principle C and Head Movement' by Carlo Cecchetto and Caterina Donati, is entirely theory-internal. Based on examples mostly from English, and occasionally from Italian and other European languages, it looks at the two algorithms which govern phrase structure building:

> In {H, α}, H a lexical item (LI), H is the label.
> and
> If α is internally merged to β forming {α,β}, then the label of β is the label of {α,β}.

Cecchetto and Donati argue that these two algorithms can be reduced to one:

> The label of a syntactic object {α,β} is the feature(s) that act(s) as a Probe of the merging operation creating {α,β}.

The rationale for this reanalysis of the axioms is that the second does not obey minimalist requirements 'because it is specifically restricted to movement configuration and, by doing so, it does not allow reduction of movement to (Internal) Merge'. The axiom which they propose is based on generalising the first to incorporate the second: 'in a nutshell, the Probe of a Merge operation always provides the label.'

The examples of language used to illustrate this argument are all constructed and analysed by the writers; no native or naïve subjects are used, no real-time data is used. All reasoning in this paper is based on streamlining the theory and making it internally consistent.

All three of these papers make multiple references to Chomsky. The first supports him against an alternative argument, the second uses his theory to illustrate a point about a natural language, while the third is primarily concerned with furthering his theory. All three use intuition as a methodology to some extent. The first might conceivably be read by a non-specialist, the other two certainly not. They therefore seem to exhibit many of the outward trappings of normal science produced within a paradigm. In their shared theoretical vocabulary, as much as in their exclusivity, they show a web of interrelated concepts which are dependent on the theory at hand for their postulation, and therefore for their confirmation. This issue of *Syntax*, then, gives a good a cross-section of TGG practice. As I mentioned earlier, this cannot be seen as a definition of TGG, nor as a comprehensive survey of its scope. However, it is accurate in that it shows the type of operations, posits, vocabulary and activities which TGG involves.

3.2 Sociolinguistics

Sociolinguistics is often presented as a reaction to TGG, to the inward-looking, atomising world of mentalist grammar[48]. Sociolinguists are held

[48] And also, it has been argued, the male world of TGG. Newmeyer (1996:17-23) has a fascinating discussion of how sociolinguistics is driven by 'feminine' concerns, as opposed to the 'masculine' approach of Chomskyan linguistics.

to be driven by the desire – and they would certainly say the need – to study language *in situ* (see, for example, Hymes 1977:206).

I noted in chapter one that sociolinguistics is not a homogenous field, reflecting several methodological and institutional facets. First, there is no Chomsky-type figure in sociolinguistics who dominates the field. Labov is hugely influential, and his brand of variation studies is perhaps the standard sociolinguistic approach. However, Dell Hymes and John Gumperz developed alternative approaches to language and society, which could perhaps be seen as more 'socio-' and less 'linguistic'. In particular, they advocated a bottom-up, anthropological study of language in context rather than solely analysing linguistic features quantitatively. Figueroa articulates the tension inherent in sociolinguistics:

> This study is focused on the sociolinguistics of language rather than the sociolinguistics of society: on what sort of linguistics is sociolinguistics – what does sociolinguistics say about theories of language. However, one could equally ask what sort of sociology or anthropology is sociolinguistics. (1994:11)

Figueroa goes on (1994:11-15) to give a comprehensive overview of different views regarding that tension, concentrating on the split between Labov on the one hand, and Gumperz and Hymes on the other. Other commentators, such as Duranti, analyse the split in terms of labels: Labov's varieties are more 'sociolinguistic', working on 'language choice and language change', whereas the work of Hymes and Gumperz is 'linguistic anthropology', whose theoretical concerns are performance, indexicality and participation (1997:13-21).

In the last ten years 'sociocultural linguistics' (see Bucholtz and Hall 2008) has emerged as a distinct branch of sociolinguistics, drawing on ethnographic approaches as much as on quantitative analysis, and defines itself (partly) through opposition to what it sees as the dominant or traditional

Labovian approach. SCL can be seen as a modern development of the work of Hymes and Gumperz. Labov, on the other hand, continues to be enormously influential for the rest of the field.

In order to more closely examine what sociolinguists typically do, we will look at the April 2010 issue of *Journal of Sociolinguistics*. This journal 'promotes sociolinguistics as a thoroughly linguistic and thoroughly social-scientific endeavour', and publishes 'articles that build or critique sociolinguistic theory, and the application of recent social theory to language data and issues' (according to its website).

The issue under discussion features four articles:
'Ethnolinguistic repertoire: Shifting the analytic focus in language and ethnicity' by Sarah Bunin Benor;
'A phonological study of the spatial diffusion of urban linguistic forms to the varieties of the Nile Delta' by Dario Ornaghi;
'Focusing, implicational scaling, and the dialect status of New York Latino English' by Michael Newman;
and 'Constructing identity with L2: Pronunciation and attitudes among Norwegian learners of English' by Ulrikke Rindal.

The first introduces a theoretical construct, 'ethnolinguistic repertoire', and as such is aimed mostly at furthering and deepening the theoretical vocabulary of sociolinguistics. Its main aim is not to introduce new quantitative data *per se*, but to use various data to propose a new construct. It takes its cue from Labov and his work on dialectal studies, but also from Hymes, Gumperz and other work in 'socio-cultural linguistics', including Bucholtz and Hall.

The second paper focuses on two variables in local Arabic dialects in the Nile Delta, and their changing distribution by age based on the influence of nearby prestige dialects. The data was gathered through interviews, and is quantitatively analysed according to fairly traditional Labovian standards.

The third paper is an evaluation of Benor's notion of 'ethnolinguistic repertoire', in quantitative analysis of Latino teenagers in New York. It uses implicational scaling to analyse the occurrence of four variables in the speech of twenty subjects, gathered through semi-structured interviews.

The fourth is a quantitative examination of attitudes in Norway towards British and American English accents. It studied 23 Norwegian teenagers, and looked at their production of English, both in pre-selected word lists and informal conversation. It analyses the relevant variables quantitatively, but also discusses the question of how those variables might act as identity markers for young Norwegians. In this sense it mixes ethnographic and quantificational approaches.

All four of these papers have some form of quantitative analysis, but its role and prominence varies. All four use real-time data, and spend some time explaining how their data was gathered. Labov is cited in all four texts, Milroy in three, Bucholtz-Hall in two, and Hymes and Gumperz, in one. In these texts, we see a common set of commitments to the study of language as it is used, but a wide variety of practices, and no common framework in which the work is done; instead, we see several established approaches.

This gives a fairly representative cross-section of the influences of sociolinguistic work, in its various forms and influences.

3.3 Schools, disciplines, topics; paradigms, theories and programmes

In this section I want to consider the institutional (and other) delineations and affiliations of groups of linguists. In chapter one I pointedly used the terms 'type' and 'form of linguistics' to refer to TGG and sociolinguistics. This was because there is a certain amount of significance in which terms these groupings use to identify themselves. I have already looked in some detail at the Kuhnian notion of a 'paradigm'; in this section I will look more closely at the difference between a paradigm, a 'theory' and a 'programme'. I will also look at three terms used to describe groups of researchers: 'school', 'discipline' and 'topic'. In varying ways, these terms relate to institutional and theoretical differences, all of which I will describe below. One area this section does not address is the question of which, if any, of these forms of linguistics should properly be called 'sciences'; this question is discussed in the next section, and in chapter three.

3.3.1 a paradigm, a 'theory' and a 'programme'

Theories are relatively unproblematic, in the sense that everyone knows what they are.[49] Scientific theories are proposals about the world which may or may not turn out to be (in some sense) 'true'. It is a truism that in practice scientists follow Popper's formula, whereby a theory accepted as true is one which has not yet been shown to be false (see the discussion of Popper at the beginning of this chapter). However, a theory is only the idea, the fact or the knowledge (however we wish to characterise it). I have already discussed in detail what 'paradigm' means in Kuhn's account of the history of science, and I will continue to use the word in the Kuhnian sense. What is relevant to repeat here is that Kuhn incorporated the so-

[49] Theories, like everything else in philosophy, are extremely problematic. See Giere in Newton-Smith (ed.) (2000:515-524).

ciological and psychological reality of scientists as humans into his account of scientific paradigms. However, as also discussed earlier in this chapter, Kuhn's account was often seen as too human – it presented scientists as irrational trend-followers, and failed to explain why science could be so successful when it was practised by unimaginative, irrational 'sheep' (see section 2.3 of this chapter).

Imre Lakatos coined the phrase 'research programme', in his explanation of how science proceeds. His thesis was a synthesis of Kuhn and Popper's ideas, one which defended the rationality of scientific enquiry, whilst explaining why scientists did not always proceed according to the idealised criteria which Popper had described. His thesis was that a research program has a 'hard-core' of ideas which are not to be challenged (the 'negative heuristic'). These ideas are surrounded by a protective belt of supplementary hypotheses, which develop the program and explain anomalies which might otherwise threaten the hard-core (the 'positive heuristic). As long as these supplementary ideas produce results (in terms of expanding the theory, predictive success, explaining *new* anomalies etc.) then the program can be said to be 'progressive'. If the protective belt is merely that, if the hard-core continues to require supplementary hypotheses, but these have no progressive role in the program, then the program can be said to be 'degenerate', and will eventually be abandoned (Lakatos 1970:132-138).

The chief attraction of Lakatos' account of research programs is that it retains, to an extent, Popper's falsificationism as the chief criterion of scientific epistemology, while at the same time allowing that scientists may have perfectly good reasons for not following Popper to the letter. If one anomaly can be explained by an auxiliary hypothesis, perhaps only on a temporary basis, then this might be preferable to abandoning the whole program and starting again. As well as acknowledging the fact that scientists are often unwilling to abandon an idea which they have invested considerable time and effort into, Lakatos also acknowledges that scientists have hunches about the future productivity of an idea which, for the moment, may not be entirely justifiable from a Popperian point of view.

Lakatos' account is of particular interest here because the current incarnation of TGG is named 'the Minimalist Program'. This contrasts with the previous *theories*, such as Government and Binding theory, the Extended Standard Theory etc. (see chapter four for a chronology of these). Minimalism is a programme, not a theory, because

> [...] it asks questions and follows guidelines that are broad enough to be pursued in a great many directions. This flexibility, this room for alternative instantiations of minimalism, is what the term 'program' emphasizes. (Boeckx 2006:5).

Chomsky appears to be doing little more than acknowledging the limitations of minimalism by calling it a program rather than a theory: 'This is, of course, a program, and it is far from a finished product [...however ...] It gives at least an outline of a genuine theory of language, really for the first time (Chomsky 2000:8).

This discussion of the terms 'paradigm', 'programme' and 'theory' is intended to show that there are at least some genuinely significant differences between the three, and that linguists and other researchers are often aware of the. People tend not to use 'paradigm' approvingly anymore, simply because of the controversy which Kuhn engendered. 'Program' is more loosely defined and more tentative, and this seems to be at least why Chomsky uses it. However, Boeckx warns us that

> A quick look at the literature on theory, theoretical models, programs etc. reveals that philosophers of science, historians of science, and scientists themselves have not been consistent in their uses of these terms. (Boeckx 2008:6)

3.3.2 'school', 'discipline' and 'topic'

'Topic' is not a particularly problematic term. Most people feel comfortable with the difference between a topic or subject (e.g. physics) and a theory (relativity, string theory). However, Chomsky has stated that generative grammar is a topic not a theory:

> Generative grammar is sometimes referred to as a theory, advocated by this or that person. In fact it is not a theory, any more than chemistry is a theory. Generative grammar is a topic, which one may or may not choose to study. Of course, one can adopt a point of view from which chemistry disappears as a discipline (perhaps it is all done by angels with mirrors). (1986:4-5)

Chomsky is saying nothing controversial here, if we read him as saying that we do all carry knowledge of language in our heads, and that each of us implicitly knows a set of rules which stipulates all and only the set of acceptable sentences in our language. However, Chomsky's formulation could be accused of involving a sleight of hand – it is certainly possible to infer from this that Chomsky's Transformational Generative Grammar is a topic, not a theory. TGG produces theories of course; in the past, these have included G&B, EST etc. If generative grammar really is a topic, not a theory, then Chomsky's theories have been some among many in this field; we have seen that there are other grammatical theories which do not use transformations (see above). This leads, however, to the sense that the 'generative' part is redundant, and that perhaps we could just call the whole thing 'linguistics'.

Moving on from topics, Dell Hymes has an interesting formulation of what the subject entails. For Hymes, 'Linguistics is a discipline and a science, and its history is part of the general history of disciplines and sciences' (1974:1). As I have already indicated, the 'science' part of this will come in the next chapter. Bucholtz and Hall use 'field' and 'perspective' when they want to be non-specific (2005:585-7; 2008:401-5). The Prague School were a school and a circle, and Matthews (1993:6) sees no need to discriminate between the two. He also refers to various 'Chomskyan schools' (ibid:233-4). Matthews' examples illustrate that what a 'circle' and a 'school' have in common is a closer social network, at a sub-disciplinary or sub-paradigmatic level, and imply a selective membership. Murray (1994:10-12) analyses, in similar terms, the various characterisations of the 'invisible college' – essentially the web of contacts unique to each researcher, which nevertheless interact to form communities. However, Murray is not much interested on which label (school/paradigm/invisible college) ought to be attached to such communities, and 'group' is normally sufficient for him.

As I noted above, in the section on criticisms of Kuhn, he himself had 'Second thoughts on paradigms' (Kuhn 1974). We saw that at the end of this paper he concedes that 'we shall be able to dispense with the term 'paradigm', though not with the concept that led to its introduction' (1974:319). We also saw that Masterman breaks the term 'paradigm' down into three main senses: metaphysical commitments, artefacts, and social groupings (1970:65). Kuhn himself says that 'a paradigm is what the members of a scientific community, and they alone, share. Conversely, it is their possession of a common paradigm that constitutes a scientific community of a group of otherwise disparate men' (1974:294). The phrase 'scientific community', then, is close to the social aspect of what a 'paradigm' is.

Kuhn goes on to say

> Let me now suppose that we have, by whatever techniques, identified one such community. What shared elements account for the relatively unproblematic character of professional communication and for the relative unanimity of professional judgement? (1974:297)

He then explains that one sense of the word paradigm, as used in *SSR*, refers to these 'shared elements'. However, at this point he would prefer to use the term 'disciplinary matrix':

> "disciplinary" because it is the common possession of the practitioners of a professional discipline and "matrix" because it is composed of ordered elements of various sorts, each requiring further specification. (ibid)

This dispenses with the ambiguity inherent in Kuhn's notion of a 'paradigm', and is an attractive option for describing in a non-question-begging way different forms, schools, sub-schools (etc.) of academic practices and communities.

However, there is still a problem with substituting 'paradigm' or 'school' with 'discipline and 'disciplinary matrix'. It is not immediately obvious how we would distinguish physics, sociology and art history on the one hand, from professional football, prostitution or baking on the other hand, as these appear to be 'professional disciplines' with 'ordered elements of various sorts, each requiring further specification'. Nevertheless, 'disciplinary matrix' in Kuhn's sense does provide a useful and relatively neutral way of describing communities and their professional habits.

With no set definitions, 'school', 'topic', 'program' and 'discipline' are all interchangeable in the sense that they refer to groups of people researching things, without getting involved in a philosophy of science dispute about paradigms (etc.). 'Science' is certainly more problematic, and this is addressed in the next chapter. So when Hymes call linguistics a discipline *and* a science (my emphasis), he is at once stating something obvious ('linguistics is a (professional) discipline) and something contentious (linguistics is a science).

Before moving on to the next section, it is worth looking at non-Kuhnian models of human scientific progress. Hymes and Becher approach this question in two very different ways. First, Hymes (1974:9-14) provides an alternative to Kuhn's 'paradigm shift' interpretation. By looking at linguistics, and only linguistics, he suggests that language studies contain different 'cynosures', or foci of investigation. This has an intuitive appeal, because it is tailored to linguistics. However, there is a danger of prejudging the question here, if we take Hymes to mean that Kuhn's system does not really work for linguistics because linguistics is not really a science, and that therefore its historical aspect should not be identical, but similar. This may be true, but it only has a partial bearing on the use of Kuhn's philosophy by practising linguists.

Becher (1989) looks at the structure, development and behaviour of academic disciplines from a purely sociological, rather than normative or theoretical, point of view. He does not specifically look at linguistics, but includes it in the social sciences, of which he provides an analysis in terms of standards of proof and types of material. He draws on other writers' dissection of disciplines into various categories, most of which separate the social sciences from the hard sciences. If there are fundamentally different types of subject, we should expect the practitioners of social sciences, and their practices, to differ from the hard sciences. That they do is reasonable evidence that they are different types of subject. Becher (1989:11) approaches the history of disciplines with a set of criteria taken from Biglan (1973) which are more fine-grained than a simple scientific/unscientific dichotomy. First, he points out the complexities in comparing disciplines. There are three distinctions which he uses:

1) Hard – soft, which relates to 'the degree to which a paradigm exists'
2) Pure – applied, which relates to 'the degree of concern with application'
3) Life system – non-life system, separating 'biological and social areas from those that deal with inanimate objects'.

This contrasts heavily with a simple demarcationist 'science versus non-science' approach. Becher looks at a wide variety of disciplines, such as engineering, law and literary criticism, not just sciences and subjects which aim to be sciences, and he concludes that very few subjects form 'clusters' across all three distinctions. For example, while physics and chemistry fall into the same three categories, biology is separate according to the third distinction.

Becher's divisions are appealing, and probably very useful from a neutral taxonomic viewpoint, but they do not seem to have had much influence in the way that disciplines, including linguistics, see themselves.

Part 4: Rationalism and Empiricism[50]

In chapter four I consider the influence of philosophies from the seventeenth and eighteenth centuries on the development of linguistics in the late twentieth century, the most well-known of which is the influence of Descartes on Chomsky. In particular I am interested in two epistemological approaches: Rationalism and Empiricism. These are vexed terms, and broad labels for broad concepts will inevitably be objectionable for some. Nevertheless, the two were largely seen as opposites for much of the 300 years which separate Descartes and Chomsky.

Harris (1993:66) provides as good an account as any of the standard view of the difference, and is worth quoting at length:

[50] A note on labels. 'Rationalist' and 'Empiricist' will be capitalised, to distinguish them more fully from those who use reason, and those who use empirical evidence. Obviously Empiricists are not wilfully irrational, and Rationalists are not unwilling to use empirical evidence. As I noted previously, conflation of the two has led to confusion in the past. However, in quotations I will retain non-capitalisations as they occur in the text. See chapter four for a discussion of the different definitions of these words, including subdivisions such as 'positivism'.

> Empiricism: all knowledge is acquired through the senses.
> Rationalism: no knowledge is acquired through the senses.
>
> Nobody in the history of epistemology, naturally, has bought (or tried to sell) either position; the only function they have served is as straw men in various polemics. The members of the loose philosophical school known as British Empiricism – a school with a varying roll, but which usually includes Locke, Hume, Berkeley and Mill – held positions that fall more fully within the first definition than within the second, along with several other eminent minds, such as Epicurus, Aquinas and Ayer. The opposing tradition is ably represented by Plato, Descartes, Spinoza and Leibniz. But even the most casual reading of any of these thinkers makes it clear that the only useful definitions here are fuzzy rather than discrete, and that the quantifiers should be tempered to reflect genuinely held beliefs:
>
> Empiricism: most knowledge is acquired through the senses.
> Rationalism: most knowledge is not acquired through the senses.
>
> Even with this tempering, however, we have to keep in mind that *knowledge* refers to domains like mathematics, language and hitting an inside fastball, not to the name of your sixth grade teacher or where you left the car keys. But the definitions are workable.

Harris's tempering of definitions is useful. It is easy to misrepresent either side, and people frequently do; but in this tempered form, both definitions have a *prima facie* plausibility. Neither is obviously illogical, and eminently clever and/or sensible people have adhered to each position.

However, given the straw-man nature of the 'strong' version, the 'weak' version throws up problems. What could 'most' mean in such statements? Knowledge is not something which can be measured or counted out. After all, as Harris says, we are not talking about where you left your keys, but about things like mathematics, so a general knowledge pub quiz will not clear the matter up. It is not immediately obvious what sort of evidence would count in deciding between the two weak formulations above. Despite these preliminary doubts, all the participants in this debate approach it as a serious question; that is, one that potentially has an answer, or answers.

Of course, I am particularly interested in a specific type of knowledge, our linguistic capabilities, and in this respect there is perhaps a clearer divide between the two positions than there is, say, with regard to mathematical abilities. Either we learn language, in the normal sense of the word 'learn', and consequently 'languages could differ from each other without limit and in unpredictable ways'[51] (Joos (1957:96), quoted in Harris (1993:64)), or we have a specifically constrained cognitive hardwiring which only allows as human languages a subset of a much larger set of conceivable languages.

However, there is a difference between arguing about the specifics of how much languages can vary, and arguing about the fundamental nature of human minds. It would be possible to see language as largely innate, while maintaining a broadly Empiricist epistemology, or vice versa (this point is elucidated in chapter 4). By comparison, no Empiricists have ever claimed that breathing is a 'learned' activity. But language tends to be seen as a bellwether: if language (or the structure of language, or our propensity to acquire language) can be shown to belong to one side of the debate or another, then this is evidence for a broader view of our cognitive abilities[52]. As one of the more accessible and salient aspects of our 'knowledge' (or our capabilities), language is iconic in our search for knowledge about the functioning of the mind.

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[51] Note that it is quite possible that Joos did not literally mean this. For a full debate, see http://linguistlist.org/issues/2/2-112.html.

[52] See, for example, Pinker (1994:17), Lyons (1991:209), Saussure (1974 [1916]:7) Katz (1981:1-18) Smith in Chomsky (2000:vi-vii) and Locke (1964 [1690]:259).

One modulation to this picture needs to be added, however. Empiricism was generally seen as the no-nonsense progenitor of modern science; Empiricism was supposed to have provided the Renaissance and Enlightenment roots which spawned so many eighteenth- and nineteenth-century taxonomic sciences, leading on to nineteenth- and twentieth-century Positivism and the continued success of the scientific project in mapping, explaining and predicting the universe.

Rationalism, in its Cartesian guise, had once been very much bound up with science, in that its founder Descartes was also a physicist of serious renown. However, up to the 1950s the inheritors of his legacy were generally deemed to be such figures as Hegel in the 19th century, (although Figueroa contrasts Hegel and Descartes, see chapter four) and Husserl and Heidegger in the 20th. Although these philosophers might seem to belong to a variety of schools, they were generally *not* seen as providing the basis for a reliable physical science. They did form the basis of much of our modern views on social and human sciences, but Empiricism in the Anglo-Saxon mould seemed to have the natural sciences sewn up, as described by Harris (1993:66, and see chapter four).

With these caveats we can accept the very broadest definitions of Rationalism and Empiricism, and accept the epistemological opposition which the two theories represent. Chomsky explicitly and frequently claims Descartes as a forebear; in the next two chapters I will show how and why he claims this, supporting his analysis and arguing in a similar fashion that sociolinguistics is steeped in an Empiricist view of the mind.

Conclusion

The purpose of this section has been largely to define certain terms and ideas. These terms and concepts are paradigms and incommensurability (particularly in Kuhn's formulation); relativism, natural and social science; TGG and sociolinguistics; and Rationalism and Empiricism. It should be clear by now, if it was not obvious before, that defining any of these is ultimately an impossible task – as long as these terms have existed, their definitions have been debated. However, it is possible to give an overview of how they are most commonly used and interpreted.

In this chapter I have also introduced various controversies over the meaning or interpretation of ideas such as 'paradigm' or 'Rationalism', because in the next two chapters I describe historical and philosophical processes and arguments which turn on the interpretation of these terms and ideas.

Chapter Three: Linguistics, science and Kuhnian paradigms

In chapter one I outlined my theory of reference for terms in scientific theories. This theory of reference is motivated by a set of problems (or questions), and is meant to solve these questions. In chapter two I gave definitions and details of the key theories and terms on which this thesis is based: Kuhn's paradigms, Kuhn's incommensurability, TGG, sociolinguistics, and Rationalism and Empiricism.

In this chapter and the next I will look at two specific problems which have arisen from the interplay of the terms and theories which I discussed in chapter two. The first of these, discussed in this chapter, is the debate about whether or not TGG in particular (but also, as is occasionally argued, other types of linguistics) can be convincingly presented as a scientific discipline; and if it can, then whether it can be seen to instantiate a Kuhnian paradigm. Most of the data in this chapter comes from disputes and self-justificatory arguments about whether or not a particular form of linguistics can claim to be science, and whether or not they can be justifiably called a Kuhnian paradigm. From this data I draw two negative conclusions – that neither TGG nor sociolinguistics can claim to be mature sciences, and that neither can claim to be Kuhnian paradigms. In some cases a clear link has been made between claiming paradigmaticity and claiming scientificity.

The second problem, discussed in chapter four, is concerned with claims about the links between Rationalism and Empiricism and modern forms of linguistics. In chapter five I show how these problems – scientificity and epistemology – can be explained with reference to the idea of incommensurability; how TGG and sociolinguistics can be said to have incommensurable vocabularies; and how this incommensurability can be explained by my theory of reference.

The first section of this chapter unravels some arguments from the history of linguistics claiming that TGG embodied a Kuhnian-style revolution and paradigm. I look at arguments for and against this, and the implication that paradigmaticity *confers* scientificity. I have already shown why this is a fallacy, and in this chapter I present evidence which suggests that this implication has often been made.

In chapter 2.1 I gave details of Kuhn's theory of paradigms. I also looked at why it originally applied only to those forms of study or research which are unquestionably scientific, and why, for this reason, it cannot be used as a proof or indication that some other field of study has attained scientific status. Having looked at Kuhn's theory in detail, we can assess it for *prima facie* problems that might arise in its applicability to the history of linguistics. By looking at some of the ontological issues surrounding language study, we can see why it differs in important respects from those sciences which are uncontroversially 'scientific', such as physics and chemistry, and which are the focus of Kuhn's theory.

We can then use these insights to assess the claims that the recent history of linguistics does or does not fit the Kuhnian model. With a better understanding both of the nature of Kuhn's theory and of the nature of the ontological issues concerning language as an object of study, we can examine the claims for linguistics fitting the Kuhnian model, and any concomitant implications that this reinforces its status as a science.

The last part of this chapter looks at the historiographical use of Kuhn's model from within linguistics, and the propagandist or self-justificatory value of Kuhn's philosophy. By this I mean that Kuhn's theory is well known, and has been interpreted as providing a definition of 'science' by examining the historical progression of scientific disciplines. The implication is that if a given area of study fits this model, then it must be a science, but I should point out once again that, in my opinion, Kuhn does not espouse this particular argument himself, implicitly or explicitly.

1.0 Is linguistics scientific?

The benefits of attaining 'scientific status' for a discipline should be obvious, in a trivial sense. If your discipline hopes to discover and describe the world as it is; if you want your findings to be taken as fact; if you want to discover truths about the world, rather than give an interpretation; then what you are aspiring to is 'science'. In one sense, 'science' is just a word, of course:

> Nor does linguistics need the nominal blessing of *science*. It is some sort of systematic, truth-seeking, knowledge-making enterprise, and as long as it brings home the epistemic bacon by turning up results about language, the label isn't terribly important. Etymology is helpful in this regard: *science* is a descendant of a Latin word for knowledge, and it is only the knowledge that matters. (Harris 1993:11)

Whether or not we agree that linguistics does not need 'the nominal blessing of *science*', there is still more than a label at stake. The methods of the natural sciences still provide a target, or template, for other disciplines which hope to base themselves on rigorous empirical discovery[53]. This is not just propagandist. I think that nearly all the linguists mentioned in this essay genuinely feel that their subject is scientific and that, when taking a break from the serious business of actually doing linguistics, they have a right, or perhaps a duty, to make claims for the proper status of their subject. They are not charlatans, and if they think that their subject

[53] I use 'empirical' in a non-technical sense here, but see below for detailed discussions of the use of this word.

has progressed to the point where it is much more like a physical science than anything else, then why not explain why this is so?

I have plenty of sympathy with these claims – having received formal training in both generative linguistics and sociolinguistics, I have only come across linguists who genuinely try to further the boundaries of knowledge, and who treat methodology seriously. However, I do not feel that any form of linguistics can be said to belong unquestionably in the natural sciences. My theory of reference for scientific terms, outlined in chapter one, should show why this is the case: any discipline founded on metaphorical posits, whether it is more like a pre-paradigmatic science or a social science, has fundamental ontological differences with the natural sciences, with methodological consequences.

Moreover, by examining the arguments which have occurred between linguists of different persuasions over whether they or their opponents practise science or not, we can illustrate that:

- Neither of the forms of linguistics under discussion can be fairly called a science, as shown by my theory of reference and other criteria (see this chapter and chapter 5)
- They are incommensurable. This explains the confusing nature of the debate (see chapter 5)
- Their incommensurability can be explained by my theory of reference (see chapter 5)

1.1 'Claiming scientificity', that is, explaining how their field should properly be considered a science

There are many viewpoints from which it is possible to argue that a given subject should be considered a natural science. In this section I will consider the two broadly contrasting viewpoints which I have already addressed in previous chapters: the 'standard' hypothetico-inductivist, and the (Kuhnian) historico-relativist.

1.1.1 Science from a 'standard' point of view

When claiming scientificity, there are many philosophies which could be pressed into service, and many strategies which could be used. One of the most popular is that used by Smith (1999:8-11), who makes much of the scientific nature of TGG, but along Popperian lines (or, if not strictly Popperian, then traditional hypothetico-inductivist with a Baconian heritage), rather than Kuhnian lines:

> One of Chomsky's achievements has been to make plausible the claim that linguistics is scientific in the more interesting sense that it can provide not only explicit descriptions but also explanations for the classification. There are several strands to such a claim. The first is that linguistics provides a general theory explaining why languages are the way they are: each language is a particular example of a universal faculty of mind, whose basic properties are innate. The second is that the theory should spawn testable hypotheses: like a physicist or a biologist, the linguist manipulates the environment experimentally to see what happens and, crucially, he or she may be wrong. The experiments are not usually as high-tech as those in the hard sciences, but they allow for testing: if your analysis entails that English speakers should find *John speaks fluently English* as acceptable as *John speaks English fluently*, then it is wrong and must be replaced by a better one. A corollary of this emphasis on seeking testable explanations is that the central concern is *evidence* rather than *data*. (1999:8)

The contrast which Smith is making is between data (raw observation) and evidence *for or against a particular theory*. Naturally, what counts as data and what counts as evidence is determined by the standards of the science in which the scientist is engaged (or the paradigm, as Kuhn would put it).[54] Later in the same chapter he argues further for the inclusion of linguistics among the natural sciences:

> Like physics, but unlike logic or literary criticism, linguistics is an empirical science. That is, on a Chomskyan interpretation, which takes the speaker's mentally represented grammar to be the correct focus for investigation, it makes sense to claim that one analysis is right and another wrong. Every time a linguist describes a sentence or postulates a principle, he or she is making innumerable empirically testable predictions. Those linguists who claimed that subjects precede objects in all languages were simply wrong: interestingly wrong, because the refutation of their claim has led to a greater understanding of the nature of language, but wrong. (1999:11)

These excerpts represent a classic explanation of the scientific method, as it is commonly understood, and a defence of Chomsky and Chomskyan linguistics on the grounds of this adherence to scientific norms. These scientific norms include using evidence rather than data. This distinction is critical in science: data is raw information about the world, with no meaning; evidence, on the other hand, means evidence for or against testable hypotheses. This is why both Kuhn (1962:15) and Smith (1999:8-9) refer to the pre-scientific practice of 'data-gathering' (although there is no mention of Kuhn or any other philosopher of science in Smith's account). The norms of science are presented as context-free and unquestioned.[55]

Chomsky himself tends to avoid the word 'science', using the word 'theory' instead.[56] However, in the introductory chapter of *Rules and Representations* (1980), which, like most of his books, sets out metatheoretical considerations before addressing the actual linguistics, Chomsky does, for once, address the idea of 'science' as opposed to linguistic 'theory'. He de-

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[54] The concept of what is and isn't data is discussed again in chapter five.
[55] As a minor point, I might point out that there is nothing particularly Chomskyan about the discovery that some languages have OVS or OSV word order.
[56] This has changed a little in recent years. See the passages on the 'science of human nature' in Chomsky (2000b), and the discussion with Krauss and Carroll (2006) http://www.chomsky.info/debates/20060301.htm.

fends the 'Galilean style' in physics (and, by implication, the other natural sciences), on the grounds that 'we have no present alternative' (1980:9). The question which he addresses is 'To what extent and in what ways can inquiry in something like "the Galilean style"[57] yield insight and understanding of the roots of human nature on the cognitive domain?' (ibid). Chomsky's answer is positive, of course, and he argues against

> the "bifurcation thesis", that is, the thesis that theories of meaning, language and much of psychology are faced with a problem of indeterminacy that is qualitatively different in some way from the underdetermination of theory by evidence in the natural sciences. (ibid:16)

This 'bifurcation thesis' comes in two versions, as advocated by Quine and Putnam. Putnam is the main target, when he argues that

> "the barbarous idea" of "scientizing the social sciences" collapses [...] because of the problems of indeterminacy of translation, "knowledge of such a *simple* fact as '*shemen* means oil' [in Hebrew] cannot be justified/confirmed by following the paradigms of inductive logic". (ibid:17)

What Putnam is arguing, and Chomsky denies, is that meaning and other linguistic objects cannot in the end be studied in the same way as other natural objects, because of the indeterminacy which stands in the way of our knowledge of them. Indeterminacy, for both Putnam and Quine, holds that there can be no fact of the matter about mental representations, because any theory about language and the mind is always underdetermined by the evidence, and other theories are always possible (Chomsky 1980:14-15). For Chomsky, this is no problem, because *any* science is subject to a certain amount of indeterminacy. After all, 'theories are underdetermined by evidence, or they would have no interest at all. What

[57] In this passage Chomsky uses Husserl's definition of the 'Galilean style' in physics as 'making abstract mathematical models of the universe to which at least the physicists give a higher degree of reality than they accord the ordinary world of sensation'.

seems implausible – at least, quite unargued – is the bifurcation thesis' (ibid:21).

Chomsky concludes that 'The crucial question, then, is whether psychology is part of the natural sciences' (1980:20). He is keen to show that the mind, language and psychology can indeed be studied scientifically, in the same way as other natural sciences, and that language and the mind are not subject to particular constraints on their study (indeterminacy):

> I do not believe, then, that consideration of [...] indeterminacy sheds any light on the enterprise I have been discussing, nor does it suggest that the effort to isolate systems of the mind that can be studied in the manner of the natural sciences must come to grief. I will therefore continue to pursue the working hypothesis that there are aspects of the study of mind that lend themselves to inquiry in "the Galilean style". (ibid:24)

As noted in chapter two, Hymes too has aimed towards a scientific but non-Chomskyan linguistics. The opening statement of *Studies in the History of Linguistics: Traditions and Paradigms* (1974:1) is unequivocal: 'Linguistics is a discipline and a science, and its history is part of the general history of disciplines and sciences'.[58] That linguistics is a professional discipline should be uncontroversial (as discussed in chapter two, with the caveat that it therefore shares characteristics with football, prostitution and bakery); here I want to address the second half of his conjunction, the idea that linguistics is a science. However, Hymes' discussion here is notably negative, and gives many examples of what science is not, and what paradigms are not, without giving a positive evaluation of how to do his type of linguistic ethnography scientifically. In 'Models of the Interaction of Language and Social Life', he again claims that the description of language 'is among the oldest of man's scientific enterprises' (1972:35, in Gumperz

58 See also Hymes & Gumperz (1972:35) for discussion of the contemporary status of sociolinguistics.

and Hymes 1972). He then goes on to describe how to do sociolinguistics, with the implication that this is how to do it scientifically.

1.1.2 What would a scientific linguistics look like from a Kuhnian point of view?

An alternative to the hypothetico-inductivst approach to science is the Kuhnian historical approach. According to Kuhn, an assumption of a mature science is that any of its puzzles will be solvable within the assumptions of the paradigm. Arguing against Popper, Kuhn (1977:274-276) argues that this, rather than testing and falsifying hypotheses, is the real day-to-day work of scientists.

Generative linguistics[59] seems to fulfil these criteria. TGG has a set of 'exemplars', or at least maxims which are central to the paradigm, and appear to be indisputable. The following list, while neither definitive nor exhaustive, represents these universal truths which are taken as both proof and motivation for the Chomskyan paradigm:

- Human babies are all the same with regards to potential linguistic ability. That is, any baby, brought up anywhere in the world, will learn its mother tongue equally well.

- A human language is ridiculously complicated – too complicated to be learnt via a process of trial and error.

- All human languages are potentially infinite, and all native speakers of each language are capable of understanding and producing an infinite number of sentences.

[59] As I noted in the introduction, this chapter focuses on TGG. It may or may not be the case that sociolinguistics also fulfils these criteria; this is an open question which I do not go into in this chapter.

- All native speakers have 'intuitions' about their language, and these intuitions have several features in common for all speakers. For example, all speakers can distinguish between a well-formed sentence (*I like eating apples*) and a badly-formed one (*I apples like eating*). They can also all distinguish between well-formed nonsense (*colourless green ideas sleep furiously*) and badly-formed nonsense (*heron slam-dunk why envelope cogitate*). They can also all agree that a sentence is ambiguous (*hectoring lecturers should be avoided*).

- These intuitions do not vary according to age (after the first few years), intelligence, education, other talents, other abilities or lack of (blindness, deafness, perfect pitch), hair colour, shoe size etc. So language must be a discrete human ability, hard-wired into the physical make-up of the entire population, and highly specified in its structure and development.

- It follows from this that humans learn languages 'naturally', that language learning is, to an extent, hard-wired into the brain.

- Therefore, all languages (or dialects, or linguistic varieties) share a basic structure which allows any one of them to be randomly 'imprinted' onto the brain of the new-born.

- To describe children as 'learning' a language is erroneous. Perhaps 'grow' would be more appropriate.[60]

These maxims throw up puzzles, and individual linguists can choose which of these puzzles to attempt. These puzzles include defining exactly

[60] These exemplars, or maxims, can be found in chapter one of just about any introduction to generative grammar. Two examples separated by nearly twenty years are the first two chapters of Smith and Wilson (1979), and the first chapter of Radford (1997).

what count as linguistic universals and discovering the transformational rules of different languages within a generative framework. [61] There is certainly a community of peers who can give approbation to or judgement on the results obtained, and the results are largely uninterpretable to non-linguists. Also, students tend to learn from textbooks (although Chomsky often makes an appearance on reading lists too, and again, see chapter four for a discussion of the role of classic or early modern philosophy in linguistics).

However, none of these social or institutional norms can really be seen as defining a science, and certainly not as demarcating it from pseudo-science. With the exception of puzzle-solving, these are essentially the outward institutional trappings of a science, and cannot be taken as 'proof' of the scientific nature of the enterprise. We can compare this line of argument with the formation of 'cargo cults', whereby Pacific Islanders were said to have observed the arrival of 'cargo' at hastily built airstrips, and concluded that if they built their own airstrips then 'cargo' would arrive for them. Following this logic, they built replica airstrips complete with landing lights and control towers, usually out of straw and wood. The aeroplanes, however, did not arrive (Jarvie 1964/ 1967:55-73). I am not suggesting that TGG (or any other type of linguistics) falls into the cargo cult category, just that it is a similar fallacy to regard Kuhn's description of the outwardly observable aspects of a successful science as criteria for the practice of a successful science. The presence or absence of such outwardly observable facets of a successful enterprise might be necessary for normal science to take place, but without the unobservable substance which actually causes the enterprise to function, they are not sufficient.

[61] As we saw in chapter two, both TGG and sociolinguistics have standard ways of solving 'puzzles', or at least conducting research within their paradigms.

1.2 Attempting to demonstrate that another sub-discipline is not scientific

Hymes' view implies that it is possible for a discipline to be scientific without Kuhn's model being applicable to it. The more radical possibility for sociolinguistics (or any other type of non-TGG linguistics) is to explain either why TGG is misguided and broadly meaningless, or why it is non-scientific. (One does not necessarily entail the other. We now know that Newton's laws of motion were 'wrong' in some sense, but we still regard their discovery as 'good science'; conversely there are many sentences which are no doubt true, but not scientifically provable). One advantage of describing a rival discipline as non-scientific without reference to Kuhn is that it is not question-begging ('Why should we take Kuhn's account as definitive?'), and measures a discipline against more 'objective' criteria.

It appears that older writers (contemporaries of Chomsky's or not much younger) dismiss his ideas most vociferously (see Hockett 1968, Yngve 1996 below) – which is exactly what Kuhn would expect (1962:151). More recent linguists tend to see TGG as a permanent fixture in modern linguistics, and (for some) containing plenty of merit, but only in one corner of the linguistic field. However, there have always been dissenters who claim TGG to be not just wrong, but also unscientific. Labov has been a leading critic of the methodology of TGG, which uses intuition rather than quantificational analysis or other less 'subjective' data. Ironically, he has also been a supporter of generative linguistics, and originally planned to incorporate his sociolinguistics into a generative framework (Figueroa 1994:99-101). Labov, then, saw TGG not as entirely wrong, but as using data whose validity may be questionable, therefore leading to questionable results (see the discussions on intuitions and idealisation below).

One of Chomsky's most consistent critics is a former ally, Paul Postal. Chomsky, famously, never loses an argument (see Botha (1991) for a detailed analysis). He does, however, lose friends. In *The Linguistics Wars* (1993), Harris chronicles at length how Chomsky's earliest and brightest young students ended up turning away from him, in particular John Ross, George Lakoff, James McCawley, Paul Postal and Jerrold Katz, in the generative semantics dispute. This conflict forms the central theme of *The Linguistics Wars* (1993) by R.A. Harris, which I discussed at length in chapter two. Harris gives a fairly detailed explanation of how this separation occurred; in brief, Postal seems to have been the most passionate pro-Chomskyan in the early days (1994:68-73), and converted this to equally anti-Chomskyan zeal (ibid:199). The original dispute was purely about linguistics, however. [62] In their subsequent careers all of those linguists mentioned above have criticised Chomsky to some extent; some more than others, and in Postal's case, much more. Much of his current work revolves around the idea that he is sticking to the principles of generative grammar, while Chomsky ignores the most basic scientific standards. Postal (2004) accuses Chomsky of base rhetorical tricks:

> Passages like (1) [a quotation from Chomsky] make no attempt to consider criticisms of the favoured view, nor do they deal with arguments, many of considerable detail and depth, that other, competing views, of NL [natural language] syntax are far superior to the GB view. Work in lexical functional grammar (LFG) and head-driven phrase structure (HPSG), categorical grammar, and so on, is unmentioned. In short, such passages partake more strongly of the character of factually empty propaganda rather than of serious scholarship[63]. (2004:4)

[62] See also Huck and Goldsmith (1994) for a treatment of the same disputes.

[63] Lexical Functional Grammar and Head-Driven Phrase Structure are two non-Chomskyan theories of generative grammar. They are 'generative' in that they generate the sentences of a language, but they differ from TGG in that they are non-transformational. See Bresnan (2001) and Pollard and Sag (1994).

Although here Postal advocates a broad consideration of different theories of language, for the most part he concentrates on attacking Chomsky over his methodological approach. As such, his main criticism is about 'serious scholarship', or to put it another way, standard scientific practice:

> It is one thing to lack insight, to propose defective principles, to suggest generalizations that do not stand up. All this is a regrettable but nearly inevitable part of normal inquiry. It is quite a different thing to flout minimal standards of scholarly procedure; to ignore the literature; to claim such and such a generalization holds when one knows or should know it does not; to generalize to grand claims from a few selected cases; to develop techniques, rhetorical and otherwise, for avoiding falsification; to deliberately cite certain facts that support one's proposals while deliberately not citing those that do not; to fail to respond to criticisms and to restate criticized positions as if no critique existed not because the challenges do not merit a response but because one lacks a viable response; to utilize other people's ideas without credit; to claim that someone whose work one is criticizing has said such and such when there is no basis for such a claim; and so on. Combinations of various of these and other unacceptable procedures inevitably yield something that, while purporting to be linguistics, is actually junk linguistics. (2004:9)

Postal's book is an in-depth examination of how Chomsky and Chomskyan linguists are guilty of all of the above.[64] Pullum and Scholz (2002) make similar comments about the lack of empirical support for one of the key tenets of TGG – the poverty of the stimulus – although they do so in far less personal and far more constructive terms than Postal does. Trask also criticised the Chomskyan project for being constructed on shaky theoretical ground[65].

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[64] Harris more diplomatically, repeats this charge, saying that Chomsky has a 'cavalier' approach to intellectual property. He suggests that Chomsky is not 'the common-thief variety of idea absorber ... [he is] ... as happy to give ideas away as he is to appropriate them.' (1993:255).

[65] For a trenchant interview with Larry Trask, shortly before his death, see http://www.guardian.co.uk/science/2003/jun/26/scienceinterviews.artsandhumanities

There are also arguments from TGG about the non-scientific nature of sociolinguistics, [66] but I will not concentrate on these, as the main issue of this chapter is not about whether or not sociolinguistics forms a Kuhnian paradigm. Consequently I will restrict myself to the arguments from within sociolinguistics on the supposedly non-scientific nature of TGG.

Two methodological issues are commonly raised to make this point. They concern the use of intuitions as data, and the nature of idealisations. I will briefly discuss these here.

1.2.1 Intuitions

The first point concerns TGG's use of native speakers' intuitions about the well-formedness of sentences in order to discover the grammatical rules of a language. This has led to discussion about how intuitions are obtained. Typically the linguist makes up a set of sentences which are (or are not) acceptable to him or her, and deduces rules of movement and blocking (for example) from these. Their judgements on the acceptability or otherwise of the sentences come from their tacit knowledge of their native language; that is, they know without thinking about it whether the sentences are acceptable or not. As Radford (1997:4) says:

> It would perhaps not be too much of an exaggeration to say that whereas traditional grammars concentrate on *grammaticality* [...] work on grammar within the Chomskyan paradigm tends to focus more on explaining *ungrammaticality*.

We all have a mechanism for spotting badly-formed sentences. We know when a sentence does not conform to the norms of our dialect, and we have intuitions about what is acceptable. So a native English speaking

[66] There is a commonplace assumption in TGG that variation is not amenable to scientific study – see Figueroa (1994:83).

linguist will know that 1) is acceptable and that 2) is not, and will deduce rules of English grammar from this:

1) My armadillo has lost his fork[67]
2) * His my lost armadillo has fork

However, there are problems with this bald division of sentences into 'acceptable' and 'unacceptable'. I cover them in detail in chapter four; the following serve as examples for the purposes of clarity in this chapter.

It seems that linguists have slightly different intuitions to everyone else, either through overexposure to normally unacceptable sentences, or just through thinking about it too much. Snow and Meijer (1977:175) present a case study which makes exactly this point. They gave the same questions, based on Dutch word-order, to three groups: native Dutch speakers with no linguistics training, a group of second-language Dutch speakers, and a group of professional linguists[68]. The results were that 'the correlation [...] is higher between native speakers and non-native speakers than between native speakers and linguists.' Moreover:

> Only one of the non-native group could be said to be a perfect bilingual. Two more were very good bilinguals and the other five spoke Dutch considerably less well than their native language [...] Yet the correlation between the group of three excellent bilinguals and native speakers was not higher than the correlation for the poorer Dutch speakers with native speakers. This suggests that skill in speaking a second language can be developed without developing 'better' (i.e. more native-like) syntactic intuitions.

[67] Of course, this supposedly 'grammatical' sentence is also semantically incongruent – a device used by Chomsky to show the independence of grammar from word meaning (1957:15).

[68] This three-way division of the subjects (natives, non-natives and linguists) is complicated, and perhaps compromised, by the fact that Snow and Meijer do not mention whether the trained linguists were native Dutch speakers or not. From the phrasing, my guess is that they were, but this is not clear.

This passage reveals two interesting problems with intuitions. First, the intuitions of speakers of a second language, of varying levels of proficiency, closely match those of native speakers. In other words, the native intuitions do not apparently provide a 'better' source of data than non-native ones. Second, and more damning, linguists correlate with each other, but not with linguistically naïve (i.e. untrained) non-native- or native-speakers: they form their own group. This suggests that their intuitions have been learnt, or shaped, by their linguistics training.

One way to avoid this issue of whether linguists' intuitions have been somehow changed by their training is to concentrate solely on the intuitions of non-linguists. While more time-consuming, this at least looks more likely to provide a repeatable experiment, and avoids the problems outlined above. However, this also runs into problems, and Greenbaum (1973) describes one of these. Where 'a trained linguist' is fairly well defined, linguistic naivety in general is a much fuzzier concept. Greenbaum replicated an experiment (as the scientific method both allows and requires), to see if the results matched those in the original. In the first experiment, the researchers had given out lists of four sentences, each exemplifying a different construction. For example:

(A) Sophia Loren was seen by the people while enjoying herself
(B) The people saw Sophia Loren while enjoying themselves
(C) Judy was seen by the people while enjoying themselves
(D) The people saw Karen while enjoying herself

The problem was as follows:

> It appears (at least there are no indications to the contrary) that the four sentences were presented to the informants in an identical order and in the order that the investigators hypothesized would have a decreasing rate of acceptability. The order might have given a clue to informants and therefore might have

> prejudiced the results. Moreover, there might be a general tendency to judge earlier sentences differently from later sentences. For example, it could well be that exposure to a set of deviant sentences [...] will increase the tolerance of informants. (1973:204-205)

This suggests that some of the informants were actually changing during the test, and that therefore it is possible to become less linguistically naïve and change intuitions, at the same time as informing on those intuitions. If this is right, then it sounds disastrous for the idea that intuitions constitute reliable evidence. Greenbaum is relatively optimistic about this, saying that it is a matter of the experimenter being careful with how the test is constructed so as to minimise this kind of accident, but if non-quantifiable factors such as 'linguistic naivety' can be important, then this impinges on the possibility of conducting experiments in a controlled environment.

Smith (1999:29) defends the use of intuitions in this way:

> It is worth emphasizing that reliance on intuition is not a retreat from the usual canons of scientific rigour: 'intuition' is simply another word for 'judgement', with no mystical or unscientific overtones. There is no difference in principle between our linguistic judgement about sentences of our native language and our visual judgement about illusions such as the Muller-Lyle arrows. It is simply a fact that we perceive the lines as different in length, and psychologists try to devise theories of visual perception that explain this fact. Likewise, it is simply a fact that we judge *they are flying planes* as ambiguous, and linguists try to devise theories of competence that explain why.[69]

While intuitions may have no 'mystical' overtones, they do, as we have seen, attract the suspicion that their use is 'unscientific', or, at least, not quite as scientific as we might like.

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[69] See McCawley (1982) for a critical discussion of the status of intuition regarding 'sense data' as opposed to 'perceptual data'.

This supposed failure of empirical study in TGG is often cited as a serious flaw. Labov, contrasting TGG (unfavourably) with his own quantitative methods says

> When we study what people do rather than what they think they do, we get a much simpler and more understandable view of the linguistic system. (Labov 1989:53, quoted in Figueroa 1994:99)[70]

For Labov, the use of intuitions is an unnecessarily fraught occupation, when there exist alternatives for studying language which conform to the standards of normal empirical science. Through observation and quantification it is possible to draw conclusions without recourse to intuitions.[71]

Apart from the question of whether or not competence is accessible, there are other procedural objections concerning the use of intuitions as data. These include claims that TGG has never come up with a model to explain intra-speaker variation, and instead chooses to idealise it away. Another objection is that inter-speaker variation is also assumed, whereas there may in fact be important differences which are ignored this way, depending on factors such as gender differences, handedness, age and personality (Schütze chapter 4).

On top of this, there has been a long-running dispute about linguists' intuitions. This takes two forms: first, that over-exposure to deviant sentences might affect their judgements; second, that advanced knowledge of grammar might affect their judgements; and third, most seriously, that their intuitions might be affected, either consciously or unconsciously, by a desire to make the facts fit the theory. As Labov says:

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[70] A good example of this is *Introducing English Grammar* by Borjars and Burridge (2000), which uses extracts from *the Big Issue* instead of invented examples. It should be noted, however, that these are written rather than spoken examples.

[71] Of course, this method has its problems too, such as the observer's paradox, which defenders of TGG could throw back at sociolinguistics (or, indeed, almost any human science).

> [A]s linguists become more deeply involved in [...] theoretical issues, it is likely that their intuitions will drift further and further from those of ordinary people and the reality of language as it is used in everyday life [...] Linguists cannot continue to produce theory and data at the same time. (1972:199)

The problem of linguists' intuitions is, of course, easily solved – do not use them whenever it is possible to use linguistically naïve subjects.[72]

Another procedural problem with acceptability judgements, dealt with at length by Schütze (chapter 5), concerns the instructions given to the subject by the researcher. The problem lies in consistency, or lack of, in the way that instructions are given out. Where linguists usually have well-defined concepts of, for example, grammaticality or acceptability, their linguistically naïve subjects may not. So if a linguist asks a subject whether a sentence is 'acceptable' or not, the interpretation of what 'acceptable' means may vary from subject to subject.

Schütze gives a humorous example of this problem, an early exploration of the difference between linguists' and non-linguists' intuitions by Hill (1961):

> He used 10 subjects, of which 3 were linguists and several others were English professors [...] They were instructed to "reject any sentences which were ungrammatical, and to accept those which were grammatical" [...]. Two rejecters of the sentence *I never heard a green horse smoke a dozen oranges* changed their judgments to accept it once it was pointed out to them that the sentence was true. (Schütze 1996:131-2)

[72] Interestingly, the subject of whether or not linguists' intuitions differ from those of the general public, and whether or not this difference is systematic, has become a sub-field in its own right. See Schütze (1996) chapter 4 for a wide review, as well as Greenbaum (above).

This example illustrates two points. First, the understanding of key concepts may not be shared by researchers and subjects. Second, and more importantly, the instructions given by the researchers may not always be clear, and even if they are clear and well-defined in one test, that clarity and definition will almost certainly (in practice) not be carried across to other tests. This means that the results of one test cannot be compared to those of another, with two important implications. The replication of a test (a cornerstone of any truly scientific enterprise) may be confounded by performance factors related not to the sentences but to the way in which they are presented; and the results of different tests may be incomparable because the instructions given to candidates may differ in crucial but unacknowledged ways.

Schütze argues that all these procedural problems can be overcome. However, it is certainly true that the lack of standardisation in this area may have profound repercussions for the validity of *any* data gathered in this way.

A methodological argument about the validity of certain types of data is unlikely to bring down an entire paradigm[73]. While these objections about intuitions may prove valid, there are generativists who work with data other than intuitions, especially in child language acquisition. For TGG intuitions are a convenience, albeit a notorious one, rather than a cornerstone, so any attack along these lines may not, after all, prove fatal to it.

1.2.2 Idealisation

The second methodological aspect of TGG which has been described as unscientific is that of idealisation (again, I address these issues in more

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[73] This is a Lakatosian observation. See chapter two for a discussion of Imre Lakatos' 'Methodology of scientific research programmes' (Lakatos and Musgrave (1970, especially 132-8)).

detail in chapter 4). In the context of TGG, this can mean two closely connected things.

First, idealisation can refer to the 'idealised speaker-listener' as part of a 'homogeneous speech-community' (Chomsky 1965:4). This is the theoretical individual whose knowledge of language is the subject of TGG models. The idealised speaker-hearer is 'unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random and characteristic) in applying his knowledge of the language in actual performance' (ibid). This idealised speaker-hearer is therefore a theoretical construct devised to access linguistic knowledge, or competence, rather than actual production and interpretation of language, or performance.

Smith puts forward the straightforward argument in defence of idealisation:

> All of science is characterised by the need to exclude from consideration those factors which, while undeniably real, are not pertinent to the issue under investigation. We know that heavenly bodies are not mathematical points, but they can be treated as such for the purposes of gravitational theory. (1999:12)

Figueroa (1994:83) quotes Suppe[74] (1989:65) making the point in a similar way:

> A science does not deal with phenomena in all of their complexity; rather, it is concerned with certain kinds of phenomena only insofar as their behavior is determined by, or characteristic of, a small number of parameters abstracted from these phenomena. Thus in characterizing falling bodies, classical particle mechanics is concerned with only those aspects of falling-body behavior which depends upon mass, velocity, distance travelled over time, and so on. The color of the object and such are aspects of the phenomena that are ig-

[74] Frederick Suppe defends the semantic conception of scientific theories against the 'received' (i.e. broadly logical positivist) conception. See Suppe (1989).

> nored; but the process of abstraction from the phenomena goes one step further: We are not concerned with, say, actual velocities, but with velocity under idealized conditions [...]

Figueroa continues: 'In this way Chomsky is concerned with the idealized conditions of species homogeneity (for universal grammar) and idealized speech communities (for the study of specific grammars)' (ibid). It seems uncontroversial to say that in order to discover an individual's grammatical knowledge, we do not need to study factors which lie outside of that structural system. This is the same point that Chomsky was making above when he discussed doing science in 'the Galilean style'.

Hymes suggests that it is not quite that simple. Where Chomskyans separate pragmatics from syntax, Hymes sees them as inextricably linked: 'for Hymes knowledge of a language also entails the ability to use it' (Figueroa 1994:54). Hymes points out that 'a person who can produce all and any of the sentences of a language, and unpredictably does, is institutionalized' (Hymes 1974:75), meaning that the social knowledge of how and when to use language is absolutely central to knowledge of language. We need to know not just how to form a question, but when to do so and which questions to form. Whether or not this is a convincing argument turns on whether different linguistic abilities can be separated out and studied individually (competence, performance, pragmatics, syntax, etc), or whether they constitute an inseparable whole. I will not go into this issue here, but the argument shows the potential methodological problem for TGG.

The second type of idealisation is not of the human, but of (parts of) language. TGG studies sentences, while sociolinguistics studies utterances. To put it another way, one studies types and the other studies tokens. Of course, the 'types', which are known as 'sentences', are generated by the idealised speaker-hearer, and studying them instead of the tokens is motivated by the same concern for simplicity of structure. The distinction is worth making as questions about the ontology of humans are different to questions about the ontology of sentences. However, the following problem arises: it is only the tokens that physically exist, not the types, so perhaps it makes more sense to study them. TGG studies sentence types, and therefore does not study 'the real world'.

This is a vast philosophical question, one which continues to be debated[75]. I do not intend to solve the problem of particulars and universals here, although it recurs in chapter five. Figueroa (1994:158-163) provides a thorough analysis of the ontological issues involved from a specifically linguistic view. In later chapters I also look in detail at the opposition between competence and performance, which is a closely related issue.

These are the two perceived problems with linguistic idealisation, opinions about both of which divide along the TGG-sociolinguistics lines. In my opinion this area provides a more cogent and potentially more damning opposition to the whole Chomskyan project than the objection to intuitions outlined above. In questioning the validity of 'carving up' language into competence and performance, and instead viewing it as holistic, Hymes et al force an all-or-nothing approach to TGG.

1.3 Conclusion to section one of chapter three

What is gained by analysing linguists' arguments that their discipline is scientific, and arguments that other disciplines (or forms of linguistics) are not scientific? First, we gain an understanding of what motivates linguists. Few participants in the debate are happy to concede that what they do is unscientific. All of them are trying, in some sense, to 'bring home the epistemic bacon', and in explaining why they believe that their

[75] See Plato's *Republic* and Wittgenstein's *Philosophical Investigations* for two of the better-known solutions, and the Stanford Encyclopedia of Philosophy for a good discussion of the current state of the art.

approach is scientific, we gain an understanding of what they are trying to achieve.

An analysis of the arguments for why another form of linguistics is not scientific also gives us an understanding of what motivates linguists and what they are trying to achieve. However, on a more fundamental level for my purposes, we get an insight into what kind of objects linguists believe themselves to be dealing with, and what they believe to be the best way to study those objects. In terms of my theory of reference, you can only do science when the posits of a theory have a fixed reference. When we are dealing with arbitrarily referenced mental posits, we do not have to agree on the reference of a term, and therefore we do not have to agree on the best way to study that posit. This then takes the form of both ontological and methodological incommensurability.

To sum up, then, TGG sees itself as scientific, as do some non-Chomskyan schools of linguistics, and there are many ways, not necessarily involving Kuhn, for adherents of TGG to support this. The non-Chomskyan schools rarely invoke Kuhn as positive evidence for their own discipline's scientific status, as this necessarily involves somehow proving that TGG is unscientific. While there are complaints about the allegedly unscientific intuitionist methodology often used in TGG, most sociolinguists at least do not plan to wipe TGG off the map, but rather assume that it will continue as one of many 'separate but equal' types of language study.

Instead, when non-Chomskyan schools discuss Kuhn with reference to TGG, they do so in order to show either that he is wrong, or that his model is not applicable to language study, and that therefore TGG does not represent a Kuhnian paradigm. This forms the subject of part two of this chapter.

2.0 Linguistics and paradigms: the use and abuse of Kuhn's philosophy in linguistics

2.1 Introduction to section two

> It is rather puzzling, then, that so many commentators, generativist and non-generativist alike, have taken the Chomskyan revolution to exemplify Kuhn's conception of a scientific revolution (see, for example, Katz and Bever 1976:11; Koerner 1976:709; Maclay 1971:163; Searle 1972:16; Sklar 1968:213; Thorne 1965:74). (Newmeyer 1996:179n)

The previous section looked at instances of linguists claiming that their subject is scientific, or claiming that their rivals' is not. Following on from that, this section is concerned with the charge that Chomskyans have, at times, tended to justify their own existence and written their own history in Kuhnian terms, incidentally, and perhaps unintentionally, giving tacit validation to Kuhn in the process (see Koerner (1994a:1.0), and see below for more examples). In brief, this means showing that the history of their discipline fits the Kuhnian model, with the possible implications that their discipline is therefore a science and that Kuhn's model is a correct account of the development of science. Other linguists addressing this issue, whatever the nature of their disagreement with Chomskyan linguistics, tend to take issue with the view that TGG might form a paradigm, whether or not there is any evidence that such a claim has been made in the first place, preferring the line that Kuhn's philosophy of science is not very useful in a subject like linguistics. It will also become clear in section 2.3 of this chapter that the instances of Chomskyan linguists presenting TGG as a Kuhnian paradigm are far outnumbered by the instances of complaints – both from Chomskyans and non- or anti-Chomskyans – that this has been too frequently done.

The primary motivation on both sides concerns the appearance (or reality) of a Kuhnian paradigm. As we saw above, a Kuhnian paradigm is a set of assumptions which totally dominate a subject when normal science is being done. In times of normal science there is no dissent (Kuhn 1962:23-34) and no challenge to that set of assumptions. I say 'appearance' because it is obvious from the most cursory glance that Chomskyan linguistics, while perhaps dominant, is not paradigmatic in the discipline in the strict Kuhnian sense of being the only commonly-held approach to the study of language (see Newmeyer below). Historical linguists, sociolinguists and many others all thrive in universities and journals across the world; some linguists oppose Chomsky, and others simply have no need for him. It sometimes seems that Chomskyans like to exaggerate their dominance, though, in order to give the impression that they and only they are doing real linguistics, while a small minority have other approaches, and sociolinguists *et al* are engaged in sociological or historical research which, while interesting, is emphatically *not* linguistics[76]. On the other hand, linguists working outside the generative grammar model often present the situation as the opposite, that Chomskyan linguistics is one of many types of linguistics. According to this point of view, TGG enjoys the lion's share of resources and fame, and no doubt does interesting and worthwhile work, but it is only one type of language study, and therefore it constitutes neither a theoretical nor a political paradigm.

In order to examine the instances of linguists using Kuhn's theory to argue for or against linguistics (or a certain type of linguistics) as instantiating a paradigm, I have broken this section down into four parts, each examining a different aspect of the issue. Each of these describes a strategy or motivation which is used to argue for or against the applicability of Kuhn's model to various types of modern linguistics (usually TGG, in keeping with

[76] See Katz, (1981:220n), Newmeyer (1986:5), Chomsky (1986:4-5) and (2000:1-2), and Smith in Chomsky (2000:vi-vii). Also see Hymes (1974:12,16), who complains about this practice, and Labov (1972a p183).

the rest of this chapter, but with occasional diversions into other types of linguistics), or for or against the claims of a particular school of linguistics to scientificity. These strategies are:

- Using the works of Kuhn in order to bestow scientific legitimacy on their own (sub-) discipline (as discussed in section 2.2 below).
- Claiming that another sub-discipline has misused Kuhn for their own self-serving purposes (2.3).
- Attempting to demonstrate that the other (sub-) discipline does not, in fact, follow the Kuhnian model (2.4).
- Attempting to demonstrate that Kuhn's model is wrong, and that therefore that it has no bearing on the scientificity or otherwise of a linguistic (sub-) discipline (2.5).

My own opinions will be elucidated in more detail later on, but it will become clear that the main point of this section is that there are phantom voices in this debate. Some Chomskyan writers have claimed that their subject instantiates a Kuhnian paradigm, occasionally insinuating that this reinforces its status as the pre-eminent, or perhaps most scientific, form of linguistics. However, just as many writers accuse Chomskyans of abusing Kuhn's philosophy in this way.

2.2 Using Kuhn in the service of their own (sub-) discipline, in order to bestow scientific legitimacy on their work

The use of Kuhn's work, *The Structure of Scientific Revolutions* (1962), in linguistic historiography is simply motivated: to show that one's discipline fits in with the Kuhnian account of the history of science is to show one's discipline to be scientific, and to be scientific is unquestionably a desirable quality for a discipline to have, as I argued above. I will briefly recapitulate why such a position would be wrong.

One aspect of the philosophy of science implicit in these comparisons is the idea that by describing science, Kuhn provides some kind of yardstick by which to measure scientificity. On this reading, Kuhn has described the history of science, and therefore anything which fits this model must be science. As I argued in the introduction, this is to commit the classic fallacy of turning an 'is' into an 'ought': it is a mistake to interpret his theory as either demarcationist or prescriptivist in this sense, as he gives a sociological history of science rather than a recipe for constructing a scientific paradigm. As I mentioned in chapter two, he includes astrology and many other activities which we nowadays regard as non-scientific in his description. For this reason, following Kuhn's model can be no guide to present or future science, history being contingent on non-recurring events.

The apparent fit between Chomskyan linguistics and Kuhn's philosophy has driven many hypotheses on the history of linguistics. However, there is a circularity involved in their justification of each other. It is not possible to claim that TGG (or indeed, any other type of linguistics) is a science on the grounds that it fits with Kuhn's account, because it is by no means clear that Kuhn is right. Conversely, the emergence of TGG does not vindicate Kuhn's account, because it is not clear that TGG should be regarded as 'a science', however closely its history seems to fit Kuhn's model.

The mutual benefits are obvious, though. TGG *does* fit the Kuhnian model in some ways, and for those who oppose TGG on ideological, methodological or any other grounds, this is infuriating. The insinuation that what they did was unscientific is unpleasant for older practitioners of linguistics, even if this is not what an accurate reading of Kuhn entails. It is annoying for sociolinguists to be told that their study is somehow inferior, on the grounds that it is not 'real science' like TGG. And it is annoying for all non-Chomskyan linguists to see the majority view positioning itself as *the* paradigm, that is, the only linguistics in the field, with a predestined claim to ownership of the field of language studies.

I am making a bold claim, as none of the sources listed below makes the point explicitly that because it follows the Kuhnian pattern, TGG must therefore be a science. However, I think this is a reasonable inference to make, for the following reasons.

Anyone who claims that the history of a particular field accords with Kuhn's account of the formation, development and overthrow of scientific paradigms is claiming that that field is scientific. Clearly this may be left implicit in the description, but if this implication is not made, then we must ascribe extreme naivety or absolute unfamiliarity with Kuhn's work to the writer. Kuhn wrote about the history and philosophy of science; that was his job. Any discussion of Kuhn must be assumed to be within the field of history and philosophy of science. Of course, there is nothing wrong with drawing parallels, pointing out that the field whose history is being addressed, while not a natural science, nevertheless fits the Kuhnian mould; but if this is not made explicit, it is almost impossible not to draw the conclusion that natural science is the area under discussion.

There is also the question of why anyone would ever bother to note the fit between TGG and Kuhn's theory unless there was some further claim being made. If we assume that there is no further claim being made, then noticing the fit is akin to looking at a cloud and noticing that it looks like a camel, or the London underground – pleasing in its coincidence, but nothing more than that.

Narratives which hold up TGG as typifying a Kuhnian paradigm claim that Chomsky's *Syntactic Structures* (1957) swept away the old post-

Bloomfieldian paradigm, and that the paradigm of linguistics since then, and, by extension, all serious linguists, have been Chomskyan. A possibility which is less frequently encountered in the literature is that Chomskyan linguistics forms the 'first paradigm'[77], that is to say that linguistics was not scientific until Chomsky. This would be entirely in keeping with Kuhn's account of science, but it is rarely explicitly stated, (although Koerner (1994a:1.1.3) quotes Newmeyer (1980:250), claiming that "More has been learned about the nature of language in the last 25 years than in the previous 2500". Also, Newmeyer 1980:20 has a chapter called 'Syntactic Structures: Linguistics Made a Science'). Instead, it is more often asserted that TGG forms a new paradigm, replacing the old. This implies that linguistics was scientific before Chomsky, but that Chomskyan linguistics is better than structuralist linguistics.[78]

In a passage which provides several references on this point, Newmeyer says that

> It is rather puzzling, then, that so many commentators, generativist and non-generativist alike, have taken the Chomskyan revolution to exemplify Kuhn's conception of a scientific revolution (see, for example, Katz and Bever 1976:11; Koerner 1976:709; Maclay 1971:163; Searle 1972:16; Sklar 1968:213; Thorne 1965:74). (Newmeyer 1996:179n)

Here we have six references to claims that 'commentators, generativist and nongenerativist alike' endorse the Kuhnian interpretation of the 'Chomskyan revolution'. I will look at what they say about Kuhn one by one, to check what they actually say about Kuhn, paradigms and science.

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[77] See Figueroa (1994), discussed below, who floats the possibility but does not endorse it.
[78] As discussed in section 2.3 of this chapter, Saussure felt that his approach to linguistics was 'scientific', as did Müller in the late 19th century (Müller 1862 *passim*). Below I address the idea that TGG *is* structuralism, and the idea that TGG might form the first paradigm, in much more detail.

Katz and Bever say 'The transformationalist revolution in linguistics fits Thomas Kuhn's (1962) account of scientific revolutions.' It is hard to find much room for equivocation in this in its portrayal of the Chomskyan revolution as a Kuhnian one.

Maclay says:

> Chomsky's work has led to a genuine scientific revolution in that his approach has redefined the goals and methods of linguistics and thereby delineated a set of relevant problems with which linguists may be properly concerned [...] Chomsky's impact is due in no small part to his ability to offer solutions to a wide range of problems that had been either ignored or handled by structuralist methods. (1971:163-4)

Maclay, like Katz and Bever, stresses the 'scientific revolution' nature of TGG. Unlike Katz and Bever, however, he does not explicitly call it a Kuhnian revolution.

Searle (1972) is fairly explicit about the Kuhnian nature of TGG. However, Searle is in a minority here in claiming not only that TGG solves problems, but that pre-Chomskyan linguistics was full of 'nagging counterexamples'[79]. Searle says:

> His [Chomsky's] revolution followed fairly closely the general pattern described in Thomas Kuhn's *The Structure of Scientific Revolutions*: the accepted model or 'paradigm' of linguistics was confronted, largely by Chomsky's work, with increasing numbers of nagging counterexamples and recalcitrant data which the paradigm could not deal with. (1972:2)

Searle goes on to describe two standard 'problems' which TGG has always claimed to be able to deal with, and which structuralism could not deal

[79] More realistically, perhaps, Chomskyan linguistics solved problems which the old linguistics did not *try* to solve. See page xxiii of Culler's introduction to Saussure (1974 [1916]).

with. These are the infinite set of sentences that constitutes any language, which by definition cannot be 'catalogued' (as Searle characterises structuralist methodology); and the pair of sentences 'John is easy to please/John is eager to please', which demonstrate the limitations of describing 'surface' forms of language when dealing with syntactic phenomena (1972:4-5). However, Murray (1989:159-160) explicitly denies that pre-Chomskyan American linguistics was undergoing any kind of 'crisis'.[80]

We then come to Thorne (1965:74):

> It seems to me indisputable (though I know that there are very many who would dispute it) that a revolution of the kind Kuhn describes has recently taken place in linguistics – dating from the publication of Chomsky's *Syntactic Structures* in 1957. That is to say, for many linguists now the subject matter of linguistics is not what it was before that date and what, of course, for a great number of linguists, it still is. For these linguists the paradigm of linguistics has changed. The student who learns linguistics from *Syntactic Structures* is, in effect, learning a different subject from the student who learns linguistics from, say, Zellig Harris's *Structural Linguistics*. This explains why at the moment so many discussions appear inconclusive, so many misunderstandings fundamental. Kuhn points out that scientists working within different paradigms consistently 'talk through' each other.
>
> Seen through this point of view *Constituent Structure* represents the first post-revolutionary example of a textbook that partly rewrites the history of linguistics. The technique Postal employs is exactly that which Kuhn describes.[81]

Thorne here does not necessarily endorse the Chomskyan approach, nor does he imply that TGG is in any way more scientific for being a new paradigm. However, he does endorse the paradigmatic approach to the history of science.

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[80] Stephen Murray's views on this issue are addressed in detail in part 3.3 of this chapter.
[81] For a tribute to Thorne http://www.benjamins.com/cgi-bin/t_bookview.cgi? bookid= CILT % 2065.

So four of Newmeyer's six references clearly make the link between TGG and Kuhn. Of the other two, Koerner is lukewarm in his endorsement of this interpretation (Koerner 1983:151), and rather scathing about those who take it too far, who he also claims to be mostly 'non-linguists', such as Sklar (1968) – Newmeyer's final alleged culprit – and Yergin (1972), both of whom were writing in non-academic magazines, Dingwall (1971), and Greene (1972), (although his attack on Greene seems unfounded).

2.2.1 Talking about a revolution but not mentioning Kuhn

> The theory of principles and parameters which has been developed over the last two decades is probably the first really novel approach to language of the last two and a half thousand years. It is conceptually so different from previous account of language, either traditional or generative, that for Chomsky this is the first time that linguistic theory might justify the description "revolutionary", more usually accorded to his work of the 1950s. (Smith in Chomsky 2000b:xi)[82]

Newmeyer (1986a:1) defends the 'revolutionary' interpretation of the emergence of TGG because 'the idea that the field ever underwent a "Chomskyan revolution" has been challenged in recent years, and the challenges appear to be on the increase'. Newmeyer's paper has a Kuhnian feel in that it argues that the majority of linguists at the time (1986) were Chomskyans, but that they did not hold 'institutional power'. This is against those such as Murray (1980 and 1994:239) and Antilla (1975) who define the change as a 'palace coup' and a 'coup d'etat' respectively (see Koerner (1994a:1.1) for an expansion on this theme). There is clearly a political point to this. By 1986 generative linguists could hardly be seen as the 'young turks' of the 1950s and early 1960s, who precipitated the paradigm shift. However, by claiming that generative linguists

[82] C.f. Chomsky (2000c:90) 'In fact I think it is fair to say that more has been learned about language in the last 20 years than in the preceding 2000 years'. Note that this is a different 20-year period to the 25 years referred to by Newmeyer above.

represent the majority, and that the work they do is *non*-revolutionary, Newmeyer could represent TGG as 'normal science'. Moreover, by claiming widespread acceptance for TGG *despite* its proponents not holding the reins of power, Newmeyer can attribute to TGG the progressive problem-solving ability which a theory must possess, according to Kuhn, for rational scientists to adopt it as a new paradigm.[83]

However, Newmeyer is careful not endorse TGG as a 'Kuhnian' paradigm, because there is not, and never has been, 'uniformity of belief' across linguistics.[84] Because of this:

> The conclusion seems inescapable: the 'Chomskyan revolution', if there was one, was not a 'Kuhnian revolution' (Newmeyer 1996:29)/

Newmeyer, as we have seen, endorses the revolutionary reading of TGG, but is opposed to framing it in explicitly Kuhnian terms. Many other TGG writers have described TGG as a revolution, or as a paradigm, or in other palpably Kuhnian terms, without actually mentioning Kuhn, and this forms the subject of this sub-section.

It should be clear from the sources I have used so far that, when writers on the recent history of linguistics refer to the 'Chomskyan revolution', they are not necessarily using the word 'revolution' in the strictest Kuhnian sense. First, Kuhn's arguments are open to interpretation, and *The Structure of Scientific Revolutions* has given rise to many interpretations, so there is no definitive measure of how TGG should be assessed for

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[83] See Murray (1994:246) for a robust perspective on this. 'Chomsky (1982:42-43) told interviewers "As I look back over my own relation to the field, at every point it has been completely isolated, or almost completely isolated..." I find it hard not to consider this delusional.'

[84] Kuhn does not, of course, insist that a mature paradigm have no opposition, but rather that this is largely the case: 'before [the transition from the pre- to the post- paradigm period] occurs, a number of schools compete for the domination of a given field. Afterward, in the wake of some notable scientific achievement, the number of schools is greatly reduced, ordinarily to one' (1969 (1962):178).

its revolutionary nature. Second, many of the writers quoted above do not explicitly mention Kuhn. Although it will be readily understood by many people that a reference to an intellectual or scientific 'revolution' is to be interpreted in Kuhnian terms, the word 'revolution' was neither invented by Kuhn, nor copyrighted to his theory. It is possible to refer to the emergence of TGG as a 'revolution' merely in layman's terms, with the general sense of political and/or intellectual turmoil suggested by that word, without involving the specifics of Kuhn's theory. Similarly, Kuhn popularised, but did not invent, the term 'paradigm', so again any use of this term which is not explicitly within a Kuhnian framework does not have to be interpreted in a strict or precise way. One example of this is *Modern Linguistics: The Results of Chomsky's Revolution* by Neil Smith and Deirdre Wilson (1979), which does not even have Kuhn in the bibliography, let alone refer to his concept of 'revolution' (as noted by Koerner 1994b:13). Similarly the third (1991) edition of *Chomsky* by John Lyons has a whole chapter called 'The Chomskyan Revolution: A Progress Report'. This is an update on the original 1977 edition, which had, in part, assessed the revolutionary nature of Chomsky's work. Lyons sometimes uses the phrase 'Chomskyan revolution' in inverted commas, sometimes not, as if to hedge his bets over the rhetorical import of the term. He begins:

> I have made it clear that I share the common view that Chomsky's early work did indeed have a revolutionary impact upon both the theory and the practice of linguistics. (1991:156)

After almost apologising for being partly responsible for introducing the phrase 'Chomskyan Revolution' in the 1977 edition, and giving extensive caveats about the term, he concludes:

> None of [these caveats] should be interpreted as implying anything other than admiration for Chomsky's astonishing achievement and gratitude for what 'the Chomskyan Revolution' has taught us about language and perhaps about the human mind'. (ibid:209)

Like Smith and Wilson, Lyons does not refer to Kuhn at any time during his discussion about 'the Chomskyan Revolution'.

These writers do not deny that their use of the word 'revolution' has a Kuhnian sense, but the fact that they use it without referring to Kuhn at all could be construed as a little devious. The word 'revolution' in the context of an academic discipline strongly hints at a Kuhnian revolution, even if this is not explicitly stated. Certainly when the writer is sympathetic to the discipline in question they are most likely to be referring to a desirable type of revolution (a Kuhnian one, perhaps) rather than a less desirable type (Pol Pot's in Cambodia, for example).

Koerner (1994a1.1.3) gives one more possibility:

> During the late 1960s and early 1970s, many enthusiasts of TGG spoke of a revolution in linguistics (cf. in addition to those mentioned at the outset of section 1.0 above: Dingwall 1971:759; Greene 1972:189; Yergin 1972). It is interesting to note that more recent publications that maintain the same argument (e.g., Smith & Wilson 1979:10; Newmeyer 1980:20) no longer make an explicit reference to Kuhn's (1962) book on scientific revolutions, perhaps because the ideas therein appear to them as a *chose acquise* that need no longer be demonstrated.

So there are two possible reasons for using the word 'revolution' without mentioning Kuhn. First, to imply a Kuhnian revolution without having to back this up with details from Kuhn's theory; and second, because it is so plainly obvious, and universally agreed upon, that a Kuhnian revolution took place.

Finally, Kuhn is not the sole arbiter in these matters. Not everyone accepts his theory and not everyone refers to it. There is a certain amount of consensus that a 'Chomskyan revolution' (of some description) took place.

Whether or not it was a Kuhnian revolution, however, is a different question.

One significant part of Kuhn's model says that a new paradigm is demonstrably better at solving problems than the old one (1062:77-91). It is part of the myth of Chomsky that he demolished, destroyed or laid waste to (never 'refuted' or 'repudiated') the behaviourist theories which were prevalent up to the 1950s, and Smith's description of the impact of the *Review of Skinner* fulfils this:

> His [Chomsky's] review of Skinner's major book, *Verbal Behaviour* (1957), perhaps the most devastating review ever written, not only sounded the death-knell for behaviourism, but also laid the foundation for current mentalist linguistics and cognitive science more generally. (Smith 1999:97)

Here the sentiment is tangibly Kuhnian, with the young turks blowing away the old paradigm with a better, more effective way of approaching the subject.

To conclude, although none of the writers cited in this section make the absolutely explicit claim that being Kuhnian entails being scientific, it is nevertheless reasonable to infer that, in many of the above passages, this claim is being made. Some invoke Kuhn without the concomitant claim that this confers scientificity; others invoke scientific revolutions without invoking Kuhn. Either way, it is certainly enough, as we shall see in the following section, to provoke a reaction.

2.3 Claiming that another sub-discipline has misused Kuhn for their own self-serving purposes

While there are only a few examples here of generative linguists presenting TGG as a Kuhnian paradigm, there are many examples of writers claiming that this has been the case (as we saw with Newmeyer in the previous section, who finds this practice 'rather puzzling'). For example, Winston (1976:25) describes the situation like this:

> It has become commonplace to claim that a scientific revolution has taken place in linguistics as a result of Noam Chomsky's contributions to the theories of syntax, linguistic metatheory and the philosophy of mind [...]. The cliché has also been encouraged by the appearance of Thomas Kuhn's *The Structure of Scientific Revolutions.*

Sampson (1980:158-9) is categorical on this issue:

> [M]any linguists of the Chomskyan school have enthusiastically embraced Thomas Kuhn's doctrine of the history of science as a series of 'Gestalt switches' [...] in each of which no reasoned grounds can be assigned for the adoption of the new intellectual 'paradigm'.

Matthews (1993:28), showing little inclination towards Chomsky or Kuhn, says:

> If I were still in a Sellar and Yeatman mood I would unhesitatingly describe this [Kuhn 1962] as the Worst Thing that has happened to the historiography of twentieth century linguistics; not, of course, because of what Kuhn said [...]; nor because I do not believe that the mainstream of American linguistics changed course at this time; but because it led so many of Chomsky's supporters to make events fit Kuhn's model.

Hymes (1974:9) makes similar comments, and we saw above Newmeyer making a similar complaint (and also see Hymes Labov 1975:128 in section 2.5 below). By portraying this mass of Chomskyans erroneously trying to squeeze TGG into the Kuhnian model, Matthews, Sampson, Winston and Hymes can imply that a partial version of history is being written (and Newmeyer, from a generativist perspective, can look exquisitely scrupulous). The implication is that the Chomskyans are trying to turn their numerical supremacy, which one could call dominance of the field, into a Kuhnian paradigm. If it is repeated often enough that TGG represents a true Kuhnian paradigm, then this will become accepted fact; and by showing that their linguistics fits with Kuhn's account, they are implying that all other linguistics is either irrelevant or outside the scientific mainstream. The supposed proliferation of these claims becomes more sinister than an incorrect reading of history. It becomes a plot to rewrite history. And if this attempt to rewrite history were successful (the conspiracy theory would have it), then TGG could present itself not just as the best variety of linguistics in the late twentieth century, but as the only type of linguistics worth studying, with all the others relegated to some sort of non-scientific dustbin.

Sampson is particularly vitriolic, but unfortunately lacking in evidence. The example he gives of the 'many linguists of the Chomskyan school' who 'have enthusiastically embraced Thomas Kuhn's doctrine' is Percival (1976:292), who explicitly denies that paradigms are useful for describing the 'Chomskyan revolution'.[85] However, as we saw above, both Newmeyer (1996:179n) and Koerner (1983:151) provide lists of Chomskyans who have made approving reference to Kuhn and/or paradigms in connection with the development of TGG, and so there is more to the claim that 'many linguists of the Chomskyan school have enthusiastically embraced Thomas Kuhn's doctrine' than mere paranoia from people who disagree with Chomsky. Finally, it is worth noting that there are approximately as many writers in these lists as there are in the list of those who disapprove of the appropriation of Kuhn.

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[85] Percival, moreover, was never really a 'Chomskyan' in any meaningful sense, despite working alongside him on the Machine Translation Project at MIT. He has concentrated more on Renaissance grammar. See http://people.ku.edu/~percival/Resume.html.

2.4 Attempting to demonstrate that the other sub-discipline does not follow the Kuhnian model

It is not enough, however, merely to claim that Kuhn is being misused by generative linguists for their own purposes. The critic of the view that TGG is a Kuhnian paradigm must show why this is not the case. Hymes[86] (1974:16) presents compelling evidence on this front:

> Kuhn takes for granted that a new paradigm, a new outlook, is not just different from a preceding one, but successful because superior; in particular, the new paradigm explains things that the old could not, but it continues to be able to explain what the old one could as well. Within linguistics, the successive 'paradigms', or cynosures, have not fully had both properties, which account [sic], of course, for much of their failure to command complete authority within the field.

Hymes here takes the view that Kuhn's paradigms are not merely irrational gestalt switches, but that moving from one to the next is rational because of the increased problem-solving ability of the new paradigm. While this may be true, for example, of the shift in physics from Newtonian mechanics to the theory of relativity, it is not true of linguistics, according to Hymes. Successive schools of linguistics have had different foci, and have tried to answer different questions. TGG cannot, and does not try to, establish the relationship of the languages in the Indo-European family, or the significance of the presence or absence of rhoticity in New Yorkers' speech. Most linguists working today do so side by side with linguists from other areas (generative grammarians, sociolinguists, historical linguists, dialectologists etc.) with only occasional overt conflict. No one would suggest that researchers in proto-Indo-European should instead be studying pragmatics, say. For this reason Hymes advances the notion of a 'cynosure', or a guiding light, which is a focus for a collection of individu-

[86] See section 2.5 of this chapter for specific criticisms of Chomsky from Dell Hymes.

als working on a particular aspect of language. In other words, a cynosure rather than a paradigm allows for the possibility of coexistent and non-contradictory sub-disciplines, which is exactly the case in the field of linguistics.

Koerner (1994a:1.1.3) gives a different argument for TGG not being a Kuhnian paradigm:

> In short --- and as will become still clearer from what follows --- it seems that, upon closer inspection, the term 'revolution' does not properly apply to TGG. Despite many disclaimers, TGG is basically postSaussurean structuralism.

Where Hymes concentrates on the contemporaneous existence of different schools of linguistics, unable to put each other out of business, Koerner looks at the historical succession. Successive paradigms should be incommensurable, that is, they should present theories or facts in completely different ways, which prevent communication across paradigms. Koerner, however, sees the Chomskyan 'revolution' as a development of structuralism, not incommensurable with the preceding post-Bloomfieldian theories, but a natural progression from them. If this is the case, then TGG does not represent a paradigm itself, but is a phase within a larger paradigm, presumably starting with Saussure, continuing through Bloomfield, and still continuing today. Arguments in favour of this viewpoint would highlight such things as Saussure's division of language into *langue* and *parole* (1974 [1916]:9-13), which closely mirrors Chomsky's division of language into competence and performance (1965:3-14). Joseph (1999) not only notes these similarities, but goes one further by arguing that Chomsky was actually *more* structuralist than Bloomfield and Sapir. This was because Saussure was quite happy to talk about mental objects, whereas Bloomfield's approach regarded 'anything "mentalistic" as being inherently metaphysical, and therefore not amenable to scientific study' (1999:24). For this reason, Chomsky's 'revolution lay partly in convincing American linguists that the behaviourist rejection of the mind was misguided' (ibid:25).

Another viewpoint, in some ways more damaging for the claim that TGG forms a Kuhnian paradigm, holds that TGG forms a development and continuation of the Saussurean structuralist paradigm. It is easy to see how this conclusion is arrived at. Saussure's *Cours de Linguistique Générale* (1916) reads like self-conscious paradigm formation *avant la lettre,* and Saussure defines both the object of study and the best way to study it. For example, in the chapter titled 'Subject matter and scope of linguistics: its relations with other sciences', he asserts that

> The scope of linguistics should be:
> a) to describe and trace the history of all observable languages [...]
> b) to determine the forces that are permanently and universally at work in all languages, and to deduce the general laws to which all specific historical phenomena can be reduced; and
> c) to delimit and define itself. (Saussure 1974 [1916]:6)

This is as clear an example of 'paradigm formation' as is possible. Saussure is telling linguists that linguistics ought to be studied in a particular way, just as Chomsky does on the first page of *Syntactic Structures*:

> Syntactic investigation of a given language has as its goal the construction of a grammar that can be viewed as a device of some sort for producing the sentences of the language under analysis. Linguists have been concerned with the problem of determining the fundamental underlying properties of successful grammars. The ultimate outcome of these investigations should be a theory of linguistic structure in which the descriptive devices utilized in particular grammars are presented and studied abstractly, with no specific reference to particular languages. (1957:11)

In these two extracts from both the *Cours* and *Syntactic Structures*, Saussure and Chomsky say how linguistics *should* be studied. Moreover, Chomsky's suggestion that linguistics should present and study 'descriptive devices [...] abstractly' calls to mind Saussure's remarks on the object of linguistic enquiry – 'the general laws to which all specific historical phenomena can be reduced'.

The main difference between Saussure and Chomsky, or perhaps more accurately, Chomsky's major advance on Saussurean linguistics, is the postulation of transformations, as Culler notes in his introduction to the *Cours*:

> The notion of rule-governed creativity – of individual creativity that is made possible by a system of grammatical rules – is what he [Saussure] lacked, and it was left to Chomsky to show how the linguistic system could account for sentence formation without denying the freedom of individual speakers. (Culler (1974:xxiii)

Culler is clearly suggesting that Chomsky's advances rested heavily on Saussure's work. Whether transformations constitute a new mode of study within Saussurean structuralism or an entirely new linguistic paradigm is debatable, but on a neutral reading of Kuhn it is hard to see how the case for a new paradigm could be made; it would certainly be difficult to argue that the Saussurean and Chomskyan paradigms are incommensurable, as a true Kuhnian reading of the situation would have it.

In some histories written from within TGG (e.g. Newmeyer 1980:20-1, Smith 1999:9) there is the implication that TGG represents the first paradigm. Although this is neat from a Kuhnian point of view, it is a dangerous strategy as it implies that all language study pre-Chomsky (including Saussure, as discussed above) was unscientific, an implication which can sound both ignorant and arrogant unless carefully worded. Saussure certainly saw his own approach to language as scientific – the opening sentence of the *Cours* asserts that:

> The science that has been developed around the facts of language passed through three stages before finding its true and unique object. (1974 [1916]:1)[87]

Interestingly, in this sentence Saussure not only asserts the scientificity of the study of language, but the existence of previous paradigms which, although not to be mocked, were nevertheless not quite on the right track. So from Saussure's point of view, he himself had found the right and proper way to study language scientifically, and the study of language had passed through various stages beforehand. This would seem to rubbish any Chomskyan claims that TGG forms the first paradigm. It is fairly clear, then, that the relationship between Saussure's structuralism and TGG provides serious problems for the claim that TGG was the first linguistic paradigm.

If neither TGG nor Saussure form the first linguistic paradigm, then there are other candidates for the first scientific paradigm in linguistics, but none is convincing,. There is no obvious 'Eureka!' moment or Newtonian figure. Many textbooks date 'modern' linguistics to 1785 and William Jones' speech to the Royal Asiatick Society[88], but this speech did not give principles and methods for the scientific study of language, so much as note the interesting similarities between Latin, Greek and Sanskrit. It does not hold the same place in the history of linguistics as, say, Newton's *Principia Mathematica* does in the history of physics. *Studies in the History of Linguistics* (1974), edited by Dell Hymes, has a section entitled 'First Paradigm (?): Comparison and Explanation of Change' [Hymes' punctuation]. This consists of seven papers, none of which cite TGG as the first

[87] These three stages are grammar, philology and comparative philology (ibid:1-5).
[88] E.g. Harris (1993:15), Robins (1967:134), but see Aarsleff (1982:314-5) for a dissection of this claim.

paradigm, which further complicates the line of argument that TGG may have been the first paradigm.

Koerner notes, though, that there is a difference between a theoretical revolution, which is perhaps the 'purer' revolution in the Kuhnian sense, and a 'sociological' revolution. He notes that:

> it cannot be denied that many young men and women in linguistics during the 1960s and 1970s *believed* that they were witnessing a revolution in the field, and it appears that this widespread belief (and the associated enthusiasm that young people tend to generate) has been at the bottom of the 'Chomskyan revolution'. (1994a:1.1.3)

Perhaps slightly sarcastically, he observes that if people *believe* that a revolution has taken place, then as far as the practitioners of that field are concerned, and for many of the writers of history, a revolution *has* taken place. In part 2.3 above, I noted the slightly paranoid tone of non-Chomskyans complaining of the proliferation of accounts of the revolutionary nature of TGG. Koerner here adopts a different approach: generative linguists are not deliberately rewriting history, but accidentally allowing a mistaken impression to take hold. They are caught up in hysteria rather than plotting to write their predecessors out of the history books, and for the young turks of the 1960s, it is more rewarding to believe that they took part in a revolution than to believe that they merely carried on with an established tradition.

Murray, on the other hand, does accuse Chomskyans of re-writing history to serve their own myth-making purposes – and he goes as far as to accuse Newmeyer (1986c) of writing 'Stalinist history [...] wildly biased and unsuitable as a textbook either for history or linguistics' (1989:156). One of these myths is that Chomsky fought a one-man struggle against the establishment forces at the beginning of his career. Murray takes the example of Chomsky's alleged inability to find a publisher for his first book *The*

Logical Structure of Linguistic Theory. According to Murray, however, it was Chomsky who pulled out of an agreement to publish it (1999:350). He concludes:

> For nomothetic theory as well as for ideographic [sic] history,[89] it bears stressing that the Kuhnian expectations fostered by Chomsky, Lees, and their followers is rejection by the *ancien régime* pushing people to become revolutionaries. The evidence [...] is not merely lacking, but the evidence [is] in the *opposite* direction, i.e., rather than rejection there was encouragement – and even solicitation from those controlling the means of linguistic publication. (1999:351, and see also Murray 1994:230-234)

This is endorsed by Joseph, who suggests that:

> [Chomsky's] own accounts of his relation to the neo-Bloomfieldians read like classic hero myths, key elements of which include the hero's being self-generated and overcoming obstacles placed in his path. It would take nothing away from Chomsky's greatness if he tried coming to grips with Murray's account, not as an attack, but as a potential source of insight into the workings of his own mind. But I wouldn't hold my breath. (1995:388-9, and see also 1999:26)

Newmeyer (1986b:21) accepts Koerner's assertion that TGG is structuralist, but insists that it was nevertheless, revolutionary:

> Chomsky's structuralism, however, no more disqualifies his theory from being revolutionary than does Einstein's Newton-like search for physical laws undermine the revolutionary nature of relativity theory. Saussure's victory was the victory of structuralism, just as Newton's victory was the victory of a lawful universe. We would no more expect the next revolution in linguistics to be an antistructuralist one than we would expect the next revolution in physics to return to divine intervention as an explanatory device.

[89] Nomothetic: 'Relating to or concerned with the study or discovery of the general laws underlying something'. Idiographic: 'Concerned with the individual, pertaining to or descriptive of single and unique facts and processes' (OED).

Newmeyer's difference with Koerner rests on whether 'structuralism' is a cover-all term for (as Newmeyer puts it) 'Saussure's great insight that at the heart of language lies a structured interrelationship of elements characterizable as an autonomous system' (ibid)[90]. Put uncontentiously like this, there is little reason to doubt that a revolution could occur within structuralism, as Newmeyer says it did. However, 'structuralism' also applies more narrowly to the Saussure/Bloomfield/Sapir approach, which was certainly displaced by TGG, not least in epistemological terms (see chapter four), so the question comes down to whether or not it is more useful to focus on the similarities in approach outlined by Koerner above, or the differences as emphasised by Newmeyer. From this perspective, the difference is merely one of nomenclature, that is, there was certainly a large change within linguistic study, whose nature both sides more or less agree on, so one can choose to call it a revolution, or a change of focus, or a banana. Of course, the argument is not over whether the phrase 'change of focus' or the word 'banana' are appropriate in this historical context, and the reason is Thomas Kuhn. The word 'revolution' always had emotive power, because of its political connotations, but Kuhn gave it a specific rhetorical force.

To sum up, there are, then, different ways of approaching this matter. There are different aspects of Kuhn's theory which can be shown not to apply to TGG, and there are different motivations which can be ascribed to Chomskyans who claim to have been part of a revolution.

Kuhn's model, as Koerner noted (above), has both theoretical and sociological aspects, and it is in the sociological realm that linguistics seems furthest from the Kuhnian model. We have already seen that, although it is said that TGG represents a revolution, we do not find near-unanimous adherence to this model which we would expect in a true Kuhnian para-

[90] However, see chapter four for a discussion of Yngve's approach to linguistics, which explicitly denies many of Saussure's more fundamental claims.

digm (as discussed by Newmeyer above). Another sociological difference also concerns adherence to the Kuhnian norm. According to Kuhn, scientists have little interest in challenging the status quo while that status quo remains tenable. In linguistics, on the other hand, it is almost *de rigueur* to try to point out why prevailing models are incorrect, and to propose one's own model, and many well-known linguists have their own model of 'how language works'.[91]

2.5 Attempting to demonstrate that Kuhn's model is wrong, and therefore that it has no bearing on the scientificity of a linguistic (sub-) discipline

Arguments against TGG being a true Kuhnian paradigm focus not only on the inapplicability of Kuhn's model to linguistics, but also on the deficiencies of Kuhn's model itself. For example, Labov emphatically rejects Kuhn:

> It is suggested that we have two incommensurable 'paradigms'. This is a fashionable view, and the construction of such paradigms is a favourite occupation of those who would prefer to discuss the limits of knowledge rather than add to it. (Labov 1975:128, quoted in Figueroa 1994:74)

It is arguable that it is symptomatic of the nature of linguistic study that it is not possible to use Kuhn in this context. To do so one would have to do the following: prove that one's own subject is scientific; prove that other linguistic fields are neither scientific nor linguistic; and, prove that one's own discipline, uniquely in the field of linguistic study, is the only one which fits Kuhn's account. Apart from being extremely difficult to do, this would also involve the assertion that all other types of linguistic study are no better than astrology, a claim which is both offensive and arrogant.

[91] Apart from Chomsky's and Hymes' (*passim*), two others are Halliday's systemic functional grammar (Halliday and Matthiessen 2004) and Tomasello's usage-based theory of language acquisition (Tomasello 2004).

While sociolinguists and generativists may each suspect that the other is not doing science, it is very hard to prove this is the case, and it is virtually impossible to 'prove' that they are not doing linguistics (see 2.1 above).

Figueroa (1994:8) belongs to those who argue against Kuhn's model of the historical development of science, rather than using the model as ammunition against the validity of another conception of linguistics:

> Kuhn's version of history is too categorical and lacking in attention to developmental processes [...]. Kuhn's scheme does not adequately account for the history of linguistics where one finds throughout its history, co-existence of competing paradigms. (Of course in Kuhn's defense one could simply claim that linguistics is and always has been an 'immature' science, hence the existence of various approaches at any given time).

I explained in chapter two that Kuhn's philosophy is heavily schematic (or 'categorical' as Figueroa puts it), so the first part of this argument holds little force for me. In the rest of this quotation, however, Figueroa is actually hedging her bets. Since she says that Kuhn is wrong and Hymes is (more) right, it doesn't matter which parts of linguistics constitute a paradigm. However, even if Kuhn were right, linguistics would not be a science in Kuhnian terms, and so Kuhn would be irrelevant to the history of linguistics. Despite this, she is more vehement in her rejection of Kuhn: 'I do not subscribe to Kuhn's notion of how a change from one scientific paradigm to another takes place' (ibid).

Figueroa is much more in agreement with Dell Hymes than with the Chomskyans mentioned above or even Labov over the fit between TGG and Kuhn's model, and this view denies to TGG *ab initio* the paradigmatic status which can seem so keen to claim. In stating that she does not agree with Kuhn's assessment, Figueroa denies that same status to other types of linguistics, such as sociolinguistics (although she does attribute some aspects of paradigmaticity, especially incommensurability, to both TGG and sociolinguistics (1994:27-8).

Dell Hymes is one of the leading dissenting (i.e. non-Chomskyan) voices in the field. With his brand of sociolinguistics, the ethnography of communication/linguistic anthropology, he too aspires to a scientific study of language (1974:1, 8-9)[92], and so has his own motivation for dispelling the idea of TGG as *the* linguistic paradigm. If non-TGG linguistics is to defend its scientificity, it must either present TGG as incorrect (and therefore moribund); or it must explain the co-existence of two or more scientific linguistic approaches, which entails either explaining why Kuhn's ideas do not apply to the science of language, or why Kuhn is wrong. All three of these are found in the literature; however, Hymes' own view is closer to the supposed inapplicability of Kuhn's model to linguistics, rather than its inapplicability to *any* form of study.

Conclusion to chapter three

This chapter has looked at two closely related issues. First, whether or not any form of linguistics can be accurately described as scientific; and second, whether any form of it can be accurately described as a Kuhnian paradigm. With respect to the first question, there is far more evidence of linguists claiming that their form of linguistics is a true science, comparable to physics or chemistry. Regarding the second question, there is a fair amount of evidence that Chomskyans have tried to argue that TGG is a Kuhnian paradigm, and plenty arguing that TGG was *in some way* revolutionary. What is less clear, and what I believe ought to be inferred, is the question of why such claims would be made. I think it is a fair assump-

[92] It might be objected here that ethnography does not usually present itself as a natural science, but is as social a social science as it is possible to be. However, Hymes is unequivocal in his commitment to science in these passages.

tion that any claim for paradigmaticity implies a claim for scientificity, although this is not necessarily the case, nor is it central to my argument.

By looking at these arguments as they have been conducted by linguists, we can see two things. First, that their interpretations of what 'science' and 'paradigm' ought to mean are significantly different. Second, that they do not agree on the best way to study language. These disagreements, concerning what 'linguistics' is (or should be) and its sociological and theoretical history, betray a deep-seated disagreement about the nature of the object of study and its consequent practice.

In the next section I look at the competing epistemological standpoints which underpin different forms of linguistics, and examine the effect of those standpoints on the ontological commitments which they entail.

Chapter four: claiming Rationalist and Empiricist forebears

The previous chapter looked at arguments concerning whether or not linguistics can be scientific, whether or not any form of linguistics can accurately be described as a Kuhnian paradigm, and what counts as linguistics anyway. In order to elucidate how these arguments come about, I will now look at a different set of disagreements between linguists, over the epistemological commitments of their respective forms of linguistics.

Chomsky has made much of the relationship between Descartes' Rationalism and TGG, and several people have taken issue with this formulation, either by questioning the accuracy of that relationship, or by questioning the validity of a Rationalist epistemology. In this chapter I examine this aspect of the 'Chomskyan Revolution'. First I look at the nature of the Rationalist-Empiricist split, its re-emergence in post-war linguistics, and its implications for science. The second part of this chapter explores the claims made by Chomsky regarding the purportedly 'Cartesian' nature of his subject. These claims have subtly altered (in content and in frequency) over the course of Chomsky's career, and I look at the chronological development of Chomsky's Rationalism. In part three I look at counter-arguments against Chomsky. Some of these enlist Empiricist philosophers in the service of non-Chomskyan linguistics, others look further back in time, but all of them argue that Chomsky's commitment to a Rationalist epistemology in some way compromises his linguistics.

In the previous chapter I examined the history of linguists appropriating Kuhn's philosophy for their own purposes, and such considerations come into play again in this chapter. In Kuhn's analysis of the emergence of scientific paradigms, he notes that while they are struggling for acceptance, new paradigms will use philosophy to bolster their claims:

> No natural history can be interpreted in the absence of at least some implicit body of intertwined theoretical and methodological belief that permits selection, evaluation, and criticism. If that body of belief is not already implicit in the collection of facts [...] it must be externally supplied, perhaps by a current metaphysic, by another science, or by personal and historical accident. (Kuhn 1962:17)

Kuhn goes on to describe the exact manner in which such ideas are disseminated:

> In history, philosophy, and the social sciences [...] the elementary college course employs parallel readings in original sources, some of them the "classics" of the field [...] As a result, the student in any one of these disciplines is constantly made aware of the immense variety of problems that the members of his future group have, in the course of time, attempted to solve [...] Contrast this situation with that in at least the contemporary natural sciences. In these fields the student relies mainly on textbooks [...] (ibid:165)

According to these passages, one characteristic of a nascent Kuhnian paradigm is a collection of classics in the field (or 'exemplars') which define that science. In the absence of these, a field must enlist other forms of support to defend itself against claims of non-scientificity. The history of philosophy provides a resource for this, as it deals with such issues as epistemology and methodology which are central to the establishment of any scientific discipline. I noted in the introduction that this is not an issue which Kuhn highlights in his account of the development of the sciences; however, it is germane from the point of view of linguistics because it has given rise to a debate whose literature far outweighs the importance Kuhn accorded the issue.

I will not assume that the participants are acting in a self-consciously 'Kuhnian' way and are trying to fulfil his criteria for a scientific paradigm; whether or not they are self-consciously 'Kuhnian' is less relevant to this chapter than to the last. However, in citing philosophical forebears, certain schools of linguistics follow Kuhn's template; and this does not just provide us with a pleasing symmetry, but allows us to see if Kuhn's explanations of the emergence of paradigms can shed any light on why different types of linguistics might claim different epistemological commitments, and how those commitments might explain the incommensurabilities which form the subject of chapter five.

1.0 The re-emergence of Rationalism after centuries in the wilderness

In *The Linguistics Wars* (1993), Randy Allen Harris gives an account of Chomsky's progress from the 1950s to the 1980s. Although the primary focus is on the rise and fall of Generative Semantics in the 1960s and 1970s, Harris first sets the scene. In describing Chomsky's emergence, and his eclipsing of his structuralist predecessors (who Harris refers to as 'Bloomfieldians'), Harris notes that one fundamental difference in outlook was epistemological: where the Bloomfieldians had been Empiricists, Chomsky was a Rationalist.

Chomsky's radical epistemological proposal was for a linguistics based on innate knowledge rather than learnt behaviour. Harris presents this departure from epistemological orthodoxy as not just deviant but almost perverted, according to the standards of the time:

> [...] Whitehead had defined the general disregard for rationalism by saying "we no more retain the physics of the 17th century than we do the Cartesian philosophy of [that] century" (1929:14). It was passé philosophy. Its perennial

> opponent in the epistemic sweepstakes was, largely due to the work stemming out of the Vienna Circle, on top. Empiricism was *au courant.* (1993:66)

Where Empiricism seemed to have evolved into something like an epistemological prerequisite for any science of human behaviour and brain function, Rationalism retained an air of Neo-Platonism[93] which could clearly be seen in Descartes' work, and subjects such as souls which played a large part in Descartes' thinking were certainly not fit for twentieth-century scientific enquiry. Harris makes much of the sense of epistemological outrage being felt:

> Trager, keying on the mysticism most Bloomfieldians equated with rationalism, condemned Chomsky as 'the leader of the cult [that has] interfered with and interrupted the growth of linguistics as one of the anthropological sciences for over a decade, with evil side-effects on several other fields of anthropology' (1968:78). The sky was falling. The sky was falling. (1993:68)

The work which in which Noam Chomsky made his Rationalist heritage most explicit was *Cartesian Linguistics* (1966a). In this he expounded at length his views on the supposed underpinning of TGG by ideas which date back to the seventeenth and eighteenth centuries, and in particular the Rationalist-Empiricist epistemological debate. Chomsky argued that TGG, then only about a decade old, received epistemological backing in the work of various key thinkers throughout history, beginning with Descartes, all of whom had shared a common nativist outlook on human knowledge, but who had been largely overlooked in the study of language in the preceding couple of centuries.

What was unpalatable about his appropriation of Descartes was that it suggested that the philosophy of science which had established itself in European thought was itself untenable when it came to studying some-

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[93] Alexandre Koyre's introduction to *Descartes: Philosophical Writings* (1950, eds. Anscombe and Geach) makes this Neo-Platonist heritage explicit.

thing as apparently 'empirical' as language. Language is not like other aspects of human behaviour. It is very much rule-governed, regular to a large extent, and predictable in non-trivial ways. The study of language should therefore, according to standard Empiricist reasoning, fit in with other natural sciences, rather than be exposed to the vagaries of continental philosophy. By mixing and matching like this, Chomsky was upsetting an epistemological apple cart. It should not have been possible to use a Rationalist epistemology as the basis for an empirical science, but that was what he was, apparently, doing.

Going back a decade, Chomsky's *Syntactic Structures* (1957) is generally seen as marking the start of a new type of linguistics. Opinion is divided as to whether practitioners of the old style of linguistics (referred to variously as Bloomfieldian, neo-Bloomfieldian, structural or taxonomic), had any idea that the end was nigh. Newmeyer (1980:1) says 'If American linguistics was in a state of crisis in the mid 1950s, few of its practitioners seemed aware of it'. Murray (1994:237) concurs with Newmeyer: 'Reading the linguistic literature of the mid-1950s, one does not find evidence of a sense of crisis'. Murray here is using the term 'crisis' in an explicitly Kuhnian way. Having quoted Kuhn saying 'Paradigm-testing occurs only after persistent failure to solve a noteworthy puzzle has given rise to crisis [1962:144]', Murray argues that 'there is no evidence that *Syntactic Structures* discussed (let alone solved) puzzles that the previous generation of linguists had been trying unsuccessfully to solve' (ibid).

In contrast, Harris (1993:36), describes an exchange in 1962 between Trager and Sledd over Trager and Smith's *Outline of English Structure*, which Harris describes as 'a self-conscious exemplar of the [Bloomfieldian] program'. Sledd argued that Trager and Smith's system needed 'overthrowing', an aim which Harris characterises as 'for all the world like a symptom of the historical stage in the growth of a science that Thomas Kuhn calls a *crisis*'. If Harris is right, then from a Kuhnian point of view 'linguistics, as an abstract and collective entity, *was* looking for a savior' (Harris, ibid). If Murray and Newmeyer are right, then linguistics (and particularly American linguistics) seemed to think that it was enjoying rude health.

One way of arguing for the latter can be seen in the state of Bloomfieldian linguistics' philosophical foundations, and with reference to Kuhn's argument that a lack of concern with the philosophical bases of one's discipline indicates nothing so much as confidence in it (1962:35-40). Empiricism was indeed '*au courant*' in the mid 1950s, in the sense that most linguists would have regarded their methodologies as Behaviourist and/or Positivist, which marked out twentieth-century scientific Empiricism from the prototypical (but still fundamentally correct) Lockean seventeenth-century version. This entailed a commitment to science and a rejection of metaphysical investigation (especially into 'meaning' – not by Bloomfield himself, but by his followers. See Murray (1994:130-2) and Harris (1993:25-28)). Bloomfield's *Language* (1933), the bible of structuralist linguists, engages confidently and without "metaworries" (Lass (1980:ix), quoted in Figueroa (1994:17)) with the application of 'scientific method' to linguistic data, and, when philosophers were consulted, it was always modern philosophers such as Quine, who was helping to develop the Empiricist/ Positivist view of science, never relics such as Descartes, Locke or Leibniz (see Harris (1993:24-6) for a fuller discussion of this).

The work of Charles Hockett[94] provides a good example. A brief survey of five of his books in Sheffield University library (all, tellingly, relegated to the basement) published between 1958 and 1987 shows one reference to a pre-twentieth-century philosopher, Hegel, and this is only tangential to the main argument. Bloomfield's *Language* does run through the history of

[94] Harris describes Hockett (1916-2000) as 'the Bloomfieldian boy-wonder' (1993:43) and 'the Bloomfieldian-most-likely-to, the late master's favored son' (1993:53).

linguistic thought, but has little to say on the philosophical history of its methodological principles.

Although, as I showed in chapter three, it is fairly commonplace to describe Chomsky's effect on linguistics as 'revolutionary', this clichéd standard account does not always delve into the question of what *kind* of revolution it was. Was it a revolution in the methodological aspect of linguistics, or was it deeper, a re-examining of the philosophical basis of the approach to language?

> Rationalism and empiricism [...] illustrate just how deep the Bloomfieldian-Chomskyan division rapidly became. What looked to most of the old guard like a new way to do syntax mushroomed in less than a decade into a new way to do linguistics, a new way to look at human beings, and a new way of doing science; new, and completely inverse. They were baffled and enraged. (Harris 1993:67)

It was not obvious in 1957 that Chomsky meant to do away with Empiricism, and substitute Rationalism for it. Transformations, interesting as they were, did not present a *prima facie* threat to an Empiricist framework (Murray 1994:239), and Murray claims that Chomsky was significantly aided by many of the older, Bloomfieldian, generation (1994:230-4). The Chomskyan program *did* end up fairly quickly rejecting the bases of the old Empiricist philosophy, though this was not inevitable. Did Transformational Generative Grammar *entail* Rationalism, as Chomsky would have it, or was it a matter of faith on Chomsky's part (as Sampson claims) which led him from philosophical Rationalism to linguistic nativism? We can discount the possibility that Rationalism entails TGG specifically, if only because such a thing has never been suggested even by the wilder participants in the debate (although Chomsky's Rationalist faith[95] could conceivably have led him, via a nativist view of linguistic and cognitive

[95] 'Whether one is an Empiricist or [a Rationalist] must be a matter of faith. (1976:963-4)' Sampson. See section 3.5 for further discussion of the role of faith in epistemology.

ability, to the internal structure of the human language faculty). Instead we will look at the arguments that seem to have led Chomsky from linguistic nativism back to Descartes.

I think that this is the correct way round, as Chomsky does not seem to have started from first philosophical principles, unlike his inspiration, but from primary linguistic facts, such as child language acquisition, infinite language from finite means, etc. Most of all he was trying to provide a formal account of our pan-species linguistic knowledge. In the terse introduction to *Syntactic Structures*, his stated goal is 'a theory of linguistic structure in which the descriptive devices utilized in particular grammars are presented and studied abstractly, with no specific reference to particular languages' (1957:11). This relatively modest mission statement contains no reference to any school of philosophy, or any intention of overturning the prevailing epistemology of the time.

This 'linguistic structure', of course, had a non-specific ontology, in that Chomsky claimed no physical basis for such knowledge, other than the non-specific 'language faculty', attributing it instead to 'mind', or what Descartes had once referred to as '*res cogitans*'. Chomsky argues that studying the mind should not be problematic, in that we can study the structure of our thoughts without understanding their neural (or otherwise) origins.

> [T]here has been no coherent formulation of metaphysical dualism or the mind/body problem, in my opinion. Suppose that we investigate some of the functions of the brain (call them "mental functions") in isolation from the brain structures themselves. This can be a perfectly legitimate and reasonable procedure, but we should be careful not to draw unwarranted conclusions from it. The procedure is not restricted to "mental functions"; other properties of the world can be studied in a similar way, and regularly are. Thus, one can study the solar system as a system of point masses, within "rational mechanics," basically a branch of mathematics. And one can study chemical proper-

> ties in isolation from properties of particles in motion; in fact, that is pretty much the way chemistry *was* studied until the quantum theoretic revolution made it possible to unify chemistry with a radically different kind of physics. Chemistry achieved its "triumphs . . . in isolation from the newly emerging science of physics," a leading historian of the subject points out (Arnold Thackray). The same was true of genetics prior to the discovery of the mechanisms involved, and there are many other examples. I do not think that the mental aspects of the world are different in this respect from others. (from Cela-Conde and Marty 1998:20)

By comparing the mind-body problem to any other abstraction in any scientific discipline, Chomsky is essentially saying that the 'mental' nature of linguistic data is as unproblematic as the abstract subject matter of mathematics. Earlier in this interview Chomsky rejects Descartes' fundamental division of matter into mental and physical, while retaining other of his 'Cartesian' ideas. That some phenomena are 'termed mental' is a useful shorthand for the idealisation which has been made, but irrelevant in the wider philosophical context.

For me this point is crucial. Harris's quotation about the sky falling suggests that Chomsky's claims about innateness should be seen as shocking, where innateness refers to the physical species, the genetic endowment of the embodied individual. However, a great deal of what he has written is solely about mental structure – his claims about innateness are about things innate to the mind, not to the body, and this presents the link to Descartes which some found so unnecessary.

The behaviourist is, as Quine pointed out, 'knowingly and cheerfully up to his neck in innate mechanisms of learning-readiness' (1976:56-58). It was not the idea that humans were genetically endowed to learn a language that came as a philosophical surprise, but the idea that our minds are limited in such a way that we all learn languages which share essential similarities.

Quite what these similarities might be is a matter of debate, and all of the following have been debated.[96] Options range from mechanical processes such as transformations, raising, passivisation, re-write rules etc.; to traditional interpretable features such as nouns, verbs, pronouns; to posited functions such as θ-roles (Chomsky 1981); to lexical terms such as body parts (Brown (1976); to tendencies (sequencing of colour terms (Berlin and Kay 1969).[97] If Chomsky is right, then some of these features (or some other features) characterise all human languages, and any human language is constrained by them. Quine (above) and others of the same view would argue that, naturally, humans are limited in the way in which they learn things, and in what they can learn; but that this does not entail a specific linguistic initial state and set of universals which is identical across all human languages.

Having made his claim about innateness, Chomsky then linked it back to the disagreements between Locke and Descartes. He appropriated Descartes' conception of *res cogitans* and set it in opposition to Empiricist/Behaviourist theories of language. If, like Chomsky, you believed that language was a genetically endowed, species-specific ability, with limited variability, then you were with Descartes in believing that the object of study was mental substance. If you believed that language could vary unpredictably, like Joos (see chapter two, part four), then you believed that humans were born with a multi-purpose blank slate, as good for learning more or less complicated languages as for learning the rules of cricket or how to be nice.

So it was the nature of mind that was under discussion, at least from Chomsky's point of view. For the Bloomfieldians this was not under dis-

[96] See Sampson (1997:107-136) for a comprehensive argument against universals.
[97] The Universals Archive of Konstanz University catalogues proposed universals exhaustively.

cussion, because the matter had been settled already. The nature of mind was no more a matter of debate than geo- or helio-centricism. However, when Chomsky resurrected the spectre of Rationalism, what came under debate was not the substantial nature of mind, but its structure. No one ever really suggested that Chomsky's conception of mind was similar to Descartes' in its substantial form (and nor did Chomsky use it to prove the existence of an all-powerful God). The similarity was supposed to lie in their respective *formal* conceptions of mind, or in other words, the knowledge we can glean of the structure of the human mind.

Chomsky's innateness hypothesis comes from the poverty of the stimulus argument and related concepts such as the critical age hypothesis (e.g. 1986:150-2 *et al*). These make the argument that language is not just constrained by general rules about possible languages, but that human languages are specifically constrained by human brains. In other words, there should be lots of possible languages, or possible features of languages, which turn out never to occur in actual human languages. This looks like a testable claim, rather than a philosophical one, and it is one I will return to in section 3.2[98] of this chapter. For now it is enough to note that this is an area which looks promising for a genuine difference in kind between Empiricist and Rationalist concepts of mind.

If this is such a split, then it would help to explain something much decried of TGG – its concentration on a formal picture of language. For many, omitting to describe the functional nature of language is akin to omitting the flying when discussing birds, or analysing fish in the absence of their swimming ability. Bloomfieldians before Chomsky, and sociolinguists in reaction to him, have noted that language is used as a tool for certain things, and that this is done by a mixture of learned paradigmatic

[98] For details of an experiment involving supposedly 'impossible' languages, see Smith and Tsimpli (1995:137-155). See also Newmeyer (2005) for a full treatment of this issue.

strategies and by more or less free creativity. A blank slate offers infinite possibilities, leaving our creativity constrained by non-linguistic features.[99]

For Chomsky the possibilities would not be so open. Although he makes much of the infinite nature of language (1957:11, 1965:4-5 and see chapter one), his conception of it is very much of a constrained infinity (1986:55, 205n, 1988:148-9). Chomsky is led from what he believes to be the most interesting aspect of his 'discoveries' about language (the fact that we are programmed in a certain way to only understand certain types of all possible language) to the study of what that structure actually is. This is, seemingly, to the detriment (or even exclusion) of any other type of study, especially of the social variety.

The next two sections of this chapter survey the historical use of Rationalist and Empiricist philosophers from within linguistics. However, first there are three conclusions to note from this introduction concerning the early development of TGG and Chomsky's wholesale embrace of Rationalism. First, TGG did not have to entail Rationalism in the broadest sense; noting that we have a predisposition to learn language does not necessarily lead directly to Descartes. Having said this, TGG did entail a *kind* of Rationalism, in that it laid down innateness constraints regarding what kind of languages humans are capable of learning. Second, the focus on the formal nature of language was again a product of TGG, but not an inevitable one. TGG could have been much more open to the possibility of studying the functional nature of language, but it did not turn out that way. Third, the interplay between TGG and Rationalism, and arguments against it, has Kuhnian overtones, and this lends support to the idea that the forms of linguistics under discussion, if they follow Kuhn's template in this sense, might be subject to the incommensurability which he suggests arises from competing epistemological and ontological commitments.

[99] Sampson (1997:139-41) makes critical points about Chomsky's use of the word 'creative'.

2.0 Chomsky, Descartes and 'Cartesian linguistics'

This section details Chomsky's use and citation of Rationalist philosophers, concentrating on the early days of TGG, but continuing throughout his career. Such citations were rather more common during the emergence of TGG than they are now. This accords with Kuhn's account of paradigm-formation, as I noted in the introduction, and shows that an emerging school/subject/ paradigm has to do some work in order to recruit new members, and that some of this work can be rhetorical rather than scientific. The more rhetorical (or metaphysical) the persuasion, and the less empirical, the more we might surmise that they are dealing with arguable (i.e. posited) objects, whose ontology is supported by arguable philosophical theories.

Chomsky does not mention Descartes in *Syntactic Structures* (1957), but within ten years he had written a book devoted to uncovering the Cartesian foundations of the new subject. He would continue to write about Descartes throughout his career although, in the 1980s, such references began to drop off. In this section I will look at a selection of Chomsky's works, noting how and to what extent Chomsky presented TGG in relation to Descartes and other Rationalists. I concentrate on Chomsky's book-length publications in this section, because it is only in the longer format that he tends to concern himself with metatheoretical and historical questions; shorter papers tend to focus on issues in current linguistic theory[100].

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[100] This in itself is worth noting, from a Kuhnian point of view. Journal articles and papers are the preferred method of disseminating research in the natural sciences, whereas Chomsky's book-length publications come across as self-conscious pre-theoretic 'paradigm-building'.

This section is arranged chronologically into four parts. Broadly speaking, the first part covers the 'birth' of TGG; the second covers its first real flourishing as a major force, coinciding with Chomsky's first explicit references to Descartes and Rationalism, as well as coinciding with the first serious challenge to TGG, generative semantics; the third phase covers the 1970s and 1980s, during which time an institutionally entrenched TGG covered more than one 'theory', (the standard theory, extended standard theory and government and binding); and the fourth phase is minimalism (everything since about 1993). Not surprisingly, those periods in which Chomsky wrote extensively about Descartes are covered in more detail than those in which he did not.

This arrangement partly overlaps with Harris's (1993:172) much more precise division of TGG into four parts: early transformational theory (or Chomsky's 'Harrisian' period – Zellig, not R.A.) from 1955-1964; the standard theory, 1965-71; the extended standard theory, 1972-1980; and government and binding/principles and parameters, 1981-1993. The differences are down to Harris concentrating on the changing nature of the theoretical core of TGG, and his omission of minimalism, as his book was published in 1993. The theoretical changes, for example from the 'standard theory' to the 'extended standard theory' to the 'revised extended standard theory', are not central to my analysis. However, it is worth bearing in mind that as the chronology unfolded, Chomsky's theory of language was constantly shifting and evolving. This is normal for a nascent paradigm, and helps to explain the concomitant recourse to early modern philosophers.[101]

[101] There are other ways of dividing up Chomsky's career. Newmeyer 1996 (chapter 5) divides it into 'rule-oriented' and 'principle-oriented' eras.

2.1 Early TGG

Syntactic Structures (1957) contains no references to Descartes or any other philosophers from previous centuries. In itself, this fact does not 'mean' anything; we saw earlier in this chapter that neither Bloomfield or Hockett, in the years preceding Chomsky, felt any need to do so. *Syntactic Structures* is fairly dense, and mostly reads as a new-ish approach to syntax. However, as noted in the introduction, Harris makes a vital distinction between the different ways that the ideas contained in *Syntactic Structures* can be viewed:

> What looked to most of the old guard like a new way to do syntax mushroomed in less than a decade into a new way to do linguistics, a new way to look at human beings, and a new way of doing science; new and completely inverse. (1993:67)

Turning to Chomsky's metaphysical aims, the intention of *Syntactic Structures* may not have been revolutionary on the level which it has come to be viewed. Chomsky may have meant it as a new way of doing syntax, or even as a new way of doing linguistics[102], but there is little suggestion of metaphysical revolution in the book. In the introduction, he notes that:

> During the entire period of this research I have had the benefit of very frequent and lengthy discussions with Zellig S. Harris. So many of his ideas and suggestions are incorporated in the text below and in the research on which it is based that I will make no attempt to indicate them by special reference. Harris' work on transformational structure [...] proceeds from a somewhat different point of view from that taken below [...] (Chomsky 1957:6)

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[102] Harris' formulation is not as easily understood as might be assumed, as the distinction between 'syntax' and 'linguistics' for the Bloomfieldians was not the same as that of the Generative Grammarians.

So although in the text Chomsky takes issue with some contemporary conceptions of language, he does not claim novelty in this regard; and he acknowledges many other contemporary philosophers and linguists to whom he owes debts, such as Quine, Goodman (both p.14) and even Hockett (p.86). The whole book seems more like 'a new way to do syntax' than 'a new way to look at human beings'.

The 'new way to look at human beings' emerged in due course. Chomsky's 'Review of *Verbal Behavior* by B. F. Skinner' (1959) marks his first serious proposal of an innatist view of language capabilities. It is not really a review, of course, more a wholesale attack on Skinner's behaviourist theories, and has been described as 'perhaps the most devastating review ever written' (Smith 1999:97). Harris (1993:55-8) also describes it as 'devastating'; Murray describes it as 'ferocious' (1994:231); and Newmeyer says it 'knocked out the underpinnings from the behaviourist psychology' (1996:148). The consensus is that this was Chomsky's first major metatheoretical work – the first time he set out his innatist stall and went on the offensive (*Syntactic Structures* is notably polite compared to the *Review*). Having said that there is at least some debate over who or what it was 'devastating' for. According to Sampson (much more of whom below):

> Chomsky's later writings often refer back to Skinner. But to treat Skinner's unreasonable theories as representative of the centuries-old tradition of empiricist thought is a travesty. So far as I know, Skinner was never much read outside the USA. To expect the world at large to believe in innate knowledge, because some half-forgotten American psychology professor did not believe in minds at all, is surely a bit rich. (1997:50)

These arguments are a mixture of metaphysics and rhetoric. Skinner was (or is) hardly a 'half-forgotten psychology professor', and Chomsky's review of *Verbal Behavior* certainly didn't end Skinner's career. On the other hand, the review was (and remains) a defining moment in Chomsky's career.

The schematic form of this history (whereby one theory rules and then another takes over) is, of course, not to be taken too seriously. In chapter two (section 1.1) I addressed the overlap period between one paradigm and the next, according to Kuhn. However, Sampson hints at the more relevant problem here, which is the idea that behaviourism, or empiricism, or whatever else stood in opposition to Chomskyan linguistics, was somehow monolithic. That is to say, that there was a coherent theory known as 'Empiricism' which was capable of being confirmed or rejected by either logical or empirical means. This was clearly not the case. 'Behaviourism' may have been, to an extent, such a scientific field, but as Lakatos pointed out (1970:132-81), scientific fields do not succumb to one article; instead, they 'patch and mend' in response to it. Positivism was not a scientific field, of course, but a loosely defined set of epistemological (and methodological) axioms defining an approach to science which was not empirically vulnerable. 'Empiricism' is technically an epistemological theory, but colloquially it can have a more general meaning. As Sampson points out (1997:1-6), what we call 'Empiricism' can range from an epistemological position in philosophy, to a methodological position in science, to what most people would describe as 'common sense'.

More to the point, in the 'Review of *Verbal Behavior*' Chomsky's object of attack is 'behaviorism', not 'Empiricism'. However, in his preface to the 1967 edition, he adds that the piece is a 'critique of behaviorist (I would now prefer to say "empiricist") speculation', and continues to equate the two throughout the preface. For my purposes, this equation of Empiricism with Behaviourism is notable because of what it leaves out. Chomsky was explicitly attacking a contemporary theory of mind, not a centuries-old tradition. Moreover, while he presents his alternative innatist hypothesis of human language capabilities, he does not mention his own Cartesian roots. This does not come into play for several more years, and will be discussed in the next section.

Chomsky's attack on Skinner was largely an epistemological one, that is, an attack on Skinner's view of the mind (or lack of it). Naturally this also entailed an attack on his methodology. What it did not do was to show beyond reasonable doubt that the mind could not be blank, that it must contain innate knowledge. To do that would require a comprehensive, and positive, research programme (to borrow Lakatos' terminology again). If in *Syntactic Structures* and the 'Review of *Verbal Behavior*' Chomsky promised a new linguistics, then he would need to do so in the ensuing years.

2.2 Phase Two

To return to the philosophy, there is no mention of Descartes in *Syntactic Structures* or in the 'Review of Skinner'. The reliance on 17th-century philosophy starts in the early 1960s. *Current Issues in Linguistic* Theory (1964) and *Aspects of the Theory of Syntax* (1965) contain various references to Descartes, Locke, Leibniz, Du Marsais[103], Humboldt[104] and the *Port-Royal Grammar*[105], among others. That was followed by *Cartesian Linguistics* (1966a), whose title is self-explanatory, and *Language and Mind* (1968), which places contemporary generative linguistics in the context of its Cartesian/Rationalist forebears.

Aspects really does feel like a book outlining 'a new way to do linguistics, a new way to look at human beings, and a new way of doing science'. It is

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103 Cesar Du Marsais (1676-1756) was a French grammarian, cited by Chomsky as a 'Cartesian linguist' who 'follows the Port-Royal grammarians in regarding the theory of deep and surface structure as, in essence, a psychological theory' (Chomsky 1966:50). However, see section 3.1 of this chapter for controversy over just how 'Cartesian' he really was.

104 Wilhelm von Humboldt (1767-1835) is often cited by Chomsky, particularly Humboldt's observation that language can 'make infinite use of finite means' (Chomsky 1965:8).

105 The *Port-Royal Grammar* (1660) was a 'Cartesian' work on language, in that it explored universal aspects of language, irrespective of the particular language; again, it is often cited by Chomsky.

the first of Chomsky's works to follow what became a familiar pattern over the ensuing thirty years or so. It begins with an account of the historical development of Rationalist ideas, including – as mentioned above – such figures as Descartes, Du Marsais, Humboldt, and the Port-Royal Grammar. It then goes on to explain why Chomsky's view of language is fundamentally correct, and analyses various aspects of language which support this view, while improving and revising the theory. *Language and Mind* does something similar. Its first chapter places generative grammar within a historical context, and the second looks at contemporary linguistics.

In between these two works in 1966, Chomsky published *Cartesian Linguistics*. This most curious of books is entirely devoted to explaining the history of Rationalism through the ages, beginning with Descartes and taking in, again, Humboldt, the Port-Royal Grammar, and various minor 'Cartesians', with the implied endpoint of Chomsky and TGG.

In the introduction Chomsky explains his purpose in writing *Cartesian Linguistics*:

> Questions of current interest will, however, determine the general form of this sketch; that is, I will make no attempt to characterize Cartesian linguistics as it saw itself, but rather will concentrate on the development of ideas that have reemerged, quite independently, in current work. My primary aim is simply to bring to the attention of those involved in the study of generative grammar and its implications some of the little-known work which has bearing on their concerns and problems and which often anticipates some of their specific conclusions. (1966a:2)

So Cartesian Linguistics is primarily a reference book for current practitioners of TGG. It shows how some of the problems now faced by TGG have been tackled in the past, and which 'little-known' thinkers might be of interest today. These 'concerns and problems' are four classic

Chomskyan themes, and form the chapter titles of the book: 'Creative aspect of language use', 'Deep and surface structure', 'Description and explanation in linguistics', and 'Acquisition and use of language'.

The first, 'Creative aspect of language use', takes Descartes' observation that our freedom of thought and our ability to express it marks us off from other animals. Our bodily functions, like those of animals, are mechanistic, and could be reproduced by a machine, whereas our intellectual creativity could not (according to standard 17th-century thought).

The second theme is 'Deep and surface structure', (although this terminology has now been dispensed with in Chomsky's thought (Chomsky 1995:186-191)). Chomsky describes it in its most basic form as follows:

> The former [deep structure] is the underlying abstract structure that determines its [a sentence's] semantic interpretation; the latter [surface structure], the superficial organization of units which determines the phonetic interpretation and which relates to the physical form of the actual utterance, to its perceived or intended form. In these terms, we can formulate a second fundamental conclusion of Cartesian linguistics, namely, that deep and surface structures need not be identical. (1966a:33)

Chomsky compares this basic tenet of TGG with an example from the seventeenth-century Port-Royal *Grammaire Générale et Raisonnée*:

> When I say 'Invisible god created the visible world', there are three judgements in my mind embedded in this proposition. For this states first that 'God is invisible'. 2. That 'he created the world'. 3. That 'the world is visible'. And of these three propositions, the second is the foremost and essential part of the proposition. But the first and third are only incidental, and only form part of the principal, of which the first forms the subject, and the second the attribute. (*Arnault and Lancelot* 1660:68, quoted in Chomsky 1966a:33, my translation)

He comments on the above example in the following terms:

> The deep structure that expresses the meaning is common to all languages, so it is claimed, being a simple reflection of the forms of thought. The transformational rules that convert deep to surface structure may differ from language to language. (ibid:35)

Here Chomsky is indeed not 'characteriz[ing] Cartesian linguistics as it saw itself', and deliberately uses three terms with specific theoretical uses in TGG: 'deep structure', 'surface structure' and 'transformational rules'.

After analysing the *Port-Royal* approach to language in some detail, Chomsky provides a short chapter on 'Description and explanation in linguistics'. Here he separates 'Cartesian' grammars, which looked for explanation of observed phenomena, from contemporary (i.e. Bloomfieldian) 'descriptive' grammars.

The last chapter, on 'Acquisition and use of language', again enlists Descartes, Cordemoy (another 'Cartesian' seventeenth-century French philosopher) and Humboldt, among others, and describes their arguments as to the innate nature of linguistic structures, as evidenced by what is now known as the 'poverty of stimulus' argument. Chomsky rounds off the chapter as follows:

> Contemporary research in perception has returned to the investigation of the role of internally represented schemata or models and has begun to elaborate the somewhat deeper insight that it is not merely a store of schemata that function in perception but rather a system of fixed rules for generating such schemata. In this respect too, it would be quite accurate to describe current work as a continuum of the tradition of Cartesian linguistics and the psychology that underlies it. (ibid:72)

So Chomsky hedges his bets in *Cartesian Linguistics*, by renouncing any claim to portraying the historical characters in question as they saw themselves. Instead, he shows that there is a tradition of thought stretching at least as far back as Descartes which adheres to certain core principles about the nature of language and thought, notwithstanding the changing intellectual priorities of different centuries. By placing himself within this tradition of thought, Chomsky allies his concepts of 'deep structure', transformations' et al., to the previous Cartesian concepts of innate dispositions, creativity, and the rest.

Ten years after establishing the field, Chomsky made his most comprehensive examination of his roots (and it remains so – he has not devoted so much space to the issue since). Previous works had focused on contemporary concerns such as the current state of linguistic or psychological theory. Having fairly comprehensively won those debates, when the field seemed more secure, he then situated his ideas in a historical context – the process which Harris calls 'enlisting the grandfathers' (1993:61). Of course, Chomsky's persuasive work was not done. Almost simultaneous with the publication of *Cartesian Linguistics* was the emergence of Generative Semantics (see Harris 1993 for the full story). This did not involve the 'grandfathers', so I will not go into it in detail here, but the aftermath meant the emergence of a new form of Chomsky's theories, and a consequent restatement of his epistemological position.

2.3 The 1970s and 1980s

In the 1970s and 1980s, TGG covered several different 'theories': EST (Extended Standard Theory) and REST (Revised EST), (often referred to as lexicalism, e.g. Harris 1993:144 and Newmeyer 1996:54), GB (Government and Binding), and P&P (Principles and Parameters). Two of Chomsky's book-length publications from the first half of this period are *Reflections on*

Language (1975) and *Rules and Representations* (1980). These are liberally sprinkled with references to Descartes, particularly *Reflections on Language.* The theme of these references has not changed much since *Cartesian Linguistics:*

> Despite the plausibility of many of the leading ideas of the rationalist tradition, and its affinity in crucial respects with the point of view of the natural sciences, it has often been dismissed or disregarded in the study of behavior and cognition. (1975:9)

Chomsky goes on to restate what the 'Rationalist tradition' stands against:

> Empiricist speculation and the "science of behavior" that has developed within its terms have proved rather barren, perhaps because of the peculiar assumptions that have guided and limited such inquiry. The grip of empiricist doctrine in the modern period, outside of the natural sciences, is to be explained on sociological or historical grounds. (ibid:11-12, and see footnotes 8 and 10)

There is little sense of moving on here. Chomsky is making exactly the same points as in 1959 and the 'Review of Skinner', mixed with the historical perspective of *Cartesian Linguistics.* There is a clean two-way epistemological and methodological split between Empiricism and Rationalism, especially regarding cognition and the human mind, and his linguistics falls unequivocally on the Rationalist side. Chomsky is also making two other points in these passages: first, that his Rationalism is scientific, while the prevailing Empiricism is damagingly unscientific, and second, that 'sociological or historical' factors can determine the acceptance or rejection of a way of thinking (a distinctly Kuhnian interpretation of the role of society and circumstance in the development of an epistemic enterprise). So between the sixties and the seventies, this aspect of Chomsky's metatheoretical self-justification does not alter or wane in any significant respect.

In this period, Chomsky's book-length publications begin to divide into those intended for the (highly) educated general public, and those for a specialised academic audience. For example, *Essays on Form and Interpretation* (1977) contains far fewer references to Descartes or any other of the grandfathers. The book is more technical than those of 1975 and 1980, and lacks the long introductory discussion on linguistic metatheory which characterises his other books. In writing for a more specialized audience, perhaps, Chomsky was more confident of their adherence to his paradigm.

The same pattern continues into the 1980s. From 1981 to 1992, the principal theoretical tenet was that of Government and Binding. The founding texts for this were *Lectures on Government and Binding: The Pisa Lectures* (1981) and *Some Concepts and Consequences of the Theory of Government and Binding* (1982). These do not contain references to Descartes or other 'grandfathers', but this omission can be explained by a passage from the introduction to the former:

> The Pisa Lectures were highly "theory-internal", in that a certain theoretical framework was pre-supposed, and options within it were considered and some developed, with scant attention to alternative points of view or the critical literature dealing with the pre-supposed framework. (1981:ix)

This passage is revealing, in that it shows just how confident TGG as a paradigm was becoming by the early nineteen-eighties. After the upheavals of the late nineteen-sixties and early seventies, Chomsky had once more emerged dominant, and felt less need to lay down the entire scope of the paradigm every time he published a book aimed at his peers.

However, in *Knowledge of Language* (1986), Chomsky continues to cite Descartes, the Port-Royal Grammar and Humboldt. This book is aimed at a wider audience than that of the *Pisa Lectures* – it is part of an interdisciplinary series and contains an interesting mix of linguistics and politics. *Language and Problems of Knowledge: The Managua Lectures* (1988) also discusses Descartes, Humboldt and Hume at some length, although these references are fewer than in previous works and there is no sustained historical passage in the book.

2.4 Minimalism

This brings us to the present phase of TGG, the Minimalist Program, which dates back to 1992. The separation of Chomsky's works into those books and aimed at a specialist linguistic audience, and those aimed at the general public, becomes even more pronounced in this period, whose two founding theoretical texts are *A Minimalist Program for Linguistic Theory* (1992) and *The Minimalist Program* (1995). In *The Minimalist Program* (1995), there are exactly no references to Descartes; the oldest cited work is Chomsky's own Master's thesis from 1951. *The Architecture of Language* (2000a) also dispenses with historical exegesis, as do most of the journal articles, in line with earlier periods of TGG (e.g. Chomsky 2005 and 2008). *The Minimalist Program* looks and feels like a scientific tract,[106] in a Kuhnian sense, in that it is completely impenetrable to the lay reader, which accords perfectly with Kuhn's description of the working publications of a mature science (Kuhn 1962:20).

New Horizons in the Study of Language and Mind (2000b), on the other hand, is once again full of references to various historical philosophers. This book summarises not only his current thought but also the history of his work in linguistics and science for the lay reader (although in typical Chomskyan style, it makes serious demands of his readers). Nor does it advance the theory of generative grammar, focussing instead on philoso-

[106] However, see Lappin, Levine and Johnson (2000 and 2001) for arguments that the shift from GB to the Minimalist Program was motivated purely by social rather than scientific factors, and that Minimalism is profoundly unscientific.

phy of mind and language. As with the discussion of *The Pisa Lectures*, this says quite a bit about Chomsky's assumptions about the intended audience. In this case, it would include sceptics and non-linguists (and not many TGG linguists). The book is only partially aimed at sceptical linguists however, and carries with it the confidence of a programme of study which is well-established.

In *New Horizons in the Study of Language and Mind*, references to Descartes, Humboldt *et al*, while still numerous, also compete with Hume. Perhaps this is inevitable in any book which looks to trace competing ideas about science,, reference and epistemology. In particular, the chapters on the possibilities of naturalism and dualism as scientific concepts refer back to Humean ideas, thoughts and concepts' (ibid:85), and his ' "science of human nature" [which] "sought to find the secret springs and principles by which the human mind is actuated in its operations" (1748/1975:14, section 9)' (ibid. 141). However, Chomsky does not count Hume as a genuine Empiricist, especially when it comes to the relevant parts of his philosophy, and manages to reaffirm his Rationalist stance while embracing Hume:

> All of this [discussion about the extent of the richness of the conceptual structure determined by the language faculty] is much in accord with traditional rationalist conceptions and even, in some respects, the so-called "empiricist" thought of James Harris, David Hume, and others. (2000b:64, and ibid:133 for more emphasis of agreement between Hume and Descartes)

From this survey of Chomsky's purely 'professional' work since 1957, then, we can see a reasonably clear pattern. On the other hand, when he addresses a lay audience, he starts at the beginning and goes over the subject's origins and philosophical foundations, working on the assumption that in a work aimed at the general public, he must fight all of those battles all over again. A newcomer to the subject may nevertheless hold opinions about Rationalism and Empiricism, and if that is not addressed, they are unlikely to accept any subsequent arguments based on the nature of the human mind and the way in which it acquires knowledge.

In the works aimed at professional linguists, references to Descartes go from none, up to a peak, and then slowly down to none again. It appears that Chomsky has completely given up trying to convince linguists of the foundations of the subject, and this growth in confidence, culminating in absolute certainty, is something that Kuhn's theory predicts. What began as 'a new way of doing syntax' grew into 'a new way to look at human beings'. As this syntactic revolution grew into an epistemological one, a more thorough historical grounding was needed. By the 1980s, however, TGG had its own 'exemplars', and just as important, as Newmeyer points out, '[after *Lectures on Government and Binding* in 1980] for the first time in over fifteen years, the majority of people doing syntax were working within the framework currently being developed by Chomsky' (1996 (1989):63), and therefore less need of the 'grandfathers'. To put it another way, after thirty years TGG found itself entrenched, and success breeds confidence. As we have already seen, Kuhn argues that the stronger a discipline's institutional bases, the less need it has to convince itself or the outside world of its worth (1962:35-40). By the 1990s, this process is complete, and with regard to this one aspect of Kuhn's philosophy of science, TGG looks very much like a 'normal-scientific' paradigm.

We should not, of course, be surprised that there are changes of focus in Chomsky's work over the course of his career. *Syntactic Structures* was published in 1957, just four years after the description of DNA, and in the intervening years the possibilities of what science can do, and especially what we can learn about the brain, have multiplied beyond what was even thinkable at the beginning of Chomsky's career. What is impressive is his constant attachment to Rationalism, through good times and bad. This suggests that Sampson's observation – that the choice between Rationalism or Empiricism is a matter of 'faith' – is fairly accurate. Rather than ever waver in his Rationalist faith, Chomsky would rather co-opt Hume as a Rationalist.[107]

2.5 Conclusion to chapter four, section 2

This section has shown that Chomsky's enlistment of the grandfathers served in establishing the metatheoretical bases of the new paradigm. That I am phrasing this in Kuhnian terms does not mean that Chomsky intended it that way (although it does show an interesting symmetry); nor does it entail that TGG must therefore be a Kuhnian paradigm or a science. What it does show is that an emerging discipline needs metatheoretical support, and philosophy is one way of doing this (as Kuhn said). It also shows that metatheoretical bases for *ad hoc* posits are potentially irreconcilable with other metatheoretical bases for other *ad hoc* posits. If those *ad hoc* posits are of dubious (or metaphorical) ontological status, then their underlying epistemology will reflect this.

The next section will show how alternatives to Chomskyan linguistics share this property.

3.0 People who have taken issue with Chomsky

To call a section of a chapter 'people who have taken issue with Chomsky' is to invite ridicule. Although TGG looks from the outside like a progressive and secure school of linguistics (and what looks for all the world, especially to insiders, like a mature Kuhnian paradigm), we saw in the previous chapter that he regards himself as 'isolated'. He certainly has opponents.[108] Harris says that he has 'inspired blood-boiling animosity'

[107] C.f. Chomsky's earlier and starker assessment of Hume (1965:51).
[108] The existence of *The Anti-Chomsky Reader* (2004) is fairly good evidence of this.

(1993:26), and over the years he has defended himself against his share of attacks (ruefully documented by Botha (1991)).

In this light, my choice of Chomsky's opponents to focus on might seem arbitrary. However, the writers which are the subjects of this chapter form a very particular sub-circle of anti-Chomskyan thought. Geoffrey Sampson, Esther Figueroa and Victor Yngve have all written on the philosophical roots of linguistics, from different perspectives. Of course, they are not the only people who have addressed this topic, but I have selected them for the following reasons. First, they all take on Chomsky's Rationalism directly, attributing the wrongness of the generative enterprise to his metatheoretical commitments, or vice versa. Second, they present clear alternatives to Chomsky's Cartesian Rationalism. According to Sampson, Locke's Empiricism offers a much more coherent picture of the human mind. Locke is used as the basis not just for how to do linguistics, but also as a model of how we learn and use language. For Figueroa, the 'Hegelian' approach to language study leads to a more comprehensive account of how language works (even if this approach has little to do with Hegel) and has led to a more convincing, socially constituted linguistics. Yngve attributes the misunderstanding of linguistics to an ancient category mistake, and feels that this is the key to a properly founded study of language.

Chomsky and others have also argued about the nature of language from the point of view of evolution, but this section is not about evolution *per se*. Obviously the question of what kind of mind we have stems, at some point, from the question of what kind of mind we evolved to have. Two points about evolution are addressed in footnotes below. First is the argument between Hauser, Chomsky, and Fitch (2002), on the one hand, and Pinker and Jackendoff (2005) on the other. Essentially this is about whether language evolved as an exaptation, with a very small syntactic component which is language-specific (Chomsky et al.), or whether it evolved piecemeal as adaptations (Pinker and Jackendoff). There has also been an ongoing debate between Bickerton and Chomsky about what is unique to human language in terms of human (and more general animal) cognition. For Chomsky it is recursion, whereas for Calvin and Bickerton (2000) the development of words played a vital role in the evolution of language (Calvin and Bickerton 2000).

These debates surface in my discussion of Sampson and Figueroa below, but they are only tangential to the main point of this chapter. Evolutionary selection did indeed give us the mind we have, but there is no necessary connection between, on the one hand, arguments about how language evolved and, on the other hand, what kind of linguistics our epistemologies lead us to practise. This is not to deny that evolutionary considerations could *in some sense* settle the question of how innate language is; however, they cannot prove that one or another form of linguistics is ultimately misguided because it is based on a misguided epistemological tradition. Whatever turns out to be the language-specific part of our biological heritage will not definitively show whether or not following a Cartesian epistemology is the correct way to go about studying language. To put it another way, there is no necessary connection between the arguments in *Cartesian Linguistics* and in Hauser, Chomsky and Fitch (2002).

On top of this, neither Calvin and Bickerton nor Pinker and Jackendoff embrace an Empiricist methodology. Both argue for the existence of innate language structures, and both are concerned with arguing about what kind of structures we should posit. My main focus, on the other hand, is on those writers whose epistemology entails a rejection of Chomskyan linguistics. Pullum and Scholz (2001), also briefly addressed below, is tangential to my discussion for the same reason. While they discuss our present linguistic endowment (rather than its evolutionary history), their argument concerns what type of nativism we should embrace, and picks holes in Chomsky's extreme nativism, which they believe has not been shown to be correct in any meaningful way, and lacks serious evidence, rather than linking nativism as a mistaken epistemology with a mistaken form of linguistics. Trask's and Postal's (see chapter 3) criticisms of Chomsky are in a similar vein – they argue that the primary problem with TGG lies not its theoretical/epistemological foundations but in the mundanity of its day-to-day practice (or malpractice).

Figueroa, Sampson and Yngve, then, represent three similarly-founded, but differently constituted, metatheoretical strands of attack on Chomskyan linguistics. For each of the three, the appropriation of seventeenth century Rationalist philosophy by Chomsky exhibits a conscious, and ill-founded, attempt to bolster his subject historically. For each of the three a correct epistemological historical alternative exists which can point towards a well-founded linguistics. However, before I address the arguments of Sampson, Figueroa and Yngve I will first look at an essay by Hans Aarsleff.

3.1 Aarsleff

Hans Aarsleff's paper 'The History of Linguistics and Professor Chomsky' (1970, in Aarsleff 1982:101-119) attacks Chomsky's history of philosophy less for its philosophical arguments, and more for its historical inaccuracies. Aarsleff has no epistemological axe to grind, and his stated purpose is merely to correct mistakes. Paradoxically, this independence means that his paper is only tangential to my dissertation, even if it is more accurate than the other texts I examine in this chapter. It is tangential because this chapter deals with perceptions of the status of contemporary schools of linguistics and their historical antecedents, and the Kuhnian sense in which arguments are made linking the two. It is paradoxical because for all the energy expended in these arguments, their actual historical accuracy is irrelevant. Kuhn was always keen to stress that winners write the history, whether in politics or in science (1962:136-43); and if there is a perception that TGG follows in a Cartesian lineage, then from a Kuhnian point of view that perception is more interesting than a dispassionate historical analysis of seventeenth- and eighteenth-century metaphysics and logic. Certainly Aarsleff's criticism did not affect Chomsky's willingness to continue to place his work within a 'Cartesian' lineage, as discussed in the previous section.

However, Aarsleff's arguments against Chomsky's historical claims are not presented here just for the sake of completeness. His essay is a masterpiece in thoroughgoing research, of the type Koerner would no doubt approve (see chapter one), and his conclusion on *Cartesian Linguistics* is as follows:

> I must conclude with the firm belief that I do not see that anything at all useful can be salvaged from Chomsky's version of the history of linguistics. That version is fundamentally false from beginning to end – because the scholarship is poor, because the texts have not been read, because the arguments have not been understood, because the secondary literature that might have been helpful has been left aside or unread, even when referred to.
>
> Professor Chomsky has significantly set back the history of linguistics. Unless we reject his account, we will for a long while have no genuine history, but only a succession of enthusiastic variations on false themes. (1982:116-7)

Aarsleff gives two criteria which need to be met for an enterprise such as *Cartesian Linguistics* to be successful:

> adequate scholarship and the overall coherence of the entire history that is presented, without omission or neglect of material that is relevant. (1982:102)

Aarsleff presents an array of instances where he believes Chomsky fails to fulfil these criteria. I will give just one example of each. The first, which concerns 'adequate scholarship', relates to Chomsky's appropriation of Du

Marsais as an ally, and by extension, as a 'Cartesian'. However, 'when Chomsky (1966a:53-4) does refer to D'Alembert's[109] eulogy of Du Marsais, he uses a passage which is closely preceded by the statement that Du Marsais was anti-Cartesian'[110].

The second criterion involves coherence, without omission or neglect of relevant material. Aarsleff takes issue with Chomsky's presentation of Locke in this regard. First, he says Chomsky 'relies on outright inferior sources' such as the 'laughable notes in Fraser's wretched edition of the *Essay*' (1982:103). Aarsleff then notes that Chomsky fails to mention that Du Marsais was not only anti-Cartesian, but positively pro-Lockean, citing as evidence Du Marsais' contention that 'I could cite a great many authorities, and among others that of Mr. Locke in his *thoughts concerning education*, in order to justify what I say here' (1982:113). Such details concerning Chomsky's attitude towards Locke are summed up in a quotation from one of the first (favourable) reviews of *Cartesian Linguistics* (Kampf 1967):

> 'Locke emerges as the hero, Descartes the villain, from the histories of the conflict. Chomsky forces us, at last, to reconsider the influence of empiricism on the development of science and scholarship.' It should be unnecessary to point out why this statement is absurd, in both fact and interpretation. But it is worth noting that Locke is made out by that reviewer, as by Chomsky, to be a villain, or at least a sort of nincompoop in matters of language and the philosophy of mind. (1982:102)

For Aarsleff, Chomsky's greatest sin is this, that the distinction between Cartesian Rationalism and Lockean Empiricism is consistently presented

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[109] Jean Le Rond d'Alembert was an 18th century French philosopher and editor of the *Encyclopédie*.

[110] In Chomsky's defence he says on page two that 'several of the most active contributors to them [the developments described in *Cartesian Linguistics*] would surely have regarded themselves as quite antagonistic to Cartesian doctrine'. However, if this rescues Chomsky from Aarsleff's criticism, it also makes his choice of title sound slightly disingenuous, and leaves Chomsky to show why those who 'would surely have regarded themselves as quite antagonistic to Cartesian doctrine' were nevertheless 'Cartesians'.

as black and white, and the writings of Locke presented as untenable. This point is echoed by Roy Harris, who says that 'Chomsky invoke[s] rather vague and facile distinctions between "Rationalism" and whatever supposedly stands in opposition to it e.g. "Empiricism"' (2003:169).[111] Aarsleff does not, as far as I know, have a Lockean bias, and it is hard not to agree with him that Chomsky's presentation of Locke's ideas make them appear so wrong that it is impossible to see why he was so influential at the time, and why he continues to be widely read today.[112]

I have already noted that the accuracy of Chomsky's claims about Descartes and Locke, and the accuracy of counter-claims from other linguists, are less important for my purposes than the perceptions which such arguments engender. However, in what follows Aarsleff's points should perhaps be borne in mind, if only as a reminder that, when it comes to the history of linguistic thought, the historical element is as important as the linguistic. At this point we can return to criticisms of Chomsky's historical writings from within linguistics.

3.2 Sampson's Position

Geoffrey Sampson dislikes everything about Chomsky, and as we shall see later, links his linguistics and epistemology to his politics. His absolute anti-Chomsky position is highly entertaining. Sampson's work is primarily philosophy-driven: since Chomsky's linguistics is based on Rationalism, Sampson argues, it must be wrong, and Sampson uses arguments

[111] Roy Harris has given his own account of the development of the relationship between linguistics and philosophy (see Harris 1996).
[112] Chomsky did not reply to Aarsleff's paper, because, (most uncharacteristically) 'I've never bothered'. See Barsky (1997) *Noam Chomsky: A Life of Dissent*, and Harris's review of Barsky (1998) for further (highly-charged) details of what Aarsleff and Chomsky may or may not have meant.

from Empiricism rather than from linguistics to argue against Chomsky.[113]

Sampson has consistently attacked the Chomskyan project, from his 1980 work *Schools of Linguistics*, which contains a chapter attacking Chomskyan linguistics at every level, to 2001's *Empirical Linguistics*, which contains a chapter entitled 'What *was* Transformational Grammar?' (my italics). Perhaps the most complete expression of his distaste for Chomsky, and the main focus of this section, is his 1997 book *Educating Eve: The Language Instinct Debate*. This takes issue with Rationalism and the idea of there being any innate knowledge, and instead supports a 'Lockean' Empiricism, which, Sampson says, has driven all western scientific thought and development for the last 300 years or so. The fact that this Empiricism has had such tangible results is proof of its essential correctness:

> [A]t least in the English-speaking world Locke's empiricist point of view has been broadly taken for granted during almost all of the (past) 300 years [...] (1997:6)

This 'broad consensus' view of Empiricism (at least – with a hint of Anglo-Saxon chauvinism – in the English-speaking world) is often invoked by Sampson. In the introduction to *Educating* Eve he states that 'I believe the common-sense reaction [Empiricism] is essentially correct. I am sure the idea of human knowledge as biologically built-in is quite wrong' (1997:2). *Educating Eve* is strategically negative, in that most of the arguments it contains are against the possibility of Rationalism holding any water. The positive arguments in favour of Empiricism take a back seat, while the 'ludicrous' nature of Rationalism is dismantled until common-sense Empiricism is the only plausible alternative. Consequently, Sampson's book

[113] In fact, the main target of *Educating Eve* is not Chomsky but, as its subtitle suggests, *The Language Instinct*, the best-selling 1994 book in which the Harvard cognitive linguist Steven Pinker set out a comprehensive argument for the innateness of language.

takes Empiricism for granted, and requires Rationalism to prove its point, rather than vice versa. His view of innate linguistic structures is similar to the way most us of view fairies at the bottom of the garden – it is not incumbent on us to prove their non-existence. It was noted above that preference for one or the other may be simply a gut feeling (or 'faith'), and, given this, there may be an element of traditionalism to Sampson's position: Chomsky upset the apple cart of standard Empirical thought, and this is clearly something which Sampson holds dear.

Sampson (1997:12-3) takes a stand against both Chomsky and Pinker. He pairs them up as two sides of the same coin, which is broadly accurate – despite their differences[114] they are both unapologetic Rationalists.[115] Pinker is chosen as Chomsky's partner in crime because of his enormous success; Sampson assumes (probably correctly) that, when it comes to popular linguistics (rather than politics), far more people have read Pinker than Chomsky.

Sampson frequently complains about a 'straw man' Empiricism, which Rationalists use when they want to argue their position. This straw man involves a completely blank human mind, which has no structure and no dispositions (1997:18). It is simply 'learning stuff' which happens to be brilliant at picking up human attributes. This is indeed a straw man – no one has ever held that position exactly, including Locke, as Harris noted in chapter two (1993:66). Today, of course, we see mind in terms of evolutionary selection rather than immaterial substance, and despite occasional appearances to the contrary, Rationalists do not hold the monopoly on evolutionary approaches to mind. It is perfectly possible that we have an evolutionarily-adapted mind which accords with the Empiricist model, de-

[114] See the debate between Hauser, Fitch, and Chomsky, and Pinker and Jackendoff (which is, of course, about evolution, not philosophy).
[115] See, for example, Pinker's 2002 book *The Blank Slate*, which is one long dismantling of the (occasionally straw man) Empiricist position.

spite what Chomsky argues (1975:9, on different ways of studying the mind and the body).

Sampson counters this tactic, though, by employing what could be described as a straw man Rationalism of his own. Plato did *not* claim that we are born 'knowing' that Cambridge won the Boat Race in 1939, as Sampson suggests Rationalism must entail (1997:5). Similarly, while Chomsky does *sort of* claim that children are born with 'knowledge of language' in their heads, this is not the same thing as saying that they are born knowing a language (1975:11). In the same way, no one has ever claimed that a newborn baby 'knows' a natural language, such as Tagalog or Catalan. By the same token, Rationalists do not tend to claim that a newborn baby 'knows' some kind of universal language, a dialect of which will emerge during the course of maturation. Rationalism at heart is an argument about what sort of mind we have. It argues that humans have a specific design, which under normal circumstances will produce normal results. Just as our legs will develop to walk and run, so our minds will develop to do certain things and not others. We will never fly, we are just not built that way. By the same token, we will never speak Martian. Empiricism in its most basic form says that we could have a stab at Martian, even if the results would be poor, in the same way that most English people's Tagalog abilities would probably amount to very little unless they were particularly dedicated. Rationalism says that there would be no point trying to speak Martian. We are a different kind of thing, and our brains are not built for it[116].

Sampson's occasional abuse of the Rationalist straw man is more or less par for the course in this kind of debate (it was, after all, provoked by abuse of the 'Empiricist Straw Man', of which there are many examples,

[116] Interestingly, this is one empirical way of deciding the matter. All we have to do now is find the Martians.

such as Newmeyer 1980:3; Chomsky1975:132). The main thrust of his argument is that 'Empiricism should be seen as the default position' (1997:6), partly for its antiquity and partly for its 'common-sensical' nature.

Perhaps it is in the nature of a blank-ish slate that there is little to say about it. Sampson's common-sense Empiricism is not given a particularly thorough exposition – only five pages out of 160 are given over to his 'positive' vision of the human mind (14-19). References to Locke move on to references to Popper, and his falsificationist theory of science. The human mind is kitted out to acquire knowledge by forming theories and rejecting them according to their experience (this part of Sampson's book is called 'Guessing and Testing'). Popper's theory of science held that scientists form theories, test them, and either accept them *pending further confirmation*, or reject them (Popper 1963:33-66).

Paralleling Chomsky's use of child language acquisition as evidence for his theories, Sampson transfers the Empiricist model of science to newborn babies, saying that this human propensity to form and test hypotheses works for language acquisition just as it does for quantum physics (1997:17). If language is a cultural rather than a biological phenomenon (the standard Empiricist position), then we do indeed 'learn' a language, just as we learn to bake pies. We learn from the generations of humans who have developed languages into what they are, just as chefs learn from knowledge which has been passed down through the ages. A child learns a language by 'forming theories' based on the given data – the language which it hears spoken around it, all day every day.

This example, of how child language acquisition can be explained in Empiricist rather than Rationalist terms, is to be applied to all other aspects of human knowledge. Sampson's Empiricism explains all human knowledge in terms of the barest cognitive structures necessary to handle such knowledge. Everything else is a learned cultural product, the product of generations of trial and error.

Sampson sees his Empiricism as qualitatively different from Chomsky's Rationalism (just as Chomsky sees his Rationalism as non-trivially different from Empiricism). It is worth repeating that no one, naturally, denies that we have inbuilt, instinctive capacities (see the frequently-cited Quine (1976:56-8) above). We do not learn to walk in the same way that we learn the rules of cricket. There exists a qualitative difference between the two. The question of how language and similar mental capacities develop is non-trivial in this sense. It is not somewhere in the middle, it is significantly closer to one or the other.[117]

Sampson really comes into his own when he is arguing against Chomsky's Rationalism. One of his most impressive arguments concerns the reasons for the tree-branching structure of language:

> One of the chief genuine contributions Noam Chomsky has made to science is to show us that tree structuring in grammar is an empirical finding, not a logical necessity. (1997:111)

Chomsky did not invent trees of course (even in the linguistic sense), but he did show, from 1957 onwards, that human languages must work on a tree-like structure, involving transformations effected onto branching nodes. He added that human languages are constrained into a particular type of structure which they apparently do not 'need' to have (1980:144-146), and that this is why some languages would be impossible to learn (1988:149-50). However, Sampson points out that this argument depends on an ambiguity in the meaning of 'need'. It may not be a logical necessity to have the type of tree structure which natural languages do, but

[117] See Pullum and Scholz (2001) for an argument which places Sampson and Chomsky at extreme ends of a spectrum, with several variants of nativism in the middle.

> [C]omplex entities produced by any process of unplanned evolution, such as the Darwinian process of biological evolution, will have tree structure as a matter of statistical necessity, even if tree structure is not logically necessary to them. (1997:113)

Sampson goes on (ibid:113-121) to use various writers on evolutionary theory to show that tree structure is statistically rather than logically necessary in just about any evolved system, whether that system is biological, cultural, or anything in between. This being the case, there is no wonder that language has the tree-type structure it does. However, crucially, this does not make that tree structure a 'language universal', a genetically encoded feature of human languages which is biologically but not logically necessary. By introducing this third type of 'necessity', Sampson neatly sidesteps the evolutionary aspect of the argument. There are many ways in which languages could have developed the way that they have, and to see every innovation or linguistic tool in terms of innate structures is a lazy epistemology.

Another interesting point he makes concerns very old texts, such as the Old Testament. He argues that the non-recursive nature of the language in the oldest extant versions of Genesis shows how language has developed qualitatively alongside the technological and cultural development of modern society (1997:74-75). This is a fairly radical departure from the orthodoxy that holds that language in its modern form has been the same in terms of complexity since its 'evolution' somewhere in the region of 50-100,000 years ago (e.g. Crystal 1997:6, 293), and has not undergone any significant development in that time. This is consistent with Sampson's claim that we use our minds to do what we need, or have learnt to do, not according to preset functions and capabilities ('like a fully featured washing machine or video recorder' (1997:162)). In *Language Complexity as an Evolving Variable* (2009) Sampson expands on this counter-argument to one of the central Chomskyan tenets – that biological constraints mean that all human languages are of equal complexity and expressive power.

These arguments – about the expressive power of different languages at different times, about why languages are as they are, and about whether or not we are narrowly constrained in the range of languages we are capable of learning – betray a fundamental conceptual mismatch between Sampson and TGG over what kind of thing language actually is. After all, what kind of biological developmental processes could account for the stylistic differences in language being used between the early writers of Genesis and the later parts of it? For Sampson we really do learn languages; in TGG, 'growing a language' is often used to emphasise the pre-determined nature of the process (e.g. Harris 1993:67).

Interestingly, Sampson is one of the few people who explicitly link Chomsky's linguistics and politics, something which in general Chomsky is unwilling to do, and which Dell Hymes (1996:26) calls 'principled schizophrenia'.[118] For Sampson, the Empiricist view of mind guarantees individual trial and error, and therefore personal autonomy. A Rationalist view of the mind lends itself to centralised engineering of society, with a determinist view of human nature. This erodes freedom and dignity, and risks disaster.

At the end of the book he comes clean about this:

> All of us, surely, would rather be what most of us have supposed we are: creatures capable of coming to terms with whatever life throws at us because of our ability to create novel ideas in response to novel challenges – able to take the best ideas and ways of life of our predecessors and build on them, genera-

[118] Joseph (2006:126) notes the disparity between Chomsky's general attitudes towards personal and political freedom, and his attitudes towards linguists who disagree with him, such as the generative semanticists, who had Chomsky 'impose the one true interpretation of his theory in a fashion that can only be described as dictatorial'.

> tion after generation. Who would not prefer this picture to that which portrays biology as allotting to the human mind a range of available settings, like a fully featured washing machine or video recorder, and allows us to select the optimum intellectual setting to suit prevailing conditions? The former concept of Man is far nobler. The evidence suggests that it is also more accurate. (1997:162)

Sampson makes his political feelings about the human mind clear here. However, there is a serious flaw. Even if we would all prefer the Empiricist ('noble') view of the mind, wishing for it does not make it come true. This provides one of the most explicit, fascinating, and difficult moments for Sampson: his argument is, at base, one for humanity, and, for Sampson, human dignity and nobility require the Empiricist position to be correct.

There does seem to be a hole in Sampson's analysis of the link between Chomsky's politics and his epistemology. Chomsky is neither a communist, nor a fascist, nor any other type of authoritarian. It may be the case that both communists and fascists have deterministic views about society, and that this stems from a deterministic epistemology. However, this is arguable. Those on the other side of the epistemological fence from Sampson might argue that the blanker the slate, the more malleable the society (Joseph 2006:124), and that Empiricism, rather than Rationalism, invites totalitarianism. Chomsky, on the other hand, is a libertarian anarchist,[119] or, as Sampson would have it, his views are 'an engaging but slightly dotty version of anarcho-syndicalism' (1997:11), and it is hard to find a political position more enamoured of freedom than that. On top of that, Chomsky, as we have seen, refuses to draw a definite link between his politics and his linguistics, so it seems an unfruitful argument for Sampson to make.

[119] According to several interviews on his own website:
http://www.chomsky.info/interviews/20020322.htm

Sampson also takes the view that using a Rationalist methodology is incompatible with free and proper scientific practice (just as it is incompatible with a free society), and that it can be shown that Chomsky is guilty of serious deviations from proper scientific practice on account of his Rationalism. The main claim is that Rationalism is incompatible with reasoned and evidence-based argument, instead relying on internally found certainties.

For example, after talking about the claims of Chomskyan linguists that they were part of a Kuhnian revolution (see chapter three), he goes on to say that

> The thoroughgoing rationalist [...] [i.e. Chomskyan] [...] is obliged to prefer revolution to constitutional reform (in science and in politics); if the correctness of a theory, or the desirability of a form of society, is knowable by the pure light of reason rather than by practical experiment, then no means of peaceful persuasion are available when an opponent obstinately persists in claiming to see things differently. Naturally, those Chomskyan linguists who follow Kuhn, like political revolutionaries, lay much more stress on the notion that it is legitimate for them to come to power through an irrational Kuhnian 'paradigm-shift' than on the corollary that an irrational paradigm-shift which unseated them would have to be accepted as equally legitimate. (1980:159)

Here Sampson says that while Empiricism may turn out to be incorrect, compared to Rationalism, nevertheless it would be wrong to abandon the empirical method. Since TGG does not use the empirical scientific method, it must be unscientific. However, this use of 'Empiricism' and 'Rationalism' conflates two different meanings, which we have already touched upon.

It is probably unfair to characterise TGG as being purely 'Rationalist' in its methods. Generative linguists do not simply present their theories and refuse to discuss them on the grounds that they are right, and they know they are right. Sampson's delineation of Rationalist and Empiricist methodologies, as opposed to Rationalist and Empiricist theories of human knowledge, is an interesting and subversive take on Chomsky's claims about 'Cartesian linguistics', but it does TGG a disservice. It is perfectly possible to be a Rationalist (that is, to believe that human knowledge is discoverable from first principles found innately in humans), while coming to this conclusion via empirical methods.

Sampson also explores another difference between the Empiricist and Rationalist positions:

> In general, empiricist philosophy encourages one always to think 'I may be wrong, and the other man may well be right'; rationalism encourages one to think 'I know the truth, so the only point in talking to the other man is in order to show him the light'. When scholars of these contrasting frames of mind encounter one another, it is clear which man is likely to win the debate. (1980:158)

Again, Sampson's distinction between Rationalist and Empiricist beliefs (as opposed to their synonymous but not necessarily linked methodologies) leads him to a denunciation of Rationalism, but it seems misplaced. TGG did not achieve its pre-eminent position simply by shouting at other linguists 'in order to show them the light'. Some older linguists were convinced, by both argument and demonstration, as were many younger students (see Newmeyer (1986:38-39); and see Searle (1972:8) for the view that Chomsky *only* converted younger linguists). Of course, most generative linguists today are no more likely to abandon TGG wholesale than physicists are to abandon the theory of relativity. However, this does not point to an irreducibly 'rationalist' standpoint, which refuses to accept the possibility that it may be wrong. It is simply how most disciplines function. People work within a theory, which, if it produces consistent and cumulative results, is generally not challenged much[120].

There is a difference here between what Sampson describes as Rationalist epistemology ('I know the truth, so the only point in talking to the other man is in order to show him the light') and either Popperian unfalsifiable tenets, which render a theory unscientific (Popper 1963:37), or Kuhnian tenets which form the framework inside which research is carried out. Kuhn (1962:43) states that all sciences contain tenets which are central to the project, and are therefore highly unlikely to be abandoned. Popper adds that if these are not to be abandoned *under any circumstances*, then the discipline in question is not a science. However, Sampson indicates that generative grammarians produce conclusions which are unarguable, rather than initial premises, and that these are backed up by an alternative, Rationalist epistemology, which stands in contrast to normal scientific 'Empiricist' epistemology. It is this difference which gives Sampson's argument its special force, and makes it so startling. Where Marxists, according to Sampson, accept an argument from authority, generativists accept arguments from their own authority.

While TGG is controversial, it is practised by rational beings, and they have not staved off all the attacks on them simply by insisting that they are right.[121] It seems that, when rhetoric requires it, Sampson can commit the fallacy which he warns against, that of conflating a philosophy with a methodology.

As I mentioned above, Sampson is brilliantly entertaining. Like just about everyone who has attacked Chomsky's ideas head-on, he has not accumulated a large following. However, what is important for the purposes of

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[120] This description of how disciplines function is Lakatosian as well as Kuhnian. As has already been mentioned, Lakatos' philosophy was in part derived from Kuhn's.

[121] Although see Harris (1993:68-73) for references to Chomskyan 'mad dogs' who 'abandoned the ordinary conditions of scholarly fair play'.

this discussion is that his Empiricism prevents him from seeing any sense in any of Chomsky's writings (and for good measure, his political ones too).

The roots of incommensurability in Sampson's work are much clearer to see in politics than in linguistics. It's obvious that when right-wing conservatives talk about 'liberty' they mean something very different from left-leaning liberals, or Marxists, or anarcho-syndicalists. However, beyond the politics, Sampson cannot conceive of a way in which the mind can be said to be pre-determined to learn a circumscribed form of language, as I discussed above. Although he presents rational arguments against Chomsky's position, it is hard not to draw the conclusion that this disagreement between Chomsky and Sampson is 'a matter of faith'.

3.3 Figueroa's Position

In chapter one and two we saw that sociolinguistics is significantly more diverse than TGG. Figueroa's *Sociolinguistic metatheory* (1994) addresses this diversity by looking at three leading lights of sociolinguistics: Dell Hymes, William Labov and John Gumperz. Although emphasizing their differences, she also draws attention to the similarities, especially between Hymes and Gumperz. She suggests that on one level 'Labov's attempt at a synthesis [between autonomous linguistics and social reality] fails. He does not incorporate the social dimensions of language into his linguistic theory' (1994:106). This contrasts with her conclusion about Gumperz, that 'he has demonstrated that a linguistics of particularity is possible' (ibid:140). Although she presents this distinction, and emphasises that Gumperz's and Hymes's metatheories are well-founded, she nevertheless places all three within the 'Hegelian framework' (see below), although she does not place Labov within the 'linguistics of particularity' (1994:176n).

Incommensurability between sociolinguistics and TGG is explored explicitly in *Sociolinguistic Metatheory*. Although, as we saw in the previous chapter, she finds Kuhn's model insufficient to describe linguistics, she sees his ideas on incommensurability as 'one of the more lasting points to be taken from Kuhn's work' (1994:27). She specifically frames the difference between sociolinguistics and TGG in Kuhnian terms:

> Markova (1982) has pointed out that the normal science paradigm in psychology, and in all "scientific" fields for that matter including linguistics, has traditionally been the Cartesian framework and not the Hegelian framework [...][122]
>
> The received linguistics paradigm has been Cartesian and formalist. Given these facts, sociolinguistics may be seen as part of an evolving revolutionary science paradigm, one which offers an alternative to the normal Cartesian assumptions. It is very difficult to participate in normal science [...] and also to question it. (1994:27)

This book is more mainstream than Sampson's, in that it comes from within sociolinguistics, is destined for sociolinguists looking at the foundations of their subject, and probably would not find a larger audience outside the discipline. Despite the conventionality of its audience, however, its metatheoretical nature means that the focus is not on the practice of sociolinguistics *per se*, but the underlying Empiricist/Rationalist debate and its political ramifications. By political, I mean the underlying currents of why it might be held to be correct to choose a particular discipline to study. As we shall see, this tends to be driven by anti-elitist or other egalitarian principles.

The initial motivation for sociolinguistics partly involved a backlash against TGG. This was not just epistemological, it was also topical. Many

[122] This is not a universally held view. Sampson quotes Pinker arguing against Empiricism: 'According to Pinker, this Standard Social Science Model has dominated intellectual life since the 1920s – it is "the secular ideological victory of our age"; "in the rhetoric of the educated, the SSSM has attained total victory"' (1997:104).

linguists felt that studying language in a vacuum, or without taking into account its communicative and social aspects, was useless. The abstract question here, not always explicitly articulated but lurking in the background of every conflict between sociolinguistics and TGG, is about the purpose of language, rather than its nature or form. TGG sees language as being *for* thinking, while sociolinguistics sees it as *for* communicating. This may seem a rather pointless distinction to make, as it obviously does both. However, the way you look at language affects the way you study it.[123]

The idea of 'the purpose of language' clearly rests on a semantic conflation. First, it could refer to the evolutionary development of language. In this sense, it would refer to those aspects of language which developed first, and those which were by-products or exaptations[124]. There are physical features which serve as evidence for both. For example, the form of the entire vocal tract seems to indicate that a lot of selectionary pressure has gone into developing our ability to communicate (Bickerton 1990:141-5). However, the vastly complicated structure of natural languages (infinite recursion and self-reflexive metalanguage, for example) seems like overkill if all that language is for is to point out food sources.[125]

There is a second option, however. If the question is not seen in evolutionary terms it can be read as referring to the primary function of language. This is a little more difficult to spell out. For formalists, such as Chomsky, it means that language simply cannot be understood without focusing on the structure of the linguistic knowledge of the individual; this

[123] See chapter five for more discussion of the function of language.

[124] '[M]any features of organisms are non-adapted, but available for useful cooptation in descendants [...] features that now enhance fitness but were not built by natural selection for their current role. We propose that such features be called exaptations.' (Gould and Vrba 1982:1)

[125] Recall that Hauser, Chomsky and Fitch (2002) have argued that *all* language is an exaptation of other cognitive features, but this is different from trying to distinguish *which* features of language are exaptations and which are adaptations.

knowledge makes communication possible, but the communication is merely a manifestation of that knowledge. For functionalists, such as Figueroa, language is social glue, which exists to make and bond communities. Any mental 'knowledge of grammar' can only be seen within those terms (Figueroa 1994:21-5).

This question, in contrast to the Empiricist-Rationalist debate on the nature of language, seems open to a 'somewhere in the middle' treatment. Despite this, the debate between sociolinguistics and generative grammar, as conducted in Figueroa (1994) and elsewhere, gives clues that having a gut-instinct about the answer to this question seriously affects the way that language is studied. If you see language as a mental, thought-based phenomenon, then its abstract structure will require studying. If, on the other hand, you see language as a social phenomenon, then you will study it in social contexts.

While this is not a 'political' argument, there are further factors involved. Using the self as evidence, or conducting 'tests' on native speakers, could be said to dehumanise the process. However, if the aim of the research is to investigate the contents of their minds, then this will not seem a drawback. Seeing humans as inextricably social creatures means examining them *in situ*, and it means regarding the content and function of their minds as a social, interactive, and to some extent a collective phenomenon. It also means that a great variety of people and their use of language can be studied (hence the label 'variation studies' for certain aspects of sociolinguistics). This is again 'political' in that it contrasts with the perception that most university-employed linguists are white, middle-class men; and if their primary source of material is their own intuitions about language, then the language itself will be heavily focussed on that one dialect.

Figueroa's analysis of the philosophy of sociolinguistics is guided by this political and epistemological divide. The most numerous references to philosophers from previous centuries come at the beginning of the book, where she analyses two opposing 'large-scale world views or philosophical-cultural frameworks which have remained quite consistent over the time-span of what one might call the Western intellectual tradition' (1994:19). These are the 'Cartesian' and the 'Hegelian' frameworks, as proposed by Markova (1982). These 'frameworks' are characterised as follows:

Cartesian framework	**Hegelian Framework**
Nature of mind is individualistic.	Nature of mind is social.
Mind is static and passive in acquisition of knowledge.	Mind is dynamic and active in acquisition of knowledge.
Knowledge is acquired through algorithms.	Knowledge is acquired through a 'circle returning within itself'.
The criterion of knowledge is external.	The criterion of knowledge is internal.

(Figueroa 1994:19-20, after Markova 1982:6)

Figueroa is careful to qualify these 'frameworks', which are supposed to represent two basic and opposing conceptions of mind in the Western philosophical tradition:

> It is unfortunate that Markova chose to name the two frameworks Cartesian and Hegelian since [...] one can find great disagreement as to what either men [sic] really stood for. Markova's frameworks are adopted in this study as representing real divisions in Western thought, but with the caveat that no claims are being made about either Descartes or Hegel [...] (1994:29n)

This raises the question of why these labels are used at all. Of course, Figueroa is using other people's labels (Chomsky's and Markova's), and giving her own caveat. However, she says (I believe correctly) that they 'represent real divisions in western thought'. Whether or not scholars of Descartes and Hegel would accept these labels, the linguists who use them certainly see the distinctions as valid, and tie their linguistic theories to their epistemological heritages. The gap between the label and the intent is notably mirrored in explanation of the Cartesian nature of Cartesian linguistics:

> Chomsky's opening hypothesis in *Cartesian Linguistics* is that contemporary linguistics had lost touch with an earlier European tradition of linguistic studies, which he identified as Cartesian The term "Cartesian" is not used here according to its generally accepted definition. Chomsky extends that definition to encompass, as he puts it, "a certain collection of ideas which were not expressed by Descartes, [were] rejected by followers of Descartes, and many first expressed by anti-Cartesians"(1998:105-6)

If the Cartesian framework is not representative of Descartes, and the Hegelian one not representative of Hegel, then this is a schematic division[126], two convenient labels to represent 'real divisions in western thought'. However, the associations of 'Cartesian' with 'Chomskyan' are strong, and there is little doubt that by employing this word Figueroa is deliberately associating a mistaken epistemology (Cartesian) with a mistakenly-founded discipline (TGG), and attributing TGG's mistakes to its epistemological foundations.

In setting up the opposition, Figueroa does formulate a clear division between ways of thinking. The combination of the 'internal' versus 'external' criteria for knowledge, and the individual versus social views of mind, represents one of the basic problems which Western thought has dealt with repeatedly. 'Hegelian' might seem like an acceptable, if vaguely used, alternative to 'Cartesian', one which embodies everything which generative

[126] In the context, this is not necessarily a problem, after all, Kuhn's entire theory is self-confessedly schematic (1962:xi).

grammar does not offer, such as a social attitude towards knowledge acquisition, amongst other things.[127]

The terms 'Hegelian' and 'Cartesian' might more usefully be called 'Formalist' and 'Functionalist', and Figueroa goes on to use these terms in the ensuing discussion (1994:21). 'Formalist' and 'Functionalist' are more neutral terms, in that they do not beg the question of whether a particular author meant to say what the interpreter takes them to say, and they are self-descriptive. A Formalist studies the form of language, a Functionalist studies the function of language.

Given that the terms 'Formalist' and 'Functionalist' seem to be more apt for the distinction which Figueroa is making, and are readily available, we are again drawn to the question of why there was ever any need to introduce (and implicitly endorse) the Cartesian-Hegelian distinction. It would be arguable that Hegel is being presented as an alternative to 'Cartesian' thought because he is sometimes seen as a forerunner of Marx (Popper 1963:333, Hollis 1994:71), and consequently the egalitarian, anti-establishment line of thinking which stretches through the 20th century. This would contrast with Descartes and the scientific establishment which has dominated Western (and especially capitalist) history. Of course, this directly contradicts what Sampson says about freedom and Locke.[128] Certainly, sociolinguistics has strong currents of left-wing and egalitarian representation in its sub-disciplines (see, e.g. Mesthrie *et al.* (2000:30-32) for a brief overview of Marxist sociolinguistics, and (ibid:213-241) for an entire chapter about sociolinguistics and gender. There is no such thing as Marxist or feminist transformational generative grammar).

[127] There is a further problem with drawing up these particular battle lines. Hegel and Descartes may in some sense be considered adversaries, but for some they are (loosely) part of the same Continental tradition (Popper 1963:324).
[128] And see Hymes (1974:25) for an alternative view.

Figueroa's use of Hegel and Descartes emphasises the political, epistemological and methodological gulf between sociolinguistics as it ought to be practised, and 'received linguistics' (for which read 'Chomskyan' or 'TGG') as it is in fact practised. It also illustrates that the ideological split between Formalist and Functionalist approaches to language is not limited to modern linguistics, and is not a new phenomenon. The Cartesian-Hegelian split is not as commonly recognised as the more usual Rationalist-Empiricist split, which does not seem to apply to Figueroa's debate, as there is nothing in, say, Locke's work which commends it to a Functionalist point of view. However, it helps Figueroa to show that she is dealing with more than a localised affair, as it makes the claim that, just as TGG claims to belong to a centuries-old philosophical position, so does sociolinguistics; and it helps to unite the various diverse strands of sociolinguistics under a single metatheoretical banner.

Figueroa's work will be addressed again in the next chapter. In this section I have argued that, perhaps more explicitly than anyone else, she regards TGG and sociolinguistics as instantiating two different paradigms, and expresses it in Kuhnian terms. This entails not just different areas of study, but different conceptions of the object of study, different conceptions of how to study it, and different epistemological traditions with which to back up these stances. In short, incommensurable approaches to the study of language.

3.4 Yngve's Position

The argument that linguistics is on the wrong path because of its methodological commitments does not have to focus on the Rationalist-Empiricist split. In this section I look at the work of Victor Yngve. Yngve trained as a physicist, and then worked on the machine translation project at MIT in the 1950s and early 1960s. An iconoclastic linguist, he never joined the Chomskyan programme, and his 1996 book *From Grammar to Science* explains how all linguistic enquiry, from Plato to Chomsky, rests on a fundamental misunderstanding of the nature of language, and how, consequently, linguistic enquiry has completely failed to be at all scientific. He has developed and expanded on those ideas in the self-explanatory *Hard-Science Linguistics* (2006).

Yngve's argument is that *all* linguistics is misguided, and has been for well over two thousand years. His primary epistemological thesis is that language was wrongly classified by the ancient Greeks, and that it has remained wrongly classified ever since. The Stoics divided philosophy into the physical, the logical and the ethical. Language was categorised as part of logical, not physical, philosophy, and has remained stuck there ever since. This has led to students of language not looking at the 'real-world' nature of language, which in turn leads to language not being studied scientifically. He goes on to make his position clear on most of what has been considered linguistics up to now. After mentioning ten or twenty different viewpoints on what language is[129] and how it should be studied, he concludes:

> [T]here seems to be no scientific way of deciding among the many contenders or among the various ways they propose for analyzing linguistic materials. Instead we find positions and methods being promoted like a new movie or defended with withering polemics or taken up like the latest fad [...]. This is not what one would expect to find in a science; it is more like literature, philosophy, politics or religion, which do not pretend to be scientific. (1996:11-12)

Yngve takes it for granted that linguistics ought to be a science, and this is entirely consistent with his premise that language should be seen as a physical phenomenon in the natural world. He also takes it for granted

[129] These include 'a natural phenomenon, the object of a science, a type of faculty, a type of module, a type of stuff, a type of system' (1996:10) and many others, recognisably connected to various linguistic theories.

that, if language were being studied from the correct point of view, then there would be a 'scientific way of deciding among the many contenders or among the various ways they propose for analyzing linguistic materials'.

Yngve's argument is about language and linguistics in general, but naturally any criticism of 'linguistics' will take in Chomsky at some point. Yngve does not always single out Chomsky. However, when he does so, he uses the same argument that he uses against all other types of linguistics, which is that our hopelessly confused metaphysical conception of language has made it impossible to study language scientifically:

> Noam Chomsky in his recent publications begins with a number of implicit and explicit assumptions for which he provides no scientific justification. In fact they cannot be scientifically justified and are probably all false. (1997:8n)

This is fairly strong stuff, and is representative of Yngve's views in general (see also Yngve 1996:39-45, and Yngve and Wasik 2006:xi).

Yngve does not dwell overly long on the history of philosophy, or its relationship with linguistics. He is not exactly dismissive of philosophy, but the whole thrust of his argument rests on the distinction between the logical domain (logic, mathematics, etc.) and the physical domain (physics, chemistry, etc.). In trying to reposition the study of language in the physical (and therefore scientific) domain, it is necessary to abandon the logical-philosophical tradition which has mistakenly taken in the study of language in the past, and for this reason Yngve has little inclination to lean on old philosophers for support. When they do occur in his argument, it is usually as an example of the mistakes of the past. For example:

> Chomsky [...] like Descartes, would muddy the distinction between science and philosophy. (1996:69)

He takes a similar line with Locke:

> These [Yngve's] methods [...] promise to bring into the realm of science things that have been discussed since at least as early as when John Locke wrote on the association of ideas in his *Essay Concerning Human Understanding* (1690), and that have often been approached through speculation, intuition and introspection. (1996:288)

Yngve's arguments about studying language as part of the natural world echo other anti-Rationalist ideas about making the study of language more empirical, such as Labov's statement, quoted in chapter three, that

> When we study what people do rather than what they think they do, we get a much simpler and more understandable view of the linguistic system. (Labov 1989:53, quoted in Figueroa 1994:99)

Yngve does not argue that linguistics ought to follow an Empiricist epistemology, as Sampson does, but there is nevertheless a parallel desire to free linguistics from 'speculation, intuition and introspection'.

Descartes, along with Bacon and Galileo, receives some lukewarm praise for his role in leading Western society towards a rigorous conception of science:

> Francis Bacon, Rene Descartes, and Galileo Galilei, emphasized that one must begin by doubting received opinion. (1996:21)

However:

> Bacon's thought [...] would have us rely too much on the blind collection of data [...] Descartes's science was flawed in that it relied too heavily on intuition [...] Of these three, Galileo's view has been most influential in the development of science. (ibid.)

In other words, we learn from Descartes' mistakes rather than by taking a more positive reading of his work. This might sum up Yngve's general approach to philosophy: it has been useful in the past, and helped in the development of science, but the two should not be confused. Logic does not lead to the discovery of facts about the natural world.

In chapter three, we noted Newmeyer's approving description of 'Saussure's great insight that at the heart of language lies a structured interrelationship of elements characterizable as an autonomous system' (1986b:21). Saussure's 'great insight' was based on exactly the same dichotomy between the mental and the physical as Yngve is interested in. However, Saussure explicitly places language within the mental rather than the physical domain (1974 [1916]:8). Yngve congratulates Saussure on noting the distinction, but goes on to criticise him:

> Saussure did not follow up on this crucial insight, but he did worry about it [...] He even exclaimed that the illusion of things naturally given in language is profound. The illusion certainly is profound [...] But Saussure was right. It is an illusion. (1996:30)

Yngve's analysis of Saussure's insight, and failure to follow it up, locates TGG firmly within the Saussurean structuralist tradition, and that tradition within a larger tradition stretching back to the Stoics. On this reading, there are two paradigms: the old one and Yngve's.

Yngve's whole thesis is based on a metatheoretical assumption – that language is part of the 'physical domain', not the 'mental domain'. It is this (and only this) which leads him to reject modern linguistics. So while philosophy cannot be used constructively as a basis from which scientific knowledge develops, it can be used destructively, to demonstrate the bankruptcy of so-called 'sciences' which are based on mistaken philosophical assumptions. Yngve uses the Stoic mental-physical distinction to *explain* what he sees as the hopeless state of modern linguistics, but to make it a science, he must turn to scientific practice and not the philosophy of science.

Yngve's distrust of philosophy as a way of achieving any practical results becomes even clearer later, when he gives the reader some recommendations about how to best do science.

> In describing the criteria, assumptions and methods of science here, my aim is not to be prescriptive but simply to characterize the best practice of scientists. In doing this I am definitely not following the lead of any philosophers of science, although some philosophers of science may well agree with the description given here. Rather, I am laying out my own understanding of how science operates learned during the course of my training and experience in physics. (1996:94)

In a footnote to this passage he gives an outline of his training in physics at the University of Chicago (1996:320n). It is central to his thesis that he knows how to do science through professional training and practice. Rather than relying on 'philosophers of science', who do not actually practise science, he relies on his own experience of the 'hard sciences'. This could potentially lead to the accusation that he has no definition of 'science', only a set of methods which he has accepted unquestioningly from his teachers and colleagues.[130] Without some kind of definition or method, 'science' might sound tautologous to the outsider. 'Science is what scientists do' might sound unconvincing, just as 'art is what artists do' is unlikely to convince a sceptic about modern art. On the other hand, this does not stop it being an accurate description of the nature of science.

Interestingly, he does not see the definition of 'science' as problematic at all. He goes further, saying that science does not need the blessing of a

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[130] Later in this section I will discuss how this analysis of science could be seen as 'Kuhnian'.

philosophical or non-circular definition, as it has always done well enough without one. Furthermore, he has little interest in convincing non-scientists that this is the case:

> I don't believe that anyone with extensive training in science would take exception to the characterization of science given here. It's quite standard and universally accepted. (1996:94)

However, he footnotes this passage with the caveat that this characterisation of science is

> Universally accepted, that is, in the more highly developed sciences [...] Readers who happen to be familiar with the views of Noam Chomsky should be cautioned that they cannot rely on his writings for an understanding of science. His work is basically in the logical domain and is rationalistic and philosophical in its outlook and method. It is unfortunate that in its rhetoric it makes repeated claims to be scientific and as a result many linguistics students have been misled into erroneous views of science. (1996:320n)

Again, this passage explicitly contrasts the 'rationalistic' and 'philosophical' with the 'scientific'. Yngve is not conflating the terms 'rationalistic' and 'philosophical', merely suggesting that Chomsky is both, and that both contrast with 'scientific' in non-overlapping ways.

Yngve does give a positive account of how science functions[131]. This has two parts, the first of which is little more than a description of common sense, as practised by anyone trying to gain firm knowledge about the world – and in particular by detectives, the analogy which Yngve uses to show the non-mystical nature of scientific practice:

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[131] On top of explaining how science in general ought to be done, he has continued to develop what he claims to be a linguistics practised according to his recommendations. He gives an extensive account of how this has been done, along with a collection of papers in this vein by practising linguists, in *Hard-Science Linguistics* (2006).

> The conduct of science is not a matter of following a cut-and-dried prescription of 'scientific method' despite what some logicians, philosophers of science, and elementary textbooks have claimed. It requires [...] an optimistic 'can do' attitude, creative imagination, alertness to the smallest clues, willingness to question received opinion, boldness in forming hypotheses and following leads, and sometimes dogged perseverance against repeated setbacks. (1996:96)[132]

In the second part of his description of science, Yngve gives four 'assumptions' which are central to the successful practice of science. These are: the 'ontological assumption, that there actually is a real world out there to be studied'; the 'regularity assumption, that the real world is coherent so we have a chance of finding out something about it'; the 'rationality assumption, that we can reach valid conclusions by reasoning from valid premises'; and the 'causality assumption, that observed effects flow from immediate real-world causes' (1996:101-102). These assumptions are, as noted above, 'common sense', in that they accord with how we go about our daily lives. Just as Sampson refers to babies as 'little research scientists' (1997:17), Yngve implies that we carry out many of our actions in a 'scientific' way.

The success rate of this approach in everyday life extends to more complicated scientific discovery as well:

> These are the standard assumptions of all science. Although I have represented them as assumptions taken for granted and assumed true without evidence or proof, there actually is good reason to accept them as a foundation for science. Not only do they accord with common sense, but more important, they have worked in science. (1996:103)

Yngve finishes his discussion of the scientific method by giving two reasons why we positively ought not to read philosophers of science if we are

[132] This has, presumably coincidental, Feyerabendian overtones – see chapter two for discussion of Feyerabend's 'anything goes' philosophy of science.

to become responsible practitioners of the scientific study of language. First, referring back to philosophy will only prolong the confusion which Yngve is trying to dispel:

> It would not be appropriate to consult philosophers or the philosophy of science to learn about science, and I recommend against it. Consult scientists and the literature of science instead. One reason is that linguistics, in trying to become scientific, must break away from philosophy. Although linguistics has its ancient roots in philosophy and owes much to philosophy, philosophy is not science. The literature of philosophy contains much critical analysis and opinion about science, but it is not designed to teach one to be a scientist. (1996:105)

Second, philosophy is not like science. Its practitioners cannot always be trusted as they have ulterior motives, which scientists cannot have if their efforts are to 'work':

> The only way to tell which philosopher if any to believe about science when they differ is to have studied science firsthand oneself. Philosophical writings often have particular philosophical axes to grind that are of little concern to us and may even prove destructively confusing. (1996:106)

Yngve is very clear about what makes good science and what doesn't. To be a scientist is to have practised science. Philosophers of science are simply irrelevant.

Without a conscious definition of science, Yngve could be open to the accusation that he advocates the Kuhnian 'sheep-like' behaviour which so many people found unappealing in Kuhn's characterisation of science (see chapter two for extensive discussion of this point). However, there is a way out of the circle, which might be described as 'science is what scientists do, and what scientists do is science'. The 'way out' is the functionality of science; in other words, as he bluntly puts it, 'it has worked' in the past, and it is only rational to expect it to work in the future. Philosophy of science doesn't 'work' on any practical level. So rather than the circular definition 'science is what scientists do', the definition is 'science is science because it works'[133]. This is, however, akin to Kuhn's analysis of science. Rather than lay down theoretical tenets for the successful practice of science, he examines the behaviour of practitioners of successful science. Kuhn's philosophy of science, then, is identical to Yngve's rejection of philosophy of science.

To conclude, apart from phonetics, the object of study of most linguistics is signs, symbols and other abstract objects, and this disparity leads to Yngve's radical critique of all previous study of language, tracing it back to the Stoics' distinction between the physical and the logical domains, and their mistake in including language in the logical, not the physical, domain. The distinction rests on one basic epistemological difference:

> Efforts in philosophy to bring the methods of science to bear in the logical domain are clearly misdirected, as most philosophers realize, as the efforts to redefine science into something that would also study the logical domain. Such a move would deny the distinction between invented objects and real objects. (Yngve 1996:93)

In adopting this distinction, wherein abstract entities must be studied in one way (logically), and 'non-invented' entities studied in a physical way, Yngve tries to remove all ontological and epistemological controversy concerning language at a stroke. Language, he says, does *not* belong in the logical domain like mathematics: it is physical, in that it deals with real physical occurrences. As such, these real things must be studied in a scientific way. What is not possible is to make the category mistake of studying language as part of the logical domain, while at the same time trying to apply the methods of the physical sciences to this domain.

[133] Or, the proof of the pudding is in the eating; or, 'by their fruits ye shall know them' (Matthew 7:20).

If Yngve is right, and language has been misconstrued, then only phoneticians have been doing anything remotely right. Everyone else has been putting square pegs in round holes, or perhaps square pegs in non-existent round holes. The crucial, and basic, division is between 'invented objects' and 'real objects'. For Yngve, the domain of 'real objects' would have to include patterns of airwaves, or acoustic disturbances, and neural activity. It would *not* include words, phonemes, nouns, meanings and transformational rules.

Of course, words, phonemes, nouns, meanings and transformational rules are exactly what have been studied for the last couple of hundred years by linguists. If Yngve is right, they should have run into some significant problems, beholden as they are to a fundamental category mistake.

This concurs with the view that the fundamental difference between linguistics (and other social/human sciences), on the one hand, and natural sciences on the other, is the provisional nature of what is studied. All the 'types' of things to be studied are postulated, not observed, e.g. utterance, phoneme, meaning, etc. Things which take place can be interpreted as one thing or another. This is exactly what Yngve identifies as a mistake. Postulation is free – anyone can postulate anything. The fate of old postulates is instructive, though. Physical postulates are either confirmed or not, and things like phlogiston or the ether are disconfirmed. This does not happen with mental postulates; instead, they fall into disuse (recall Jackendoff's reference to "disillusioned Kuhnian debris" left in Chomsky's wake, from chapter two). Similarly, different postulates can be invoked to account for the same phenomenon, with, in the worst case scenario, no proof able to demonstrate one's superiority over the other. If Yngve's analysis of the situation is correct, then this is exactly what leads to incommensurability between disciplines which studied provisionally posited mental items.

There are two ways to see Yngve's linguistics, especially within the context of this thesis. First, we could view his brand of linguistics as espousing a different epistemology, and therefore a different approach to the object of study, from other schools of linguistics. Or we could see him as he sees himself, and view him as outside both traditional linguistics and philosophy of science. Either way, his epistemology leads to incommensurable views over the object of study and the best way to go about it.

3.5 Sampson, Figueroa and Yngve

This section has shown that different writers have made different uses of the classical and ancient philosophers to make epistemological attacks on the current practice of linguistics.

Yngve and Sampson make inverse uses of the history of philosophy. Sampson uses empirical data and argument to attempt to prove his epistemological standpoint (or, why Rationalism must be wrong). Yngve reverses this, using a philosophical argument to dispose of an 'empirical science' (Chomskyan linguistics, and any other type of modern linguistics with scientific claims), which he sees as fundamentally flawed, while simultaneously decrying the philosophy of science in all its forms.

Figueroa grants equal status to the current theory and the historical roots, but she is in some sense preaching to the choir. Her book is not really aimed at converting TGG linguistics, more at examining and explaining the historical roots of sociolinguistics to its practitioners.

For Yngve, philosophy is literally pre-theoretical, in a chronological sense. Once a phenomenon has been correctly isolated and identified, and a scientific method of systematically studying the phenomenon at hand has been established, then there is no further need for philosophy. Yngve does not disparage philosophy *in toto*, but puts it firmly in its place, and that place is not within science.

Sampson takes a more holistic approach to the matter. Although philosophy and linguistics are separate disciplines, the *idea* of science bridges empirical science and Empiricist philosophy. We can do science because we are naturally scientists. Without an Empiricist conception of the human mind, we are unable to 'know' anything about the world – 'know' in Sampson's characterisation of knowledge as 'the totality of guesses which we have put up for potential refutation and which we have not yet succeeded in refuting' (1997:16).

Yngve, as we have seen, shares this conception of the human mind. His description of science is a refined variation on common sense. However, he places little weight on grinding 'philosophical axes'. His epistemological views are little more than an illustration of how humans can go about discovering the world, or doing science. It is the mirror of Sampson's contention that science is a specialized application of human epistemic capabilities.

Figueroa concentrates on each aspect equally. She assumes that an incorrect epistemology will accompany an incorrect approach to studying language empirically, and that a correct approach to the empirical study of language will accompany a correct epistemology. However, she does not use one to prove the other. Rather, she presents the two sides as inevitably opposed on both the theoretical and the metatheoretical level:

> To state the obvious, therefore, sociolinguistics on a metatheoretical level is not well served by the Cartesian framework nor is it part of the formalist linguistic paradigm. (1994:27)

This accords slightly with the comment of Sampson's quoted earlier in this chapter:

> [T]he issue between the two philosophies [*characterised in this piece as between Empiricism and Hermeneutics*] is not to be settled by rational debate, since what counts as rational debate is very much part of the issue. Whether one is an Empiricist or [a Rationalist] must be a matter of faith. (1976:963-4)

Here Sampson is talking about a kind of meta-epistemology, which precedes debate on which depiction of the mind is correct. Obviously you have to start somewhere, and there is an aspect of this debate which does demand a kind of faith, or at least an intuition that you are on the right side. Having bought into a 'side', consistency demands that you adopt the concomitant view of language (or epistemology, depending on where you enter the debate).

However, this contrasts to an extent with Sampson's insistence throughout *Educating Eve* that the nature of mind is an empirical matter – that is, a debate which ought to be settled with empirical evidence. He often repeats the assertion that Chomsky agrees with him on this. By employing empirical data in his arguments for his Rationalistic view of mind, Chomsky implicitly concedes that the Rationalist-Empiricist debate is an empirical, not a conceptual, debate:

> Chomsky does not normally claim that his own view of language as a 'biological organ' is the only view which is logically coherent [...] Contingent facts cannot be evidence for or against a logical truism. So, by putting forward empirical observations in support of his own view of the language acquisition process [*the specific focus of this passage, but the argument is applicable to a more general theme*], Chomsky implicitly concedes us the right to construct an alternative account [...] (1997:26)

This apparent contradiction need not be seen as particularly damaging to Sampson's case. First, people change their minds, and Sampson is entitled to do this over the twenty years which separate these two works. More importantly, there is a subtle difference regarding what can be viewed as an empirical debate and what is a matter of intuition, or 'faith'. Choosing between, say, Empiricism and Hermeneutics, with regard to the human sciences, as Sampson discusses above (1976:963-4), is partially a matter of faith because it turns on what logic you are prepared to accept, and this is not something which can be argued with empirical evidence.

So Figueroa may be broadly correct in the implication that a certain view of the mind will tend to accompany a certain view of the nature of language. For example, those who see language as an innate and universal human capacity will probably see it as part of a network of other innate capacities. However, this does not preclude debate based on empirical data. Salient facts or experiments should force a partial revision of whichever philosophical position has been taken, although this will most likely be a partial revision.

As I explained at the beginning of this chapter, Kuhn describes the emergence of paradigms not just in theoretical terms, but also in terms of group membership. He goes on to say:

> At the start a new candidate for paradigm may have few supporters, and on occasion the supporters' motives may be suspect. (1962:159)

Of the three writers described in this section, none wants to look dominant; they prefer to portray themselves as put-upon minorities, struggling against an intransigent and bullying majority (e.g. Figueroa (1994:9); Sampson (1997:11)). In line with the quotation from Kuhn above, we can add that this majority is also likely to be characterised as either mistaken or wilfully misleading. In this case that majority is, of course, the Chomskyan one.

All three writers present TGG as the dominant paradigm, but this carries the implication that it will, under Kuhn's account, become redundant, as all paradigms must – although Kuhn does not provide histories of revolutions which swing *back* to tenets held by previous paradigms. Without the institutional advantages which come with being the dominant paradigm, all they have is the reasonableness of their arguments, and it is a paradox of Kuhn's philosophy that they can appear both out-of-date and present themselves as the future of their discipline at the same time. This suggests (and I think rightly) that the motive for Sampson's and Yngve's books is to cause a scientific revolution. I have already examined how Figueroa's engagement with Kuhn is more nuanced than aiming for a straightforward revolution.

However, there is a problem, which I have already touched on. Of the beginning of paradigms, Kuhn says:

> If a paradigm is ever to triumph it must gain some first supporters, men who will develop it to the point where hardheaded arguments can be produced and multiplied [...] Because scientists are reasonable men, one or another argument will ultimately persuade many of them. But there is no single argument that can or should persuade them all. Rather than a single group conversion, what occurs is an increasing shift in the distribution of professional allegiances. (1962:158)

This 'increasing shift in the distribution of professional allegiances', in essence, attaining a majority, is of course something that both Yngve and Sampson have failed to do, and it leads on to the second point about Kuhn. When a paradigm is established, and 'normal science' is the order of the day, being in the majority allows you to dismiss opponents, especially individual ones, as cranks.

In practice, what we see is that starting from a historical argument rarely converts people. Kuhn was right that what attracts people to a new paradigm is puzzle-solving ability, and the mass-movement of one's peers. Yngve and Sampson did not attract many disciples. Figueroa and Chomsky only enlist the grandfathers into an already established approach to linguistics.

Conclusion to chapter four

In this chapter I have shown that the practice of claiming kinship with older philosophers and philosophical traditions has played a significant part in the development of metatheoretical debates about the validity or otherwise of TGG and its methodological approaches to linguistic investigation. Kuhn gives an account of the appropriation of such philosophers in the myth-making aspects of paradigm formation, although his account is underdeveloped. His main point is that any nascent paradigm needs to write its own history, although exactly how it does this can vary.

Metatheoretical arguments over the nature of language and the most appropriate or fruitful way to study it can take many forms, whether defensive or attacking. While, on the whole, any serious empirical enquiry should offer first of all positive arguments in its own favour (that is, showing how it obtains results from scientifically valid premises and procedure), sometimes it will be necessary to resort to meta-analysis of these procedures. More than many disciplines, linguistics suffers from serious disagreement over the ontological status of its subject matter (language), and most types of linguistic enquiry will at some time be forced to defend their choice both of philosophical assumptions and the methodology used to address this ontological problem.

While the majority of such disputes will be methodological (that is, bearing on the philosophy of science and the appropriate way to analyse the data in question), sometimes it becomes necessary (or at least possible) to reach back further into the history of philosophy and co-opt major figures as support for an ontological or epistemological position. These 'grandfathers' (in Harris' felicitous and not entirely sarcastic phrase (1993:17)) provide gravitas and moral support. Writers who find themselves swimming against the tide of received academic opinion can at least find company in the past, where there will nearly always be a seventeenth- or eighteenth-century philosopher who held similar views.

We saw in chapter three how much effort was, at one stage at least, put into history-writing which stressed the paradigmatic nature of TGG according to Kuhn's model. In this chapter I have illustrated just how much Chomsky has appropriated Descartes and other 'Cartesians' to his cause, a practice which reinforces the appearance of a Kuhnian paradigm. I have also shown how much effort has been put into refuting Chomsky's claims to such a heritage, both from inside linguistics and outside it. Both sides of this argument fulfil Kuhn's description of how paradigms are formed, although the Kuhnian nature of the argument is likely to have been largely unconscious, as this is not such a well-known aspect of Kuhn's philosophy of science.[134] This nevertheless gives credence to the idea, to be developed in the next chapter, that we can show different types of linguistics to have incommensurable vocabularies, based on their incommensurable epistemological commitments.

[134] Of course we should be wary of the Kuhnian fallacy, which I have tried to stress throughout this dissertation. Fulfilling the sociological, historical or institutional facets of a paradigm does not in itself make a science. The account I have given in this chapter shows TGG fulfilling Kuhn's criteria for the outward appearance of a paradigm; it also shows other forms of linguistics either arguing against such claims from TGG, or making such claims on behalf of their own discipline. However, such outward criteria do not make a Kuhnian paradigm – they are the symptoms rather than the cause.

This is not to criticise any of the participants in the debate. I have already noted that, unless explicitly stated, I assume their good faith in making these points. What this and the last chapter show is that there is a strong correlation between Kuhn's account and the development of modern linguistics, sometimes noted and exploited by TGG linguists, sometimes not.

In the next chapter I will show how these philosophical disagreements about the intellectual forebears of different strands of linguistics tally with the developments of incommensurable technical vocabularies, which have come to be the source of significant disagreements. In this section I have talked about three different anti-Cartesian schools which are incommensurable with TGG. The next chapter focuses on just one of these – sociolinguistics.

Chapter five: incommensurability and its roots, and the solution provided by my theory of reference

> One of the more lasting points to be taken from Kuhn's work on paradigm differences is the incompatible nature of competing paradigms. Kuhn points that there can be no real dialogue between competing paradigms because, to put it colloquially, each side is missing the point of the other side. Though they might seem to be speaking the same "language", they are not talking about the same thing. The logical progression of argument in one paradigm is irrelevant or nonsensical in another because it is based on assumptions which are not held by, or are rejected, by the other paradigm.
>
> This is important to keep in mind given the often contentiousness of differing positions held by linguists who are arguing from completely different starting points and therefore have very little, if any, common ground. Rather than insisting that there be only one authentic way of doing linguistics, or that there be a scalar hierarchy of more to less linguistic, it is more accurate to admit genuine diversity based on differences. (Figueroa 1994:27-8)

In this chapter I draw together all the strands of my thesis. First I recapitulate the disagreements described in chapters three and four, and the broad outline of the opposing camps (part one). Next I show that the vocabularies as used in those arguments are incommensurable with each other, and show how those vocabularies are interlinked in the way that Kuhn described incommensurable vocabularies (part two). In part three I show that this incommensurability can be explained by the theory of reference which I outlined in chapter one.

I will do this by showing how the varying sides in the various arguments can be broadly seen to coalesce around two sets of ideas, which have been so far exemplified by the writings of Chomsky – extensively supported by, amongst others, Newmeyer and Smith – on the TGG side, and the sociolinguistics of Hymes, Gumperz, Labov and Figueroa on the other. I concentrate on these sets of writers (as opposed to others such as Sampson and Yngve, who have been discussed at length in previous chapters) because I believe they represent the most convincing examples of Kuhnian paradigms among all the works which I have so far mentioned. TGG is fairly self-conscious of its status, and I will not discuss that any further. Sociolinguistics, underpinned by what Figueroa referred to as the 'Hegelian tradition', is represented by Hymes, Gumperz, Labov and Figueroa. In this chapter I also look in some detail at the writings of Bucholtz and Hall, who have updated the Hymesian tradition; and *Sociolinguistic Theory* (1995/2003) by J.K. Chambers and *An Introduction to Sociolinguistics* (1986/2006) by Ronald Wardhaugh. Simon Dik's theory of functional grammar[135] is also addressed, as a consequence of the amount Figueroa references it. I noted in the previous chapters that Figueroa draws a division between Labov, on the one hand, and Hymes and Gumperz on the other, on the grounds that the latter pair aim for a linguistics of particularity (Figueroa's preferred approach). The works of Mary Bucholtz and Kira Hall clearly fall into this Hymes-Gumperz tradition (and not the Labovian one). However, this difference is not, I believe, a fundamental epistemological or ontological one. It is a methodological choice about how to do linguistics, but the division nevertheless warrants the broader grouping as epistemologically opposed to TGG. Wardhaugh and Chambers have been chosen for several reasons. First, they engage with the metatheory under discussion. Second, both of their books are textbooks aimed at undergraduate students, and they are paradigmatic in two different senses. They describe and represent the paradigm, in that they present the field as it is, and include the major exemplars (Labov in NYC, Trudgill in Norwich, Dorian in Scotland etc.). We might refer to this as unconscious paradig-

[135] See below for further discussion of functional and formal approaches to grammar.

maticity. However, they also build and proclaim the paradigm as healthy, vibrant and mature. For example, Chambers begins:

> This book is about language variation and its social significance. By now, the research literature on this topic [...] amounts to a formidable accumulation. It includes, by any reasonable yardstick, some of the most incisive discoveries in the long history of humanity's inquiries into the structure and function of language. (2003: 1).

We might refer to this as self-conscious paradigmaticity.

1.0 A review of the arguments from chapters three and four

In chapters three and four I looked at two areas of disagreement between TGG and its opponents. The first was over the scientific nature (or otherwise) of the way linguistics is done, and the paradigmatic (or otherwise) nature of the study of language. The second concerned arguments about the correct philosophical basis for studying language.

In this section I will review these arguments, with a view to setting up the next section, where I demonstrate the incommensurability that underlies these arguments.

Science: The science debates subdivide into three questions. First, whether or not it is possible to study linguistics scientifically, or in 'the Galilean style' as Chomsky puts it, and which Putnam denies. Second, whether or not the methods used in TGG properly belong to the natural sciences, and whether or not any other approach to linguistics can be considered more scientific. The third controversy is over whether or not TGG follows its own self-proclaimed standards of scientific practice, which

Postal (and others such as Pullum and Trask – see chapter three) forcefully deny.

The first argument is mostly based around Chomsky and Putnam, and obviously there is no suggestion that Putnam is a sociolinguist (or any kind of linguist for that matter). Although it is a radical departure from orthodoxy to equate Putnam with sociocultural views of language, what they have in common is an approach to language which emphasises its fluidity, unpredictability and ephemerality. For Putnam, the intentional and arbitrary nature of language means that it cannot be studied scientifically, while for writers such as Bucholtz and Hall, the emergent nature of language requires that it be observed *in situ* (2005:586-8). In both of their approaches the fundamental facts of language are intimately bound up with idiosyncratic aspects of human behaviour, and cannot therefore reveal ultimate immutable theories, as is usually expected in the natural sciences (also Figueroa (1994:170-2) on the 'linguistics of particularity'). Chomsky of course rejects this.

The arguments about whether or not the methods in TGG disqualify it as a natural science pit Chomsky, and Neil Smith in his defence, against Hymes, Labov, Figueroa, Snow and Meijer, and Greenbaum. Smith argues that, as a branch of psychology, there is nothing untoward about the use of intuitions in linguistics, and Chomsky argues that we can idealise away some aspects of language, just as astronomers can sometimes view planets as mathematical points rather than lumps of rock or gas.

Where Snow and Meijer and Greenbaum argue that the practice of examining intuitions is in itself fundamentally suspect (backed up exhaustively by Schütze), Hymes and Figueroa aim to give an alternative account of the most fruitful or realistic framework for studying language, one which looks at the way that language is used by real humans in real situations. This account makes little or no reference to speaker intuitions about which sentences are grammatically acceptable. Instead, they focus on what people say, and the situations in which they say them. The theory is independent of the informants; sociolinguists don't ask their subjects 'why did you say that?', or 'is this sentence correct?'

The third set of arguments concerned whether or not TGG follows its own self-proclaimed standards of scientific practice, forcefully denied by Postal; they are less germane to this chapter then the first two. This is because incommensurability across disciplines does not depend on those disciplines following correct scientific procedure, but rather depends on them formulating theories in the manner of the natural sciences. For this reason, it does not matter whether they are the finest, most honest form of scientific enquiry, or the worst form of pseudoscience.

Paradigms: The key argument concerning paradigms is whether or not TGG ought to be considered a paradigm, as they are described by Kuhn. The ancillary arguments are over whether or not any other type of linguistics is a paradigm, and whether it is a useful term or concept for describing the study of language; whether or not TGG can be considered 'revolutionary' (in a Kuhnian sense or otherwise); whether or not historians partial to TGG have misused or misinterpreted Kuhn; and what effect all of this has on the status of TGG and any other type of linguistics as sciences.

These debates are harder to break down in terms of two opposing sides. This is partly because some of the contributors, such as Koerner, have altered their positions over time. It is also because those who oppose the view that TGG is a Kuhnian paradigm do so from a variety of positions and motivations: some are practitioners of rival forms of linguistics (Hymes and Figueroa, Sampson to some extent), while others, such as Murray, Koerner and Matthews, write primarily as historians of linguistics and are less interested in whether or not TGG is the (or a) correct way of studying linguistics, instead seeing the whole issue as either propaganda or unnecessary obfuscation.

Rationalism and Empiricism: The arguments over the epistemological inheritance of contrasting forms of linguistics are simpler. This is in part due to the extensive and consistent work done on this by Chomsky (and backed up by Chomskyans such as Smith). It is also because those who engage in the debate tend to do so in a fairly black-and-white fashion. Either Rationalism or Empiricism is the correct epistemological basis for the study of language, according to these arguments, and there is not much middle ground (although, as we saw in the previous chapter, those who discuss innatism from an evolutionary point of view tend to do so in terms what – or how great – our innate linguistic endowment is, not whether it's there or not). Those who argue against Chomsky do so for two reasons: either because they espouse an alternative epistemology for their form of linguistics (Figueroa, Hymes, Yngve), or they feel that Chomsky's appropriation of Descartes is either misguided or in some way inaccurate (e.g. Aarsleff).

Just as with the view of language as fluid, unpredictable and ephemeral which is vaguely shared by Putnam and sociolinguistics, it is possible to make the case that what Hymes, Figueroa, Labov et al. all share is an opposition to Chomsky's mentalism, on the grounds that language is a real object in the real world, which is situation-dependent, physical and dynamic.

If the problem is mentalism, then the obvious question is 'what is the mind?' This question was addressed in chapter three, and is again discussed in section 2.1.1 below, regarding the object of study of linguistics. The obvious conclusion is that mind is literally the object of study for Chomsky (something quite literally studiable in 'the Galilean style'), whereas for everyone else it is some form of chimera, metaphor, or unreachable mess. Different disciplines have different ontological commitments, so this should not be seen as particularly surprising, but we will see in section two of this chapter that it relates directly to my theory of reference, as it concerns what is chosen as the focus of study, which objects are given names, the process by which objects are selected for naming, and how that process differs from natural language referencing.

What these arguments, which I set out in detail in chapters three and four, have in common is incommensurability; they are not simply about vocabulary (whether polysemy or synonymy). It is not a case of British people saying 'aubergine' and Americans saying 'eggplant', while referring to the same species of the vegetable kingdom. It is more like Christians and Hindus arguing about god(s). Where for Christians 'god' contains within its definition an assumption of singularity, for Hindus the definition is a family term. So when a Hindu talks about the relationship between two gods this is not, from a Christian point of view, a factual error (like, say, 'aubergines grow underground'), but a conceptual error ('aubergines can't spell'). However, from a non-partisan objective it is a conceptual mismatch rather than an error.

2.0 Incommensurable vocabularies as used in those arguments

> Gumperz' view is simply that sociolinguistic work requires a different set of concepts and methods. (Dil in Gumperz 1971:xiv)

Having established the two broad groups of opponents, we can look at the language which they used in the debates which were examined in the previous chapters, and examine which groups of words can be seen to be incommensurable. In each case I will present vocabulary or concepts which can be shown to be interlinked, and show how they are incommensurable as they are used by each side.

The reason that incommensurability seems such a valid option in this case is because the 'equipment' used in TGG and sociolinguistics, such as it is, is mental. Part of the 'tool-kit' for these methods is the mental/social entities posited by TGG and sociolinguistics – both types of linguistics rely on a structured network of mental posits in order to explain what they try to. In other words, both use mental objects of speculative ontological status, whose existence is both assumed and confirmed by the theory being tested.

Most of these are relatively uncontroversial. Words and sentences are commonly accepted things whose existence does not arouse much debate. The same goes for intentions, relationships and communities. However, some posited mental objects are less accepted, and these will all be discussed.

2.1 Defining the paradigm: mentalism, variation, I- and E-language, true linguistics, natural science and social science

I will begin by looking at arguments about what kind of practice linguistics is, or ought to be. This broadly parallels the discussion in chapter three about which, if any, of the forms of linguistics under discussion can be seen to instantiate a Kuhnian paradigm.

2.1.1 Mind and variation

This section shows that each side makes its claim about the object of study, and that these claims either preclude or exclude the other's. I will start by looking at two different conceptions of what language is, when it is taken to be the object of study of linguistics. Linguistics is, of course, the study of language, but only in the trivial sense which leaves language further undefined. When language is defined in terms of something else, then the object of study of linguistics becomes less well-defined.

For example, if	'linguistics is the study of language'
and	'linguistics is the study of X'
then	'language is X'.

Or, if	'linguistics is the study of language
and	'language is Y'
then	'linguistics is the study of Y'

Two of these further definitions sometimes put forward are that linguistics is the study of the mind (i.e., psychology), and that it is the study of social facts, or variation.

We have seen in the previous two chapters that Chomsky has defended at length the idea of the mind as a valid object of study of linguistics, while sociolinguists such as Hymes have defined language in terms of its use and its users. It is worth noting here the non-polar nature of their definitions. In focusing on knowledge of language in the mind, TGG does not exactly rule out variation, but fails to have a definition for it altogether. As will be discussed below, Chomsky has no time for the study of variation (1965:4). In the same vein, focusing on variation and use of language by culturally situated users, sociolinguistics fails entirely to define the notion of mind.

Chomsky's commitment to studying the mind has not wavered over the years:

'linguistic theory is mentalistic, since it is concerned with discovering a mental reality underlying actual behavior' (Chomsky 1965:4 and see 193n)

'we can only characterize the properties of grammars and of the language faculty in abstract terms' (1975:36)

When I use such terms as "mind", "mental representation", mental computation" and the like, I am keeping to the level of abstract characterization of the properties of certain physical mechanisms, as yet almost entirely unknown. There is no further ontological import to such references to mind or mental representations and acts.' (1980:5)

Generative grammar is concerned with those aspects of form and meaning that are determined by the 'language faculty', which is understood to be a particular component of the human mind.' (1986:3)

The approach is 'mentalistic', but in what should be an uncontroversial sense. It is concerned with 'mental aspects of the world', which stand alongside its mechanical, chemical, optical, and other aspects. It undertakes to study a real object in the natural world – the brain, its states and its functions – and thus to move the study of the mind towards eventual integration with the biological sciences. (2000b:6)

We can contrast this with two 'paradigmatic' statements' about what sociolinguists do and find interesting:

Variability is an integral part of the linguistic system. (Labov 1966:3)

Labov's New York Survey demonstrated [...] that language variation is not only amenable to analysis but also linguistically interesting and socially revealing. (Chambers 2003:26)

A recognition of variation implies that we must recognise that a language is not just some kind of abstract object of study. (Wardhaugh 2006:5)

[M]eaningful insights into language can be gained only if such matters as use and variation are included as part of the data which must be explained in an

> adequate linguistic theory; an adequate theory of language must have something to say about the uses of language. (Wardhaugh 2006:6)

Sociolinguistics assumes that language is used differently by different people at different times, and this fundamental aspect of language is the rock on which variationist studies are built. To say, then, that variation is a feature of performance to be explained away through idealisation (Chomsky 1995:14), and the assumption in TGG that variation is a problem, would be literally non-sensical to a sociolinguist unaware of the pre-theoretical posits of TGG. It would be similar to an evolutionary biologist being told that variation between species was a problem, rather than the object of study. The mental and social entities posited are bound up with the views of the nature of language assumed by TGG and sociolinguistics. For TGG inter-speaker variation is a problem to be circumvented (Schütze chapter four), whereas in sociolinguistics it is the object of study. This gives us a straightforward example of incommensurability.

If TGG is the study of the mind, then it is a part of psychology (Chomsky 1975:36-44), and if it is the study of variation in behaviour then it is part of sociology. This leads on to territorial claims about what does and doesn't constitute real or true linguistics.

2.1.2 True linguistics

> If we hope to understand human language and the psychological capacities on which it rests, we must first ask what it is, not how, or for what purpose, it is used. (Chomsky 1968:62)

The above contrasts are not merely evidence of different fields with different foci: TGG, as we saw, needs to explain away variation, in the same way that sociolinguistics needs to explain why individual minds in isolation are insufficient material for linguists; for example, Wardhaugh quotes

Hudson (1980:19) saying that 'an asocial view must lead to a linguistics which is essentially incomplete' (2006:6). However, the different conceptions of what linguists can and should study lead to territorial claims about what 'is linguistics' and what isn't; these tend to focus on the locus of language and the object of study, and have given rise to vexed arguments and accusations.[136]

Hymes (1974:12), from within sociolinguistics, does not claim that some sub-disciplines are linguistics and some not, but attributes this claim to unnamed sources:

> Some people indeed write histories, or paragraphs on history, that come to little more than hailing moments of 'true' linguistics, and speculating as to causes of the mysterious prevalence of 'false' linguistics at other times.

Just as in the debate over whether or not TGG is a Kuhnian paradigm, there are phantom voices here, an unnamed group of propagandists making unjustified accusations. This quotation indicates a general siege mentality, rather than offering details of the debate. However, there are some examples of this type of claim.

Labov, Katz and Chomsky have all used the implication that what they do is linguistics, to the exclusion of others. Figueroa (1994:69) presents Labov's position in this way:

> For Labov sociolinguistics "is a somewhat misleading use of an oddly redundant term" (Labov, 1972a p183). It is "misleading" because it somehow implies that sociolinguistics is something other than linguistics, and it is "oddly redundant" because it implies that there can be a linguistics which does not consider language socially.

Milroy also speaks for Labov in similar terms:

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[136] See Carlson (2003:80-1) for further discussion of the ontological issues surrounding the study of language.

> As is well-known, William Labov [...] was initially not at all happy that the label *sociolinguistics* should be attached to work that in his view, and I believe in the view of all of us who are practitioners of this science, should really have been referred to as linguistics. (1987:ix)

A similar line is taken by Bucholtz and Hall:

> It has been recognized that language is an embodied practice that must be analyzed as such (2008:407)

Katz, with one foot in the Chomskyan camp[137], counters that

> Sociolinguistics [...] is the sociology of language, and its interests concern the discovery of what linguistic forms are found where, what forms are correlated with what social features, etc. Thus what linguists like Labov [...] seem to have overlooked is that since sociolinguistics is the sociology of language, it is sociology, not grammar. (1981:220n)

Chomsky (1986:4-5) also hints at exclusivity in his approach to his field of study (in a passage quoted in chapter two section 3.3):

> Generative grammar is sometimes referred to as a theory, advocated by this or that person. In fact it is not a theory, any more than chemistry is a theory. Generative grammar is a topic, which one may or may not choose to study. Of course, one can adopt a point of view from which chemistry disappears as a discipline (perhaps it is all done by angels with mirrors).[138]

Earlier in his career, in the paradigm-consolidating *Aspects of the Theory of Grammar*, he took the line that

> Observed use of language or hypothesized dispositions to respond, habits, and so on, may provide evidence as to the nature of this mental reality, but surely

[137] Jerrold Katz's *Language and Other abstract Objects* (1981) attacks Chomsky's ontology, without dismissing his theoretical advances in syntax.

[138] There are those linguists who, like Brian Patten quoted at the beginning of the thesis, would like to review their disbelief in angels.

> cannot constitute the actual subject matter of linguistics, it this is to be a serious discipline. (1965:4)

These two approaches pull back from attacking other approaches by name, but the implication is clear: the respective types of study are somehow autonomous and correctly founded, and therefore any other type of linguistic study is irrelevant.

These two approaches break down along competence-performance lines, with Labov seeing performance as providing the primary linguistic data, and Chomsky concentrating on competence[139]. However, the distinction between the two approaches extends to seeing one's chosen focus as providing the *only* linguistic data, with the other type of study being interesting, perhaps, but not true linguistics.

Figueroa sets out these opposing approaches, and explains them in terms of their philosophical bases:

> Labov appeals to mundane realism, that his data corresponds with what people actually do, and Chomsky appeals to psychological realism, that his data corresponds with the actual workings of the brain [...] Labov and Chomsky therefore place the locus of language in two different places, and thus their object of enquiry is not the same [...] The questions of where language is located and what the object of linguistic enquiry should be are closely related. For Labov the locus of language is not in the individual but in the community [...] the object of linguistic enquiry for Labov is therefore the community and not the individual. (1994:80-1)

The justifications for pinpointing a locus of language, either in 'the individual' or 'the community', naturally have an effect on the conception of 'the object of linguistic enquiry'. However, Figueroa shows that there are metatheoretical questions which affect the choice of object of enquiry. We

[139] See below for further discussion of competence and performance.

have seen that Chomsky claims a Cartesian heritage, and for Figueroa this is instrumental in his approach to what counts as a legitimate object of enquiry:

> Labov considers language to be a social fact and locates language in the community-at-large rather than in the individual. Chomsky on the other hand considers language to be a mental property and locates language in the species rather than in any social group [...] Because the language faculty is species specific, one can assume that any (normal) member of the species will be endowed with such a faculty and therefore any (normal) member of the species is acceptable as a specimen. If one adds to this the Cartesian notion that knowledge is attained most immediately and completely through intuition, one arrives at the conclusion that the individual and individual intuitions are acceptable means of approaching reality. (ibid:81)

Figueroa here explicitly links Chomsky's ontology – his view that the species-specific language faculty of an idealised individual is a legitimate object of enquiry – with a Rationalist methodology which states 'that knowledge is attained most immediately and completely through intuition'. It is crucial for Figueroa's argument that the disagreement over the correct object of enquiry is based on differing conceptions of how knowledge about anything can be acquired. Practitioners of TGG may well complain that Chomsky does not quite claim that 'knowledge is attained most immediately and completely through intuition'. Apart from Descartes, few Rationalists would go this far, but in TGG intuition is certainly seen as equally legitimate as other forms of data-gathering.[140]

This section has shown that the paradigmatic statements about the focus of sociolinguistics and TGG, and corresponding territorial claims on the focus of linguistics, show immediate and significant differences. While these statements may be weak evidence for incommensurability between

[140] Intuition is not, of course, the only methodology used in TGG. Smith (1999:22-25) describes various types of pathological cases which provide real-time utterance data, which are then used as evidence for UG.

the two disciplines, they provide a basis for the next two sections to build on.

2.2 Science and methodology

This section looks at aspects of the methodological practices of TGG and sociolinguistics which might be seen as incommensurable. This follows on from the section of chapter three which examined claims that one or another form of linguistics does not practice science properly. It will be shown that those arguments stem from incommensurable views of what the object of study is, and therefore the best way to go about studying it.

2.2.1 Access to competence and performance

> A grammar of a language purports to be a description of the ideal speaker-hearer's intrinsic competence. (Chomsky 1965:4)

In Chomskyan grammar, 'competence' refers to the 'tacit knowledge of [a native speaker's] language – i.e. of how to form and interpret words, phrases and sentences in the language' (Radford 1997:2)[141]. This contrasts with what Hymes termed 'communicative competence' (Hymes (1971), and see Saville-Troike 1989:20-26), and which Hymes postulated in response to Chomsky's notion of 'competence'. For Chomsky, this competence is an innate ability pertaining to human beings in normal circumstances, by which they can produce well formed sentences in their native language, and judge the well-formedness or otherwise of the sentences produced by other people. For linguists such as Hymes, however, well-formedness is not the vital aspect of language which separates us from chimpanzees. Just as important, if not more so, is the ability to use language appropriately in a social context. We have already seen that not to

[141] Radford's book is an undergraduate introduction to generative grammar, then in the very early stages of the minimalist program.

do so, but to produce any well-formed sentence at any time, as the generativists are so fond of pointing out that we can, would be taken as clear evidence of insanity:

> [A] person who can produce all and any of the sentences of a language, and unpredictably does, is institutionalized. (Hymes 1977:123)

In other words, knowing the language involves knowing how to use it, and our power to generate an infinite number of grammatical sentences is only one aspect of that knowledge, and one that needs to be carefully constrained. Wardhaugh too challenges Chomsky's notion of competence:

> [T]he kind of competence that must be explained involves much more than Chomsky wishes to include, and indeed includes much that Chomsky subsumes under what he calls performance. (Wardhaugh 2006:3)

One particular aspect of the competence-performance distinction raises further methodological issues. If 'competence' in the Chomskyan sense is accepted as a valid posit, then this entails that the linguist needs to somehow gain access to it in order to study it, and this is usually done through intuition, specifically through acceptability judgements about the well-formedness of sentences. In order to understand the role of acceptability judgements in TGG, we must first accept the existence of the thing which we are trying to describe – competence. Of course, competence forms part of the very definition of 'language' as understood within TGG. To those outside the 'paradigm' however, it takes learning a new language to break into the circle of meaning between 'language', 'competence' and 'grammaticality' (etc.).

Schütze (1996) shows that since its inception TGG has been subjected to constant criticism, both intra- and extra-paradigmatic, of its primary methodology. Although most practitioners of TGG accept the posits of the theory (the competence-performance distinction and so on), the reliability of acceptability judgements has been questioned on many grounds, as we saw in chapter three, section 1.2.1.

Competence in particular draws attention, because in some important respects it cannot be accessed at all. Schütze (1996:19-36) summarises in detail the differences between grammaticality and (grammatical) acceptability. The first is an aspect of competence, and is not, therefore, directly accessible, whether through intuition or anything else. Acceptability *is* accessible, being a feature of performance, but its relationship to grammaticality is not necessarily perfect:

> [T]he paradox of linguistics: the only possible way of determining whether or not a grammar is correct is by consulting the speaker's intuitions, but they are inaccessible. (Householder 1973:365n, quoted in Schütze (1996:26))

> Although one might propose various operational tests for acceptability, it is unlikely that a necessary and sufficient operational criterion might be invented for the much more abstract and far more important notion of grammaticality. (Chomsky 1965:10-11, quoted in Schütze (1996:25))

Therefore, it could be argued, the initial posit, competence, which Chomskyans are primarily interested in, is destined to remain a posit, as it is inaccessible in principle. It could further be argued that by the law of Occam's Razor we should reject it; however, acceptability judgements give us some kind of window onto competence.

2.2.2 Ontological priority, form and function of language, directionality

If we accept the competence-performance distinction, whether in Chomskyan terms or not, and we accept that the methodological approaches so far discussed are (to some extent) valid for the examination of competence and performance, we are still left with the question of which, if either, we ought to approach first when it comes to studying language. This has sometimes been framed in terms of ontological or epistemological 'priority', which forms the subject of this sub-section. Given the language involved, it is tempting to view this in a Cartesian light. Descartes saw the universe as being made of two substances, mind and matter (1954 [1637]:32). Matter was mutable and therefore imperfect, while mind was eternal, unchanging and non-physical, and therefore perfect. It follows that matter must have been created by mind, and not vice versa, because something greater cannot be produced by something lesser (ibid:33-4), and this is the sense in which mind can be considered ontologically prior to matter, both in status and in chronology. Chomsky's Cartesian reading of our linguistic abilities does not (particularly) draw on this aspect of Descartes' thought, but his assumption that the contents of the mind are somehow more perfect than the products of our bodies is distinctly Cartesian (e.g. Chomsky 1995:168 and 2000b:9).

When Chomsky talks about 'the mind' he is, of course, referring to something quite different from Descartes' 'mental substance'. Where for Descartes mind and matter were literally two different types of thing, modern science tends to see them as ultimately the same type of thing[142], with mind as a phenomenal process arising from the interaction of the physical components of the human body, in particular the central nervous system and the brain.[143] This is not to imply that all of what we refer to as our 'minds' is phenomenal, and therefore conscious. Plenty of what constitutes our minds is unconscious, and some is no doubt destined to remain so. Some, on the other hand, seems amenable to discovery, and this is the goal of TGG.

[142] Chomsky entertains the interesting parallel that mind may turn out to be like dark matter – 'crucially different from the 10 per cent of the world about which there are some ideas' – without actually endorsing it as probable (2000b:85).
[143] Some philosophers would say 'supervening on' rather than 'arising from' – see Davidson (2001:207-225).

So the Chomskyan 'mind' is not the Cartesian mind. It is heavily dependent on the body, but, for the purposes of scientific examination, separable from it. Given this, the 'objects' which it contains must be seen as non-physical (i.e. mental) objects, and the relationship between those (posited) mental objects and the corporeal human body is therefore, for the purposes of this discussion, not so different from the relationship between Descartes' 'mental stuff and physical stuff' (1954 [1642]:66-71).[144]

We have seen again and again that ontological questions mesh with epistemological ones. Figueroa (1994:19), after Markova (1982), characterises one difference between competing types of linguistics as a difference between competing epistemologies. We saw in chapter four that she argues that where TGG is based on a Cartesian epistemology, sociolinguistics is Hegelian. The first sees the mind as passive, with acquisition of knowledge following a deductive path, while the second sees the mind as dynamic, and the acquisition of knowledge based on dialectic. This leads to a different view on what the 'knower' is. For the Cartesian:

> "the inner world is epistemologically prior to the outer world". Therefore, "interaction between the thinking subject and the rest of the world [is] not considered by the Cartesian paradigm" (Markova, 1982, p. 20,23).

If the inner world is 'epistemologically prior', then it is incumbent on linguists to gain a sufficient understanding of its nature before addressing the outer world. In other words, 'epistemological priority' means chronological priority. As well as there being time-based constraints on what the linguist ought to study, there are also contrasting attitudes towards time and change across the two epistemological standpoints:

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[144] Recall from the previous chapter (Cela-Conde and Marty 1998:20) that Chomsky does not accept the mind-body distinction as a valid dichotomy.

> Evolutionary and developmental processes in the Cartesian framework are considered in terms of innate predetermined structures and functions, and are viewed therefore as ahistorical states. (Figueroa 1994:20)

For the Hegelian, on the other hand, 'it is through interaction with the world that consciousness develops, and the subject object relationship is not unconnected: "both partners in the interaction, both the knowing subject and the object of his knowing, are gradually transformed" (Markova, 1982, p. 178)' (Figueroa: ibid).

One way of solving the question of ontological priority might be to focus on the function of language:

> [I]n line with the Cartesian framework, formalist linguistics holds that the primary function of language is thought [...] In line with the Hegelian framework, functionalist linguistics states that the primary function of language is communication. (Figueroa 1994:23) [145]

We saw in chapter three that many participants in those debates have addressed this question by looking at what we use language for, and how and why it evolved. The natural response is that we use language for thinking and for communicating, and that it evolved for both. In evolutionary terms, this is unproblematic. Evolution bootstraps itself, and evolved mechanisms can change their function over time, as was discussed in chapter three on exaptations. Nevertheless, we saw in chapter three that there has been significant debate over whether language evolved mostly as exaptation with the purely linguistic feature a single, narrow grammatical module (Hauser et al 2002), or as a collection of adaptations suited to more general cognitive and communicative tasks (Pinker and Jackendoff 2005).

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[145] This debate is old. It goes back at least to the 18th century – see Rousseau and Herder in Moran (1966).

This historical argument differs slightly from the synchronic argument that one or the other function has priority. This might seem an odd argument to pick, when there does not really seem to be an argument to be had. However, one's attitude towards the 'primary function' of language goes hand in hand with, or perhaps determines, the possibilities for studying it. Figueroa (1994:22-3) approvingly quotes Dik who, as befits the architect of a theory of functionalism, discusses what he calls two 'paradigms' in linguistics,[146] the formal and the functional (1978:4). Under the formal paradigm 'a language is a set of sentences', and 'the primary function of a language is expression of thoughts'. Under the functional paradigm, 'a language is an instrument of social interaction', and 'the primary function of a language is communication'. Such a distinction may be commonplace, but the intent behind it is crucial.[147]

Even if we cast aside the rhetorical load of describing the mind as in some way 'ontologically prior', and therefore superior to, the physical body, we are still left with the conclusion that the mind is methodologically prior to the body in Chomskyan linguistics, and this priority retains the epistemological problem discussed in chapter three, unit 1.2.2: how do we derive the physical tokens, of words or sentences, from the prior mental objects? The problem is more than just that of a name for a type ('a sentence') and the observable tokens (the utterances). The problem lies in the interpersonal knowledge that speakers of a language share – they have individual knowledge of language, while the contents of that knowledge include things like sentences which are not individual, but shared across the community. Our 'knowledge of language' (see below, section 2.3.2 for more on this) might be taken as uncontroversial by most linguists, in that strong arguments have been made to show that we all have it, and that we have it remarkably uniformly across our species, but as a theoretical posit on

[146] I put 'paradigms' in quotation marks because such usage obviously begs the question, in a Kuhnian sense.

[147] An alternative is to be found in Newmeyer (1998), writing from a formalist perspective, who presents a conciliatory approach.

which to base our study of human linguistic abilities, it presents philosophical problems – these are further explored in section 2.3.3 below).

As noted above, there is a significant overlap between the issues of ontological incommensurability and methodological incommensurability. This arises from the basic observation that what scientists or social scientists believe to exist (the posits of the theory) dictates how they go about investigating those posits (see Kuhn 1962:111-135, especially 123-9); this leads onto the question of directionality of meaning. The idea of meaning, that it attaches to linguistic symbols, is a property of them, or is a relation between them, relates to the question of what kind of things linguistic symbols are held to be. In particular, it relates to the question of the ontology of sentences and utterances, and their relationship with meaning. The meaning of 'paradigm' here is a framework of study which directs the scientist; in this case it means outlining the relative scope of difference subdisciplines (Kuhn:1962:43-51):

> Formal paradigm: syntax is autonomous with respect to semantics; syntax and semantics are autonomous with respect to pragmatics and the priorities run from syntax via semantics to pragmatics.
>
> Functional paradigm: pragmatics is the framework within which semantics and syntax must be studied; semantics is subservient to pragmatics and the priorities run from pragmatics via semantics to syntax. (Dik 1978:4 quoted in Figueroa 1994:23)

As always in these schematisations, it is not clear whether or not anyone has ever held either of these positions in such a simple format. However, for the purposes of this dissertation they illustrate two different viewpoints on language. These two paradigms have completely different attitudes towards the directionality and the number of entities involved in meaning relations, and this is especially interesting with regard to what is autonomous of what. For example, in the formal paradigm, syntax is autonomous of semantics, and this allows it to generate semantic functions while being independent of both semantics and pragmatics. For the functional paradigm, on the other hand, there is no way to make sense of the idea of meaning independently of the pragmatic circumstances.

On top of this, the functional paradigm places syntax and semantics 'inside' pragmatics, while semantics is 'subservient' to pragmatics. This implies that there is a bidirectional relation between semantics and pragmatics, and between pragmatics and syntax, but in each case pragmatics is assumed to contain or provide a framework for the subservient function or study (semantics/syntax). The formal paradigm only provides a unidirectional relation between each area: syntax > semantics > pragmatics. Syntax is independent of semantics, semantics of pragmatics, but the flow does not reverse in any substantial sense.[148]

This section has explored an interconnected series of binary oppsotions constructed along both conceptual and vocabulary lines: epistemological priority, methodological priority, Hegelian and Cartesian frameworks, formalist and functionalist paradigms, universals and particulars as objects of study. Despite the schematic nature of these divisions, the fact that they are postulated and taken seriously shows that there are substantial and epistemological issues at stake between the two types of language study, and suggests that the differences are not just about taste or inclination.

[148] It should be pointed out here that according to Chomsky (2000b:132), 'it is possible that natural language only has syntax and pragmatics [and not semantics]', but this is based on his internalist view of reference – he does not deny that sentences are meaningful.

2.2.3 Theory choice/data

Underlying all of this is a basic ontological and epistemological difference concerning the nature of science, theories and reality. To return to Figueroa's analysis of the disagreements between Chomsky and Labov:

> Part of Labov's disagreement with Chomsky may be traced to a difference in conception of what a theory is and the role that a theory plays in scientific enquiry [...]
>
> Chomsky is concerned with the idealized conditions of species homogeneity (for universal grammar) and idealized speech communities (for the study of specific grammars). Theory building for Chomsky is therefore neither driven by a need to correspond to mundane reality nor necessarily derived from data found in real life situations, because the object of enquiry is not realistic in this sense. Furthermore, it is the theory which drives the data; that is, the data is deduced from the theory rather than the theory induced from the data.
>
> [...] It is not that Labov does not abstract away from phenomena or idealize, but that he has a more positivist notion of what a theory is. Labov's realism is of the sort that the objects he claims to exist do in fact exist. In other words, for Labov a grammar should not be a construct of the linguist which corresponds to some idealized reality but should instead correspond to particular observable facts. (ibid:83-4)

Figueroa has an agenda to pursue; this characterisation of Chomsky's approach to theory formation and the relationship between the theorist and the theory would almost certainly be challenged by practitioners of TGG, especially in its rather provocative assertion that a Chomskyan theory is 'neither driven by a need to correspond to mundane reality nor necessarily derived from data found in real life situations', and that 'it is the theory which drives the data'.

However, from the point of view of sociolinguistics, especially the strand to which Figueroa adheres, it certainly appears that TGG selects theories which are not data-driven. This is because the respective definitions of 'data' in the two disciplines are different. Where for sociolinguists data is something that happens in the course of human communication, in TGG it is something which is accessed via introspection. If the definitions of data do not match up, then misstatements about the other side's use of it are bound to follow.

Equally arguable is Figueroa's characterisation of Chomsky's approach to theory choice as being driven by indeterminacy:

> Labov proposes that the means by which one can choose between disparate analyses is to choose the one which best corresponds with the data; the one which best demonstrates the existence of the theoretical entities generated by the linguist's theory [...] Labov therefore argues against the position he suggests Chomsky holds, that 'there will always be many possible analyses for each body of data, and we will need internal evaluation measures to choose among them' (Labov 1972a, p. 1202). Instead Labov maintains that there is indeed only one correct analysis, that its correctness can be established externally, and that correctness can be established in a theory independent way. Labov's positivism is demonstrated through his objection to the Kuhnian claim that there is no neutral basis for choosing between theories, and his objection to the theory driven (thus un-neutral) nature of Chomsky's explanation. Labov and Chomsky may be seen therefore as fundamentally disagreeing over the nature of theory and data. (ibid:83-84)

We have already seen that Chomsky has argued *against* Putnam's indeterminacy (see chapter three), and it was, after all, 'positivist' philosophers such as Quine who initially espoused the idea with regard to reference (e.g 1960:26-79). Chomsky has argued that there *is* one correct analysis (in the sense that it is possible in linguistics as much as it is possible in the natural sciences (1980:14-23), but if 'correctness can be established externally, and that correctness can be established in a theory independent way', then we can see Labov espousing a most 'positivist' position, in distinction to Chomsky's Rationalism.

It is this that gives the ultimate basis for theory choice. For both sides the interplay between ontology, epistemology and methodology leads to incommensurable theoretical vocabulary. Epistemological concerns entail metatheoretical commitments about what constitutes valid arguments, valid data and valid conclusions. In the absence of agreement on these points, overlapping vocabulary as it is used in different theories becomes uninterpretable, and there is no way to move between vocabularies without translation.

2.3 Epistemology and ontology

The previous two sections discussed terms which refer either to the definitions of the linguistic paradigm under examination, or those terms which define how that paradigm goes about its work (in other words, what is or is not acceptable practice according to its own rules). This section looks at more specific vocabulary items which purport to relate to the entities which are studied under those paradigms and according to those rules. It broadly complements chapter four, which discussed epistemological views of language and the ontological commitments concerning the object of study which different forms of linguistics make.

2.3.1 Language and knowledge of language

According to the most basic definition, linguistics is the study of language (Radford 1988:1, Smith 1999:8, many others). In this section I will explore the ontological commitments to basic concepts such as 'language' which are assumed by the rival epistemological backgrounds which are under discussion. Language is, of course, the object of study of linguistics. According to the OED, language is 'In generalized sense: Words and the methods of combining them for the expression of thought', and I noted that for the purposes of this dissertation, this definition begs the very questions which I am interested in: what is a word? what are those methods? what is the relationship between language and thought, and how can language express thought?

We can begin by looking at two definitions of language, as given by the participants in the debates outlined in chapter three. Smith gives a fairly straightforward TGG assessment of what language is:

> Why is Chomsky important? He has shown that there is really only one human language. (Smith 1999:1)

> Language is definitional of what it is to be human, and the study of language is a way in to the study of the human [...] mind. (ibid:7)

Chomsky himself introduces language to the lay reader by assuming that

> The faculty of language can reasonably be regarded as a "language organ" [...] We assume further that the language organ is like others in that its basic character is an expression of the genes. (2000b:4)

> Suppose that Peter's language organ is in state L. We can think of L as Peter's "internalized language". When I speak of a language here, that is what I mean. (ibid:5)

Chomsky and Lasnik are self-consciously outlining a new program for linguistic study when they define language as follows:

> When we say that Jones has the language L, we now mean that Jones's language faculty is in the state L, which we identify with a generative procedure embedded in performance systems. To distinguish this concept of language from others, let us refer to it as *I-language*, where I is to suggest "internal",

> "individual", and "intensional" [...] When we use the term *language* below, we mean I-language. (1995:15)

Of course Chomsky is not actually claiming here that the exhaustive definition of 'language' is 'a generative procedure embedded in performance systems' – the above definition is more usually referred to as an I-language, as opposed to an E-language. However, this narrowing of the focus has methodological side-effects. Smith proposes that 'I-language is amenable to scientific investigation in a way that E-language is not' (1999:31). Chomsky goes further, saying 'one might define E-language in one or another way, but it does not seem to matter how this is done; there is no known gap in linguistic theory, no explanatory function, that would be filled were such a concept presented' (1995:16). Previously (1986:29) Chomsky had said that a certain amount of misapprehension may have been caused by nomenclature in the past; E-language being a 'derivative and largely artificial construct', he suggests replacing 'I-language' with 'language'.[149]

This is a rhetorical trick frequently used by Chomsky. First, isolate that aspect of language which he is interested in; second, show how that aspect of language is central to linguistics, possibly because it is the only one which is systematically studiable; third, let the assumption creep in that it is the most important or central aspect of language (e.g. 1986:15-46; Murray (1994:445), not for the first time, describes this approach as 'Stalinist').

As we saw in the previous section, where TGG studies the 'ideal speaker-listener', and therefore studies the formal properties of linguistic knowledge, sociolinguistics studies the functional use of language as it is produced in real-life situations. On the surface, such positions might appear

[149] Recall also Hauser, Chomsky and Fitch (2002) which defines the 'purely linguistic' items of cognition from an evolutionary perspective.

unproblematically complementary; however, the clash comes from whether or not it is possible to separate the underlying structure from its real-life manifestations. Is it in any way meaningful to abstract the form away from any functional manifestation, or is the formal structure of language bound inextricably to its functional role? For Labov, the latter is clearly the case:

> It is difficult to avoid the common-sense conclusion that the object of linguistics must ultimately be the instrument of communication used by the speech community; and if we are not talking about *that* language, there is something trivial in our proceeding. (Labov 1972:85)

Saville-Troike who has an impeccably Hymesian heritage and orientation[150] takes a similar line, arguing that language is 'first and foremost [...] a socially situated cultural form', and that 'to accept a lesser scope for linguistic description is to risk reducing it to triviality' (1989:3). Wardhaugh takes a similar line – 'the definition of language includes in it a reference to society' (2006:1) – as does Fasold (1984:ix).

Markova notes the problems inherent in this conception of language:

> The existence of a universal grammar implies the existence of language independent of any actual use [...] the investigation of the role of language in verbal reasoning which stems from Chomskyan linguistics also assumes that the rules of reasoning cab be discovered by inspection of natural language without appeal to actual use. (1978:3)

Hymes holds that

> It is indeed something of a contradiction, an irony at least, that we have today a general linguistics that justifies itself in terms of understanding the distinct-

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[150] Saville-Troike practises and develops the ethnography of communication along the lines advocated by Hymes. See, for example (2003:viii) where Saville-Troike describes Hymes as 'truly the father of the field' and (ibid:1-40) for a detailed discussion of the all-pervasive influence of Hymes on her practice.

> iveness of man, but has nothing to say, as linguistics, of human life [...] Linguistics cannot claim to be a science of *language* without constituting itself on an adequate functional foundation. (1977:147).

Hymes believes that linguistics can and should study language *in situ*, which entails political engagement with that context. For example:

> If competence is to mean anything useful [...], it must refer to the abilities actually held by persons. A salient fact about a speech community, realistically viewed, is the unequal distribution of abilities, on the one hand, and of opportunities for their use, on the other [... *for example* ...] even a cursory look at the globe discloses definition of women as communicatively second-class citizens to be widespread. When, where, and what they may speak, the conceptions of themselves as speakers with which they are socialized, show again and again that from the community point of view, they at least are not "ideal speakers", though they may on occasion be ideal speakers. (1977:205)

He goes on to give a manifesto-like list of sociolinguistic commitments, based on the principle that language is not substantially separable from its use (ibid:206).

Chomsky, as we have seen, uses the inverse argument, that *only* the aspect of language which he is studying is amenable to scientific study. So we have different conceptions of the make-up of the object of study. For TGG it is possible, and necessary, to compartmentalise language. From the point of view of the sociolinguistic web of reference, the social side of language cannot be hived off from the cognitive.

From 'language' we move on to 'knowledge of language' and 'knowers (or speakers) of language'. Chomsky has said on this theme:

> Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as mem-

> ory limitations, distractions, shifts of interest, and errors (random or characteristic) in applying his knowledge of the language in actual performance. (1965:3-4)

His 1986 book *Knowledge of Language: Its Nature, Origin, and Use* naturally provides a paradigm-forming definition of this term:

> (i) What constitutes knowledge of language?
> (ii) How is knowledge of language acquired?
> (iii) How is knowledge of language put to use?
>
> The answer to the first question is given by a particular generative grammar, a theory concerned with the state of the mind/brain of the person who knows a particular language. The answer to the second is given by a specification of UG along with an account of the ways in which its principles interact with experience to yield a particular language; UG is a theory of the "initial state" of the language faculty, prior to any linguistics experience. The answer to the third question would be a theory of how the knowledge of language attained enters into the expression of thought and the understanding of presented specimens of language, and *derivatively*, into communication and other special uses of language'. [my italics] Chomsky (1986:3-4)

This quotation shows how causally a definition can exclude other approaches to the same object of study. In Chomsky's definition here, knowledge of language is put to use *derivatively* – its use is not part of its definition. Instead, knowledge of language is an entirely tacit knowledge, identifiable with a 'state' (see above), rather than a conscious knowledge of what it is and how to use it.

As Wardhaugh notes (2006:1), the earlier passage is 'extensively quoted'. This is because it stands in opposition to what many people feel to be intrinsic to knowledge of language:

> Knowing a language also means knowing how to use that language since speakers know not only how to form sentences but also how to use them appropriately. (Wardhaugh ibid:3)

This is not just theoretical talk; it has significant real-life consequences for those who fail to obtain this knowledge:

> [A] person who can produce all and any of the sentences of a language, and unpredictably does, is institutionalized.[151] (Hymes 1977:123)

A definition of knowledge of language rests on the definition of language, as accepted by the respective paradigms.. If language is 'internal, individual, intensional', then knowledge of it is also internal, individual and intensional. If the definition of language 'includes in it a reference to society', then knowledge of language 'means knowing how to use that language', and not doing so leads to ostracism from society. Following these contrasting notions of what kind of thing language is, the next section examines the consequent ideas of how language is constituted.

2.3.2 Sentences and utterances

In the preceding chapters I have outlined two approximately contrasting views of language, with Chomsky on one side and an unlikely amalgam of sociocultural linguists, Yngve, Sampson and Putnam. In this chapter I have narrowed this down to TGG and sociolinguistics (in particular what Figueroa describes as the 'Hegelian framework'). The kernel of the debate, shared by all, is whether or not to accept 'mentalism' in its broadest form; that is, whether or not the mind *per se* ought to be the object of study of linguistic research, or whether it can only be reached by indirect means, such as through behaviour or by studying more general critical, communi-

[151] By 'institutionalised' I assume that Hymes means, in the passive sense, 'committed to a mental illness institution', rather than the adjectival sense of 'accustomed to being in a particular institution'.

cative or cognitive capacities (this was addressed in chapter three, and section 2.1.1 above). In this section I will show that mentalism entails an ontological commitment to linguistic types which is held by its opponents to be exactly backwards.

In chapter three, section 1.2.2 I explored the debate over the usefulness or otherwise of idealisation, where I showed that whether or not idealisation is seen as acceptable depends on whether or not types (i.e. sentences) are accorded as much ontological respect as tokens (i.e. utterances).

We have already looked at the OED definition of language: 'Words and the methods of combining them for the expression of thought'. Although this does not sufficiently define language for our purposes, it does point to an obvious facet of language which needs further discussion – that language involves the combination of words, not just words themselves:

> Word: A combination of vocal sounds, or one such sound, used in a language to express an idea (e.g. to denote a thing, attribute, or relation), and constituting an ultimate minimal element of speech having a meaning as such; a vocable. (OED)
>
> Sentence: A series of words in connected speech or writing, forming the grammatically complete expression of a single thought [...] In Grammar, the verbal expression of a proposition, question, command, or request, containing normally a subject and a predicate (though either of these may be omitted by ellipsis). (OED)

This section look at two terms commonly used in linguistics, 'sentence' and 'utterance', and three questions investigating their relationship and ontological dependencies: what is a sentence?; is it the same as an 'utterance?; and if not, what kind of distinction is there? Lyons gives one explanation of why this debate exists:

> Linguists tend to spend far less time these days discussing the nature of sentences. But this is not because there is now some generally accepted criterion, or set of criteria, in terms of which it can be decided what is and what is not a sentence. The reason is simply that linguists have been less concerned recently with questions of definition. (Lyons 1977b:629, from Figueroa 1994:156)[152]

Figueroa (1994:155-163) comprehensively surveys the literature on the question, looking at a variety of textbooks and critical literature, based around pragmatics and functionalist perspectives, which deal with the sentence-utterance distinction as a matter of course. She starts with the following:

> Utterances are physical events. Events are ephemeral. Utterances die in the wind [...] A sentence is neither a physical event nor a physical object. It is conceived abstractly, a string of words put together by the grammatical rules of a language. A sentence can be thought of as the ideal string of words behind various realizations in utterances and inscriptions. (Hurford and Heasley 1983:15)

The work cited is a semantics coursebook, but the definitions hold reasonably well for any type of linguistic study. Figueroa goes on to cite three definitions of the relationship between the two. An utterance 'is the issuance of a sentence' (Levinson 1983:19); utterances are 'realizations (implementations) of sentence patterns in the act of communication' (Danes 1970:133); sentences are 'grammatical entities derived from the language system' while utterances are 'instances of such entities' (Leech 1983:14, all three from Figueroa 1994:156). Levinson, concentrating on sentences, says that utterances are the issuance of a sentence, but does not explain that term. Danes and Leech, from a more functionalist perspective, define sentences in terms of their derivation from utterances. From all these perspectives, the relationship between sentences and utterances is defined

[152] Lyons, as we saw in chapter three, is the author of *Chomsky* (1991) which, while not uncritical, is largely and enthusiastically supportive of Chomsky's ideas.

metaphorically in terms of one or the other, depending on the focus of the author. This unidirectional definition has important repercussions, which are discussed below.

Gumperz attempts to avoid this by focussing on particulars, mistrusting the narrow focus of abstract universals:

> Structural abstractions [...] are quite adequate as long as interest is confined to language universals or typology and to comparative reconstruction [...] but when we turn from a study of language as an institution to the analysis of speech behavior within particular societies, more detailed information is required. (1968:461)

Figueroa endorses Gumperz's 'linguistics of particularity' (1994:170-2)) as an attempt to circumvent the need for universal abstract structure. On the other hand Lyons, writing about semantics, defines these terms as follows:

> Utterances are unique physical events [...] the linguist, however, is not generally concerned with utterances as unique observational entities. He is interested in types, not tokens, and the identification of utterance-tokens as instances of the same utterance-type cannot be carried out in terms solely of external, observational criteria. (Lyons 1977:28)

Lyons – generally writing from a Chomskyan perspective – clearly stakes a claim about what linguists 'are interested in' here. Utterances, 'tokens' of speech, are not their concern, and he casts doubt on whether they are amenable to any kind of observational study.

Chomsky makes a similar claim, right at the beginning of his career, when he says that

> The fundamental aim in the linguistic analysis of a language L is to separate the *grammatical* sequences which are the sentences of L from the *ungrammati-*

> *cal* sequences which are not sentences of L and to study the structure of the grammatical sequences. (1957:13)

This might be uncontroversial, except that he goes on to claim that

> [I]t is obvious that the set of grammatical sequences cannot be identified with any particular corpus of utterances obtained by the linguist in his field work. (ibid:15)

One reason for this is that any corpus will be finite, while a language is an infinite set of sentences; however, the dismissal of corpora obtained through fieldwork is telling. Later, Chomsky breaks down the sentence into its constituent parts, for the purposes of constituent analysis, as follows: '*Sentence* → NP + VP' (1957:26). This is, of course, not quite a definition of a sentence, but it sums up what is and is not studied under Chomskyan syntax. Nearly half a century later, this definition has evolved into 'I have taken an expression to be a pair <PHON, SEM> constructed from lexical items LI, each a complex of properties, including I-sound and I-meaning' (Chomsky 2000b:175) – an approach which Chomsky describes as 'traditional' (ibid:173). The idea that an expression is 'a pair <PHON, SEM>' is a long way from a definition of utterance which states that 'every utterance and its hearing bear the marks of the framework of participation in which the uttering and hearing occur' (Goffman 1981:3, from Figueroa 1994:145). These definitions would be unproblematic if they described different types of entity with a well-defined mutual relationship but they do not.

From within a theory of pragmatics sympathetic to TGG we have: 'Generative grammars abstract out the purely linguistic properties of utterances and describe a common linguistic structure, the sentence, shared by a variety of utterances which differ only in their non-linguistic properties' (Sperber and Wilson 1986:9). However, Lyons holds that 'some utterances, actual or potential, are sentences, whereas others are not. Some utterances are non-sentences, because they are grammatically incorrect; others because they are grammatically incomplete' (Lyons 1981:27). So even the idea that sentences are abstracted out of utterances, and represent the linguistic kernel, is controversial. The very idea of sentences is more or less rejected by some: 'The problems with the sentence exist at every level – historical, ontogenetic, psychological, and universal-grammatical' (Hopper 1983:131, quoted in Figueroa:161).

The ways that different forms of linguistics view sentences and utterances, or more broadly, 'types' and 'tokens', and their relative importance in the study of language, perhaps gives us the clearest indication of the ontological and epistemological commitments made on either side:

> Thus [...] one is still not informed as to how one gets from one level to the next, if system-sentences are decontextualized utterances, how it is logically possible to claim therefore that utterances are derived from sentences. To follow this line of reasoning one must accept the competence-performance distinction, one must accept that sentences are part of competence while utterances are part of performance, and one must accept that competence is ontologically prior to performance. (Figueroa 1994:159)

Figueroa here describes fairly succinctly the issue at stake, and the standard interpretation of the problems it causes. There are 'sentences', and these are in some sense non-corporeal, idealised mental objects. Then there are 'utterances', which are somehow temporal/physical instances of a sentence. Naturally, this type-token distinction exists for any noun: there is rice, and there is a bag of rice; there is chess, and there is a game of chess; and there is the atom, and there are all the atoms in the universe. However, in some forms of linguistics, such as TGG, the idealised sentence is the object of study in itself, while the spoken or written instances of it are relegated to the status of imperfect examples. This is a step beyond normal idealisation. Idealisation tends to be used because the reality is unavailable, not because the reality is deemed less interesting.

For TGG, then, the sentence-type actually provides more information than the sentence tokens, or utterances, which make up everyday language use.

For Chomsky, sentences are 'psychologically real' (1986:36-9); for Katz, they are 'Platonic objects' (1981:*passim*); and for some sociolinguists, they are snake oil. For this last group, utterances should be the (collective) object of study, while sentences are no more use as an idealisation than, say, 'rice', as opposed to just all the rice in the world, or examples of rice. As Figueroa points out above, a sentence, in Chomskyan terminology, belongs to 'competence', our knowledge of grammar and grammatical rules, which can also be described as the set of all possible well-formed sentences in an individual's language. An utterance, according to this division, belongs to 'performance', the physical realisation of that knowledge, or everything that gets said, ever. Performance by this definition is 'imperfect', in the sense that although their knowledge may be flawless, people make mistakes in production, and do not always talk in well-formed sentences.

Furthermore, according to this reading, performance is 'derived' from competence, and competence is 'ontologically prior' to performance. The first of these is perhaps more amenable to empirical confirmation than the second (which was discussed above). For performance to be derived from competence, we would need to show that our knowledge of grammar produces sentences which are physically processed and then uttered, with occasional mistakes in production having no bearing on the original knowledge of language. This has an intuitive appeal, on the grounds that first one thinks of a sentence, and then one produces it, i.e. people have to think before they speak. Although this is indeed intuitively plausible, there are problems.

The most important of these, for the purposes of my argument, is that, in order to demonstrate this causal link, we would need to show that competence contains one sentence, which can be produced many times while remaining unchanged. In brief, the problem is that we are looking for a causal link between a mental posit and a physical process, a perennial philosophical problem.[153] Furthermore, and more damaging, we would need to show that such a path of production, from mental type to physical token, is available to us in a demonstrable way, and in a way which does not rely on the posits of the theory (competence, sentences, etc.) to confirm it. This is the exact problem which demonstrates incommensurability – it turns on the postulation of mental objects, and the extent to which we are talking metaphorically or not when we posit them.

The two main problems in dealing with sentences and utterances, then, are, first, the idealised locus of our (phenomenal) linguistic abilities, and the connection between this knowledge and its physical manifestations; and, second, the status, metaphorical or otherwise, of the mental objects which are posited as part of a theory of language. What is particularly interesting for my purposes is that this problem is barely acknowledged as a problem, especially by practitioners of Chomskyan linguistics, who argue specifically that it is NOT a problem (see the arguments about mentalism above); the idealised nature of the sentences described is accepted and described as an inescapable and unremarkable fact of life.

These discrepancies arise from conceptual incommensurability over the nature of sentences and utterances, and these in turn arise from conceptual differences over the nature of language. Differences over the object of study entail different methods for studying it, and together these form incommensurable paradigms. Furthermore, all of these discrepancies are unidirectional – they stem from defining the other in terms of one's own postulates. This is to say that the ontological and methodological commitments made by a theory which aims to study 'utterance' preclude that theory from properly defining what 'sentence' might mean, except in nega-

[153] See Davidson (2001:207-225) on the identification of mental states with brain states, and Dupré (2000b) for a brief but comprehensive treatment of reductionism.

tive terms relative to the positive definition of 'utterance' (and vice versa). As we saw in section 2.3.1 above, 'negative definition' of this type is also something regularly done by Chomskyans. In that section I looked at the definitions of I-language and E-language; it is worth repeating them here to see how they fit into the universals/particulars debate.

> I-language is amenable to scientific investigation in a way that E-language is not. (Smith 1999:31)
> One might define E-language in one or another way, but it does not seem to matter how this is done; there is no known gap in linguistic theory, no explanatory function, that would be filled were such a concept presented. (Chomsky 1995:16).

So 'E-language' is a concept of ever-changing reference, and cannot be studied. This is exactly how Kuhn describes incommensurability – it is a set of circumstances in which concepts are inexpressible in the vocabulary of a particular theory.

2.4 The way they interlink as per Kuhn's model

We can put these definitions into tables in order to demonstrate the knock-on effects that the meaning of one word has on the meaning of another. Such 'webs' of meaning are exactly as anticipated by Kuhn (2000:35-7, 43-53' 91-94; 1962:128-9).

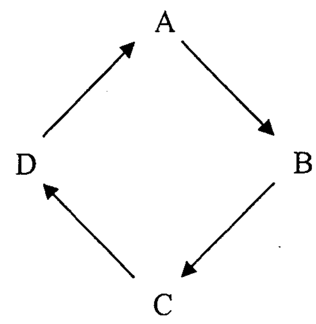
TGG	**Sociolinguistics**
PARADIGM-DEFINING TERMS	**PARADIGM-DEFINING TERMS**
The abstract structure of the **mind** is amenable to organised study. **Variation** must be idealised away. **Linguistics** studies invariant abstract language structures in the mind.	**Variation** is an integral part of language, and is amenable to scientific study. **Abstract mental structures** are meaningless when context-free. **Linguistics** studies language variation in real time and space.
SCIENCE AND METHODOLOGY	**SCIENCE AND METHODOLOGY**
Competence reflects **grammaticality**.	**Communicative competence** drives

Grammaticality is imperfectly reflected by **acceptability**. The mind is ontologically prior in the **Cartesian framework**. Linguistics studies **formal** mental linguistic structures. **Syntax** is autonomous with respect to **semantics** and **pragmatics**. **Theories** are chosen according to Rationalist criteria – **data** is produced through introspection.	**performance**. The mind is dependent on the outside world in the **Hegelian framework**. Linguistics studies the **function** of language as a communicative tool **Pragmatics** is prior to **syntax** and **semantics**. **Theories** are chosen according to Empiricist criteria – **data** is produced through human communication, and data drives theory choice.
THE OBJECT OF STUDY	**THE OBJECT OF STUDY**
Language is a module and an innate species-specific capacity. Language is an organ in a state. **Knowledge of language** is therefore knowledge of syntax, and is synonymous with **linguistic competence**. **I-language** is the proper object of linguistics, not **E-language**. Knowledge of language is put to use derivatively.	**Language** is an emergent phenomenon driven by cognitive reactions to social needs Language shorn of its communicative role is degraded of meaning. **Knowledge of language** is therefore a social ability, and is synonymous with **communicative competence**. Language includes in it a reference to society. The **I/E-language** dichotomy has no validity. Knowledge of language is synonymous with appropriate use.
Linguistics is the study of sentences as part of an abstract mental entity. The study of **language** is the study of an abstract mental posit, the ability of **linguistic competence**.	**Linguistics is the study of utterances as they arise in particular events.** The study of **language** is about the intent and function with which the users of language are able to perform the required communicative tasks made possible by their abilities to **competently use language** in social situations.

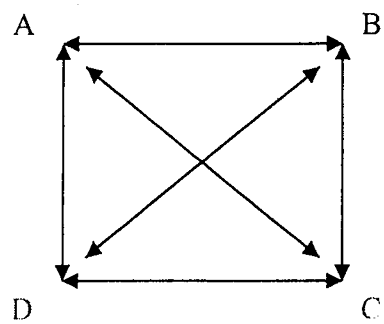
It is crucial to this Kuhnian understanding of the meaning of words that the knock-on effect is not unidirectional (1), or circular (2) but a web (3). In this diagram an arrow between two words X and Y means 'the meaning of X has an effect on the meaning of Y'.

1 A ⟶ B ⟶ C ⟶ D

2



3



Obviously the above diagram is heavily schematised (for example, on occasion the effect will be unidirectional), but the principle is sound. Whether or not we are talking about four words in a technical vocabulary or four hundred, the interdependent nature of their meanings remains. To recapitulate, incommensurability can be (and obviously is) partial. Miscommunication would only occur between, say, Ptolemaic and Copernican astronomers from the same language community when they attempted to use the technical vocabulary of their paradigm without allowing for the differences in meaning that that vocabulary had for the other paradigm. Moreover, Kuhn was keen to stress that this incommensurability, which sounds so final and divisive, can be obviated with a little good will and partial language learning (Kuhn 2000:43-7). It is not difficult for a sociolinguist to learn the 'language' of TGG, or vice versa, but it is easy for both sides to forget that they are speaking a different language to begin with.

This then allows us to explain why statements made within one school of linguistics might literally sound like nonsense to linguists from a competing school. For example, Saville-Troike mentions that 'on some occasions, proving "incompetence" may have practical benefits' (1989:26). Figueroa, writing about Gumperz, asks 'How similar does competence have to be for successful communication?' (1994:136). However, to a Chomskyan linguist this is nonsensical, as for Chomskyans 'incompetence', if such a term were in common use, would refer to pathological cases of complete lack of linguistic knowledge, which could not conceivably have practical benefits 'on occasions'.

3.0 The theory of reference for terms in scientific theories, and how it solves the problems discussed in this thesis

Having looked in detail at the incommensurable sets of vocabulary which have arisen between different schools of linguistics, it should now be clear how this tallies with the theory of reference which was given in chapter one. The incommensurability derives from ontological and epistemological commitments regarding the nature of human language and what might be acceptable ways of approaching its study. Ontological claims which are made metaphorically are liable to be incommensurable with other such claims.

My theory of reference is a synthesis of ostensive theories of reference and Lockean representational theories. Here I will briefly re-state the main arguments that theory, as it was described in chapter one:

1) Reference in scientific theories is, and should be, a different activity from natural language reference. Where natural language developed or-

ganically, and does not view fuzzy or imprecise categories as problematic, reference in scientific theories demands precision.

2) There are two main theories of metaphor in the philosophy of science: the 'standard' empiricist/positivist account, and the (supposedly) relativist viewpoint of Kuhn *et al.* The first holds that a metaphor does the same work as a literal theory in discovering natural kinds. The second rejects the existence of natural kinds, or at least our ability to discover them.

3) Ostensive theories of reference also assume the existence of natural kinds. As Kuhn describes this view, 'we accommodate language to the world' (in Ortony1979:418).

4) Kuhn argues, conversely, that we accommodate the world to language. For this reason Kuhn rejects natural kinds, as metaphor and language could have found 'other joints' in nature. This is consistent with his 'instrumental' view of science: we can get better and better at using nature, but we are not zeroing in on a Kantian *Ding an Sich* ('thing in itself'), or the 'essences' of natural kinds, which standard theories of reference and metaphor describe.

5) I propose extending Kuhn's account to viewing all of *science itself as metaphor for our understanding of the world.* This account of metaphor states that all types of theory are a metaphor for the thing in the world which they are trying to describe.

6) This can be illustrated by comparison with Locke's representational theory of language. Locke observed that words do not refer to things, but to our concepts (or 'ideas') of things (1964 [1690]:259). This theory also implies a certain scepticism about the possibility of direct knowledge of the world, and foreshadows Kant's insistence that we know nothing of the

noumena (the *Ding an Sich*), only the phenomena (see Smith and Greene 1940:330-34).

7) In the same way, science is not a description of the world; rather it is a set of theories which describes our knowledge of the world. This entails a triadic relationship between us, science and the world, just as Locke's theory of reference entails a triadic relationship between us, things and words. It is also in line with Kuhn's relativistic assertion that our scientific theories do not converge towards 'a permanent fixed scientific truth' (1962/1969:173), but instead represent our best understanding of the object of enquiry.

8) A standard empiricist account of natural science holds that, when successful, it does interact with the real world directly. The social sciences do not deal with real objects but with mental (or behavioural) posits, and one of the strengths of the social sciences is their acknowledgement of this fact along with the espousal of a methodology which acknowledges the *ad hoc* nature of the posits, expecting each researcher to refine and justify them as research proceeds.

Under this 'empiricist' account, then, only theories in social science (and subjects like TGG which also deal with mental posits) are metaphors, as they deal with mental or behavioural posits, while natural science theories are not metaphors in this sense, as they refer literally to things in the world. If natural science terms are not metaphorical then they refer to natural kinds. And if nature does indeed divide at discoverable joints, there is no reason why our descriptions of, for example, how the mind works should not match up with neural processes at some point in the future.

9) However, this does not accord with Kuhn's analysis of *all* science being our best description of the world, rather than a literal depiction of it. Nor does it fit with Kuhn's (partial) rejection of natural kinds.

Following Kuhn's theory of metaphor, then, we are directed towards the simpler and more consistent (and ultra-relativistic) view that all scientific terms, whether in natural or social science, are metaphorical, in the sense that we cannot discover the essence of natural kinds or mental objects; we accommodate the world to our language at the same time as accommodating our language to the world.

10) Social sciences (and TGG) are less 'literal' than natural science. The terms of a theory in natural science refer to our understanding of the world. However, this understanding of the world is only provisional, in that it is mediated by the language of the scientific theories used to describe it. Social science, on the other hand, remains two steps away from reality. This is because the posits in social science do not refer literally to objects observed empirically in the world, they refer to putative mental or social phenomena which may or may not be empirically confirmed. So we can maintain the traditional natural/social science divide under my 'Kuhnian' interpretation of the role of metaphor in scientific theories.

11) Metaphor and Linguistics

If this is right, then the status of TGG becomes difficult to categorise. It is not a natural science with a hermeneutic base, because of the 'metaphorical' nature of the posits which it begins with (such as VPs, cyclicity, phases etc). However, it is not a social science because it does not pair an awareness of its hermeneutic base with a hermeneutic methodology. According to this Kuhnian analysis, on the other hand, sociolinguistics fits neatly into the social sciences with very little to distinguish it, methodologically speaking.

It is not the case that TGG models neural processes and mental processes in corresponding ways, and that tree diagrams (for example) are a way of depicting our knowledge of linguistic processes. They are, rather, a metaphor for our understanding of linguistic processes, in the same way that a social science posit such as 'identity' is a metaphor for a type of human behaviour, rather than a depiction of or a reference to a thing in the world. So both TGG and sociolinguistics are metaphors for our understanding of language.

12) So TGG is epistemically equivalent to social science, but not part of it. There are natural sciences, the language of whose theories is metaphorical of our understanding of the things in the world, and there are social sciences, the language of whose theories refers to posits which may or may not coincide with the things in the world. The natural sciences are therefore one 'epistemic step' away from the world, as we interpret our knowledge of the world through a metaphorical language. The social sciences, meanwhile, are two epistemic steps away from the world, as there are both a metaphorical language *and* posits of uncertain ontological status between them.

Under these criteria, sociolinguistics is unquestionably a social science. TGG also falls into this second category, because it deals with posits. However, it is not a social science because its object of study does not lie in the social sphere. It is for this reason that I conclude that it is epistemically equivalent to the social sciences, while not belonging to them.

13) How does incommensurability arise across disciplines which appear to share an object of study? The answer lies in the theory of 'dubbing' and ostensive reference, as explained by Putnam (1975:215-271). This holds that part of the meaning of a term is determined by and refers to natural kinds as they are constituted in nature.

However, when we are dubbing mental posits, in TGG or in social science, we are one step further away from the 'unknowable essence of things'. This means that one person can dub one mental posit, and another dub another mental posit. They cannot show each other, and they do not have to tell each other. Crucially, they can come from the same speech community (e.g. English). So as theories develop based on these alternate sets of mental posits, there is little reason to expect the webs of meaning (of dubbed mental posits) to match up. With no history of ostensive reference for the word or term (e.g. 'linguistic competence'), its theoretical meaning is exactly what it is used to mean within that theory.

This explains also why TGG and sociolinguistics can be incommensurable without necessarily being in competition (as Kuhnian paradigms are usually taken to be). Their theoretical languages, while derived from English, have different (but overlapping) putative references.

14) This Lockean theory of representation does not need to stand in opposition to a theory of ostensive reference, as given by Putnam or Kripke. In these theories, there is a chain of reference which leads back to the thing itself, or the initial dubbing. All the Lockean theory does is to replace the original *Ding an Sich* with the original idea, as conceived at the moment of dubbing.

We can add a proviso to the ostensive theory of reference by adding that all dubbing is provisional, as it occurs in scientific theories. We have seen that a scientific revolution can dispose of, or completely transform, any member or subset of our ontology, so any dubbing can only refer to something as we believe it to exist within our paradigm.

15) This analysis concerns primarily the language of scientific theories. It does not necessarily apply to ordinary language. Whether or not a

Lockean theory of ideas, or a Kripkean chain of reference, is acceptable to philosophers of language at large is irrelevant to the analysis of the language of scientific theories.

We should, therefore, expect different rules to apply to the language of scientific theories, or at least variants on the normal rules. In particular, we should not be surprised to find that reference works differently in scientific theories, because the way things are named is different, and the putative contents of scientific theories are different from those of normal discourse. Science has a different, and in some ways stranger, ontology than everyday life, and consequently a different way of talking about it.

In these fifteen steps we can see how a hybrid theory of reference, an understanding of the role of metaphor in philosophy of science, and a rejection of natural kinds, can help us to understand how incommensurable vocabularies can arise across different schools of linguistics. At the same time they help us to explain, from a Kuhnian point of view, why the claims of just about any form of linguistics to the status of a natural science is bound to obscure, rather than to illuminate, the debate.

What we have gained from an examination of the disputes over Kuhn and over the relationship between linguistics and epistemology are the following:

- Linguists are keen to position their subjects within a historical lineage, by using both Kuhn and older philosophers.
- This naturally leads to arguments about linguistics and the nature of language. However, it leaves out a crucial other side of Kuhn's philosophy, namely the focus on incommensurability.
- What I have done is to go one further than Kuhn, and to show that incommensurability arises as a result of using natural language

metaphorically in scientific theories, when *dubbing* stuff that might not exist outside the head of the dubber.

As an illustration, we saw that words such as 'sentence' are adopted from natural language and given a precise theoretical definition. However, those in another 'paradigm' can adopt the same word within another theoretical framework. On the most fundamental level, this happens to the object of study itself, in this case the word 'language', as well as such interlinked factors such as 'mind' and 'variation'.

Conclusion to the thesis

The aim of this thesis has been to show that the vocabulary sets used in TGG and sociolinguistics can accurately be described as incommensurable, and to show that this incommensurability can be explained by my theory of reference. If we accept that terms which refer to posits in scientific theories have a different referential genesis from words in natural language, then it quickly becomes clear that their purported references are very different. The freedom of postulation in the social sciences results in a particular susceptibility to incommensurability, as one term can refer to two posits with nothing in common except their name.

The works of Thomas Kuhn have provided a frame on which to hang the thesis. This thesis started, for me, with an examination of why linguists were so interested in Kuhn, but it led to the realisation that Kuhn's theories of incommensurability and his ideas on metaphor and reference, could be developed in ways which explain why linguists felt the need to frame their subject in terms of his theory of paradigms, and why that model does not fit the recent history of linguistics particularly well.

Without Kuhn it is possible to make sense of linguistics, but I have filled a gap by showing that the discipline which focuses most on the idea of 'language' can use his philosophy to explain its own institutional dilemmas. Incommensurable ideas correlate with incommensurable vocabularies, and different paradigms speak different languages. The irony is that linguists, who are so adept at analysing the languages of others, have missed an interesting aspect of their own languages, and the subsequent communication difficulties which these have engendered.

The issues discussed in this thesis, regarding the underlying philosophical commitments of linguistics, the nature of linguistic explanation, and the social and institutional history of linguistics, continue to be discussed, and give rise to lively debate and disagreement. The Kuhnian or otherwise nature of linguistics, especially that of TGG, produces, is a source of fascination for both linguists and philosophers – a recent publication, *Chomskyan (R)evolutions* edited by Douglas Kibbee, brings together a large number of the more recent contributions, and the debate shows little sign of receding.

The consequences of this thesis are that, I hope, it will be possible for those whose work involves familiarity or contact with both paradigms will see them in a new light. What sometimes looks like entrenched disagreement, or plain bloody-mindedness, might in fact be incommensurable concepts obscured by identical vocabulary.

BIBLIOGRAPHY

Aarsleff, H., (1967) *The Study of Language in England 1780-1869* Princeton: Princeton University Press

Aarsleff, H., (1982) *From Locke to Saussure: Essays on the Study of Language and Intellectual History.* London: Athlone.

Aitchison, J., (1995) *Linguistics: an Introduction (4th ed.).* London: Hodder & Stoughton.

Anscombe, E., and Geach, P. (eds.), (1954) *Descartes: Philosophical Writings.* London: Open University Press.

Antilla, R., (1975) 'Revelation as Linguistic Revolution' in Makkai, A. (ed.), *The First LACUS Forum.* Columbia SC: Hornbeam.

Armstrong, D., (1978) *Universals and Scientific Realism, Vol. II. A Theory of Universals.* Cambridge: Cambridge University Press.

Auden, W.H., (1966) *Collected Shorter Poems 1927-1957.* London: Faber and Faber.

Austin, J. L., (2005) *How to Do Things with Words (2nd ed.).* Cambridge, Mass.: Harvard University Press.

Barnes, B., (1982) *Kuhn and Social Science.* London: MacMillan.

Barsky, R., (1997) *Noam Chomsky: A Life of Dissent.* Cambridge: MIT Press (accessed through University of Sheffield Library website 17/02/11). http://cognet.mit.edu.eresources.shef.ac.uk/library/books/mitpress/0262522551/cache/chpt1.pdf

Becher, T., (1989) *Academic Tribes and Territories.* Milton Keynes: Open University Press.

Bell, R., (1976) *Sociolinguistics: Goals, Approaches and Problems.* London: Batsford.

Berlin, B. and Kay, P., (1969) *Basic Color Terms: Their Universality and Evolution.* London: University of California Press.

Bierwisch, M., (1971) *Modern Linguistics : Its Development, Methods and Problems*. The Hague : Mouton.

Bickerton, D., (1990) *Language and Species*. London: University of Chicago Press.

Bickerton, D., (1995) *Language and Human Behaviour*. London: UCL Press.

Biglan, A., (1973) 'The Characteristics of Subject Matter in Different Scientific Areas'. *Journal of Applied Psychology*, 57:3, 195-203.

Bird, A., (2000) *Thomas Kuhn*. Chesham: Acumen.

Black, M., (1962) *Models and Metaphors: Studies in Language and Philosophy*. New York: Cornell University Press.

Bloomfield, L., (1935) *Language*. London: George Allen & Unwin.

Boeckx, C., (2006) *Linguistic Minimalism: Origins, Concepts, Methods, and Aims*. Oxford: Oxford University Press.

Borjars and Burridge (2000) *Introducing English Grammar*. New York: Arnold.

Botha, R.P., (1991) *Challenging Chomsky*. Oxford: Blackwell.

Bresnan, J., (2001) *Lexical-Functional Syntax*. Malden, Mass: Blackwell.

Brown, C., (1976) 'General principles of human anatomical partonomy and speculations on the growth of partonomic nomenclature.' *American Ethnologist* 3:3, 400–424)

Brown, H., (2005) 'Incommensurability reconsidered'. *Studies In History and Philosophy of Science* Part A Volume 36, Issue 1, 149-169.

Buchholz, M. and Hall., (2005) 'Identity and Interaction: A Sociocultural Approach'. *Discourse Studies* 7-4/5 585-612.

Buchholz, M. and Hall., (2008) 'All of the above: New coalitions in sociocultural linguistics' *Journal of Sociolinguistics* 12/4, 2008: 401–431

Bugarski, R. 1976 in Parrett (1976) ed. *History of Linguistic Thought and Contemporary Linguistics*. Amsterdam: Mouton de Gruyter.

Carlson, G., (2003) 'On the notion "showing something"' in Moore, J. and Polinksy, M. (eds.) *The Nature of Explanation in Linguistic Theory*. Stanford: CSLI Publications.

Calvin, William H., and Bickerton, D., (2000) *Lingua ex Machina: Reconciling Darwin and Chomsky with the Human Brain*. Cambridge, Mass.: CogNet.

Cecchetto, C. and Donati, C., (2010) 'On Labeling: Principle C and Head Movement'. *Syntax* 13:3 241–278.

Cela-Conde, C. and Marty, G., (1998) 'Noam Chomsky's Minimalist Program and the Philosophy of Mind. An Interview'. *Syntax* 1:1 19-36.

Chalmers, A.F., (1978) *What is This Thing Called Science?*. Milton Keynes: Open University Press.

Chambers, J.K. and Trudgill, P., (1980) *Dialectology*. Cambridge: CUP.

Chambers, J. K. (2003) *Sociolinguistic Theory: Linguistic Variation and Its Social Significance (2nd ed.)*. Oxford: Blackwell.

Chambers, J.K., Trudgill, P. and Schilling-Estes, N. (eds.), (2002) *The Handbook of Language Variation and Change*. Malden, Mass.: Blackwell.

Chomsky, N., (1957) *Syntactic Structures*. The Hague: Mouton.

Chomsky, N., (1959) 'Review of *Verbal Behavior* by B. F. Skinner'. *Language* 35:1 26-58. (Reprinted in Leon A. Jakobovits, L. and Miron, M. (eds.) (1967) *Readings in the Psychology of Language*. Englewood Cliffs (N.J.): Prentice-Hall).

Chomsky, N., (1964) *Current Issues in Linguistic Theory*. The Hague: Mouton.

Chomsky, N., (1965) *Aspects of the Theory of Syntax.* Cambridge: MIT Press.

Chomsky, N., (1966a) *Cartesian Linguistics: A Chapter in the History of Rationalist Thought.* New York and London: Harper & Row.

Chomsky, N., (1966b) *Topics in the Theory of Generative Grammar.* The Hague: Mouton.

Chomsky, N., (1968) *Language and Mind.* New York: Harcourt Brace Jovanovich.

Chomsky, N., (1972 [1968]) *Language and Mind (Enlarged edition).* New York: Harcourt Brace Jovanovich.

Chomsky, N., (1975) *Reflections on Language.* London: Fontana.

Chomsky, N., (1977) *Essays on form and Interpretation.* New York: North Holland.

Chomsky, N., (1980) *Rules and Representations.* Oxford: Basil Blackwell.

Chomsky, N., (1981) *Lectures on Government and Binding.* The Hague: Walter de Gruyter.

Chomsky, N., (1982) *Some Concepts and Consequences of the Theory of Government and Binding.* Cambridge: MIT Press.

Chomsky, N., (1986) *Knowledge of Language: Its Nature, Origin and Use.* London: Praeger.

Chomsky, N., (1988) *Language and Problems of Knowledge: the Managua Lectures.* Cambridge: MIT Press.

Chomsky, N., (1995) *The Minimalist Program.* Cambridge: MIT Press.

Chomsky, N., (2000a) *The Architecture of Language.* Oxford: OUP.

Chomsky, N., (2000b) *New Horizons in the Study of Language and Mind.* Cambridge: CUP.

Chomsky, N., (2000c) 'Minimalist Inquiries: The Framework'. In Martin, R., Michaels, D., & Uriagereka, J., (eds.). *Step By Step: Essays In Syntax in Honor of Howard Lasnik*. 89–155. Cambridge: MIT Press.

Chomsky, N., (2005) 'Three Factors in Language Design'. *Linguistic Inquiry* 36:1, 1-22.

Chomsky, N., (2008) 'On Phases', in Freidin, Otero and Zubizarreta (eds.).

Chomsky, N., Krauss, L., and Carroll, S., (2006) *Science in the Dock* (Discussion http://www.chomsky.info/debates/20060301.htm)

Collier, P. and Horowitz, D., (2004) *The Anti-Chomsky Reader.* Michigan: Encounter Books.

Cornips, L. and Corrigan, K., (2005) *Syntax and Variation: Reconciling the Biological with the Social.* Philadelphia: John Benjamins.

Couzens Hoy, D. (ed.), (1986) *Foucault: A Critical Reader.* Oxford: Basil Blackwell.

Crystal, D., (1997) *The Cambridge Encyclopedia of Language.* (2nd ed.) Cambridge: CUP.

Danes, F., (1970) 'One Instance of Prague School Methodology. Functional Analysis of Utterance and Text' in Garvin (ed.) *Method and Theory in Linguistics.* The Hague: Mouton.

Davidson, D., (1984) *Inquiries into Truth and Interpretation.* Oxford: Clarendon Press.

Davidson, D., (2001) *Essays on Actions and Events (2nd ed.).* Oxford: Clarendon Press.

Dawkins, R., (1995) *River out of Eden.* London: Orion.

Dawkins, R., (2006) *The God Delusion.* London: Transworld.

Demir, I., (2008) 'Incommensurabilities in the work of Thomas Kuhn'. *Studies In History and Philosophy of Science* Part A Volume 39, Issue 1, 133-142.

Descartes, R., (1633) 'The World' in Cottingham, J., Stoothoff, R. & Murdoch, D. (eds.) (1985) *The Philosophical Writings of Descartes, Vol. 1* Cambridge: CUP.

Descartes, R., (1637) 'Discourse on the Method' in Cottingham, J., Stoothoff, R. & Murdoch, D. (eds.) (1985) *The Philosophical Writings of Descartes, Vol. 1* Cambridge: CUP.

Descartes, R., 1642 'Meditations' in Cottingham, J., Stoothoff, R. & Murdoch, D. (eds.) (1985) *The Philosophical Writings of Descartes, Vol. 1* Cambridge: CUP.

Devitt, M., and Sterelny, K., (1987) *Language and Reality: An Introduction to the Philosophy of Language.* Oxford: Basil Blackwell.

Dik, S., (1978) *Functional Grammar.* Amsterdam: North Holland.

Dillinger, M., (1984) 'Review: *Language and Other Abstract Objects* by J. J. Katz'. *Philosophy of Science* 51:1 175-176.

Dingwall, W., (1971) *A History of Linguistic Science.* Linguistics Programme, University of Maryland.

Dreyfus, H. and Rabinow, P., (1982) *Michel Foucault: Beyond Structuralism and Hermeneutics.* Chicago: CUP.

Dupré, J., (2000a) 'Natural Kinds' in Newton-Smith (2000).

Dupré, J., (2000b) 'Reductionism' in Newton-Smith (2000).

Duranti, A., (1997) *Linguistic Anthropology.* Cambridge: CUP.

Fasold, R., (1984) *The Sociolinguistics of Society.* Oxford : Blackwell.

Fedorenko, E., and Gibson, E., (2010) 'Adding a Third *Wh*-phrase Does Not Increase the Acceptability of Object-initial Multiple-*wh*-questions'. *Syntax* 13:3 183-195.

Feigl, H., Scriven, M. and Maxwell, G. (eds.) (1958) *Concepts, Theories and the Mind-Body Problem.* Minneapolis: University of Minnesota Press.

Feyerabend, P., (1965). 'On the "Meaning" of Scientific Terms'. *Journal of Philosophy* 62 (10):266-274.

Feyerabend, P., (1975) *Against Method.* London: NLB.

Feyerabend, P., (1978) *Science in a Free Society.* London: NLB.

Feyerabend, P., (1981) *Problems of Empiricism: Philosophical Papers volume 2.* Cambridge: CUP.

Feyerabend, P., (1987). 'Putnam on Incommensurability'. *British Journal for the Philosophy of Science* 38 (1):75-81.

Figueroa, E., (1994) *Sociolinguistic Metatheory.* Oxford: Pergamon.

Fishman, J. (ed.), (1968) *Readings in the Sociology of Language.* The Hague: Mouton.

Fodor, J., (1975) *The Language Of Thought.* Cambridge: Harvard University Press.

Foucault, M., (1970) *The Order of Things: An Archaeology of the Human Sciences* (translated from the French). London: Routledge.

Foucault, M., (1972) The archaeology of knowledge (translated from the French by A.M. Sheridan Smith). London: Tavistock.

Freidin, R., Otero., C and Zubizarreta, M., (eds.) (2008) *Foundational Issues in Linguistic Theory: Essays in Honor of Jean-Roger Vergnaud.* Cambridge: MIT Press.

Goffman, E., (1981) *Forms of Talk.* Philadelphia: University of Pennsylvania Press.

Goodman, N., (1978) *Ways of Worldmaking.* Indianapolis: Hackett.

Gould, S., and Vrba, E., (1982) 'Exaptation – a Missing Term in the Science of Form'. *Paleobiology* 8 (1): 4–15.

Graffi, G., (2001) *200 Years of Syntax.* Amsterdam: John Benjamins.

Greenbaum, S., (1973) 'Informant elicitation of data on syntactic variation'. *Lingua* 31:2-3, 201-212.

Greenbaum, S., (ed.) (1977) *Acceptability in Language.* The Hague: Mouton.

Greene, J., (1972) *Psycholinguistics: Chomsky and Psychology.* Harmondsworth: Penguin.

Gumperz, J., (1968) 'Types of Linguistic Communities' in Fishman (ed.) *Readings in the Sociology of Language.* The Hague: Mouton.

Gumperz, J., (1971) *Language in Social Groups : Essays Selected and Introduced by Anwar S. Dil.* Stanford: CUP

Gumperz, J. and Hymes, D. (eds.), (1972) *Directions In Sociolinguistics, Ethnography of Communications.* Oxford: Blackwell.

Gutting, G., (1989) *Michel Foucault's Archaeology of Scientific Reason.* Cambridge: CUP.

Halliday, M. and Matthiessen, C., (2004) *An Introduction to Functional Grammar* (3rd ed.). London: Arnold.

Harman, G. (ed.), (1974) *On Noam Chomsky: Critical Essays.* Garden City, N.Y: Anchor Press.

Harre, R., (1986) *Varieties of Realism.* Oxford: Blackwell.

Harris, R. A., (1993) *The Linguistics Wars.* Oxford: OUP.

Harris, R. A., (1998) 'Review of Robert F. Barsky's *Noam Chomsky: A life of dissent*' *Books in Canada* 27.2, 14-17.

Harris, Roy (1996) *The Language Connection: Philosophy and Linguistics.* Bristol: Thoemmes.

Harris, Roy (2003) *Saussure and His Interpreters* (2nd ed.). Edinburgh: Edinburgh University Press.

Hauser, M., Chomsky, N., and Fitch, W. T., (2002) 'The Faculty of Language: What Is It, Who Has It, and How Did It Evolve?'. *Science* 298:1569-1579.

Herder, J. G., 1772 *Essay on the Origin of Languages* (tr. Alexander Gode), in Moran, J. and Gode, A. *On the Origin of Language* (1966) London: University of Chicago Press.

Hockett, C., (1955) *A Manual of Phonology*. Baltimore: Waverly Press.

Hockett, C., (1958) *A Course in Modern Linguistics*. New York: MacMillan.

Hockett, C., (1967) *Language, Mathematics and Linguistics*. The Hague: Mouton.

Hockett, C., (1968) *The State of the Art*. The Hague: Mouton.

Hockett, C., (1977) *The View from Language: Selected Essays 1948-1974*. Athens: University of Georgia Press.

Hockett, C., (1987) *Refurbishing Our Foundations: Elementary Linguistics from an Advanced Point of View*. Amsterdam; Philadelphia : J. Benjamin.

Hollis, M., (1994) *The Philosophy of Social Science*. Cambridge: CUP.

Hopper, P., (1988) 'Emergent Grammar and the A Priori Grammar Postulate', in Tannen (ed.) *Linguistics in Context: Connecting Observation and Understanding*. Norwood, N.Y.: Ablex.

Houlgate, S. (ed.), (1998) *The Hegel Reader*. Oxford: Blackwell.

Hoyningen-Huene, P., (1990) 'Kuhn's Conception of Incommensurability'. *Studies In History and Philosophy of Science* Part A 21:3 481-492.

Huck, G., and Goldsmith, J., (1994) *Ideology and Linguistic Theory : Noam Chomsky and the Deep Structure Debates*. London: Routledge.

Hudson, G., (2000) *Essential Introductory Linguistics*. Oxford: Blackwell.

Hurford, J. and Heasley, B., (1983) *Semantics: A Coursebook*. Cambridge: CUP.

Hymes, D., (1971) 'On Communicative Competence', in Pride and Holmes (eds.) (1972).

Hymes, D. (ed.), (1974) *Studies in The History of Linguistics: Traditions and Paradigms.* Bloomington, London: Indiana University Press.

Hymes, D., (1977) *Foundations in Sociolinguistics: An Ethnographic Approach.* London: Tavistock.

Hymes, D., (1983) *Essays in the History of Linguistic Anthropology.* Amsterdam; Philadelphia: J. Benjamins.

Hymes, D., (1996) *Ethnography, Linguistics, Narrative Inequality: Toward an Understanding of Voice.* London: Taylor & Francis.

Irzik, G., and Grünberg, T., (1998) 'Whorfian Variations on Kantian Themes: Kuhn's Linguistic Turn Studies'. *History and Philosophy of Science* 29:2 207-221.

Jackendoff, R., (1972) *Semantic Interpretation in Generative Grammar.* Cambridge: MIT Press.

Jacobs, S., (2002) 'Polanyi's Presagement of the Incommensurability Concept'. *Studies In History and Philosophy of Science* 33:1 101-116.

Jardine, N., (2003) 'Whigs and Stories: Herbert Butterfield and the Historiography of Science'. *History of Science* 41:125-140.

Joseph, J., (1995) 'The Structure of Linguistic Revolutions'. *Historiographia Linguistica* 22: 3 379–399.

Joseph, J., (1999) 'How Structuralist was American Structuralism?'. *Henry Sweet Society Bulletin* 33 23–28.

Joseph, J., (2006) *Language and Politics.* Edinburgh: Edinburgh University Press.

Kampf, L., (1967) 'Review of *Cartesian Linguistics* by N. Chomsky' *College English* 28:403-8.

Katz, J., (1964) 'Mentalism in Linguistics'. *Language* 40 124–137.

Katz, J., (1981) *Language and Other Abstract Objects*. Oxford: Basil Blackwell.

Katz, J. (ed.), (1985) *The Philosophy of Linguistics*. Oxford: OUP.

Katz, J. and Bever, T., (1976) 'The rise and fall of empiricism' in Bever, Katz and Langendoen (eds.) *An Integrated Theory of Linguistics Ability*. New York: Crowell.

Kay, P. and McDaniel, K., (1981) 'On the meaning of variable rules: discussion'. *Language in Society* 10 251-258.

Kay, P., and Kempton, W., (1984) 'What is the Sapir-Whorf Hypothesis?'. *American Anthropologist* 86 65-79.

Kibbee, D., (2010) *Chomskyan (R)evolutions*. Amsterdam: John Benjamins.

Koerner, E.F.K., (1976) 'Towards a Historiography of Linguistics' in Parrett (1976) ed. *History of Linguistic Thought and Contemporary Linguistics*. Amsterdam: Mouton de Gruyter.

Koerner, E.F.K., (1983) 'The "Chomskyan Revolution" and its historiography: a few critical remarks'. *Language and Communication* 3:147-69.

Koerner, E.F.K., (1994a) 'The Anatomy of a Revolution in the Social Sciences: Chomsky in 1962' (accessed 17/02/11).
http://www.tlg.uci.edu/~opoudjis/ Work/KK.html

Koerner, E.F.K., (1994b) 'Linguistics and Revolution with Particular Reference to the "Chomskyan Revolution"' (accessed 17/02/11).
http://webh01.ua.ac.be/apil/apil106/EFKKoerner.pdf

Koerner, E. F. K. and Asher R. E., (eds.) (1995) *Concise History of the Language Sciences: From the Sumerians to the Cognitivists*. Oxford: Pergamon, 1995.

Kramer, R., (2010) 'The Amharic Definite Marker and the Syntax–Morphology Interface' *Syntax* 13:3 196-240.

Kripke, S., (1980) *Naming and Necessity.* Oxford: Basil Blackwell.

Kuhn, T., (1962) *The Structure of Scientific Revolutions.* Chicago: University of Chicago Press (including 1969 postscript).

Kuhn, T., (1977) *The Essential Tension.* Chicago: University of Chicago Press.

Kuhn, T., (2000) *The Road Since Structure.* Chicago: University of Chicago Press.

Kuhn, T., (1989) 'The Natural and Human Sciences' (reprinted in Kuhn 2000).

Labov, W., (1966) *The Social Stratification of English in New York City.* Washington, D.C.: Center for Applied Linguistics.

Labov, W., (1972) *Sociolinguistic Patterns* Philadelphia: University of Pennsylvania Press.

Lakatos, I. and Musgrave, A. (eds.), (1970) *Criticism and the Growth of Knowledge: Proceedings of the International Colloquium in the Philosophy of Science, London, 1965.* London: CUP.

Lakoff, G., and Johnson, M., (1980) *Metaphors We Live By.* Chicago: University of Chicago Press.

Lappin, S., Levine, R. and Johnson, D., (2000) 'The Structure of Unscientific Revolutions'. *Natural Language and Linguistic Theory* Vol. 18:665-671.

Lappin, S., Levine, R. and Johnson, D., (2001) 'The Revolution Maximally Confused'. *Natural Language and Linguistic Theory* Vol. 19:901-919.

Larvor, B., (2000) 'History, Role in the Philosophy of Science' in Newton-Smith (2000)

Lass, R., (1980) *On Explaining Language Change.* Cambridge: CUP.

Laudan, L., (1977) *Progress and its Problems.* Berkeley: University of California Press.

Leech, G., (1983) *Principles of Pragmatics.* London: Longman.

Leplin, J., (2000) 'Realism and Instrumentalism' in Newton-Smith (ed.).

Levinson, S., (1983) *Pragmatics.* Cambridge: CUP.

Lipton, P., (2000) *Inference to the Best Explanation* in Newton Smith (2000)

Locke, J., (1964 [1690]) *An Essay Concerning Human Understanding.* Glasgow: William Collins.

Lyons, J., (1977) *Semantics* (2 vols.). Cambridge: CUP.

Lyons, J., (1981) *Language Meaning and Context.* London: Fontana.

Lyons, J., (1991) *Chomsky* (3rd ed.). London: Fontana.

Maclay, H., (1971) 'Linguistics: Overview' in Steinberg and Jacobovits (eds.) *Semantics: An Interdisciplinary Reader in Philosophy, Linguistics and Psychology*, Cambridge: CUP.

Makkai, A. (ed.), (1975) *The First LACUS Forum.* Columbia SC: Hornbeam.

Markova, I. (ed.), (1978) *The Social Context of Language.* Chichester: Wiley.

Markova, I., (1982) *Paradigms, Thought and Language.* London: John Wiley and Sons.

Masterman, M., (1970) 'The nature of a paradigm' in Lakatos, I. and Musgrave, A. (eds.), (1970).

Matthews, P.H., (1993) *Grammatical Theory in the United States from Bloomfield to Chomsky.* Cambridge: CUP.

McCawley, J., (1982) *Thirty Million Theories of Grammar* London: Croom Helm.

McKinney, H. (ed.), (1971) *Lamarck to Darwin: Contributions to Evolutionary Biology, 1809-1859.* Lawrence, Kansas: Coronado Press.

Mesthrie, R., Swann, J., and Deumert, A., (2000) *Introducing Sociolinguistics.* Edinburgh: Edinburgh University Press.

Milroy, L., (1987) *Observing and Analysing Natural Language.* Oxford: Basil Blackwell.

Montuschi, E., (2000) 'Metaphor in Science' in Newton-Smith (ed.).

Müller, M., (1862) *Lectures on the Science of Language* London: Longman.

Murray, S., (1980) 'Gatekeepers and the "Chomskian Revolution"'. *Journal of the History of the Behavioural Sciences* 16.73-88.

Murray, S., (1989) 'Recent studies of American linguistics'. *Historiographia Linguistica* 16: 1/2.149–171.

Murray, S., (1994) *Theory Groups and the Study of Language in North America: A Social History.* Amsterdam & Philadelphia: John Benjamins.

Murray, S., (1999) 'More on Gatekeepers and Noam Chomsky's Writings of the 1950s'. *Historiographia Linguistica* 26 343–353.

Neuman, W., (2006) *Social Research Methods: Qualitative and Quantitative Approaches (6th ed.).* London: Pearson Allyn & Bacon.

Newmeyer, F., (1980) *Linguistic Theory in America: The First Quarter Century of Transformational Generative Grammar.* London: Academic Press.

Newmeyer, F., (1983) *Grammatical Theory: Its Limits and Possibilities.* London, Chicago: University of Chicago Press.

Newmeyer, F., (1986a) *Linguistic Theory In America.* London: Academic Press.

Newmeyer, F., (1986b) 'Has There Been a 'Chomskyan Revolution' in Linguistics?'. *Language* 62:1 1-18.

Newmeyer, F., (1986c) *The Politics of Linguistics.* Chicago: University of Chicago Press.

Newmeyer, F., (1998) *Language Form and Language Function.* Cambridge: MIT Press

Newmeyer, F., (2005) *Possible and Probable Languages: A Generative Perspective on Linguistic Typology.* Oxford: OUP.

Newton-Smith, W., (ed.) (2000) *A Companion to the Philosophy of Science.* Oxford: Blackwell.

Olssen, M., (2006) *Michel Foucault: Materialism And Education.* Boulder: Paradigm.

Oppenheim, P., and Putnam, H., (1958) 'The Unity of Science as a Working Hypothesis' in Feigl, H., Scriven, M. and Maxwell, G. (eds.) *Concepts, Theories and the Mind-Body Problem.* Minneapolis: University of Minnesota Press.

Ortony, A. (ed.), (1979) *Metaphor and Thought.* Cambridge: CUP.

Parrett (1976) ed. *History of Linguistic Thought and Contemporary Linguistics* Amsterdam: Mouton de Gruyter.

Pateman, T., (1987) *Language in Mind and Language in Society. Studies in Linguistic Reproduction* Oxford: Clarendon Press.

Percival, W., (1972) 'On the Non-Existence of Cartesian Linguistics' in Butler, R. (ed.), *Cartesian Studies.* Oxford: Blackwell.

Percival, W., (1976) 'The Applicability of Kuhn's Paradigms to the History of Linguistics'. *Language* 52:2 285-294.

Pinker, S., (1994) *The Language Instinct.* London: Penguin.

Pinker, S., (2002) *The Blank Slate.* London: Penguin.

Pinker, S., and Jackendoff, R., (2005) 'The faculty of language: what's special about it?' *Cognition* 95:2 201-236.

Plato *Lysis/Symposium* (accessed 17/02/11).
http://classics.mit.edu/Plato/lysis.html

Pollard, C. and Sag, I., (1994) *Head-Driven Phrase Structure Grammar.* Chicago: University of Chicago Press.

Popper, K., (1963) *Conjectures and Refutations.* London: Routledge.

Postal, P., (2004) *Skeptical Linguistic Essays.* Oxford: OUP.

Pride, J. and Holmes., J. (eds.), (1972) *Sociolinguistics: Selected Readings.* London: Penguin.

Pullum, G. and Scholz, B., (2002) 'Empirical Assessment of Stimulus Poverty Arguments'. *The Linguistic Review* 19, 9-50.

Putnam, H., (1975) *Mind, Language and Reality.* Cambridge: CUP.

Putnam, H., (1978) *Meaning and the Moral Sciences.* London: Hutchinson.

Putnam, H., (1981) *Reason, Truth and History.* Cambridge: CUP.

Quine, W., (1960) *Word and Object*. Cambridge: MIT Press.

Quine, W., (1969) 'Linguistics and Philosophy' in *The Ways of Paradox and Other Essays (Revised Edition)* (1976). Cambridge: Harvard University Press.

Radford A., (1988) Transformational Grammar: a First Course. Cambridge: Cambridge University Press, 1988.

Radford, A., (1997) *Syntax: A Minimalist Introduction.* Cambridge: CUP.

Rawls, J., (1999) *A Theory of Justice (Revised ed.).* Oxford: Oxford University Press.

Robins, R.H., (1967) *A Short History of Linguistics.* London: Longman.

Rorty, R., (1979) *Philosophy and the Mirror of Nature.* Princeton: Princeton University Press.

Rorty, R., (1986) 'Foucault and Epistemology'. In Couzens Hoy (ed.).

Rorty, R., (2000) 'Kuhn' in Newton-Smith, W., (ed.) (2000).

Rousseau, J-J., (c.1749) *Essay on the Origin of Languages* (tr. John H. Moran), in Moran, J. and Gode, A. *On the Origin of Language* (1966). London: University of Chicago Press.

Russell, B., (1946) *History of Western Philosophy and its Connection with Political and Social Circumstances from the Earliest Times to the Present Day.* London: Allen and Unwin.

Salkie, R., (1990) *The Chomsky Update: Linguistics and Politics.* London: Routledge.

Sampson, G., (1976) 'Review of Koerner *The Transformational-Generative Paradigm and Modern Linguistic Theory*'. *Language* 52:4 961-966.

Sampson, G., (1979) *Liberty and Language.* Oxford: OUP.

Sampson, G., (1980) *Schools of Linguistics: Competition and Evolution.* London: Hutchinson.

Sampson, G., (1997) *Educating Eve: The 'Language Instinct' Debate.* London: Cassell.

Sampson, G., (2001) *Empirical Linguistics.* London/New York: Continuum International.

Sampson, G., Gil, D. and Trudgill, P., (2009) *Language Complexity as an Evolving Variable* Oxford: OUP.

Saussure, F., (1916) *Course in General Linguistics* (revised edition 1974 Bally, C. and Sechehaye, A. (eds.), translated by Wade Baskin with an introduction by Jonathan Culler). London: Fontana.

Saville-Troike, M., (1989) *The Ethnography of Communication: An Introduction (2nd ed.).* Blackwell: Oxford.

Schaffner, K., (1972) *Nineteenth-Century Aether Theories.* Oxford: Pergamon.

Scholz, B. and Pullum, G., (2002) 'Searching for Arguments to Support Linguistic Nativism'. *The Linguistic Review* 19.

Schütze, C., (1996) *The Empirical Base of Linguistics.* Chicago: University of Chicago Press.

Searle, J., (1972) 'Chomsky's Revolution in Linguistics' in Harman (1974).

Shapere, D., (1964) 'The Structure of Scientific Revolutions'. *The Philosophical Review* 73: 3 383-394.

Sheridan, A., (1980) *Michel Foucault: The Will to Truth.* London: Tavistock.

Siewierska, A., (1988) *Word Order Rules.* London: Croom Helm.

Sklar, R., (1968) 'Chomsky's revolution in linguistics'. *The Nation* (9/11/1968) 213-217.

Smith, N., (1999) *Chomsky: Ideas And Ideals.* Cambridge: CUP.

Smith, N., and Tsimpli, I., (1995) *The Mind of a Savant: Language-learning and Modularity.* Oxford:Blackwell

Smith, N. and Wilson, D., (1979) *Modern Linguistics: The Results of Chomsky's Revolution.* London: Penguin.

Smith, T. and Greene M., (eds.) (1940) *Berkeley, Hume and Kant: Philosophers Speak for Themselves.* London: University of Chicago Press.

Snow, C. and Meijer, G., (1977) 'On the Secondary Nature of Syntactic Intuitions' in Greenbaum (1977).

Sperber, D. and Wilson, D., (1986) *Relevance.* Oxford: Basil Blackwell.

Suppe, F., (1989) *The Semantic Conception of Theories and Scientific Realism.* Urbana: University of Illinois Press.

Tartaglia, J., (2007) *Rorty and the Mirror of Nature.* Oxford: Routledge.

Taylor, C., (1985) 'Interpretation and the Sciences of Man' in *Philosophy and the Human Sciences* (1985). Cambridge: CUP.

Thorne, J., (1965) 'Review of P. Postal, *'Constituent Structure'*. *Journal of Linguistics* 1:73-6.

Tipton, I.C., (ed.) (1977) *Locke on Human Understanding.* Oxford: OUP.

Tomasello, M., (2003) *Constructing a Language: A Usage-Based Theory of Language Acquisition.* Cambridge: Harvard University Press.

Urquhart, A., (ed.) (1994) *The Collected Papers of Bertrand Russell volume 4.* London: Routledge.

Wardhaugh, R., (2006) *An Introduction to Sociolinguistics (5th ed.).* Oxford: Blackwell.

Whorf, B., (1956) *Language, Thought, and Reality: Selected Writings* (Edited and with an introduction by John B. Carroll). Cambridge: Technology Press of Massachusetts Institute of Technology.

Wiggins, D., (1980) *Sameness and Substance.* Cambridge: Harvard University Press.

Williams, M., (2000) *Science and Social Science.* London: Routledge.

Winston, M. E., (1976) 'Did a (Kuhnian) Scientific Revolution Occur in Linguistics?'. *Proceedings of the Biennial Meeting of the Philosophy of Science Association* 1 25-33.

Wittgenstein, L., (1974 [1922]) *Tractatus Logico-Philosophicus.* London: Routledge and Kegan Paul.

Wittgenstein, L., (1963 [1958]) *Philosophical Investigations* (translated by G.E.M. Anscombe). Oxford: Blackwell.

Woolgar, S., (1988) *Science: The Very Idea.* London: Tavistock.

Yergin, D., (1972) 'The Chomskyan Revolution'. *The New York Times Magazine.* (3/12/1972) 42-3 and 112-127.

Yngve, V., (1996) *From Grammar to Science: New Foundations for General Linguistics*. Amsterdam/Philadelphia: John Benjamins.

Yngve, V., (1997) 'Two Foundations for Linguistics Briefly Compared'. *The Twenty-fourth LACUS Forum 1997*. (accessed 17/02/11). http://humanlinguistics.utoledo.edu/Resources/Two%20Foundations.pdf

Yngve, V. and Wasik, Z., (2006) *Hard-Science Linguistics*. New York: Continuum.

ONLINE RESOURCES

Ben Goldacre's 'Bad Science' page (accessed 17/02/11). http://www.badscience.net/

'Chomsky is an anarchist' (accessed 17/02/11). http://www.chomsky.info/interviews/20020322.htm

The Oxford English Dictionary Online (accessed through the University of Sheffield Library homepage 17/02/11)) http://dictionary.oed.com.eresources.shef.ac.uk/

W. Keith Percival's homepage (accessed 17/02/11). http://people.ku.edu/~percival/

Geoffrey Sampson's homepage (accessed 17/02/11). http://www.grsampson.net/

For a discussion on what Joos really meant (accessed 17/02/11). http://linguistlist.org/issues/2/2-112.html

The Universals Archive (accessed 17/02/11) http://typo.uni-konstanz.de/archive/nav/browse.php?number=1&PHPSESSID=da14e1c5586749454a7f1b78a3369ded

The Stanford Encyclopedia of Philosophy (accessed 17/02/11) http://plato.stanford.edu

Syntax (accessed 17/02/11)
http://www.wiley.com/bw/aims.asp?ref=1368-0005&site=1

Journal of Sociolinguistics (accessed 17/02/11)
http://onlinelibrary.wiley.com.eresources.shef.ac.uk/journal/10.1111/%28ISSN%291467-9841

Dick Hudson's Word Grammar site (accessed 17/02/11)
http://www.phon.ucl.ac.uk/home/dick/enc2010/frames/frameset.htm

Halliday and Matthiessen's SFG site (accessed 17/02/11)
http://www.isfla.org/Systemics/

A tribute to Thorne http://www.benjamins.com/cgi-bin/t_bookview.cgi?bookid= CILT % 2065.

Interview with Larry Trask
http://www.guardian.co.uk/science/2003/jun/26/scienceinterviews.artsandhumanities