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Partially connected to science:

the Luxembourg Museum of Natural History and its Scientific Collaborators

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Abstract

This thesis explores the production of scientific knowledge at the Luxembourg Museum of Natural History. It focuses in particular upon the roles and interrelationships of amateurs and professionals in this process. In doing so it explores how the boundaries of science are made and unmade.

As a contribution to the science studies literature, the production of science is examined in a rather unexplored space: a museum. Theoretically, this thesis draws on science studies in general and actor-network theory in particular. Based on empirical research – participant-observation fieldwork, semi-structured interviews and document analysis – it unpacks the 'cultural boundaries of science'. It provides an empirical and theoretical understanding of the multiplicities, heterogeneities and materialities of boundaries and boundary-work.

On the one hand, this thesis shows how the co-production of scientific knowledge and the making of a 'museum without walls' is rendered difficult through resistive agency and different articulations of space and time; on the other, it examines how this co-production is eased through the managing of boundary encounters and boundary objects, and practices such as decentralisation, brokering, and heterogeneous gift exchange. This thesis thus provides an informed account on what it means to be 'partially connected' to science.

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Table of contents

Trains of Thought

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Imagine yourself sitting in a train. You might be talking to somebody, reading a book, or listening to some Led Zeppelin music. Every now and then, your thoughts are interrupted by the creaky voice coming from the speaker, announcing the name of the next station or - especially if you travel in the UK – informing you about the delay of the train. But then this. Suddenly the voice from the speaker addresses somebody in person. The voice tells a guy called Laurent to look out of the window now, because a badger was sighted some time ago right at the spot the train is just passing. People around you look surprised. So do you probably. A what? A badger? What is going on here?

This is a true story. This is what happened once on a train in Luxembourg (this very small country sandwiched between France, Germany and Belgium). From a speaker in a public transport system, where you would normally expect impersonal and repetitive communication, something quite personal and unique was uttered. A message was transmitted from the locomotive driver to only one passenger of the train. And the message was about something very specific: the location of an animal. Of course, the locomotive driver, called Guy, knew Laurent and had recognised him when the latter got on the train. Guy also knew that Laurent was interested in badgers. In fact, both knew each other through the Luxembourg Museum of Natural History, as both are scientific collaborators of this institution. So, funny as he is, Guy told Laurent and the ears of all the other passengers of the train that he had seen a badger at the spot the train was just passing. A piece of scientific knowledge was exchanged on a train, through a speaker. A quite unusual incident.

The same locomotive driver happened to surprise me on another occasion too, one day when I was far away from Luxembourg. I was looking at a photographic exhibition in Oslo, Norway with a couple of friends of mine and we chatted in Luxembourgish. Suddenly we were approached by a man who started talking in Luxembourgish with us. We were astonished because we certainly did not expect to meet another Luxembourg citizen. The likelihood of citizens of one of the smallest countries in Europe meeting by chance in another country must be rather slim, considering that only 450,000 people live in Luxembourg and that around 80% of them speak Luxembourgish. The stranger from Luxembourg asked us what we were doing in Oslo and we explained our reasons for being here: my friends were visiting Oslo and I was studying here. The person who was

talking to us, Guy, was taking part in the 7th International Mycological Congress. The congress took place at the University of Oslo and Guy was presenting a poster.

A locomotive driver who takes part in an international congress on mushrooms and - as I found out later - is even paid the travel costs to go there? Another strange story, it seems. You would not necessarily expect a locomotive driver to be knowledgeable in natural history. More so, at an international congress on mycology you would rather suppose professional scientists dealing with mushrooms to be present. Yet, there are some people who challenge these sometimes taken-for-granted realities. People like Guy, and many of the other 200 scientific collaborators of the Luxembourg Museum of Natural History, seem somehow to belong to two worlds, that of the professional scientist and that of the amateur or the naturalist. Among the scientific collaborators of the Museum there are all sorts of people: a bank employee interested in astrophysics and collaborating with NASA, a school teacher fascinated with beetles, a teenager interested in - and even publishing about - fossils. In short, many of the Museum's collaborators are people whose profession does not bear a direct link with their scientific interests. However, they seem to be doing similar things than we expect of professional scientists: they publish articles, do fieldwork, give talks, present posters at conferences. The boundary between the activities of amateurs and professionals is not clear-cut.

Aims and objectives

I got increasingly interested in these stories and decided to write my thesis about this subject. I wanted to explore the interrelationships of amateurs and professionals by doing a case study on the Luxembourg Museum of Natural History (named hereafter the Museum with a capital M). As both amateurs and professionals are involved in the production of scientific knowledge in the Museum, this seemed to me a worthy place to explore the 'cultural boundaries of science' (Gieryn 1999). Rather than being produced only by professional researchers, science in the Museum originates from a close cooperation between specialised people and laypersons - a model termed 'co-production of knowledge' by Callon (1998a, see also Callon et al. 2001). As a consequence, amateurs come to belong to the world of the professional too. They seem to be 'amateur experts' (Epstein 1995) or 'partial scientists' (Meyer 2005). The two

questions I asked myself were: How is science co-produced at the Museum? and How are the boundaries between professionals and amateurs made and unmade?

To focus on the interrelationships between amateurs and professionals is a promising and timely exercise. It is quite recently that the social sciences have started to explore the complex interactions between scientists and non-scientists. Besides analyses of the 'public understanding of science' there has been an increase in the examination of 'the public(s)' as producers of scientific knowledge. There is a trend in 're-thinking science' (see Nowotny et al. 2001) in terms of its relation to wider society as the 2004 joint 4S/EASST conference on 'Public Proofs. Science, Technology and Democracy' and Latour and Weibel's recent book *How to make things Public. Atmospheres of Democracy* (2005), for example, suggest. Whether talking about 'agoras' (Nowotny et al. 2001), 'hybrid forums' (Callon and Rip 1991, Callon et al. 2001), or 'parliaments of things' (Latour 2004a), there seems to be a common agreement that science has to occupy a new, more open space.

Scientific knowledge should not be produced in ivory towers, in spaces occupied by scientists only, but science has to occupy a more democratic and public space. The inclusion of lay people to contribute to the making of scientific knowledge, for instance, holds the promise of plurality and promises better social outcomes (Pestre 2003:260). This, in turn, might challenge notions of expertise and maybe a third wave in science studies - the study of expertise (Collins and Evans 2002) - needs to be developed. Thus, focussing on amateurs and professionals contributes to these debates. Exploring the co-production of science in particular settings, such as a museum, can foster new models of thinking about - and engaging with - science.

What I have written so far is in some ways a rather conventional introduction. I started with two anecdotes, which point towards bigger issues of sociological interest. I framed my case: the Luxembourg Museum of Natural History and its scientific collaborators. And I mapped out the scope of the thesis, which is to investigate how scientific knowledge is made through the involvement of amateurs and professionals. One more element needs to be introduced now: myself. This thesis is not an 'objective' account; it does not look at the Museum from nowhere. Instead, it is written by somebody with a specific socio-

cultural background and a distinct history. This is a view from somewhere. Donna Haraway (1991, 1997), amongst other scholars, has argued that knowledge is embodied and situated, that the content of a scientific text is shaped by the place of its production. Introducing myself right here at the beginning is thus important as my own background inevitably influences what is written and how it is written. So where am I situated? I am a male Luxembourgish national and I initially graduated in biology. But at a certain point, I came to a decision to quit biology. I decided to study something 'about science' rather than science. While looking for postgraduate research opportunities, I did two traineeships: one at the Luxembourg Ministry of Research and another at the Luxembourg Museum of Natural History. After these practical experiences, I carried on with a master degree in Science, Technology and Society (STS). After this fascinating turn from the natural sciences to the social sciences, I decided to continue to do research in this area. The University of Sheffield Department of Sociological Studies became my base for undertaking the current project.

Is there, then, something distinctively Luxembourgish about this project? Maybe Luxembourg lends itself well to explore boundaries. As a small country, it mostly depends on the import of human resources, capital, and goods from its powerful neighbours Germany and France. A small country might thus provide a good place to study mobilities and the crossing of borders. Being brought up in a small country and having moved between cultures, languages, and places might have sensitised me to think about boundaries. This thesis might be specifically Luxembourgish since it has been carried out by a Luxembourg national, since it deals with something significant for Luxembourg, and since it investigates a case in Luxembourg. Yet, having said this, I don't assume thereby that Luxembourg is the only place where a project like this one could have been undertaken nor that the issues I explore are only taking place in Luxembourg. I will return to these issues in the conclusion.

Chapters

The thesis is divided into eight chapters. In *Chapter One*, I bring together different theoretical approaches. First, I am concerned with the spaces in which science takes

place: the laboratory, the field, and the museum. Then, science studies and in particular actor-network theory will be discussed. Thereafter, I will consider the boundaries of science. I will discuss concepts such as 'boundary-work' and 'boundary objects' before focusing on one particular boundary, that between amateurs and professionals.

The next chapter introduces the methods used for the thesis. The methods I used are qualitative in form: interviews, participant observation, and document analysis. I 'followed the actors' (Latour 1987) through a symmetrical approach, making no *a priori* distinction between categories such as human/nonhuman and amateurs/professionals. Doing this, I relied on a multi-sited ethnography, an approach that moves within different locations and follows various associations.

Chapter Three focuses on the economy, society and culture of Luxembourg. After discussing the recent shifts in the cultural and museum landscape, I will analyse the historical and political aspects of the national research infrastructure. Whereas until the mid-1980s science was mostly practiced as a 'serious leisure' activity by amateurs, there has been a professionalisation and institutionalisation of research in the last two decades. I will pay particular attention to the reconfiguration of the boundaries within and around Luxembourg science throughout these changes.

In *Chapter Four* my aim is twofold – examining centralisation and decentralisation. I first recount how the Museum was built over time; how material, legal and structural boundaries and walls were set up to demarcate a specific space dedicated to the natural history of Luxembourg. Then I look at how some walls are broken down; how the Museum surpasses its walls by different means such as buses and festivals and through the involvement of amateurs.

The places of encounter between the Museum staff and the collaborators will be the focus of chapters five and six. *Chapter Five* examines bodily and textual encounters: an annual meeting of the collaborators, a biodiversity weekend and the Museum's own journal. I explore how science is made and how boundaries are articulated at these specific 'boundary encounters'. I also consider how both the exchange of scientific knowledge and of more symbolic things are important in negotiating the relationships

between the Museum and the collaborators. Finally, the use of different languages for the communication of science will be analysed.

The digitised encounters through the Museum's databases are the focal point of *Chapter Six*. The Museum's dream to construct a complete survey of nature through databases reveals its wish to be a powerful spokesperson for nature and its attempt to build a biodiversity panopticon. But there are many things that make it difficult to construct a coherent database. The frictions between the Museum and the collaborators show that databases map the natural and the social world, that there is a politics of databases to consider. Different epistemic cultures clash along the following divides: leisure time/working time, individual/collective, private/public, etc. Some collaborators deploy a resistive agency by not sharing data. By refusing translation they maintain their individuality and their amateur identity.

Chapter Seven explores two themes. First, it examines the heterogeneity and materiality of boundary-work. It looks at the boundary between amateurs and professionals in terms of temporal, spatial and material matters. Second, I conceptualise a theoretical space between boundary-work as expansion, expulsion and protection of autonomy, that is, between exclusion and inclusion. I will particularly theorise the partial connections (Strathern 1991) between actors by arguing that amateurs and professionals belong to more than one but less than many social spaces.

Chapter Eight concludes by moving to broader issues. First, I compare the crossing of boundaries in the Museum to the crossing of Luxembourg's borders. Drawing upon the concept of 'cross-border workers' – those people who travel on a daily basis to work in Luxembourg - I develop the notion of 'cross-border workers of science' to conceptualise the crossing of the cultural boundaries of science. Second, I argue that a fourth kind of boundary-work is at stake – different from expansion, expulsion and protection of autonomy (Gieryn 1999) – involving simultaneously boundary maintenance and boundary crossing. The chapter ends with a discussion about the relevance of natural history as an egalitarian 'way of knowing' (Pickstone 2000) and the role of museums as open and democratic institutions for the production of scientific knowledge.

Chapter 1:

Exploring the boundaries of science in a museum of natural history

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A literature review does not simply *review* a body of literature. It has to do more than this: it has to identify and propose how to fill a gap. The absence I help to create and attempt to fill is the following one. I suggest that academic work has insufficiently looked into museums as sites of knowledge production and that the relation between amateurs and professionals is under-researched. In science studies in particular the relations between scientists and non-scientists, expert and lay, have received little attention until recently (Callon and Rabeharisoa 2003). I argue that museums of natural history deserve more scrutiny and that actor-network theory is a useful - if limited - approach for this endeavour.

To put together the theoretical framework for this thesis, this chapter is divided into five sections. In the first section I am concerned with some of the spaces in which science takes place, namely the laboratory and the field. Next, I examine the museum. In the third part, I focus on actor-network theory in more detail. After this I consider the boundaries of science and discuss the notions of 'boundary-work', 'co-production' and 'boundary objects'. In the final section of this chapter, I examine one boundary in particular, that between amateurs and professionals.

1.1. Spaces of science

'[S]cience must take place somewhere', Livingstone (2005:100) writes, 'location, like embodiment and temporality, is essential to knowing'. There are many spaces in which scientific practice takes place. These include laboratories, museums, the field, the home, and universities. In this section I discuss the laboratory and the field as places of scientific inquiry. Let's start with the laboratory.

The emergence of the laboratory

Today, a large amount of scientific research is carried out in laboratories. The history of most sciences is that of an extreme confinement that sets laboratories and instruments out of reach of the amateur and the layperson (Callon et al. 2001:65). The strength of the laboratory – its precision of measurement and the elimination of interferences – has

made it necessary for experiments to be done in a private and confined space (Callon et al. 2001:72). Today's laboratory is a spatially and socially organised form that can be traced back to the 'house of the experiment' (Shapin 1988). To understand today's taken-for-granted form of the laboratory, we have, therefore, to revisit the emergence of this particular space and the controversies around different modes of organising science. The rise of laboratories in 17th century England, examined by Steven Shapin and Simon Schaffer, provides a good starting point.

For Robert Boyle, the main figure in the community of experimental philosophy in 17th century England, it was important to construct and maintain a crucial boundary around his programme for experimental philosophy (Shapin and Schaffer 1985:80). His main adversary Thomas Hobbes was to be excluded from the Royal Society and his anti-experimentalism gave grounds for this exclusion.

In Boyle's view science had to be demarcated from various other fields. He and the experimentalists were on guard against dogmatists and tyrants in philosophy, and secretists who produced their knowledge-claims in a private and undisciplined space (Shapin and Schaffer 1985:78). Instead, assent was to be secured through the production of experimental findings, mobilized into matters of fact through collective witnessing (Shapin 1994, 1988). Knowledge was to be founded upon a 'collectivised individual sensory experience' (Shapin and Schaffer 1985:152). The experimentalists created a 'calm space' in which philosophers could collectively agree upon the foundations of knowledge (Shapin and Schaffer 1985:76). Giving witness traversed the social and moral accounting systems of Restoration England (Shapin and Schaffer 1985:59). Moreover, witnesses could be multiplied by 'virtual witnessing': the experimental report, besides being a narration of some prior visual experience, was also a visual source, a source of virtual witness that was agreed to be reliable (Shapin and Schaffer 1985:61). Nonetheless, as Hobbes critiqued in *Leviathan*, the Royal Society was not a public space. Witnessing of experiments was private and the space occupied by the experimentalists had a master (Shapin and Schaffer 1985:113-4). The emergent laboratory was a public space but with restricted access (Shapin and Schaffer 1985:336).

To have access to this space, philosophers had to be humble, noble, honest and trustworthy (Shapin and Schaffer 1985:130-1). Trust was an important aspect in experimental philosophy in 17th century England (Shapin 1994). Trust was crucial for demarcating the community of philosophers:

> The distribution of trust is therefore coextensive with the community, and its boundaries are the community's boundaries (Shapin 1994:36).

What counted as truth and science depended on who spoke on behalf of nature and whether he was respected as a gentleman. For Boyle and his colleagues, the success of the experimental programme rested upon the acceptance of certain social and discursive conventions and depended upon the production and protection of a special form of social organisation (Shapin and Schaffer 1985:22).

The emergent laboratory was a demarcated and protected space. Experimentalists mobilised various norms to define this space and the rules by which science was to be conducted and which members had to respect. Stated differently, the laboratory was - and still is - a very disciplined space.

Foucault's notion of discipline and enclosure is useful here. For Foucault, there are several disciplinary techniques to distribute individuals in space.

> Discipline sometimes requires *enclosure*, the specification of a place heterogeneous to all others and closed upon itself. It is the protected space of disciplinary monotony (Foucault 1979:141).

At a first level, constructions and architectures will try to resolve this problem. However,

> the principle of 'enclosure' is neither constant, nor indispensable, nor sufficient in disciplinary machinery. This machinery works space in a much more flexible and detailed way. It does this first of all on the principle of elementary location or *partitioning* (Foucault 1979:143).

Transposed to science this means that by enclosure a delimited and disciplined space for science is created and maintained; that persons (as well as things) are located by 'partitioning' in a 'fragmented space' (Lefebvre 1991:282). Not only does a boundary exist that separates science from non-science. In addition, the 'calm space' that is demarcated is ordered by various disciplinary techniques. The experimental programme of the 17th century, as well as today's laboratory, utilizes mainly three technologies: a material technology (the construction of machines), a literary technology (the dissemination of results to non-witnesses) and a social technology (the conventions to use) (Shapin and Schaffer 1985:25-79). These technologies share a disciplinary aspect since they set out the organisational form of science.

Spaces of natural history

Natural history also takes place in locations other than the laboratory. Apart from the laboratory, there are three vital spaces for natural history: the museum, the lecture hall and the field (Naylor 2002). The museum is a crucial space since it houses collections, maps, models, books, etc. The lecture hall is necessary to communicate natural history. And the field is where specimens are observed and collected. In addition, the laboratory - as in other scientific domains - is the place where analysis and experiments are performed.

For professional scientists, the laboratory is a disciplined space where experimental, discursive, and social practices are collectively controlled by competent members (Shapin and Schaffer 1985:39). Yet, if amateurs are involved in the production of science, the laboratory, as well as the other spaces of natural history, must also permit a less professional, less rigid and less disciplined use. This is possible via a partially enacted laboratory and a less disciplined field. A first strategy, then, enabling and facilitating the co-production of science can be related to the laboratory itself. The amateur, when s/he uses a laboratory, seems less likely to master the same level of competence in each technological, intellectual and structural resource as the professional does. S/he uses only *some* of the material and intellectual resources available.

Second, the field, or the lab-in-the-field, is a less disciplined space than the laboratory. Control over natural phenomena is less than the 'socionature' produced within the laboratory by controlling or excluding the natural environment. Historically the field was not considered to be a very scientific space. The act of analysing data collected by others in the field was not problematic and was admissible for subsequent scientific investigation. More so, natural history fieldwork was not a gentlemanly activity: it was considered unpleasant and inglorious, physical and dirty work, and sometimes dangerous (Kuklick 1997:53). However, accounts were not accepted as authentic until gentlemen-scientists attested to their veracity (Kuklick 1997:57). At the end of the 19th century, this changed and the best place for scientific inquiry was no longer to be the lecture hall but the site of direct inquiry: the laboratory or the field (Forgan 1994:152, Kuklick 1997). Fieldwork became the defining property of scientific research (Kuklick 1997:58-9). Put differently, the field became a more delimited and disciplined space in natural history.

Fieldwork as a scientific practice and social space is organised by specific methods and tools. The validity and reliability of data collected in the field are analysed or measured by practices 'inside' the laboratory or the museum. The field in natural history, although external to the museum and the lecture hall, is nonetheless defined by these latter (Naylor 2002:497). Being outside in the field means being partially inside the museum. Stated another way, to be able to turn the field into a space of scientific inquiry, methods and objects have to pass via a laboratory (see Latour 1984). Because of these movements, the distinction between the inside and outside of a laboratory becomes blurred (Latour 1984, see also Szerszynski, 2004). A laboratory does not stop at its walls. Nonetheless, even though transfers and translations exist between the laboratory and the field, even though the field is somehow disciplined, it is not overly disciplined. Translations, from the micro-world of the laboratory to the macro-world outside, and vice versa, transport some things, but not all. The field remains a space where amateurs can pursue scientific activity since standardised methods turn the field into a scientific space but also relieve some of the constraints and rigidity of professional science.

1.2. Museums

Having discussed the laboratory and the field, we now have to explore another crucial space of natural history: the museum. I first discuss the emergence of museums and then look at how to theorise them.

Acknowledging that there are contested accounts concerning the time and the place of the materialization of the first museums (see Abt 2006), I now briefly discuss how museums came into being and the key processes involved. According to Findlen (1994) the world's first museums emerged in Italy in the 16th century. Analysing the context and processes of this emergence, she writes:

> Humanists, natural philosophers, and collectors were not just found *anywhere* in society. They inspected nature in a precisely demarcated setting, the museum, that took its place alongside the courts and academies of late Renaissance and Baroque Italy as a space in which learned and elite culture converged (Findlen 1994:97, emphasis hers).

The museum, a demarcated setting, was a new kind of space. It was ordered by emerging scientific principles, it took on a name of its own, and, more uniquely, it was filled with objects. The museum emerged through a shift in knowledge from a predominately textual and discursive form to an increasingly visual form (Findlen 1994:199). A new way to connect things both to vision and to discourse developed, especially in natural history museums and botanical gardens (Foucault 1966:143). The museum was a space where objects were exhibited and bodies could move within.

For Bennett, the birth of the museum as we know it today took place in the 18th and 19th centuries through the development of the 'exhibitionary complex' through which institutions

> were involved in the transfer of objects and bodies from the enclosed and private domains in which they had previously been displayed [...] into progressively more open and public arenas [...] (Bennett 1995:60-1).

'Objects' and 'bodies', 'things' and persons', are still today the two most common elements in any definition of museums. Let's consider the following two definitions - one by a former director of the London Science Museum (Neil Cossons) and another one by the International Council of Museums (ICOM).

> Museums hold collections and reveal them to audiences. They are about objects and for people as they have always been (Cossons 2000:7).

> A museum is a non-profit making, permanent institution in the service of society and of its development, and open to the public, which acquires, conserves, researches, communicates and exhibits, for purposes of study, education and enjoyment, material evidence of people and their environment (ICOM 2006).

These definitions tell us that museums are still concerned with objects and with subjects: museums reveal and make objects intelligible in order to educate and entertain people. I now look at both these components of a museum: subjects and, after that, objects.

The subjects that make up museums can be further differentiated. For instance, four communities are said to be critical to science museums: scientists, public visitors, funders, and the museum staff (Friedman 2000). Accordingly it has been argued that a modern science museum should be a common natural stage for the actors from four sectors: the scientific community, the production sector, the government, and all of society (Wagensberg 2000:138). A more fine-grained definition might also differentiate between the scientific community and non-professionals who might also be actively involved in producing exhibits or scientific texts. If viewed as a place where different communities interact, we have, then, to follow these communities or actors through the museum.

According to Clifford (after Pratt (1992)), museums are '[c]ontact zones: the space in which peoples geographically, and historically separated come into contact' (Clifford 1999:438). But not only do people with different histories and geographies meet, different social and cultural backgrounds come together as well. For Clifford, contact

zones are 'places of hybrid possibility and political negotiation, sites of exclusion and struggle', they are 'places of transit, intercultural borders, contexts of struggle and communication between discrepant communities' (Clifford 1999:451). For instance, museums can be contact zones for different kinds of knowledge as museums negotiate a nexus between expert and lay knowledge (Macdonald 1996:4). As Star and Griesemer (1989) have shown, a museum might be a place of encounter between professional scientists and amateurs.

Apart from bringing together different people, museums are also contact zones in the sense that objects geographically and historically separated are drawn together. In a natural history museum, for example, species from different parts of the world and from different periods in time are brought together. But not only does the museum classify, preserve and manage such diverse objects, some of these objects inhabit multiple worlds at the same time:

> The intersectional nature of the museum's shared work creates objects which inhabit multiple worlds simultaneously, and which must meet the demands of each one [...] In natural history work, boundary objects are produced when sponsors, theorists and amateurs collaborate to produce representations of nature. Among these objects are specimens, field notes, museums and maps of particular territories (Star and Griesemer 1989:408).

For the present work, two elements of the above quote are central: the museum's intersectional nature and its involvement in the production of boundary objects (these will be discussed in section four). In this view, the museum does two things: it is a contact zone, a place where some walls are made and unmade, and it is an institution that produces things (objects but also knowledge).

Since I will be concerned with *doing*, with the making of objects and knowledge, I now briefly discuss practice. Just as science has been increasingly theorised as a practice (see Pickering 1992a, 1995), a science museum might also be analysed in terms of practice. Stated differently, museums might not only be pictured in terms of what they *are*, or what they represent, but also in terms of what they *do*. What does a museum do? Museums do

things through exhibitions: 'display not only shows and speaks, it also *does*' (Kirshenblatt-Gimblett 1998:6). Museums might be, for example, a means to spread and reinforce national consciousness (Hudson 1999:372-3). Museums also produce objects, knowledge, and science. A museum of natural history, for instance, does different things: it is a place where knowledge about nature is drawn together, analysed, interpreted and then published or exhibited, as much as it is a means to reinforce a certain value and respect towards nature and as it is a place where the making of displays and scientific texts is inherently political and reproduces a certain version of 'nature out there'.

Apart from the museum's intersectional nature and its involvement in the production of things and knowledge, I will take on board a third element. I will be interested at the Museum as a 'museum without walls'. It has been argued that the 'museum without walls' was the third step in the spatial evolution of museums (see Hetherington 1996:153). First, there was a 'pre-museum' space: collections were localised in palaces, private homes, churches, gardens, etc. Then, the classical museum developed during the 17th and 18th century to become a 'museum with walls'. Finally, the 'museum without walls' developed. Whereas for Malraux (1965) the museum without walls is located in books, brought about through the reproduction of art, 'unwalling' happens through other means, in other places too. Hetherington lists three factors involved in the breakdown of the walls of the museum: forces of commercialisation, the emergence of the heritage industry, and popular interests in sites of historical interest outside the confines of the museum (see Hetherington 1996:154). We can add five more elements. First, democratic values and, especially for natural history museums, environmental and social movements have drawn places and people formerly outside of the museum project into the heart of its preoccupation. Second, while museums still predominantly display 'things' (Macdonald 2004:upd) some museums have shifted their allegiance from real objects to real experiences (Hein 2000:87) and can break down their walls more easily. Also, third, in the 'information age' with its digital information networks and databases, the museum's collection and expert information becomes linked to other organizations, and identities become blurred (Keene 1998:17).[1] Fourth, in theoretical terms too, the 'museum refuses

[1] Keene (1998:17) writes: 'The actual museum is sharply defined by its geographical presence, its organizational identity, and its physical collections. How will museum people feel if the identity of their organization becomes blurred? This is what will happen if the basis for the museum's existence, its

to stand still' (Hudson 1998) as the concept of what a museum is has enlarged over the years (Hooper-Greenhill 2000b:180) and as the museum's walls opened up to the increasing interest of anthropologists, philosophers, economists, etc. alongside more classical academic work in history or in the arts. Finally, it has been argued that the walls between science and art are crumbling in the museum (Van Praet 1995, see also Macdonald 2003).

Foucault's insights about the carceral system are useful to think about the museum. For Foucault, the complex ensemble that constitutes the carceral system is

> not only the institution of the prison, with its walls, its staff, its regulations and its violence. The carceral system combines in a single figure discourses and architectures, coercive regulations and scientific propositions, real social effects and invincible utopias, programs for correcting delinquents and mechanisms that reinforce delinquency (Foucault 1979:271).

The museum too is a similarly complex ensemble: it is not only an institution with walls, staff and regulations but also both discourse and architecture, combining regulations and scientific propositions, with real social effects and underpinning utopias. But the 'hard' approach of the carceral system differs from the 'soft' approach of the museum in that the former works by discipline whereas the latter by example and entertainment and depends on voluntary participation (Bennett 1995:87). In science museums and science centres, often based on an interactive model, 'subjects are not disciplined, they are *allowed*' (Barry 2001:129).

1.3. Actor-network theory – the museum test

Science (and technology) studies see science as an inherently political, cultural and social endeavour. There is no 'pure' technology or science: technologies reproduce and embody the complex interplay of professional, technical, economic, and political factors

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collection and expert information, become linked apparently seamlessly to other organization's pools of expert knowledge'.

(Bijker and Law 1992a:3). Technical artefacts have politics (Winner 1986) and science is politics by other means (Latour 1987). Consequently, most science studies scholars hold that science cannot be objective. Haraway (1989:13) writes: 'The detached eye of objective science is an ideological fiction, and a powerful one'. Instead, knowledge is situated and 'only partial perspective promises objective vision' (Haraway 1991:190). A common threat that runs throughout science studies is the argument that science is both produced *in a place* and that it should be examined *as a space* - rather than placeless and spaceless knowledge. It is argued that science has a geography (see Livingstone 2005, Naylor 2002, Naylor 2005, Shapin 1998, Turnbull 1993) and that science is a pliable and supple cultural space (Gieryn 1999:xi). But what, exactly, do we mean by 'space'?

For Lefebvre, to picture space as a frame or container into which nothing can be put unless it is smaller than the recipient is a common error (Lefebvre 1991:94). Space is neither a person nor a thing, it is neither subject nor object but a set of relations between these (Lefebvre 1991:82-3, 92, 116). Space is not only something material but also comprises social space.

> Social space contains a great diversity of objects, both natural and social, including the networks and pathways which facilitate the exchange of material things and information. Such 'objects' are thus not only things but also relations (Lefebvre 1991:77).

Moreover, space is both abstract and concrete in character:

> abstract inasmuch it has no existence save by virtue of the exchangeability of all its component parts, and concrete inasmuch as it is socially real and as such localised (Lefebvre 1991:341-2).

Foucault takes a similar stance when he asserts that space is real and ideal: real in the sense that it governs the disposition of buildings, rooms, and furniture, but also ideal as it projects over this arrangement characterisations, assessments, and hierarchies (Foucault 1979:148).

Lefebvre underlines the need to construct a 'spatial code', which would bring together the 'micro' level and the 'macro' level, inside and outside, work and non-work, the durable and ephemeral (Lefebvre 1991:64). He further argues:

> The form of social space is encounter, assembly, simultaneity. But what assembles, or what is assembled? The answer is: everything that there is *in space*, everything that is produced either by nature or by society [...] Everything: living beings, things, objects, works, signs and symbols (Lefebvre 1991:101).

Lefebvre's social space in which subjects, objects, and symbols are assembled resemble actor-networks which 'draw together' humans and non-humans, the material and the social, etc. Space in actor-network theory is similarly relational (Murdoch 1998). Let us turn to actor-network theory now.

'[O]ne might represent actor network theory by **performing** it rather than **summarising** it' (Law 1997:2, bold in original). Damn! This should make it difficult to review actor-network theory (ANT), to *write about* ANT. Rather than summarising and giving an overview of ANT I will thus review some of its elements. I will selectively consider the themes I operationalise throughout this thesis. I will proceed as follows: first I discuss some of its particularities, then review some of its drawbacks and, finally, look at how to use, perform, and test it in a museum.

First of all, the notion of symmetry is central to ANT. ANT's principle of generalised symmetry is a radicalisation of the 'principle of symmetry' (first expressed in Bloor 1976) which calls for the use of the same types of causes to explain true and false beliefs. According to this principle 'true' scientific beliefs should not be seen as given by nature but they come within the scope of sociological analysis and explanation. The 'sociology of translation', the precursor of actor-network theory, extends this principle of symmetry. Three methodological principles are obeyed (Callon 1986): agnosticism (the impartiality between actors engaged in controversy), generalised symmetry (the commitment to explain conflicting view-points in the same terms and to use the same repertoire for

Nature and Society), and free association (the abandonment of all *a priori* distinction between the natural and the social).

Building upon these principles, Callon (1986) defines 'translation' as a process involving four moments. The first moment, 'problematization', defines the nature and the problems of actors. The next moment, 'interessement', consists in 'a series of processes by which' actors are locked 'into the roles that had been proposed for them' (Callon 1986:196). Successful interessement leads to 'enrolment', a set of strategies in which it is 'sought to define and interrelate the various roles allocated to others' (ibid.). The fourth moment of translation is 'mobilisation' which renders mobile actors that were not so before - these are the methods 'to ensure that supposed spokesmen [people talking on behalf of others] for various relevant collectivities were properly able to represent those collectivities' (ibid.).

To summarise, translating an actor means: defining an actor; testing, stabilising and specifying the roles of this actor; and, finally, rendering it mobile. In Law's (2002:99) words,

> To translate is to connect, to displace, to move, to shift from one place, one modality, one form, to another while retaining something. Only something. Not everything. While therefore losing something. Betraying whatever is not carried over.

The fragility of the process becomes evident. '[*T*]*raductore-traditore, traduction-trahison,* to translate is to betray' (Law 2002:99). Dissidence, or betrayal, might arise through controversy when the representativity of spokespersons is questioned, discussed, negotiated, rejected. If actors feel betrayed by those who represent them they might become dissident, they might betray an emerging network.

Out of this sociology of translation actor-network theory emerged. Actor-networks are defined as follows:

> The actor network is reducible neither to an actor alone nor to a network. Like networks it is composed of a series of heterogeneous elements, animate and inanimate, that have been linked to one another for a certain period of time [...] The actor network can thus be distinguished from the traditional actors of sociology, a category generally excluding any nonhuman component and whose internal structure is rarely assimilated to that of a network [...] An actor network is simultaneously an actor whose activity is networking heterogeneous elements and a network that is able to redefine and transform what it is made of (Callon 1987:93).

Actor-networks are both networks and points: 'they are individuals and collectives' (Callon and Law 1997:174). Even though actors should not be examined in isolation, definitions have been given: 'actors are those entities that exert detectable influence on others' (Law 1986:132); an actor is allegedly

> [a]ny element which bends space around itself, makes other elements dependent upon itself and translates their will into a language of its own (Callon and Latour 1981:286).

The list of actors in ANT case-studies is huge: scallops, doors, microbes, Louis Pasteur, Portuguese ships, scientists, fishermen, and so on.

Apart from a special definition of actors, within ANT the term network has a distinct meaning too. While the term is commonly used in social science to describe technological relations, economic forms, political structures and social processes, ANT uses the term in a way which is quite distinct from these applications:

> ANT bundles *all* these applications together for it concerns itself with the *heterogeneity* of networks, [...] how social and material processes become seamlessly entwined within complex sets of associations (Murdoch 1998:359).

One of the defining characteristics of ANT is, then, the inclusion of non-human actors in the actor-networks it proposes to analyse. In other words, actor-network theory conceives

the world which we build as a collective hybrid made out of humans and non-humans and asserts that it is not possible to study both constituents separately (Callon 1999a:67). Haraway makes a similar point: 'I insist that social relationships include nonhumans as well as humans as *socially* [...] active partners' (Haraway 1997:8).[2]

ANT is, then, decentering the subject (Lee and Brown 2002:259), which consists in breaching the boundaries that separate the human subject from the media in which it subsists: language, discourse, materiality, technology, desire.[3] Yet, the symmetrical approach of ANT does not stop at the human/non-human distinction only. ANT opposes binarist thinking of all kinds and provides a means of navigating dualisms, such as nature/society, action/structure, local/global, social/technical.[4] According to Latour, these great divides do not provide any explanations but are the things to be explained (Latour 1990:20). Rather than thinking in terms of separations, ANT conceives the world relationally. According to Law (1999a:4), ANT may be understood as a 'semiotics of materiality':

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[2] Haraway (1989:55) argues: 'The concept of social relations must include the entire complex of interactions among people; objects, including books, buildings, and rocks; and animals'. The main claim of ANT scholars is that it has opened the social sciences to non-humans (Callon 1999b:182). Yet, ANT does not stand alone with this claim. A perspective that bares some resemblance to ANT is that of the 'cultural bibliography of things' (Appadurai 1986, Kopytoff 1986). The argument is that commodities, like persons, have social lives and 'we have to follow the things themselves, for their meanings are inscribed in their forms, their uses, their trajectories' (Appadurai 1986:5). It is argued: 'that societies constrain both these worlds [the world of things and the world of people] simultaneously and in the same way, constructing objects as they construct people' (Kopytoff 1986:90). Ingold's dwelling perspective is comparable: people '"feel their way" *through* a world that is itself in motion, continually coming into being through the combined action of human and non-human agencies' (Ingold 2000:155, emphasis his). Pickering's notion of the 'mangle' (1995) also entails the recognition of material agency. These and other currents are discussed in Pels, Hetherington and Vandenberghe (2002).

[3] This is, in a sense, a kind of movement in a whole series of decenterings: as it was discovered that humans are not at the centre of creation, nor situated in the middle of space, and maybe neither on the summit and on the final step in life (see Foucault 1966:359).

[4] Other dualisms include: truth and falsehood, large and small, before and after, context and content, activity and passivity (see Law 1999a:3), prescientific and scientific culture (Latour 1990:20), primary and secondary (Latour 2000:119), common and private, objective and subjective (Latour 2004:93). The trend to be critical about dualisms can be traced back to Snow (1964:9): 'The number 2 is a very dangerous number [...] Attempts to divide anything into two ought to be regarded with much suspicion' even though his main argument in *The Two Cultures* does just this - he writes: 'the intellectual life of the whole of western society is increasingly being split into two polar groups. [...] Literary intellectuals at one pole – at the other scientists' (Snow 1964:3,4).

> It takes the semiotic insight, that of the relationality of entities, the notion that they are produced in [and that their form is a consequence of] relations, and applies this [...] to all materials.

Agency becomes a relational effect – hence the couplet actor-network.

Given its relational ontology, ANT differs from social constructivist thought. Neither prioritising the ('external') social context nor the ('internal') inherent structure, the relation between entity and context is problematized. It bypasses the question of 'social construction' and the 'realist/relativist debate', debates that might not be solved at all, as Hacking (1999:91-92) has argued. Nonetheless, although ANT tries to demarcate itself from the social construction tradition, it is clear that the way it describes science is usually closer to a constructivist and relativistic thought than to a realist thought.

The benefits of using ANT are various. As an actor-oriented approach it is valuable to uncover 'the micro-foundations of the macro framework' (Booth 1994:13). And it enables description of a world far richer than the society-nature dichotomy can allow (Castree and MacMillan 2001:212).

Critiques

ANT has been criticised for a number of (partially related) reasons.

'In ANT the T is too much' (Callon 1999:194). First of all, for the three major contributors of ANT - Callon, Latour and Law - the term 'theory' in actor-network theory is problematic: 'actor-network theory is not something in particular' (Law 1999a:10), it is 'more a method [...] than an alternative social theory' (Latour 1999:15), 'ANTs main shortcoming is that it is everything but a theory' (Callon 1999b:183), it is an 'infralanguage' rather than a theory (Latour and Crawford 1993:250). ANT represents more a sort of approach, a set of sensibilities, than a clear, framed and strong theory.

Flattening. ANT 'flattens' all distinctions between the entities which comprise networks (Murdoch 1998:367). Ontologically there is

> the problem of installing a great *indifference* between the countless things of world [...] which arises when they end up being portrayed as potentially *all the same* (Laurier and Philo quoted in Castree and MacMillan 2001:221).

This flattening practice leads to an obscuring of differences between entities. The assumption of a radical indeterminacy of the actor risks to yield a too great 'toleran[ce] that [...] ends up presenting an actor which is an anonymous, ill-defined and indiscernible entity' (Callon 1999b:182).

Dehumanising. ANT has also been criticised for dehumanising the human (Law 1997:7). As Hacking (1997) argued there are nonetheless differences between humans and non-humans as non-humans are 'indifferent kinds' and humans are 'interactive kinds' (or 'intentional' (Pickering 1995:17)). According to some, actor-network theory commits one important error in that it ignores the social, cultural and cognitive dimension proper to scientific action and contributes to create 'desocialised' representations (see Dubois 1999:64); and in that it pays too little attention to language and classification (Macdonald 2002:7).

Missing marginality and temporality. A critique raised by Star (1991) is that ANT fails to account for that which is not networked or marginalised. ANT is blind in the sense that it does not see what is excluded (Bowker and Star 1998:240). For Hetherington (1999:52), time and history also seem to be something of a blind spot for ANT since the metaphor of the network, with its emphasis on spatial relationships and distributions, seems to be in conflict with it.

Ambivalence. Ambivalence and fluidity have been downplayed within ANT (Murdoch 1998:364). For instance, ANT overlooks and conceals the ambivalence that actors might tacitly hold toward a network with which they apparently completely identify (Wynne 1992:300, 1995:383) - whereas in Callon's accounts betrayal looks like a sudden reversal, this might be only a minor shift in the balance of components of social identity. Responses to enrolment are varied along a rich continuum, including partial signings-on and partial commitments (Star 1991:49-50).

Causality. Related to these shortcomings, it becomes difficult to infer causes and explanations since ANT focuses on descriptions and 'how' questions rather than 'why' questions.

Politics and power. Another shortcoming concerns politics and power. ANT risks ignoring the possibility of some actors 'marshalling' the power of many others and not accounting for injustice (Castree and MacMillan 2001:222). ANT is often described as being too agnostic about social formations such as power and gender.

Internal paradox. For Gingras (1995), ANT is not coherent and clear. He holds that it is odd to argue that the social, technical, animate and inanimate cannot be distinguished *a priori* but at the same time use the term 'heterogeneous engineering' which suggests the contrary, namely the distinctiveness of these factors. ANT studies constantly make neat distinctions between different factors in their empirical descriptions while arguing for the contrary in introductory and concluding sections of papers.

The general shortcomings of ANT are its difficulty to discriminate, the deletion of Otherness and its flattening ontology. Concepts like fractionality, circulation, multiplicity, and overflows have appeared as a certain reaction to some of these critiques and the relatively flat and static image the notion of network encompasses. In recent years, ANT has evolved - as the book *Actor Network Theory and After* (1999) suggests - to try to grasp things that don't quite fit, that resist; to understand the contradictions that draw things apart and the complexities that can't be simplified (see also Mol and Law 2002). When studying the relation between amateurs and professionals in a museum, these concerns must be taken seriously. As we will see, amateurs are rather marginal to science, thus defying some principles of ANT. For this thesis I therefore use ANT as a temptation (Haraway 1989:6) rather than *the* theory.

The museum as an actor-network

Taking into account the benefits and drawbacks of ANT, how can we use ANT to examine a museum? Museums have not been much theorised using an ANT perspective.

Yet museums are places where scientific knowledge is made, where subjects and objects are drawn together, where heterogeneous actors meet - all themes dear to ANT. More so, Hetherington (1999:53) argues that ANT can help us to understand the museum 'as a space whose topology will alter within specific temporal, epistemological, cultural and material contexts'.

Within science studies more generally, museums as scientific sites haven't been much researched and scholarly work has mainly focused on laboratories. Discussing about the new modes of knowledge production, Elzinga (2004:16) argues:

> We hear nothing about changes in astronomy, natural history museums, language laboratories or departments of archaeology and musicology [...] the new images of scientific knowledge production have a social epistemology that is rather limited in scope.

Within STS there has not been much attention paid to study museums and academic work relating ANT to museums is rare.

ANT's relational ontology has been discussed in relation to a science museum. In her analysis of exhibition-making at the Science Museum in London, Macdonald (2002:256) has argued that agency is distributed among human and non-human actors, although not equally so. In a similar vein, it has been put forward that an exhibit does not bring together already existing objects, subjects and social groupings - 'rather, this is a conjunctural event in which the relevant objects, subjects and social groupings are co-produced' (Gomart and Hennion 1999:228). Seen through an ANT lens, the museum becomes an effect, an outcome of taxonomies, practices, negotiations, and agendas of the different actors involved, rather than a pre-existing entity. In museums things are co-produced: knowledge, objects and subjects are produced together with politics and society.

Haraway (1989:27) writes:

> Behind every mounted animal, bronze sculpture, or photograph lies a profusion of objects and social interactions among people and other animals, which can be recomposed to tell a biography.

For her, nature is constructed as a technology through social praxis, producing race, gender and class (Haraway 1989:54). She describes the American Museum of Natural History as a place where nature and culture, private and public, profane and sacred meet (Haraway 1989:29). In natural history museums, some of the dichotomies problematised by ANT tend to blur.

Translation and mobilisation both occur in museums. Discussing an illustration of the *Museum d'histoire naturelle* in Paris, Latour writes that through natural history expeditions a particular relationship permits the centre (the museum) to accumulate knowledge (Latour 1996b). An important gain is achieved through the transport and naturalisation of animals (such as birds) into the ordered space of the museum:

> The ornithologist can then quietly, sheltered, compare the relevant features of thousands of birds made comparable by immobility, by installation, by naturalization. What lived dispersed in singular states of the world, unifies, universalizes, under the precise glance of the naturalist (Latour 1996b:upd).

In this sense, the museum resembles the laboratory in that the natural and the social order are reconfigured (Knorr-Cetina 1995, 1999). Concerning laboratories, Knorr-Cetina (1999:26) writes:

> laboratories provide an 'enhanced' environment that 'improves upon' natural orders in relation to social orders.

Just as the laboratory takes its power from the 'enculturation' of natural objects, so does the museum. The museum does not need to accommodate objects as they are, nor where they are, nor cope with events when they happen but, instead, things can be 'brought home', into the museum (see Knorr-Cetina 1995:145-6, 1999:27). Museums are relational units that gain power by instituting differences with the environment: differences

between the reconfigured order in the museum and the arrangements found in everyday life (Knorr-Cetina 1999:44).

But there are differences. Comparing experimental spaces in Restoration England to museums, Findlen (1994:200) notes:

> The precisely articulated experimental etiquette circumscribing the laboratories of the Royal Society appears in marked contrast to the more fluid parameters of the museum, whose creators openly delighted in its ambiguities.

Less categorically, Kraft and Alberti (2003) hold that museums and laboratories are 'equal though different'. They state that there are continuities between museum and laboratory traditions and the laboratory supplemented rather than eclipsed the museum (Kraft and Alberti 2003:207, see also Desmond 2001, Pickstone 2000).

The difference also lies *within* museums themselves. Between the classical museum and the museum without walls there are significant differences:

> the museum without walls involves not one utopic practice like the classical museum but many [...] It is the very ambivalence and uncertainty of this space, however, in contrast to the classical museum that allows many voices to be expressed. It is also a contested space, a space with many actors who all wish to project their ideas about society, their utopics, through its space (Hetherington 1996:162).

Since a museum without walls is characterised by a multitude of voices, actors, ideas, practices, ANT is useful to account for such heterogeneities. However, the ambivalent and contested nature of such a space calls for an understanding beyond ANT.

On the one hand, the concept of 'hybrid forums' (Callon and Rip 1992:147-153) looks suitable. Hybrid forums are characterised by the multiplicity and diversity of actors that interact; actors whose interests or projects are different and who are less sensitive than others to institutional boundaries. It is hybrid since the actors, the problems they formulate

and the resources they mobilise are heterogeneous. The hybrid forum is the space in which knowledge, actors, and the identity of these actors are negotiated simultaneously. Museums, as places where both amateurs and professionals meet, where heterogeneous actors are enrolled into the production of science, can be such hybrid forums. Yet, on the other hand, there are limits to hybridisation. Or, in other words, while extending the principle of symmetry to include amateurs and professionals we have to be careful not to downplay differences, asymmetries, resistances, marginalities and ambiguities. We might well use ANT to *question* dualities, but we should remain able to *see, authorize, and explain* them.

1.4. The boundaries of science

Collins and Evans (2002:239) argue that most science studies (especially within the 'Second Wave of Science Studies'[5]) have become unable to distinguish between experts and non-experts. In science studies

> [m]uch excellent work has been done [...] by 'deconstructing dichotomies', dissolving boundaries, and the like, but like it or not, the world is made of distinctions and boundaries (Collins and Evans 2002:251).

While the tendency to dissolve boundaries is widespread in science studies, this tendency is also visible in the more general trends of post-structuralism and post-modernism. Post-structuralism, for instance,

> deconstructs the boundaries between categories, be they ontological, epistemological, ethical or material; and it demonstrates the inescapability of the leaks and flows across all such bodies of knowledge and bodies of matter (Shildrick 1997:4).

[5] Collins and Evans describe three 'waves' of science studies. The first wave clearly demarcated science from society. The second wave, emerging at the end of the 1960s, was triggered by Kuhn's work (1996).

Postmodernism prefers the shifting and unstable to the unified and integrated, and can see nothing but instability while celebrating fragmentation (Epstein 1997).

In science studies it is commonplace to argue that what demarcates science from non-science is not some set of essential or transcendent characteristics or methods but rather an array of contingent circumstances (Guston 2001:399, Evans 2005:3). It is a matter of power and authority, rather than a matter of truth (Evans 2005:7).

Barnes and Edge (1982a:18), for instance, hold that communication and rewards constitute the boundaries of science:

> [The] communication/reward system [...] erect[s] *boundaries* - between 'science' and 'non-science'; between scientists in different disciplines and specialities; and between accredited professionals and lay 'outsiders'.

They further write:

> Any scientist who can earn recognition or credibility within the system can be seen to be 'playing the game', and has established a claim to be treated as an *expert* in the particular field in question. This status is consolidated, and the boundaries reinforced, by command of esoteric *language* and skills [...] The matter is of practical consequence, since many disputes, both within and outside science, raise such questions as whether or not claims are 'scientific', [...] or who is to 'count' as a 'relevant expert'; or whether or not an expert witness has 'exceeded the limits of his area of competence' (Barnes and Edge 1982a:18, emphasis theirs)

In science, boundaries play an important role as they construct and maintain the status of experts and the territory of science. In addition, they demarcate scientific disciplines since:

Within this wave are the traditions of sociology of scientific knowledge and actor-network theory. The third wave, they claim, is the study of expertise and experience. For a critical review see Rip (2003).

> The scientific community is not a monolithic, homogeneous institution: it is subdivided into a complex of smaller units. [...] Scientific disciplines [...] have in common only a loose allegiance to widely-defined subject matters, characteristic concepts or techniques: members tend to share few, if any, specific research interests (Barnes and Edge 1982a:18).

In other words, not only are there boundaries around science, there are also boundaries within science.

Boundary-work

Boundaries are often crossed. While some work goes into 'boundary maintenance' (Barnes and Edge 1982b:241, Kuklick 1980), other forces tend to dissolve boundaries. Laboratories, for example, are sites where boundaries can be permeable.

> The traffic of objects, researchers, and information produces a *lifeworld* within which laboratories are locales, but which extends much further than the boundaries of single laboratories (Knorr-Cetina 1999:39).

In line with this, it has been argued that, in general, there are three types of bridges across boundaries: people, artefacts, and interactions (Wenger 2000:235). The material, embodied, and informational flows across boundaries can bring the 'outside world' into laboratories (see Latour 1984).

For Gieryn (1983, 1995, 1999) science is a space on maps of culture, bounded off from other territories. He writes:

> These cultural maps locate (that is, give a meaning to) white lab coats, laboratories, technical journals, norms of scientific practice, linear accelerators, statistical data, and expertise (Gieryn 1999:x).

The spaces in and around the edges are a perpetually contested terrain and what is at stake is the credibility and authority of science within 'credibility contests'. These contests

divide into three genres, into different sorts of 'boundary-work': expulsion, expansion, and protection of autonomy. Throughout expulsion 'Real science is demarcated from several categories of posers: pseudo science, *amateur science*, ...' (Gieryn 1999:16, emphasis mine). Expansion is 'when two or more rival epistemic authorities square off for [...] control over a contested ontological domain' (Gieryn 1999:16). And during protection of autonomy: 'scientists put up interpretative walls to protect their professional authority' especially if outside powers try 'to exploit that authority in ways that compromise the material and symbolic resources of science inside' (Gieryn 1999:17).

While Gieryn (1995:419) stresses the usefulness of a cartographic vocabulary to think about science, the image of maps is limited - practically and metaphorically. On a practical level, Kraeftner and Kröll (2003) have shown some difficulties to represent 'science in action' on two dimensions. They followed an evolving scientific project about genetically modified food and, as a preliminary report, elaborated a 280 square meters large 'socio-technological-graph'.[6] The map they initially wanted to draw, representing the relationships between the different actors involved, turned out to be a messy storyboard rather than a map. On a theoretical level, Martin (1997:140) has argued that

> the 'space' in which science and culture contend is too discontinuous, fractured, convoluted, and constantly changing for a map of any landscape to be useful.

What we 'need [is] an image of process that allows strange bedfellows, odd combinations, discontinuous junctures' (idib.).

There are three more aspects of boundaries worth to be stressed - process, materiality and permeability. First, boundaries are made.

[6] The scientific project they followed was about a genetically modified strain of rice called 'Golden Rice'. The messy 'storyboard' they elaborated was a preliminary report for the Ministry of Science in Austria and was presented at the Swiss National Museum in Zürich in 2002.

> Division [...] is a process. Boundaries do not exist in and of themselves. Rather, they are made and re-made, conceptually, socially, and materially (Unnamed 2003, underlined in original).

Gieryn's notion of 'boundary-work' (1983) points to the fact that boundaries are constructed. More than this, boundaries are ambiguous (Evans 2005). Gieryn writes:

> The boundaries of science are ambiguous, flexible, historically changing, contextually variable, internally inconsistent, and sometimes disputed (Gieryn 1983:792).

Yet, the notion of boundary-work is restrictive. Boundary-work

> is a rather limited [exercise], for it does not deal with the ways in which such processes of demarcation are *heterogeneous*. That is, it does not deal with the materialities of such processes of boundary work (Michael 2002:370).

This is the second aspect of boundaries worth examining: boundaries take many forms. They can be situated at structural level and at agential level, they take social, material, and embodied forms:

> [boundaries are] the lines which divide bodies of all kinds, at whatever level: institutions, people, activities or subjects. The lines may be formal or informal; they may have a legal, social or pragmatic status; they may be watertight or in varying degrees porous (Schuller 1995:3).

More so, 'boundary-work' does not take into account 'temporal boundaries' (Zerubavel 1990:172), such as the boundaries between being on duty and off duty.

Third, the boundaries of science can become permeable. Bridges are built, differences are temporarily and/or locally annihilated, exchanges across dividing lines can become easier and more intense. The 'co-production of knowledge' and 'boundary objects' are

two useful concepts to understand how science, while still demarcated, can be at the same time more closely connected to society, to the layperson, to the amateur.

Co-production

Scientific work is often very heterogeneous and conducted by diverse groups of actors: researchers from different disciplines, amateurs, professionals, patients, etc. Therefore science requires cooperation. Common understandings have to be created to

> ensure reliability across domains and to gather information which retains its integrity across time, space and local contingencies (Star and Griesemer 1989:387).

I now discuss three examples where both experts and laypersons are involved in the making of scientific knowledge. A first example is that of 'popular epidemiology',

> a process by which laypersons gather scientific data and other information and direct and marshal the knowledge and resources of experts to understand the epidemiology of disease (Brown and Mikkelsen quoted in Kleinman 1998:137).

This kind of knowledge production falls near the end of the democratic science continuum opposite scientist self-governance as laypeople are engaged in practices typically reserved for certified scientists (Kleinman 1998:138). Second, AIDS treatment activists are also involved in the production and evaluation of biomedical knowledge (Epstein 1995). In this case too, laypeople are engaged in practices typically restricted for certified scientists thus challenging the idea that only certified experts can engage in research practices. A third example where science is conducted by heterogeneous actors is the French Association of Muscular Dystrophy (Callon 1998a, Callon et al. 2001). Although this association is mainly composed of patients and their families, these non-professionals are sometimes actively involved in scientific research and collaborate with professionals.

In these three cases, the production of knowledge is a collective work where users are particularly active, rather than an activity confined only to researchers. Interactions between lay-people and professionals are permanent, forming a 'hybrid collective' (Callon 1998a:71). In this new 'model of co-production of knowledge' (Callon 1998a, Callon and Rabeharisoa 2002), a form of organization permits a close cooperation between specialised people and laypersons (i.e. the patients and their families).

The 'model of co-production' can be distinguished from two other models. These are the model of public instruction and the model of public debate, both of which share the same obsession: demarcation (Callon 1998a). In these two models laypersons are denied any competence to participate in the production of scientific knowledge. However, rather than demarcating science from non-science, the 'model of co-production of scientific knowledge' associates professionals and lay-people actively – and legitimately - into the elaboration of scientific knowledge and very often results are published in academic journals.

Let's turn to the term co-production now. The term co-production has not only been used to describe the production of knowledge through different actors, but in many other ways too. In Jasanoff's (1996) work, for example, the term co-production stands for the simultaneous production of knowledge and social order. She writes:

> the realities of human experience emerge as the joint achievements of scientific, technical and social enterprise: science and society, in a word, are *co-produced*, each underwriting the other's existence (Jasanoff 2004b:17).

In early ANT writing we can find a similar stance: 'actor networks [...] simultaneously give rise to society and to technology (Callon 1987:99). According to this 'co-production framework', scientific knowledge both embeds and is embedded in social identities, institutions, representations and discourses (Jasanoff 2004a:3).

For Callon and Latour (1992:349, see also Jasanoff 2004a:2), the term co-production initially related to the simultaneous making of society and nature. Turnbull (2002), on his side, is interested in how the cognitive and the material are co-produced with the social

as well as in the spatial co-production of knowledge and material objects. In his view people, objects, knowledge and space co-produce one another (Turnbull 2002:138).[7] Recently, the co-production of scientific knowledge and social identities has been examined (Callon and Rabeharisoa 2003). In this thesis, I use the term co-production mainly to describe the production of knowledge through amateurs and professionals.

Having discussed the co-production of knowledge, a few words need to be said about the production of knowledge. In their book *The new production of knowledge*, Gibbons et al. (1994) discriminate between two modes of knowledge production. Within the 'older' mode (mode 1), a very disciplinary knowledge production took place. This mode was characterised by homogeneity and hierarchy. The new mode (mode 2), in contrast, is defined by a transdisciplinary knowledge production,

> Knowledge which emerges from a particular *context of application* with its own distinct theoretical structures, research methods and modes of practice but which may not be locatable on the prevailing disciplinary map (Gibbons et al. 1994:168).

The organisational form of this kind of knowledge production is based on heterogeneity and heterarchy. This new mode, they claim, is more flexible and socially distributed and it has temporary forms, fluid contours, and temporary institutional spaces. As it is less firmly institutionalised and regroups a wide and heterogeneous set of practitioners (Gibbons et al. 1994:3-6), these practitioners form 'hybrid communities' - people who have been socialised differently (Gibbons et al. 1994:37).

This model is useful to examine the association between researchers and patients and the related new forms of knowledge production. However, concerning the collaboration between amateurs and professionals within natural history museums, this model has only a limited validity. Hybrid communities, which would unite amateurs and professionals, are no recent phenomena. Instead, they have characterised museums from their early beginnings, as we will see below. Since their emergence, natural history museums and

[7] Giddens has argued that to study the production of texts is to study the production of the author (Fyfe

the production of natural history knowledge were constituted by a heterogeneous group of actors.

More generally, Pestre (2003) critiques the work of Gibbons et al. arguing that the 'new' mode of knowledge production is a recent invention and that it is a too a-political model. For Pestre, mode 1 never existed in a pure form. Thus, the most interesting contrast is not in historical terms, but the fact that heterogeneous regimes of knowledge production can co-exist.

Boundary Objects

In museums of natural history, science connects participants from several distinct social worlds: amateurs, professionals, patrons, etc. If they wish to cooperate - when the world of these actors intersect - a difficulty appears: the task of reconciling the meanings of (the same) things in these different worlds (Star and Griesemer 1989:388). To examine how museums manage both diversity and cooperation, Star and Griesemer have coined the term 'boundary objects'. In their own words, boundary objects are

> those scientific objects which both inhabit several intersecting social worlds [...] *and* satisfy the informational requirements of each of them. Boundary objects are objects which are both plastic enough to adapt to local needs and the constraints of the several parties employing them, yet robust enough to maintain a common identity across sites. They are weakly structured in common use, and become strongly structured in individualist use. These objects may be abstract or concrete. They have different meanings in different social worlds but their structure is common enough to more than one world to make them recognizable, a means of translation. The creation and management of boundary objects is a key process in developing and maintaining coherence across intersecting social worlds (Star and Griesemer 1989:393).

Star and Griesemer (1989:410-1) define four types of boundary objects:

1988). In other words, texts and authors are co-produced.

- *Repositories.* Ordered 'piles' of objects which are indexed in a standardized fashion such as museums or libraries.
- *Ideal types.* Vague but adaptable objects such as the species, diagrams, and atlases.
- *Coincident boundaries.* Objects with the same boundaries but different internal contents such as maps of California.
- *Standardized forms.* Methods of common communication across dispersed work groups such as standardized forms.

In natural history, boundary objects are produced when theorists and amateurs collaborate to produce representations of nature. Among these objects are specimens, field notes, museums and maps of particular territories (Star and Griesemer 1989:408). Their boundary nature is reflected by the fact that they are simultaneously concrete and abstract, specific and general, conventional and customized, and often internally heterogeneous (Star and Griesemer 1989:408). Museums, for example, lie at the intersection of different epistemological infrastructures and serve as boundary objects between the past and the present and between the known and the unknown (Hedstrom and King 2003:35).

To the different kinds of boundary objects listed by Star and Griesemer we have to add digital libraries, as they too have been described as boundary objects.

> A digital library is a heterogeneous network of users, researchers, funders, operators, and other people; of documents, images, databases, thesauri, and other information artifacts; of practices and understandings; and of technology. It is a boundary object, both created and used by different communities for different purposes. It is the locus of multiple translations as various participants try to enroll others to ensure that the DL [digital library] meets their needs (Van House 2003:upd).

Apart from specimens, notes, museums, maps, and digital libraries, boundary objects have also become to signify non-material things: for some, boundary objects include

discourses and processes (Wenger 2000:236), while others have focused on temporal boundary objects (Davies and MacKenzie 2004). In this thesis too, I will expand the concept of boundary objects beyond mere 'thinginess'.

Apart from boundary objects, another way to ensure collaboration is the use of standardised methods for labelling and collecting. These methods can be learned more or less easily by amateurs since they don't require an education in professional biology to understand or to execute them. But, at the same time they render the information collected by amateurs amendable to analysis by professionals (Star and Griesemer 1989:406). On the one hand, data coming from amateurs must be accurate and reliable. On the other, directions for amateurs cannot be made too complicated. More abstractly, the 'allies enrolled by the scientist must be disciplined, but cannot be overly-disciplined' (Star and Griesemer 1989:407).

Museums can thus be described as 'boundary organisations' since: they provide the opportunity and incentives for the creation and use of boundary objects, they involve the participation of actors from both sides of a boundary, and they exist at the frontier of relatively different social worlds (Guston 2001:400-1). As boundary organisations, museums are involved in co-production in two ways: they facilitate collaboration between scientists and non-scientists, and they create the combined scientific and social order through the generation of boundary objects and standardised packages (Guston 2001:401).

1.5. Professionals and amateurs: history, symmetries, asymmetries and marginality

The concept of boundaries is useful to understand how professions came to be distinguished from one another - experts from laymen, science from nonscience, etc. (Lamont and Molnar 2002:177). The boundary I will be most interested in throughout this thesis is that between amateurs and professionals. In the final section of this chapter I examine this boundary from three points of anchorage: its emergence, the symmetries and asymmetries involved, and the marginality of amateurs. First, then, let's turn to the historical context of the (co)emergence of amateurs and professionals.

Co-emergence

For Findlen (1994:10) a common problem in the history of science is the neat division between 'scientists' and 'amateurs'. She argues that professional and amateur systems of knowledge could and did co-exist in the 16th and 17th centuries. It is commonly held that the emergence of amateurs paralleled that of professionals:

> As professionalization occurs, those who retain their serious, albeit part-time, commitment to the activity are gradually transformed into amateurs (Stebbins 1992:14-5).

Concomitant with professionalisation, amateurs were constructing a new identity through 'amateurisation' in the 19th century (Alberti 2001:117, 132-6, see also Taylor 1995:504). Through this evolution a semantic switch of the term 'amateur' has occurred (see Stebbins 1992:10, Drouin and Bensaude-Vincent 1996:417-8). 'Amateur' etymologically comes from *amatore*, the one who loves, and denotes a devotee who takes an interest in a particular activity out of 'love' (see Stebbins 1977). But whereas up to the 19th century amateur meant the one who loves, there was a shift of the term to signify the one who does it in spare time (Alberti 2001:116).

Towards the end of the 19th century, the contrast between 'amateur' and 'professional' was reinforced (Drouin and Bensaude-Vincent 1996:418-9). Mathematization and the growing complexity of practical laboratory work were two key factors of the disappearance of the amateur from certain sciences (O'Connor and Meadows 1976:78).

> [P]rofessionalization, involving as it did increasing stress on credentials, research apprenticeship, and sophisticated instrumentation, pushed even the wealthy amateur toward the sidelines (Lankford 1981:289).

In natural history there have been important shifts in the nature of knowledge practices which affected amateurs. There was a transition from observational and comparative

approaches (for classification and morphology) to include experimental, manipulative and quantitative techniques (Star and Griesemer 1989:394). In Pickstone's (2000) words, natural history has been supplemented by other 'ways of knowing'. *Natural history*, the description and classification of things, was superseded (although not completely replaced) by the *analysis* of things into various kinds of elements, and *experimenting* to control phenomena and to systematically create novelties.

Throughout these shifts, professional scientists have sought to demarcate themselves from amateurs. Historically, the development of the research natural history museum was an important stage in the professionalization of natural history work, and an example of the changing relationship between amateurs and professionals (Star and Griesemer 1989:391). In America during the first decades of the 20th century, professional biologists sought international credibility by distinguishing themselves from amateurs, establishing advanced degrees as credentials, and establishing specialised journals for publication (Star and Griesemer 1989:393). There were attempts to 'expulse' amateur science from professional science – to put it in Gieryn's terms. The creation of the museum was in effect a means for professional zoologists to claim greater scientific authority for their work by distancing it from activities of amateurs and conservationists (Gieryn 1995:415). It has been argued that, in general, professions seek to establish their authority and autonomy through the construction of various boundaries around themselves (Fournier 1999:282).

By the end of the Victorian era, then,

> the sciences were rapidly dividing into a multitude of specialized domains, each requiring rigorous academic training and access to expensive and complex research facilities. The process separated amateurs and professionals [...] Yet the amateur was not driven from the field (Lankford 1981:277).

Despite the increase of laboratory science, amateurs continued to collaborate with laboratory-based biologists (Alberti 2001). In natural history especially, amateur and professional developed a fruitful and continuing relationship (Lankford 1981:276) – and

their collecting practices were united (Asma 2001:113). In today's museums too, the increasing professionalisation has not entirely excluded non-professionals:

> At the same time that [...] staff hiring [is] more routinely based on certification, museums are turning to nonprofessional, noncertified, indigenous sources (Hein 2000:40).

In natural history museums amateurs were and still are a vital element. They ensure the museum's scientific success, as providers of specimens and information that scientists were and are too few to gather by themselves (Gieryn 1995:415). Thus, developments in natural history museums are to some extent dissimilar from the laboratory space discussed in the earlier sections of this chapter. The museum is a space where science doesn't seem to have closed itself off so hermetically as in the university or the laboratory.

Symmetries and asymmetries

Amateur invokes reference to the term 'professional'. Stebbins (1992:41-2) writes:

> the term 'amateur' should be used only with those activities that constitute, for somebody, a *professional* work role [...] there must be a professional counterpart to the status of amateur.

In other words, amateurs are to be relationally defined, against the background of professionals and professionalism – and vice versa. However, while doing so:

> we must avoid the unidimensional thinking that pits amateur against professional in terms of, say, little versus great skill, intrinsic versus extrinsic reward, avocational versus vocational orientation, or leisure versus work activity (Stebbins 1992:58).

The term amateur does not mean that the work amateurs carry out is necessarily less pertinent:

> In natural history especially, 'amateur' science in Victorian Britain was anything but amateur in its conduct and contribution to learning (Withers and Finnegan 2003:335).

We should therefore analyse both amateurs and professionals symmetrically:

> More symmetrical accounts will not prejudge 'amateurs' and 'professionals' so much as look at the social processes involved in their formation and the context of their existence (Desmond 2001:15).

Yet, a priori, there seem to be some differences between amateurs and professionals, as

> professionals are seen [...] as people who spend the majority of their working hours enacting their professional roles, roles from which they receive the bulk of their livelihood (Stebbins 1992:21).

Money is frequently seen as the major difference, as amateurs usually don't get paid. Yet, amateurs do sometimes receive money, even if they depend very little on it (Stebbins 1992:5). A monetary and organisational relationship is frequently established when amateurs are related to professionals (Stebbins 1992:39), even if the receipt of a fee for amateur efforts is rather symbolic (Stebbins 1992:54).

Apart from money, there are various other stated differences between amateurs and professionals. Professions are frequently distinguished from non-professions by identifying core defining characteristics:

> formal education and entry requirements; a monopoly over an esoteric body of knowledge and associated skills; autonomy over the terms and conditions of practice; collegial authority; a code of ethics; and, a commitment to a service ideal (Anleu quoted in Taylor 1995:499).

To roughly sum up the above features and translate them to science, amateurs differ in the degree and form of their socialisation into science (Lankford 1981:297). Amateurs

thus occupy a marginal status, a status that is incompletely institutionalised (Stebbins 1992:120).

Marginality

Compared to professional scientists, amateurs enjoy more freedom (Lankford 1981:298). Their marginality thus surfaces, amongst other things, as a tendency toward uncontrollability (Stebbins 1992:55-7, 2004:100-3). This uncontrollability is a feature that runs throughout the history of amateurs in natural history. Drouin and Bensuade-Vincent (1996:419) suggest that in the early 19th century,

> the difficulty was not to get the cultivators of natural history to work, since they volunteered and worked eagerly. Nor was it to gather reports, information and collections from them, since many local natural history societies included both volunteers of various degrees of training and a few paid naturalists, and were able to provide such materials. The main problem was that the cultivators of natural history formed an undisciplined crowd which the professionals would like to keep under their control.

For museum professionals to collaborate with amateurs thus means to manage an ambiguous and sometimes contradictory situation. As volunteers, amateurs are able to move more or less freely within the museum's institutionalised science; as scientists they tend to be controlled and fixed into a rigid system. However, this does not mean that this situation is temporary, that in the long run amateurs will either move towards professional science or be excluded from it. They are not necessarily on a trajectory to become either full members or non-members. Instead, amateurs can be on the 'periphery of practice',

> a region that is neither fully inside, nor fully outside and surrounds the practice with a degree of permeability (Wenger 1998:117).

The term peripherality suggests that

> there are multiple, varied, more- or less-engaged and - inclusive ways of being located in the fields of participation defined by a community (Lave and Wenger 1991:35-6).

'Peripheries [...] refer to continuities, to areas of overlap and connections, to windows and meeting places' (Wenger 1998:120). Thus, being a member or 'belonging' to a scientific community is not an all or nothing relationship.

> Belonging, then, is not just an either/or categorical matter: it is also potentially a journey [...] One can belong more or less; and one can come, over time, to belong more - and, sometimes, less (Macdonald 2001:14).

Amateurs can belong to the world of professionals since the boundary between amateurs and professionals is not clear-cut. Drouin and Bensaude-Vincent (1996:417) write:

> Between the full-time naturalists - a few paid professionals [...] - and the more or less literate lay public, there were also occasional practitioners who did not content themselves with reading but *practised* natural history by collecting specimens [...] And presumably [...] historians would find a number of other intermediate categories.

To distinguish a category between ('unserious' or 'casual') amateurs and professionals, Stebbins introduced the concept of 'serious leisure' (Stebbins 1992, 2004). He writes:

> serious leisure enthusiasts are usually more obliged to engage in their pursuits than are their less serious counterparts (Stebbins 1992:5).

For others, the amateur and the professional worlds are 'two separate yet interconnected, antagonistic, yet mutually respectful worlds' (Ellis and Waterton 2004a:upd). The boundary between professionals and amateurs is complex and porous as there are three sorts of flows between both (Waterton 2003). First, the concepts of science, the notion of scientific work as a particular expertise can be challenged. Does publishing, for example, remove amateurs from the amateur category? A second flow is that of technologies.

Published maps and card-punching technology are devices used by both amateurs and professionals. Third, money and capital is sometimes exchanged.

To describe 'amateurs as experts' (Waterton 2003) recaptures well the ambiguous situation of amateurs. Some amateurs are 'experience-based experts', that is

> members of the public who have special technical expertise in virtue of experience that is not recognised by degrees or other certificates (Collins and Evans 2002:238).

The term 'experience-based experts' refers to those whose expertise has not been recognised in the granting of certificates, while shifting the focus to experience as an important factor in the exercise in demarcation (Collins and Evans 2002:251). When we move towards experience as a criterion of expertise the boundary around science softens (Collins and Evans 2002:253). The boundary is no longer between the class of professional accredited experts and the rest; it is between groups of specialists and the rest. In this sense, amateurs can be 'lay experts' (Epstein 1995).

1.6. Summary

The main absences I have identified in academic work throughout this literature review can be summarised in five points. I would argue that these absences need more attention and it is - no surprise - these I am going to address in this thesis. Scholarly work has not paid much attention to (and in this thesis I will be):

- exploring a museum as a place where different social spaces intersect, where boundary objects are produced, and where walls are unmade;

- expanding the common focus of science studies on laboratories by concentrating on museums as sites of knowledge production;

- analysing the interrelationships between amateurs and professionals in the co-production of scientific knowledge;

- doing the 'museum test' for ANT;

- paying attention to the processes, materialities, permeabilities and ambiguities of boundary-work.

Chapter 2:

Making mess out of method

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In the previous chapter I implicitly raised some of the methods I use in this thesis. From the literature I have discussed, and the points I have stressed, three methodological implications have emerged. First, I rely on a multi-sited ethnography, an approach that moves within and across different locations and follows various associations. Second, this study applies the principle of symmetry, that is, it does not make any *a priori* distinction between categories such as human/nonhuman or amateur/professional. Third, I use a qualitative approach, relying on interviews, questionnaires, participant observations and document analysis to explore the questions I have set out.

In the literature, methods often look like clearly ordered tools, easy and ready to pick up and user-friendly. But we have to move beyond the purely 'cookbook' version of research methods (Silverman 1997:1-2). To discuss the methods of this thesis, I will be 'making a mess with method' (Law 2003). While I frame this chapter as being concerned with method and I subdivide it into different subsections (concerned with following the actors, ethnography, the field, qualitative methods, writing, translation, and reflexivity), I acknowledge the inherent messiness of methods. Discussing methods in a neat chapter with different sections implies some injustice to the actual process of designing and using them. Methods do have to be learned, practiced, and tested. Intended methods might prove difficult to work out in practice and the field might impose some methodological framings on its own. '[O]ur methods are always more or less unruly assemblages' (Law 2003:11). Accepting this messiness, I now review, discuss, and engage with the different assemblages of theory, techniques, learning, personal experience, and framings that constitute (my) method.

2.1. Following the actors: rules of method

For Latour (1999:20-1),

> [ANT is] a method and not a theory, a way to travel from one spot to the next, from one field site to the next, not an interpretation of what actors do simply glossed in a different more palatable and more universalist language.

One of ANT's methodological implications is that we should 'follow the actors'. Latour (1987:258) writes:

> We have to be as undecided as the various actors we follow as to what technoscience is made of; every time an inside/outside divide is built, we should study the two sides simultaneously and make the list, no matter how long and heterogeneous, of those who do the work.

Following the actors means leaving actors some room to express themselves (Latour 2004b). In this, the approach bears some resemblance to ethnomethodology, which asserts that it is the actors who define situations and create reality and facts and actualise rules (Coulon 1995:9, 16).

Following the actors also means applying the principle of symmetry. A symmetrical (Callon 1986) or ecological (Star and Griesemer 1989:389) approach does not presuppose an epistemological primacy for any one viewpoint. For instance, 'the viewpoint of the amateurs is not inherently better or worse than that of the professionals' (Star and Griesemer 1989:389). In practice, this means that if we are to examine the boundary between amateurs and professionals we have to follow, interview and observe both and not stick to any single viewpoint. In other words, we have to give voice to both amateurs and professionals. Apart from this, I will also give voice to both human and non-human actors, as well as considering both local and global contexts.

There are a couple of epistemological implications arising from such an approach. Lee and Hassard (1999:392) argue that:

> ANT is *ontologically relativist*, in that it allows that the world may be organized in many different ways, but also *empirically realist* in that it finds no insurmountable difficulty in producing descriptions of organizational processes.

Science studies, even symmetrical accounts, are not neutral but often directed against traditional accounts of science. Such studies usually empower the weaker and the material (Jasanoff 1996). Also, questioning the idea of boundaries is more than not

taking position: it is questioning the idea of position (Reichert 1992:16). In a related way, Clifford (1999:451) has argued that '[t]o give marginal, "between" places a tactical centrality is ultimately to undermine the very notion of a center'.

At the centre of my study is thus not 'the professional' or 'the scientist'. Instead, I am concerned with associations, with science-in-the-making, with boundaries. This is how I would rewrite Latour's (1987:258) 'rules of method' to 'fit', or perform, my research:

(1) I study science in action and not ready made science.

(2) To determine the nature of boundaries, I do not look for their intrinsic qualities but at all the transformations they undergo in the hands of others.

(3) I consider symmetrically the efforts to enrol human and non-human resources.

(4) I am undecided as the various actors I follow as to how science is co-produced; every time the boundary between amateurs and professionals is articulated, I will study the two sides simultaneously and make the list, no matter how long and heterogeneous, of those who do the work.

(5) Before attributing any special quality to the mind or to the method of people, I examine first the many ways through which inscriptions are gathered, combined, tied together and sent back.

These rules of method are inevitably linked to the theoretical framework I draw upon. They resonate with points raised in the previous chapter in which I have examined the ways in which scholars:

(1) Analyse science as a practice.

(2) Use the notion of boundary-work to explore the cultural maps of science and don't take the boundary between amateurs and professionals to be given but resulting from contingent, often ambiguous, processes.

(3) Decenter the subject and question dichotomies.

(4) Do not assume epistemological superiority for neither amateurs nor professionals and display a sensibility towards the heterogeneity of the arrangements in which science is produced.

(5) Examine the materiality and the mobility of scientific knowledge.

The theories and methods of this thesis reinforce each other.

2.2. Multi-sited ethnography

For Geertz (1973:9-10)

> Ethnography is thick description. What the ethnographer is in fact faced with [...] is a multiplicity of complex conceptual structures, many of them superimposed upon or knotted into one another, which are at once strange, irregular, and inexplicit, and which he must contrive somehow first to grasp and then to render.

To explore the relationship between amateurs and professionals I relied on an ethnographic approach. Marcus (1998:14) writes:

> The traditional assumption in planning ethnographic research is to make a subject of study something far from oneself. The necessary estrangement (or defamiliarization) of anthropological work is achieved by dramatically crossing cultural boundaries.

'Defamiliarizing the familiar' is what sociologists seek to do (Bauman and May 2001:10). This movement, from something familiar to the defamiliarization of it, is something all ethnographies have in common. In this, all ethnographies are more or less mobile, because they de- and re-contextualise. I would argue that my research methods were mobile in at least four ways: by moving across the outsider/insider status, by moving across front- and back stage, by moving across multiple sites and by moving across various 'conceptual locations'.

Insider/Outsider. One Museum staff member said to me: 'We, as insiders, certainly do not see it in the same way like you do as an outsider' (MK).[8] There is some truth in this quote. While I had to 'go native' to enter a group, I also had to find the right distance between myself and that group (Baszanger and Dodier 1997:12). The 'distance' between my work and the people I studied has to do with defamiliarizing the familiar, with theorising and talking sociologically about a case, with being critical and putting things in context. However, this is not to say that I was not able to know people in the Museum. The question if one has to be one to know one (also known as 'insider epistemology') is answered in the negative by Fay (1996): being one is neither necessary nor sufficient for knowledge of others, more so, not being one can facilitate knowing others.

Did I belong to the Museum? I was part of the Museum, much like staff members, in several ways. Physically I was inside the Museum for a year doing fieldwork. During this time, I possessed a swipe card, a Museum e-mail address, access to photocopy machines, printers; I had a phone number, a computer, and so forth. I joined staff members for lunch in the Museum's cafeteria around four times a week. I shared many things with them: a common language (Luxembourgish), interest in biology, and a common history (I had already worked in the Museum previously). I also think that I shared some ideological views with most staff members, namely ecological and democratic ideals. Contact with the Museum also extended beyond work: I went jogging and for drinks with some staff members. With one staff member I developed and still have

[8] Quotes from the interviews are referenced by interviewees' initials (i.e. MK stands for Monique Kirsch). The list of all the interviewees is in the appendix.

an ongoing love relationship. I was and still am to a certain extent somehow part of the Museum.

But even though I was in certain ways almost like staff members, and I suppose that some people might have assumed that I worked in the Museum, I was also unlike them. I was a qualitative researcher in midst of mostly quantitative researchers and a social scientist among natural scientists. I was not paid by the Museum. I was not really one of them. I did not feel like a stranger yet I probably was stranger to them than they were for me. (I have been given many different names during my fieldwork: 'Columbo', 'the spy', '*Geheimpolizei*' (secret police), 'the spirit of the Museum', 'the psychiatrist', 'our sociologist'.) Physically, I was outside the Museum for most of the time – in the relatively 'safe' surroundings of a university (Weinstein 1998:4) – when producing this thesis.

Front stage/Back stage. While studying the Museum I moved across two relatively distinct spaces: the Museum's 'front stage' and its 'back stage' (see Goffman 1969). Much of what I was looking for and I observed happened 'behind the scenes' of the Museum. The 'backstage' of a science museum is where the acquisition, conservation, storage, cataloguing and researching of artefacts usually takes place (Macdonald 2002:4, see also Allison-Bunnell 1998) It is also the place where scientists write texts, prepare talks for conferences, do experiments, manage databases, etc. As museum staff members collect and analyse data that stems from fieldwork – nature outside the museum – the back-stage area is also situated outside the museum. This does not mean that the distinction between front- and back-stage is necessarily obsolete (there still are doors, keys, and passwords that separate the latter) but that the researcher has to move within places other than the museum.

Apart from the research activities of the Museum, which are mostly hidden from visitors, I was also faced with a more public side of the Museum. The Museum's exhibitions, activity reports, web pages, official documents, books, etc. all portray the Museum in a certain way. There can be discrepancies between the official and the unofficial side of a museum. As Born (1995:7) noted in her study of a music institution (the IRCAM), an ethnography can thus

> uncover the gaps between external claims and internal realities, public rhetoric and private thought, ideology and practice.

More than this, gaps can also surface between the way certain communities are constructed in discourse and in documents, and the actual messy reality and practices of the different members of these communities. 'Scientists', 'the public', or 'museum staff' are not necessarily homogeneous categories (Macdonald 2002:7). As we will see, nor are 'amateurs' and 'professionals'.

Multiple sites. Moving within the Museum's private and public spaces, the relation between amateurs and professionals took me to multiple locations: private homes, sites of fieldwork, conference venues, etc. Amateurs especially are found in multiple locations, which requires the use of a multi-sited ethnography (Ellis and Waterton 2005). Marcus (1998:79) defines multi-sited ethnography as an ethnography that

> moves out from the single sites and local situations of conventional ethnographic research designs to examine the circulation of cultural meanings, objects, and identities in diffuse time-space.

My fieldwork took me beyond the single site of the Museum. In this way, the Museum often mirrored my ethnographical endeavours (Macdonald 1997). In the same way as the Museum tries to reach new spaces, implicate people outside of the Museum, break down some of its walls, the ethnography I did was similarly mobile, decentered, 'without walls'. Latour (1996a:46) argues:

> we sociologists have to drag ourselves around everywhere [...] Our terrains aren't territories. They have weird borders. They're networks, rhizomes.

Conceptual locations. Apart from moving between outsider and insider status, back- and front-stage as well as across different sites, I also moved across 'conceptual locations'. In Marcus' terms (1998), a mobile ethnography destabilises the distinction between lifeworld and system; in ANT parlance, distinctions such as local/global and agency/structure should not pre-exist sociological inquiry. By also taking into account the

non-human world, the present ethnography also moved beyond mere human interactions. It is, then, ethnography through 'thick and thin' in the sense that it looks at bodies and discourses, the material and the social, space and time. More so, this thesis – as most texts in science studies – also moves across disciplines. While I mostly draw on sociology, I partly rely on history and philosophy of science and I flirt with human geography.

2.3. Defining a case

I have chosen to study the collaboration of, and boundaries between, amateurs and professionals in the Luxembourg Museum of Natural History. This is my case. But what, exactly, is a case? For some, a case is a bounded system: 'certain features are within the system, within the boundaries of the case, and other features outside' (Stake 2000:436). Yet it is not always easy to say where a case ends, especially if one does not take boundaries to be given. When studying the particular, there is a problem of closure, to determine how to bound a case temporally and spatially (Castree 2003), which, in turn, affects findings. It can thus be difficult to select, define, and delineate the field, especially if researchers are faced with 'fuzzy fields', fields without clear boundaries (Nadai and Maeder 2005).

A couple of reasons pushed me to choose this particular case. Studying an institution in Luxembourg provided me with a certain 'home advantage' (Lareau 1996:199). I carried out fieldwork in the country I lived in for many years and interacted with people in their and my native language. Also, I was already familiar with the Museum. I had done a short traineeship in the Museum as a biologist two and a half years before commencing fieldwork as a sociologist. At that time, I had also started to collect documents about science and research in Luxembourg. The decision to study this Museum in particular was, on the one hand, driven by 'intrinsic' interests (Stake 2000:437). Through this study I aimed to better understand a particular case, the Museum, and to be able to situate it within the national research infrastructure. At the same time, the Museum also promised to be an interesting 'instrumental' case study (Stake 2000:437), as it could provide insights into 'boundary-work', the 'co-production of knowledge', and, as such, it could be

interesting to study something else and to redraw a generalization. There is a methodological utility of using 'homey' examples, such as bird watching, in order to purchase on esoteric and technical practices (Law and Lynch 1990:295). Through this case study I tried to combine a set of particular and general interests: to investigate the Museum, to explore science in Luxembourg, and, at a more abstract level, to use a sociological approach to study the co-production of science and to find out more about processes of boundary-work.

To empirically explore these particular and broader issues I spent a year at the Museum, from September 2003 until September 2004. Therefore, what I say in this thesis is determined – and therefore also limited - by local contingencies. Particularity and atypicality compete with the search for generalizability (Stake 2000:439). The Museum might be quite unlike other museums. Yet, while a case study is not typical it can be used to understand the typical (Silverstone 1985:2). From a geographically, historically, and culturally framed place we can talk about something that connects to other places, other times and other cultures. When doing so, when moving from the particular to the general, rigor is needed since every explanation is scale-dependent (Castree 2003).

One year before I started fieldwork, I presented my project to study the Museum to the Museum's director and the public relation manager. Doing so, I negotiated conditions of accessibility to ensure access to the information I sought (May 2001:128). Both were positive about the study. The only critical comment made was that the study was maybe too small and that I should include some other museums in Luxembourg as well. It turned out to be the opposite: I eventually narrowed down the scope of my PhD over the following years.

When I started fieldwork, I initially worked in the Museum's library and at home. After a few weeks, I got an office on my own which was situated in the top floor of the building mostly referred to as 'the annexe', the centre of scientific research of the Museum. I spent time there writing, contacting people, analysing documents, and conducting some of the interviews. For the rest of the time, I was in many other places at once. In these other places, I also had to negotiate access. In general, after I had asked people if I could

observe or interview them, access was granted quite easily and I did not encounter significant problems of accessibility during fieldwork.

2.4. A qualitative approach

Science studies tend to be qualitative rather than quantitative (Jasanoff 1996:411).[9] Sticking to this tradition, and taking into account the rather qualitative frame of the research questions, this study too uses qualitative methods: interviews, questionnaires, participant observation and document analysis. In general, the strengths of a qualitative approach are the possibility to record complexity, detail and context, which is not possible through the use of quantitative methods (Temple and Edwards 2002:4). Using a qualitative approach does not mean that there is a lack of rigour.

> A qualitative study is not an impressionistic, off-the-cuff analysis based on a superficial look at a setting or people. It is a piece of systematic research conducted with demanding, though not necessarily standardized, procedures (Taylor and Bogdan 1984:7).

I now discuss each of the methods I used.

Interviews and questionnaires

I conducted 47 semi-structured face-to-face interviews most of which with Museum staff (21 interviews) and collaborators of the Museum (20 interviews). Apart from these, I interviewed: two employees of the Ministry of Culture, Higher Education and Research, the former director of the Museum, an architect, and two persons knowledgeable in national research policy (see appendix for full list). The selection of staff members for interviews was quite straightforward. Out of the almost 100 staff members of the Museum, only 16 are involved in research activities. I interviewed almost all of them, as well as some staff members involved in educational activities. The selection of

collaborators was, however, a bit more complex. To choose some of the almost 200 collaborators I followed several criteria: I interviewed collaborators from different scientific areas; I interviewed some very active ones as well as some more 'problematic' ones; and I interviewed some recommended by staff members. Also, I decided to interview some of them on the basis of their response to a questionnaire I sent them – doing so, I selected informants who seemed especially sensitive to my area of concern and who seemed naturally reflective and more willing to reveal (see Hammersley and Atkinson 1995:137).

Before focussing on the interview process, I now briefly discuss about the questionnaire. I sent a self-completion postal questionnaire to all of the collaborators. After a second reminder (which included a pre-stamped letter as an incentive) the response rate was exactly 50%. While for a quantitative study this is usually taken to be a 'barely acceptable rate' (Bryman 2004:135), a representative sample is not always what is required in ethnographic research (Hammersley and Atkinson 1995:137). In general, qualitative researchers seek samples that are 'information rich' and are less concerned with representativeness (Plummer 2001:133). The reason I did the questionnaire was to look for common themes and to get somehow a more holistic sense of the main issues at stake. The questionnaire provided me with some more confidence in terms of what the collaboration between collaborators and Museum staff looks like. Let us turn to the interview process now.

People were very willing to be interviewed. Before each interview, I briefed interviewees on what I was doing and what I was after. Then, during the interview, I asked them at least 12 questions I had written down before. But I did not stick to a set of predetermined questions. I asked them usually some follow-up questions and sometimes additional questions. More so, the order of the questions was quite flexible and I often included a personalised question where I asked them to comment on a quote of theirs that I had noted down on a previous encounter.

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[9] Also, they tend to be deconstructive rather than paradigmatic, and self-consciously, often ironically, narrative (Jasanoff 1996:411).

The interviews lasted around 45 minutes to one hour. As most interviewees were Luxembourgish nationals I conducted most of the interviews in Luxembourgish. Others were held in French. While interviewing people in their native language certainly eased communication this engendered a practical problem: to translate quotes into English (this issue will be discussed below).

Most interviews happened in the Museum, including the ones with the collaborators. I always proposed that collaborators choose the place for the interview but only a minority of them chose their private home. Other than the collaborators and the Museum staff, people were interviewed at their working place. As the setting in which interviews take place should ideally be quiet and private, I rescheduled one interview as the interviewee could be overheard in the planned interview and I relocated some others because of noise from drilling. I tried to make interviewees feel as comfortable as possible and to do the interviews in a familiar environment. Although I tried to make sure that interviews went on without problems, I encountered many incidents: noise, batteries of the recording machine going dead during one interview, breaks through phone calls and knocks on the door. One time, both the interviewees' and my stomach were grumbling as lunch was imminent and we rushed through the last couple of questions. I tried to avoid scheduling interviews around 11am from then on.

At the end of one interview the respondent said: 'You don't talk much' (EE). To which I responded: 'That was my aim'. For the researcher, the main feature of interviews is listening, to be very attentive to the interviewee (Miller and Glassner 1997:104). But remaining silent can be hard work. I sometimes had to resist saying something after the six to ten seconds of silence it takes to make people feel a bit uneasy and make them say something or nod and say 'yes' or 'Hhhmm'. Some of the interviewees needed a bit more time to reflect on the questions than others and silences were longer. Listening to the transcripts of those interviews, I think that in some instances I was not silent long enough and I am unable to say whether interviewees might have added some more comments on some occasions. Nevertheless, interviewing is not only about being silent and only asking predetermined questions. Follow-up questions were asked and clarifications sought. One interview was extremely difficult as the respondent talked almost non-stop for interminable six hours. Even though I asked all of the intended questions, I did not direct the

interviewee enough and ended up dehydrated, hungry, and with a lot of material irrelevant to my research subject. While literature and methods courses had prepared me to listen, I was not prepared for how to stop an interview.

Interviewing is, then, about finding a good balance between remaining silent and talking. In addition to these verbal aspects of interviewing, I sometimes tried to speak with my body in order to achieve certain aims. When I noticed that people were a bit reluctant or nervous and that they crossed their arms for example, I tried to do the exact opposite: to lean back and take a more comfortable position. Or, I used smiles and bits of humour to lighten up the situation a bit. I found that these little devices usually helped to put interviewees more at ease.

For debriefing, I tried to follow Shipley and Wood's (1996:48-9) advices to 'close' interviews. They suggest four mechanisms: summarizing the major points discussed; providing the opportunity for clarifications and further questions or comments; the appreciation for the interviewee's time, interest, efforts and information provided; and an indication of further activities or actions. Although I tried to stick to these principles when the interviews came to an end, I was only rarely able to close interviews so straightforwardly. Often interviewees 'opened up' interviews again and added some more comments. On some occasions I even asked interviewees if they agreed that I pressed record again as I already had stopped the tape (I taped all the interviews). As we see, during the interview process mechanisms of closure are defied by the potentiality of overflows (see Callon 1998b).

After this part, which I enjoyed a lot - preparing questions and interviewing people - I transcribed the interviews. Although transcription is time consuming (Burgess 1984) and can be monotonous, dreadful, and utterly boring (these aspects are mostly absent from the methods literature) I preferred to work with full transcripts rather than notes. I transcribed all of the interviews myself into the original language, and while doing so, started to highlight parts I found particularly interesting. The quotes that made it into the final version of this thesis were then translated. The authors of the quotes are referenced by their initials except for in some instances where they are left anonymous.

Participant observation and document analysis

Some argue that the advantages of using participant observation in comparison to qualitative interviewing include: the ability to see through other's eyes, learning the native language, sensitivity to context, naturalistic emphasis, and encountering the unexpected (Bryman 2004:338-9). In terms of theory, one of the benefits is that theory is not so much imposed (May 2001:153). However, 'one does not start empty-handed. Theoretical ideas are not created wholly anew in each study' (Geertz 1973:27). There is no such a thing as pure and neutral observation: what I observed and noted down was clearly framed and informed by the theories I drew upon. I decided to observe some situations and not others.

The observations I made were overt ones, partly in closed settings, partly in open settings. I observed Museum staff and collaborators during meetings, informal discussions, field trips and fieldwork; I observed them in the Museum, in laboratories, and outside of the Museum. The three biggest events I observed were: the annual meeting of the Museum's scientific collaborators in 2004 and 2005, the 2004 Biodiversity weekend, and the 2003 Science festival. At these three events I became an active participant as well: at the 2005 meeting I gave a talk on my work, at the biodiversity weekend I helped the organisers with mundane tasks, and at the Science Festival I organised two debates and I performed '*La Ballade du Doctorant*' on guitar and vocals.

Yet, the collaboration between people and the boundaries between amateurs and professionals are difficult things to observe. They are not merely verbal. The co-production of knowledge and the articulation of boundaries happens in other places than observable and unsolicited situations. Relationships are not only taking place in instant conversations, but also through mails, phone calls, letters, gestures, technology, exchanges of objects, etc. This is why observations have to be, on the one hand, complemented by reconstructed, personal, active and solicited accounts: interviews. On the other hand, documents provide another means for exploration.

I looked through a great variety of documents. Some of these are in the public domain like books, catalogues, press releases, articles, annual activity reports, photographs,

plans, leaflets, brochures, drawings, and Internet pages. Other documents were not accessible to the public: letters, mails, Intranet pages, memos, etc. Collecting and analysing these documents was problematic because of a number of reasons. First of all, I gathered only very few documents from and about the Museum, as well as about science and research in Luxembourg. While I might have gathered all the possible documents there are - I checked the National Library, the Museum's centre of documentation, and I asked people for any documents - I found very little written documentation and almost no secondary sources about the Museum. On the one hand, this seems logical. In the next two chapters we will see that research in Luxembourg has only recently professionalised and that the Museum has relatively recently become an autonomous institution. A small country like Luxembourg with young institutions probably yields only very few documents. This lack of documents makes it difficult to trace the history of the Museum, for example.

Even today, the Museum does not seem to be good at self-documenting (see Atkinson and Coffey 1997:45). There are hardly any minutes produced from meetings and the single most reflexive and introspective statement about the Museum, an internal audit, does not seem to have had any significant impact. Also, the making of the book about the Museum's 150th birthday was undertaken without great enthusiasm and dedication. The Museum very seldom puts its history, decisions, actions, and aims into written words.

The quality of many documents I collected about the Museum and about science in Luxembourg was often quite poor: there were spelling mistakes, linguistic errors, poor structures, few pages, many copy-pastes, etc. Out of all the documents I read through during my PhD, written documents produced in Luxembourg and written documents produced in academia just seemed worlds apart in terms of quality, analysis and theory.

Second, document analysis proved to be problematic since the content of these documents seemed far from objective. Of course, documents cannot be regarded as producing objective accounts; they are not transparent representations (Atkinson and Coffey 1997:45). Yet, more substantial, more complete, more analytical and more correct documents would have made me more secure. While the quality and quantity of the documents I examined might provide a topic in itself, for example to talk *about* the

knowledge economy in Luxembourg, they are only a very limited resource to talk *from*. This is why I used some of the interviews, like those with the former director of the Museum and with two governmental employees, to produce also documentation and trace some of the history and politics of the Museum.

Qualitative analysis

> The aim is not to gather 'pure' data that are free from potential bias. There is no such thing. Rather, the goal must be to discover the correct manner of interpreting whatever data we have (Hammersley and Atkinson 1995:131).

Following a Latourian methodology, which is neither positivist nor interpretivist (Austrin and Farnsworth 2005:148-150), I tend to agree with this quote, except for the term 'interpreting'. Data is always already interpreted to some extent, for the researcher 'influences, if not actively constructs, the collection, selection and interpretation of data' (Finlay 2002:212). Rather than interpreting data, some have argued that we should think of interpretation as movement, as play (Strathern 2002:94). Interpreting data means selecting, associating data, it means relating data to theory, it means playing around with it and speaking from, through and with it.

Although data is brought into being through different methods, this does not mean that data has no prior substance. There should be a compromise between method and openness to situations (Baszanger and Dodier 1997:10). Producing, analysing and interpreting data emerges from a balance between using theory and method and letting facts speak for themselves. It means combining an external look at things with considering the internal stuff of these same things.

The relation of data to theory I adopt in this thesis is, then, one of balance between two extremes. On the one hand, some argue that theory should emerge after empirically gathering data. Those who use grounded theory, talk of 'theoretical saturation' (Glaser and Strauss 1967),

> the time when observations no longer serve to question or modify the theories generated from earlier observations, thus rendering the theory 'saturated' with data (May 2001:160).

On the other extreme, those taking a constructivist stance argue that every piece of data is a construct, already made, framed, selected and theorised. Theorising data does not come after the empirical gathering of it but theory already frames our way of looking at the world. Both these extremes, analytic induction and deductive approaches such as grounded theory, should, I think, be avoided for the substance of data and the ways for looking at it are interdependent, co-producing one another.

To analyse the data I gathered I have tried to take a balanced approach. I have tried to ground theory in data but, at the same time, it was through theory that I tried to make sense of the data. In this process, the subsequent chapters sometimes took shape quite differently. The main theme of chapter five, for example, in which I will talk about the places of encounter between amateurs and professionals, emerged before I did the fieldwork. I made sense of the Museum's annual conference and its journal in theoretical terms (i.e. Wenger's (1998, 2000) notion of 'boundary encounters') before I had properly observed and examined these. On the other hand, it was rather towards the end of my fieldwork that I realised that the Museum's databases were a site of rich and often conflicting data, and decided to turn this into a separate chapter (now chapter six). Also, when theorising data in the next chapters, the theories I used sometimes seem to fit quite well my observations while at other times, I found that I had to move beyond existing theory.

For me, the process of analysis does not fit nicely into the neat and distinct approaches one usually finds in method textbooks. Yes, in line with a grounded approach, I coded data, I turned data into fragments, and themes and theories were sometimes rather emergent. But then, in other places, existing theory simply did the job. Maybe analysis is best thought of as the process of bringing data and theory into resonance – into a relation in which neither data nor theory initially predominates – and then, by trying to do justice to both, to generate coherent and consistent theorisations.

2.5. Writing as method

Writing is not just an activity at the end of a research project but also a method of discovery and analysis (Richardson 2000).

> Writing texts has everything to do with method. You write a text of so many words, in so many months, for so much grant money, based on so many interviews, so many hours of observation, so many documents (Latour 2004b:upd).

A text is the result of many choices, constraints, and contexts. Choices have to be made about rhetoric and the audience one wishes to address (see Collier and Toomey 1997a:4-5). The choice of the audience can pose a dilemma in writing up: should accounts be more academic or more descriptive, more experience-near and factual or more experience-distant and reflexive (Macdonald 1997:169-73)? For me this was not much of a dilemma since I did not have much choice. As a piece of work aiming to get a PhD label, it had to be written for an academic audience and, as such, it has to be a scholarly piece of work: analytical, reflexive and - at least to some extent - experience-distant. Prescribed writing formats, such as the use of references had to be followed (Richardson 2000:923). And, as a student of the University of Sheffield, I am expected 'to produce a thesis which is coherent, well-documented and written in good English' (University of Sheffield 2005:21) and the Faculty of Social Sciences of my university guides me not to exceed 75,000 words.

Other than that, common for a PhD is the Introduction/Literature Review/Methods/Results/Discussion/Conclusion format (see Phillips and Pugh 1994:63). I decided to stick to this model - a model which is, after all, common practice, expected by readers, and probably more readable than unconventional models. However, the process of learning and designing methods, for example, is not situated between a bounded literature review and subsequent result chapters. The disparity between laboratory practice and final reports in journals (Latour and Woolgar 1979)

applies to a PhD thesis as well, final structure of which can be very different from the actual making of it.

Writing is never a neat, linear sequence of steps (Collier and Toomey 1997b:70). Writing is all over the place. And texts are messy. Insisting on open-endedness, incompleteness, and uncertainty about closure is what makes texts messy (Marcus 1998:189). Yet, they cannot be overly messy. Scientific writing takes place through the use of conventions which must be learned and which set boundaries (Collier and Toomey 1997a:8). While some have tried to overcome the boundaries between academic and other forms of writing (Stacey 1997:24), as a student, I don't feel experienced enough to cross most of these boundaries. I might do so in the future, but today I lack the confidence to do so.

While being bounded and structured in a certain predetermined way, a text, especially within science studies, has room for messiness. This unlike, say, a television documentary on science which has to be very neat:

> All of it [...] has to be ordered and framed. The disorder and contradictions of an observed reality have to be transformed into an ordered and coherent, plausible and persuasive, vision of reality (Silverstone 1985:44).

Ambiguities, ambivalences, and uncertainties can - and some argue that they should - be part of a sociological text about science.

2.6. Dilemmas of translation

Translating abstracts from interviews or documents from one language (such as Luxembourgish or French) into English affects research. Nuances of language and original multiplicities can be lost in translation (Smith 1996). One of ANT's dictums, to translate is to betray, can thus be taken in its most literal meaning here. The act of translating from one language into another means betraying some of the particularities of the former context. Consequently,

> methodological and epistemological challenges arise from the recognition that people using different languages may construct different ways of seeing social life (Temple and Young 2004:164).

There can be no equivalence across languages and there is no single correct translation of a text (Temple and Young 2004:165, Temple and Edwards 2002:2-5). Rather than trying to create absolute equivalences between languages, another way to see this – it seems, irresolvable – issue is to think of translation as mediation between two linguistic repertoires, between two cultures, and between researcher and researched (Smith 1996:162). Translating thus creates an in-between space, a space of hybrids (Smith 1996:162). Translation and writing are forms of 'border writing' in contact zones – places where cultures that were previously separated come together (Simon quoted in Temple and Edwards 2002:18).

While losing something (context, multiplicity, specificity) there are also things gained through such mediations: new insights and reflexive engagement. Another thing gained is transportability. By translating quotes from Luxembourgish and French into English I augment the mobility of my accounts within the academic world. If I had left quotes in their original language, it would have been immensely difficult to turn them into part of a PhD because of two reasons. First, I could not have done a PhD like this one in Luxembourg, as the University of Luxembourg doesn't have the authority to issue PhD degrees. Second, I cannot think of any suitable supervisors and examiners who would be knowledgeable in Luxembourgish and in this thesis' theoretical framework, thus this PhD couldn't have been supervised nor examined. (There is only one academic sociologist in Luxembourg, whose research focuses on socio-linguistics.) Hence, it was necessary to translate. To talk sociologically about a case in Luxembourg I have to convert accounts from the national language into another language and I cannot express myself in my native language. Translation seems to be essential for a small country like Luxembourg in order to analyse and theorise it and to overcome academic absences by relating accounts to other contexts, countries, and languages. Translation is a boundary practice in that it is concerned with creating coherence and maintaining a common identity across

social worlds, while adapting to the local needs, constraints and requirements to each of these worlds.

I translated the quotes I included in this thesis only after I had transcribed the interviews into their original language. In doing so, I have done fairly literal translations rather than ones which make people sound entirely fluent. Initially, I did all translations myself, but while working on the final draft, I asked my father, a professional interpreter working for the European Parliament, to check all my translations. While I contend that equivalence between initial comments and the translated version is not possible, I have tried to mediate (or interpret) with the help of a professional interpretor (thanks dad!) as best as possible between different languages.[10]

2.7. On reflexivity

Postmodernism, feminism and science studies stress (albeit in slightly different ways) that one has to reflect on one's own impact on the researched. Postmodernism argues for the existence of a personally involved, politically committed ethnographer instead of the morally neutral observer of phenomena (Denzin 1997b:274) as he or she is depicted in positivist accounts in the social and natural sciences. Following postmodernism is a greater awareness and acknowledgement of the role of the researcher as part of the construction of knowledge. '[The fieldworker's] social and emotional involvement in the research setting constitutes an important source of *data*' (Punch 1986:14). Within science studies, the argument is similar. For Woolgar (1988), those who study science should also reflexively analyse their own representational practices about science. Where one comes from informs, frames, shapes one's choices, one's take on research: the knowledge one produces is situated (see Haraway 1991). Reflexivity, then, refers to the inseparability of representation and represented (Cooper 1997:274).

So what? What can a reflexive stance tell me more than an un-reflexive one? It tells something about how I came to my research questions and the possible biases due to my

own standpoint. That I write about myself tells something about choices: about programmatic choices of academics to study science symmetrically (and therefore to study oneself) and about my choice and willingness to conform to some of the requirements to be accepted among science studies scholars. But, on the other hand, self-reflexions tell nothing more about the subject directly, about the Museum I study in this thesis. In these paragraphs, one doesn't learn much about the main subject of my study but more about the context of the research, the author, the motives. Some have argued that reflexivity cannot provide a better understanding of science (Baber 1992:114, see also Latour 1988). For Pickering (1992b) reflexivity should not be applied. He writes: 'reflexivity has no message about anything apart from itself. Its signposts lead to nowhere; we should not follow them' (Pickering 1992b:19, see also Cooper 1997:273-7, 284-6). Thus, the question I had trouble to answer is: should I, and to what extent, be reflexive in my thesis? Even as I write and read these lines, I am still uncertain. There are good arguments for both positions: being reflexive and not being reflexive. It is difficult to 'negotiate the swamp' of reflexivity as there are sometimes contradictory accounts of the rationale and practice of reflexivity and each way of approaching reflexivity has its strengths and limitations (Finlay 2002:225). As in science studies there is no commonly agreed stance on this issue, how should I, a student, know what position to adopt?

I have decided to be 'moderately' or 'reasonably' reflexive. As for some, reflexivity is a dimension of method (see Marcus 1998:97), I have made the choice to position myself in this chapter about methods – as well as in some other chapters. Yet, in doing so, I still feel that I have to be pragmatic, my priority is to answer my research questions more or less directly without getting too much sidetracked. Here is a short CV of mine: I am a male Luxembourgish national from a middle-class background who lived in Luxembourg for more than 20 years. My studies have brought me across three countries (France, Norway and England), two languages (French and English), four universities, and two subjects (first biology then sociology of science). Having crossed national, linguistic, cultural and scientific borders might have informed my interest in boundaries.

[10] Translation happens in other instances too: when converting spoken words into written texts during transcriptions or note taking, when analysing data, and finally, when the text is read by others.

The identity I have just articulated is one I would present in certain contexts only, for example in a scholarly piece of work or when applying for a university post. There are also financial and emotional aspects that bond me to the Museum. First, I chose to study a museum in Luxembourg knowing that the Luxembourg government has two criteria for attributing research grants: national interest and scientific quality. Second, I find the Museum a fascinating, warm, and friendly place to be in.

In the Museum itself, my identity as a researcher was enacted differently. During my fieldwork, I was often asked what I was doing in the Museum. I defined myself in different yet interconnected ways. While I initially tried (often in vain) to explain in great detail STS, I gradually explained myself not in one but in different ways. I gradually constructed a repertoire of answers depending on whom I was talking to: I described myself as a sociologist, student, doctoral student, science studies student, someone interested in research about research, a former biology student who has moved on to study the context of biologists. I negotiated a situational identity (Angrosino and Mays de Perez 2000: 683-4). But this is not to say that I enacted 'different selves' in Mead's (1934:142) sense when he writes:

> We carry on a whole series of different relationships to different people. We are one thing to one man [sic] and another thing to another. [...] We divide ourselves up into all sorts of different selves with reference to our acquaintances. We discuss politics with one and religion with another. There are all sorts of different selves answering to all sorts of different social reactions.

Rather then presenting different selves, I felt that I presented nuanced versions of a same self. These were not multiple identities. In a sense, it was more than one but less than many identities.

In a similar way, I did not enter and leave the field with one fixed set of ethical principles. Ethical obligations are complex and cannot be fulfilled through simple adherence to a prescriptive list of requirements (Murphy and Dingwall 2001:347). During situations where I had moral dilemmas I tried to bring two perspectives into harmony. On the one hand, I tried not to harm and offend others unnecessarily. Funding and academic intent

do not constitute license to invade the privacy of others, since 'qualitative researchers are guests in the private spaces of the world' (Stake 2000:447). Especially, as there existed (and still exists) a certain kind of contract, and a certain amount of trust between myself and the Museum, between researcher and researched, I felt obliged to the Museum to be honest, respectful and accurate. Yet, on the other hand, I have tried not to make any concessions about what I say in this thesis just to please Museum staff members. I tried to negotiate my own intellectual freedom. I felt that some criticisms can and should be addressed, for a critical understanding can improve sociological knowledge and might also induce positive changes in the Museum.

2.8. Mess, again

In this chapter I have discussed the messiness of method. Not only have I talked about method, but also about my personal standpoint, about how theories and method inform each other, and how this particular study imposed some methodological requirements of its own. I have discussed the limits of this study, my uncertainties, and the problems I faced - things which cannot be 'rinsed away' by more or better methods because they are very much part of the research process. In short, method is messy because it always relates to more than only prefabricated tools to understand the social world.

I did not merely 'use' methods, nor did I 'write up' this text in linear order after empirical data was collected. To some extent, methods, writing, and theory emerged and changed interdependently throughout the making of this thesis.

This thesis is made.

I contend that my account is constructed, situated, that I perform a certain version of the Museum: I describe the Museum in a certain way, using a specific vocabulary, drawing on certain theories and methods to explore a set of issues. Yet, this does not mean that this thesis is made *up*. I still draw on a pre-existing body of methods and theories. I quote people and documents, and I write about situations I experienced. I feel that I have accurately rendered the Museum through theory and method.

Morgan, such a personal feeling might be the lowest common denominator of method taken as a whole, the only commonality between qualitative and quantitative approaches, between postmodernist, feminist, positivist and empiricist approaches. Whatever methods researchers use, they probably feel that they use tools that are sensible to use and that, doing so, they are able to relate to some kind of reality and render it accurately. Even if the messiness of texts, theories, and methods might vary from nonexistent to overdone in final accounts, the sheer existence of a scientific text is based upon trust in method.

Maybe, dear *alter-ego-italic-writer-who-is-a-good-device-to-place-a-view-Morgan-is-unsure-about,* but that's enough about messy method for now. Let's move on. Let's turn to the next chapter and look at science, culture and research in Luxembourg.

Chapter 3:

Putting Luxembourg on the map

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'To put Luxembourg on the map' seems to be a favourite statement from Luxembourgish politicians. During my fieldwork the minister of research frequently pronounced statements like 'We have to put Luxembourg on the map of research' during her speeches. Similarly, the current minister of culture recently declared that Luxembourg cinema 'helps to situate Luxembourg [...] on the map' (Unnamed 2004a). Another quote has it like this: 'museums in Luxembourg contribute "to put Luxembourg on the map"' (Unnamed 2006).

Luxembourg seems to have two complexes. First, it is not well known for its culture and science. When Luxembourg makes it to international news it is usually related to its financial and economic situation or to the European institutions present in the country. Second, its geographic size makes it literally difficult to represent Luxembourg on maps. Luxembourg is such a small country that on most maps of Europe, there is only an L to be found instead of its full name. Related to the missing prominence of its science and culture, Luxembourg itself has not been put on the agenda of academic research - to date there is only one monograph about the sociology of Luxembourg, dating back to the 1950s (Fehlen 1999).

With this chapter I try to put Luxembourg on the reader's 'map'. I begin this chapter by mapping out the economic, cultural and societal landscape of Luxembourg to situate the context for the subsequent chapters. Then I will look at the national research infrastructure and science policy. We will see that Luxembourg is characterised by a relatively late institutionalisation and professionalisation of science compared to other European countries. Efforts to 'put Luxembourg on the map' of international research are recent and still ongoing.

3. 1. An open economy and society

Before Malta joined the European Union in 2004, Luxembourg was the smallest country in the Union in terms of geographic size, population and GDP (Fontagné 2004:59). At the same time, however, Luxembourg is twice as rich as the European average (Fontagné 2004:41). Luxembourg has been much dependent on external contributions to develop into such a prosperous country, which leads economists to describe the country as a small

space open economy. The most significant characteristic of Luxembourg's economy, they argue, is an obligatory opening up and integration into a larger space, this on the level of commodities and services and on the level of the factors of production: capital, technology and work (Schuller 1999:79, see also Bauler 2002:814). The geographical size and its socio-economic structure force Luxembourg to extreme import/export ratios, far away from self-sufficiency (Bauler 1998).[11] In an article entitled 'Economy without borders?' analysing the relationships between businesses in Luxembourg's wider region it is argued that markets only rarely coincide with political borders or national frontiers (Thomes 1996:156).

This economic integration seems to have been facilitated by the central geographic location of Luxembourg, sandwiched between Germany, France and Belgium. More so, this geography seems to facilitate linguistic and cultural diversification (Schuller 1999:80-1). Luxembourg is described as:

> a country of passage, a land of transition between the Germanic world and the Roman world, a geographic entity and a political entity on the edge of a linguistic frontier, but of a frontier that unites - rather than separates [...] (Christophory 1994:335).

At the same time, the economic and cultural openness of the country seems to ease its societal openness. A survey on the values of Luxembourg reveals that its inhabitants are rather tolerant and display an open attitude towards immigrants compared to neighbouring countries (Dubajic 2002:390). To sum up, Luxembourg's economy, culture, and society seem to be very open.

The alleged openness of Luxembourg can be examined by looking at the flows across its borders. These flows take many forms: workers, languages, money, and so forth.[12] Concerning workers, half of Luxembourg's active working population travels on a daily basis from France, Belgium or Germany to work in the country. This phenomenon is

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[11] Another quote has it like this: 'The economic and social evolution of small Luxembourg has always been, essentially, tributary on exterior factors and influences' (Kieffer 1997:176).

known as cross-border workers, or *frontaliers* in French or *Grenzpendler* or *Grenzgänger* in German (see CRP-CU 1998). Along with foreigners working or living in Luxembourg, different languages are used both in written and spoken form. Luxembourg is described as a multilingual society.

> French is what keeps it together, multilingualism (and polyglossia) is what keeps it going, and Lëtzebuergesch is what keeps it apart (Weber cited in Fehlen 2002:92).

Apart from workers and languages, the flow of money was an essential component of Luxembourg's socio-economic development. From the end of the 19th century until the mid 1970s foreign investments have helped to develop and turn the steel industry into a lucrative sector. Then, after the economic crisis in the mid 1970s until today, monetary flows have put the financial sector into a dominating position, turning banks into the paradigm for Luxembourg society. In the last decades, then, Luxembourg developed from an industry-based economy towards a service-based economy - a shift that others describe as a process of change from materialistic values towards post materialistic values (Dubajic 2002:763).

Hence, there are various ways in which Luxembourg is 'caught in-between'. First, to roughly sum up its socio-economic evolution, Luxembourg was known for its steel industry, it is presently best known for its financial sector and it aims to be better known for its culture and science. Second, Luxembourg is situated in-between the cultures of Germania and Romania, as well as between political powerlessness and the pride of being among Europe's architects (see Kreins 2003:115-25). Luxembourg has played a considerable role in taking the initiative as a cultural and political broker in the construction of Europe. Third, as I hope to make clear throughout this chapter, Luxembourg is currently caught up in-between a legacy of amateurs who produce scientific knowledge and aims to professionalise research infrastructures and careers. Finally, concerning Luxembourg's cultural infrastructures, a major shift has occurred in the 1990s. 1995, when Luxembourg

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[12] The flows of capital, technologies, goods, and people across national borders challenge clearly defined and neatly bounded national identities (Lamont and Molnar 2002:184-5).

was European capital of culture, is frequently portrayed as a turning point concerning cultural institutions and policy.

3.2. Cultural shifts

> [I]n 1995 [...] the entire country was made aware of the tardiness in matters of cultural infrastructure (SIP 2004:2).

This quote is taken from a document issued by the Luxembourgish government. In 1995, when Luxembourg was European capital of culture, people seem to have realised that the country's museums, concert venues, and theatres were underdeveloped to host such an event. But this quote seems a bit odd. As if in 1994 and the years before, 'the entire country' was not aware of these issues. If we flick through the rest of the document, we can't find what in particular Luxembourg was tardy in comparison with, although other European countries or neighbouring countries might be a good guess. Yet, to give this quote some credit, when Luxembourg was European capital of culture a whole new context was drawn into Luxembourg: more people came to visit museums, budgets dedicated to culture increased, and previous capitals of culture were probably examined in great detail by the organisers. Luxembourg's cultural infrastructure certainly looked tardy compared to previous cultural capitals and it looks so in hindsight today.

The above quote also holds that 'the entire country' was made aware of cultural tardiness. While this seems much of an overstatement, the country's museums and ministry of cultural affairs must have felt this more intensely. To put it into more critical terms, in 1995 and the years before, the government was confronted with the result of its own scarce efforts in the past to promote culture. One of the few Luxembourgish academics writing about culture holds:

> During a certain time, the State seems to have sought to escape its responsibilities on the matter [of the creation of cultural institutes], giving up this ground for private initiative (Wilhelm 1999:188).

The creative stimulus to build museums, for example, was not a priority of the state but resulted from efforts emanating from individuals and learned societies. For a long time, then, there was a gap between the legal and political framework of museums and their physical reality. The first two laws setting up the legal framework of museums (in 1960 and 1988) mostly institutionalized and officialised structures that already existed. But over the years, a more fine-grained cultural policy developed, acknowledging the importance of culture through the increase of budgets, the creation of new institutions and better-defined statuses.[13] The gap between museum reality and political will seems to have decreased during the late 1980s and 1990s.

1995 is often portrayed as a revolutionary year concerning cultural policy. Consider the following quote from a report on the impact of the cultural year:

> The success of 1995 was at the origin of a new ambitious cultural policy aiming at the upgrading and expansion of existing cultural networks, encouraging investment in new cultural infrastructure [...] In fact, a whole series of cultural buildings and institutions have been realised or are presently under construction (Myerscough 1996:upd).

The changes in the museum landscape illustrate well these cultural shifts. Before 1995, there were only a couple of small museums spread across Luxembourg and one 'big' museum, the Museum of the State, located in Luxembourg city. This latter museum was one of the only institutions collecting and representing Luxembourg culture, be it in the arts, history or science. After 1995, the number of museums rose from two to seven in the capital city alone.[14] Among the newly created institutions, the National Museum of Natural History opened its doors in 1996. However, most institutions created after 1995 were already in the making long before the mid 1990s: the Museum of Natural History

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[13] The Ministry of culture's budget dedicated to culture increased from 10 million euro in 1990 to 54 million euro in 2002 (Wagner 2005).

[14] In 2005, the *Stater Muséeën* network, a network connecting the main museums in Luxembourg city, consists of 7 institutions: the Casino Luxembourg - Forum d'Art Contemporain (opened in 1996), the Museum of Modern Art Grand-Duc Jean (opened in 2006), the Museum of the Fortress (due to open in 2006), the Museum of History of the City of Luxembourg (opened in 1996), the National Museum of History and Art ((re)opened in 2002), the National Museum of Natural History ((re)opened in 1996), and the Villa Vauban (opened in 1959).

was legally created in 1988 and its construction began a few years later; the Museum of History of the City of Luxembourg and the Museum of Modern Art were both initiated at the end of the 1980s. 1995 was less 'the origin' of a new cultural policy than a high point within a decade of development which reinforced the prominence of culture within Luxembourg's politics and society.

Apart from these national changes, 1995 was apparently the year that 'put Luxembourg on the international cultural map' (HRI 1998). However, almost a decade later, Luxembourg still seems tardy compared to its neighbouring countries, although on a very different level:

> always late on international developments, [... and on] the successive crises of the markets which surround it. Thus, Luxembourg builds museums whereas France or Germany tighten still a little bit more the bolts of their finances, enlarges and creates theatres whereas they close in Paris and Berlin, engages musicians for its official philharmonic orchestra when important orchestras lay off [...] (Hansen 2004:upd).

Luxembourg still seems insulated from global cultural trends. While other countries were affected by curatorial crises and decreasing state-funding, Luxembourg had its years of cultural expansion. How much the museums appreciated this cultural policy can be seen through the mobilization following the departure of the quite popular minister of culture who was in office from 1995 to 2004. In an eulogistic open letter signed by many cultural actors, the minister was thanked for her support and role in promoting the cultural landscape of Luxembourg. She was described as 'the enlightened craftsman of the development of the cultural activities of our country' with 'the concern of creation, the will to defend the cultural workers, their moral and social rights, and to facilitate the professionalisation of their activities' (Unnamed 2004b).

This letter looks as if politics and museums evolved in perfect harmony. Yet, there still were discrepancies. A few months before this letter was written, a law was passed in parliament that bears the trace of the museums' reluctance towards the government's hitherto most pro-active plan. In fact, the government had planned to confer on museums

more autonomy in terms of finances and recruitments by turning them from public administrations entirely funded and supervised by the state into public establishments and to instate thereby administrative councils. But this initiative was rejected by the museums because of a concern of interference. What was planned as a reform turned out to have only been a *reförmchen* (a little reform), as the first advisor for cultural affairs of the government noted (GD).

In practice this means that museums in Luxembourg remain largely independent in terms of their choices and do not have to worry much concerning accountability. But they also remain limited in terms of human resources. With their decision to safeguard autonomy, they have hindered the development of their research activities for example.

The relatively comfortable position that museums occupy materialises on other levels too. Issues which create much debate internationally - such as debates on the evaluation of museums and their privatisation and accountability - are almost absent in Luxembourg. The two biggest cultural debates of the last decades were rather centred on the place of contemporary art within Luxembourg. One of the polemics revolved around the Museum of Modern Art and the Museum of the Fortress (both due to open in 2006). These two museums are adjacent and built on the remains of the fortress of Luxembourg. In a nutshell, the debate was articulated along the following divides: international prestige versus national patriotism, (space and money for) contemporary art versus (space and money for) classic art, political will versus popular feeling.

The biggest cultural debate has without doubt been around a work of art entitled *Lady Rosa of Luxembourg*. This sculpture was an almost identical copy of what Luxembourgers call the *Golden Lady* - a golden sculpture of a woman commemorating war victims. The copy, however, was pregnant and contained inscriptions on its pedestal such as 'Madonna' and 'Whore' - words which were intended to reflect how women are portrayed in society and to show their sufferings during war. This installation caused much heated discussions around nationalist feelings, memories of war, female sufferings, and the freedom of expression of artists (see Majerus 2006, forthcoming).

Apart from these two polemics, current debates focus rather on questions like 'does one really need all these museums in Luxembourg?' or 'will there be enough content for all these containers?' (Neu 2004:133). Mistrust in cultural institutions, especially the most recent ones, is almost palpable. Nevertheless, in general, museum visits have increased over the last decade, and they figure more prominently within people's cultural activities.

It has been argued that over the years, the application domain of Luxembourgish culture has shifted from elitist to democratic and from supreme intellectual activity to distraction (Wilhelm 1999:157-8). The first of these asserted shifts seems to be quite true for museums. The history of the first (big) museum in Luxembourg, the Museum of State, is linked to the emergence of savant societies in the middle of the 19th century and their wish to create and manage collections. Members of those societies belonged to the intellectual elite of the country and access was not granted to everyone. More so, from the 19th century until the mid 20th century, the Museum of State was only partially accessible - due to moving, World War II, and reduced opening hours that limited its accessibility for visitors. It was only in the late 1970s, in a general trend of democratising and decentralising culture, that the idea emerged to transform museums into more open spaces. The appearance of museums in Luxembourg has much changed since then: educational services have been established; there was a turning away from very scientific and object-dense exhibitions to favour more accessible, interactive and entertaining exhibits; and increasingly more visits and activities to attract a larger public have been organised.

However, throughout this development the asserted shift from supreme intellectual activity to distraction is questionable - this for three reasons. First, museums in Luxembourg are today not only concerned with distraction since their general mission includes 'the study, the conservation and the development of the cultural heritage as well as sensitising, education and training activities' (Unnamed 2004c:1798). Second, the museums have not stopped to be places of intellectual activity. Although most museums in Luxembourg are primarily concerned with managing their collections and setting up exhibitions, a number of them are involved in research activities. The conferences, scientific publications, and the activities of the researchers employed by museums are all revealing of an active production of intellectual work. Third, the presence of visitors at workshops

and conferences, for instance, shows that people still come to museums to be educated. Although entertainment has visibly increased, educational and research activities have not vanished from museum activities. Quite the contrary: they have rather increased over the years.

The assumed shift away from supreme intellectual activity is thus not to be seen in a decrease of such activities *tout court,* but rather in the decreasing role of museums as important institutions in the making of scientific knowledge. In Europe the great age of scientific museums was the 19th century (Pickstone 2000) whereas today their relevance as key scientific institutions has weakened. In Luxembourg, however, until the mid-1980s the museum was a relatively important actor in both research and education. This, not so much because it produced and communicated essential knowledge but rather due to the absence or underdevelopment of other scientific and cultural institutions.

3. 3. The institutionalisation of science

It is often argued that universities have displaced museums in terms of epistemological weight. The museum's 'object-based epistemology' (Conn 1998) was displaced by an increasing prominence of more transportable and less 'object-dense' knowledge produced by universities. Yet, in Luxembourg the university has not (yet) emerged as a new key institution. A dynamic academic environment still seems to be lacking:

> An endemic deficiency of the cultural life remains: the absence of a body of university teachers, of programs, of courses, of seminars, of research centres, of organizations on a large scale of conferences and scientific congresses, of defences of theses, of publications, etc., in short of a university life [...] (Wilhelm 1999:194).

In a similar vein, a scientific collaborator of the Museum of Natural History described Luxembourg as an 'academic desert' (AF3). Just like Luxembourg's tardy cultural infrastructures discussed earlier, the above two quotes point to Luxembourg's tardiness in

matters of research infrastructures. But can we go so far to argue, as some have done, that there is no scientific tradition in Luxembourg (Calmes and Bossaert 1996:539)?

To verify the truthfulness of this statement, it is worth giving a historical synopsis of Luxembourg research. For the current purpose the time scope extends from the 1970s, a time during which there were initial efforts to better coordinate research, up to today with the creation of the National Research Fund and the University of Luxembourg. Historical developments before that time will not be covered (some elements can be found in Sprunck (1948) and Massard (1989)) while emphasis will be placed on the last thirty years.

In the 1960s, an OCDE report affirmed that Luxembourg had a deficient scientific milieu: 'There exists, on the governmental level, no organised research nor scientific program' (OCDE 1963:8). Rare were institutions and individuals who carried out scientific research. The creation of the *Centre Universitaire* in 1969 seems to have given a new impetus to research in Luxembourg. The 1970s are a decade of starting institutionalisation of research as the following quote (about biological research) reveals:

> The creation of the [*Centre Hospitalier* in 1975], of the [Luxembourg Council for Scientific Research in 1977] and the realization in 1979 of the system of detachments for research activities [...] made that biological research knew a renewal of strength, in particular within the Department of Sciences of the *Cours Universitaires*, the Museum of Natural History and the *Centre Hospitalier* of Luxembourg (Christophory et al. 1984:10-11).

The Luxembourg Council for Scientific Research, created in 1977, was the first structure of scientific policy of the country (in contrast: in most industrial nations the creation of science councils occurred in the immediate postwar period (Cozzens and Woodhouse 1995:582)). This Council aimed to better coordinate the multiform and wide-ranging research activities existing in Luxembourg and to favour publications and international cooperation (Ternes 1985, MAC 1980). It is alleged that the Council had a heteroclite composition, a rigid structure and only sporadic activities (Christophory et al. 1984). It

seems to have been an instrument of low impact which lacked means and was therefore abandoned in 1983 (Ternes 1985).

Despite these initial efforts, research was not well organised in Luxembourg. 'Scientific research [...] is hardly coordinated and remains scattered' noted a booklet on research issued by the government (MAC 1980:3). The activities of researchers remained restrained, isolated and of little importance. Most research being done was one-man research (Fayot 1980a).

Regarding biological research, the situation was described as follows:

> what continues to slow down biological research in Luxembourg is the absence of a tradition of research related to a university environment worthy of this name, as well as the absence of legal structures on which to set up biological research (Christophory et al. 1984:11).

It was alleged that research had structural deficits concerning the recruitment, training and career of personnel, regarding the organisation of research teams, and due to the absence of an organism for decision and orientation of research (Christophory et al. 1984:11). Budgets and human resources dedicated to research were limited (Christophory et al. 1984:11). That the government itself had difficulties issuing budget figures is telling: 'It is almost impossible to identify the part of Luxembourg's GDP assigned to scientific research' (MAC 1980:18). Only one professional researcher officially existed in 1980 (Fayot 1980b).

Then, in the early 1980s, there seems to have been an increasing recognition of the importance of research and innovation for the growth of economy (STATEC 2003:97). In 1987 a law was passed in parliament which set up the legal framework for the organisation of research. Three public research centres emerged subsequently and governmental research grants for doctoral and post-doctoral students were introduced.

Further shifts occurred towards the end of the 20th century. Especially from 1999 onwards power and decision structures have been reconfigured. Before that time, the

government had opted for a 'bottom-up' approach (Hansen 2001).[15] In slightly more critical terms, research in Luxembourg was marked by '*savage growth*, born out of idealism and individualistic engagements' (Hansen 2002b, emphasis added). But over the years, the 'bottom-up' approach was gradually complemented by a 'top-down' approach (Harpes 2002:22,37). The reorganisation of governmental structures is evidence of this change. Before 1999 there wasn't a proper department of research at the Ministry of cultural affairs, which was in charge of research policy. But in 1999, a department of research was created what is now called the Ministry of Culture, Higher Education and Research. Furthermore, higher education was separated from education at ministerial level. These reconfigurations of governmental structures put the state into a more powerful position to implement science policies and to try to lead scientific research into certain directions. The same year, in 1999, the creation of the National Research Fund indicates an even more proactive strategy from the State. This structure was created in order to give a supplementary impulse to Luxembourgish research by elaborating multiannual research programs, by defining priority axes, and by promoting national and international cooperation (Berche 2003) – thus aiming to create a more propitious environment for research. Finally, the creation of the University of Luxembourg in 2003 is the latest important structural development in research to date. The creation of this university is an interesting case that reveals some of the difficulties of structuring and professionalizing research in Luxembourg.

For a long time, four disconnected institutions were involved in both research and higher education in Luxembourg. This emerging university resembled a 'loosely-coupled system', a system characterised by a relative lack of coordination; differences in methods, aims and missions; infrequent inspection; a relative absence of regulations; little lateral interdependence; and the 'invisibility' of much that happens (Weick 1976, see also McNay 1995). More so, state support, international recognition, and societal acceptance were low. In 2003, these four institutions were merged into the University of Luxembourg. Within the next three years, many changes occurred, transforming these loosely coupled public institutions into a single, more entrepreneurial institution. The same changes that happened within decades in British academia, for instance, have been

[15] The Luxembourg Council for Scientific Research did 'not impose a determined direction to research'

collapsed into just a couple of years in Luxembourg: a shift from seeing students as apprentice academics towards seeing them rather as customers, a replacement of freedom as the dominant value of the academy by the value of competence, a reorganisation of weakly structured departments to clearer units within faculties, and a planned increase of private funding (see McNay 1995).

The European context also played an important role in decision-making. Today official discourses often stress that Luxembourg has to catch up a certain delay face to Europe and to conform to the objectives of the Lisbon Declaration (i.e. to spend one percent of the GDP for public research until 2010) and to the objectives of the Bologna Declaration (i.e. to instate the bachelor/master/PhD system). The University of Luxembourg will be an important element for competitiveness and an essential player in the new knowledge-based economy – so the recurrent argument.

However, the project of the University of Luxembourg was based upon the assumption that all the mobilised actors (researchers, teachers, ministry, European context, students, budgets, etc.) would behave in a certain way. The problematised actors and the theoretical links that had been spun out had to stand up to the test of reality. But one year after the legal implementation of the University in 2003, the fragility of this actor-network became evident. The university director suddenly passed away and some researchers did not 'lock [...] into the roles that had been proposed for them' (see Callon 1986:196): faculty deans withdrew from their function, some professors accepted chairs at foreign universities, and university staff often made complaints. As a consequence, the generally positive mood and enthusiasm about the potentialities of the university were tempered within one year as the terms used in the press like 'standstill', 'downsizing' (Hirsch 2004) and 'saving the university' (Fayot 2004) reveal.

These difficulties show, amongst other things, that there surely is a scientific tradition in Luxembourg. If there was none, the university could have been created from scratch without much difficulty. But this was not the case, as the university was built by drawing together loosely connected institutions – each with its own history. The far-reaching

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(MAC 1980:21).

origins of the university are even situated at the beginning of the 19th century when in 1817 the *Cours Supérieur* was created (Christophory 1994:335-6).

Although during the 19th century and most of the 20th century there were hardly any scientists in Luxembourg, there surely was a certain kind of scientific tradition.

> In the Grand-Duchy of Luxembourg, research, far from being inexistent, has developed since a long time in the scope of restricted structures [...] (MENFP 1997:32).

For a long time science was embryonic and done by amateurs. But although Luxembourg science certainly looked 'amateur-esque' compared to science in the neighbouring countries, to state that there is no scientific tradition at all, is to miss all the individual and enthusiastic efforts to practice science with spare resources, low governmental support, in a difficult environment. It is to miss the scientific activities happening in the characteristic locus of science at that time: the Museum of Natural History, the *Centre Universitaire*, the *Centre Hospitalier*, and the savant societies.

3.4. The late professionalisation of Luxembourg research

Due to the scarcity of scientific institutions and a relatively unattractive academic environment, researchers seem to have left Luxembourg or never came back after having received a university degree:

> our present and future professionals, [...] some say they do not exist or are already gone off to the bigger countries (Ternes 1985).

Before the mid-1980s, Luxembourg research was barely visible. Before that time, scientific research was carried out by

> a small number of researchers who, in the institutions of the state and in the industry, assumed research works *beyond their professional task* (MAC 1980:3, emphasis added).

Consequently, science was hardly a profession. It has been argued that biological research, for example,

> was *tainted by amateurism* in the sense that research was considered to be a *leisure occupation*. This amateurism has prevented the establishment of a structured tradition of biological research in Luxembourg (Christophory et al. 1984:10, emphasis added).

This 'amateurism' surely made the transition to professional science difficult. Amateurism probably stood in the way of professionalism. This, especially, if the same people who once were amateurs were now involved in professional science. However, many more factors also hampered the establishment of a structured practice of biological research (rather than 'the establishment of a structured tradition' which seems contradictory). Political will, funding, legal frameworks, and libraries have been relatively 'amateur-esque' in the past. The whole 'epistemic infrastructure' (Hedstrom and King 2003) of Luxembourg's science – its museums, libraries, and archives, but also the buildings in which research was undertaken – was rather unprofessional.

In addition, the use of the term 'leisure occupation' is problematic here. Although for some people research might have been a leisure occupation, for others it was rather 'serious leisure' (Stebbins 1992, 2004) – an activity of enthusiasts who are more systematic, substantial and engaged than just doing casual leisure.[16] In fact, science could only be practised as serious leisure since, until lately, there was no distinct professional career for scientists due to the absence of a legal, political and institutional framework. Until the mid-1980s, there simply was no 'professional' scientific field in

[16] For Stebbins (1992:6-7) six qualities distinguish serious leisure from casual leisure: the need to persevere; to have careers in their endeavours; significant personal effort based on specially acquired knowledge and/or training and/or skill; durable benefits (self-actualisation, self-enrichment, self-expression, recreation or renewal of self, feelings of accomplishment, enhancement of self-image, social interaction and

Luxembourg, which makes the use of terms such as 'amateurism' or 'professionalism' inappropriate. It would be more accurate to say that in the last 20 years a professional space for science gradually emerged through the creation of a legal framework, a funding structure, a university, etc.

These shifts were accompanied by a changing status of scientists. In the past, '"researchers" ha[d] other functions besides their research activity' (MAC 1980:18) - note that the word researchers is put in-between quotation marks. Those who did research often benefited from the *décharge* system (Decker 1992:255-6), a system instituted in 1979 through which high school professors, for example, could be *déchargé*, or detached, to do research. Yet, this system is declining today. In 2002, the government decided to put an end to those detachments and reallocated remunerated working hours to teaching activities only (Hansen 2002b).[17] In other words, there has been a suppression of the permeability that existed between institutions of university vocation and institutions of secondary education through the institutionalisation of a boundary.

In addition to these transitions there have been significant shifts in the kind of science undertaken in Luxembourg. Until the 1980s, natural history was a relatively central endeavour: the first official research centre in Luxembourg was created at the Museum of Natural History; in the first document the government issued about research (in 1980), natural history is mentioned at some length; and, finally, in 1984 a laboratory of geodynamics was attached to the Museum and not to another structure. In 1980, the official position of the state concerning funding of research was as follows:

> The state's principal criteria is the utility of research for the national heritage. Thus, a clear preference is given to [...] fauna and flora, geology, climatology, mineral richness, agricultural production, energy resources, the natural environment, etc. (MAC 1980:5).

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belongingness, lasting physical products of the activity); unique ethos; and strong identification with pursuits.

[17] Yet today there still are people involved in teaching at the university who are not employed by the latter: the so-called *vacataires*.

But in the 21st century natural history is not so prominent anymore: the research centre of the Museum of Natural History is not mentioned in the document entitled 'about... ...research' issued by the government in 2003; the planned public research centre on environment (that was to be run partly by the Museum) was finally not created; and demands for research budgets from the Museum have diminished over the years. Today, the three public research centres that exist in Luxembourg get more attention (and budgets) than the research centre of the Natural History Museum. The mission of these research centres is to favour applied research and to foster technology transfer with industrial partners (i.e. in the domains of nanotechnology and e-commerce). Consequently, the part of research activities responding to the needs of society and to the creation and diffusion of new knowledge seems more reduced in Luxembourg than in the rest of Europe (Dautel 2003:5). In Luxembourg, research policy gives priority to applied research, putting in perspective a contribution to the economic development of the country (Decker 1992:263). Applied, utilitarian and partially privately funded research is favoured, whereas fundamental research has a hard time (Deitz 2004).

In Luxembourg the move from natural history towards a more mathematical, analytical, and applied science, seems to have taken place much later than in other countries. In countries like England, France and Germany it was during the late 19th century that a new form of science strongly attached to laboratories emerged; natural history as a 'way of knowing' (Pickstone 2000, see also Jardine and Spary 1996) was declining in prominence compared to other ways of knowing: analysis and experimentation. In Luxembourg, the turn away from science as naturalistic and serious leisure activities took place significantly later, culminating towards the end of the 20th century. In other words, in the last two decades, there has been a significant reconfiguration of the institutional territory of Luxembourgish science as laboratory sciences now hold the central role formerly held by museums and collection-based sciences (see Forgan 1994:154).

At the same time, research budgets have significantly increased – between 2000 and 2003, for example, they were multiplied by three (see Dautel 2003). Also, until recently, systematic evaluation of research projects hasn't been common in Luxembourg; for some the country has no 'evaluation culture' (Allegrezza 2004). But increasingly, through the National Research Fund, foreign scientists are asked to evaluate research projects. This is

due, first, to the smallness of the Luxembourgish research community. Simply put, there are not enough scientists in Luxembourg to competently judge scientific work. Second, the perceived necessity to rely on foreign scientists' evaluations might be due to the problem of Luxembourgish nepotism (Christophory et al. 1984): Luxembourg scientists might produce non-objective comments - in other words, a small country seems to have an 'objectivity deficit'.

Apart from the increasing prevalence of evaluation, there are other qualitative changes worth noting. There are currently three Luxembourgish journals publishing works in the natural sciences: the 'Archives' of the scientific section of the *Institut Grand-Ducal*, the 'Bulletin' of the Society of Luxembourgish naturalists and 'Ferrantia - Travaux scientifiques du Musée National d'Histoire Naturelle'. In 2002, the latter two journals have put together editorial boards, which send submitted texts to external scientists for peer-review. This was not the case earlier and articles were usually accepted and published without critical review. But today, the traditional, personal and naturalist style of writing is not welcomed anymore. Although these journals are still not very visible internationally (they have no impact factor), there is nonetheless a tendency towards meeting international standards, by increasing the quality of published works and by publishing articles (or at least abstracts) in English rather than French or German.

3.5. Entangled in the diversification trend

In debates about Luxembourgish research, an argument often put forward is that Luxembourg is too small and that there isn't any critical mass of scientists.[18] However, the real problem seems to be situated somewhere else. Not only is there only a small pool of researchers, but research activities are thematically, spatially, and institutionally dispersed. Research seems to be entangled in the diversification trend of Luxembourg, an omnipresent force in governmental politics.

[18] It has been stated that, in general, small countries often lack sufficient critical mass or breadth of high-level technical skills (Davenport and Bibby 1999:441).

First of all, let's situate the trend to diversify in its context. Diversification was emphasised *in opposition to* something: political economy strove to rebalance the vulnerability of the monolithic industrial structure, the weakness of Luxembourg's dominant steel industry (STATEC 2003:13-5, 95-6). The deliberate policies thus implemented by the public authorities strove for diversification. While regarding the economy this seems reasonable, in matters of research there can be a conflict between efforts of diversification and the aim to construct the so-called critical mass. A too great diversity conflicts with the establishment of poles of competence through the gathering of various actors into one domain (for a similar point concerning enterprises in Luxembourg see Siweck 2003). How far should research be diversified? Or, in opposition to what should it be diversified? Although there isn't a monopoly regarding research in Luxembourg, research seems to be caught up in the momentum of the diversification trend.

Already in the early 1990s, pleas were made for a non-dispersion of research efforts and resources, deploring a natural tendency in Luxembourg to disperse efforts too much (see Allegrezza 1992:289). The actual situation at the university and at the three public research centres reveals these conflicting issues. The research and teaching facilities of the university, for instance, are dispersed on numerous sites, despite an improvement being in sight. Some see this as a serious constraint upon developing interdisciplinary research (Harpes 2002:42). It goes without saying that interdisciplinary research would be facilitated if researchers were put into one single space. And if those researchers would work in a reduced number of domains, the construction of the so-called critical mass would be easier.

The same problem has been reported by a team of experts who evaluated research at the three public research centres in the mid-1990s. In their report, the experts stated that the diversification of research can be a dilemma as well as a blessing. Concerning one of the research centres, they emphasised that, on the one hand, the multiplicity of competences assures the centre's mission of technology transfer, but, on the other, it was noted that this multiplicity impedes the coherence of research efforts and collaborations between teams whose competences are too far apart (MENFP 1997:25). Likewise, research activities of another research centre were also assessed as being too heterogeneous; the dispersion of laboratories on various sites impedes consultation in-

between different groups, making a coherent image of the centre difficult (MENFP 1997:35, 38). As a general conclusion, the report asserted that research was uncoordinated and that the links between the public research centres and other institutions have to be reinforced. A recentering on a more limited number of research axes was recommended, in particular to develop pluridisciplinary poles (MENFP 1997:57).

What is missing, according to the report, is the coherence of research efforts as well as collaboration. Similarly, it has been stressed that concerning national innovation, the interactions within the public sector, as well as within the overall system, are low in number and quality and that there is a lack of trust (De Biasio 2001:112-3). But how, we might ask, can a critical mass be obtained in such conditions? More so, what if the components to form a critical mass are already present, but what's missing is the cement, in other words the interconnectedness of the various actors of Luxembourg research? Maybe Luxembourg has had its critical mass of scientists for quite some time already, but a lack of material infrastructures, of trust, and motivation has limited its visibility and functioning.

Over the years, the situation doesn't seem to have improved much. In 2004, Luxembourg's Chamber of Commerce wrote the following:

> the high number of the actors of Luxembourg public research is largely disproportionate compared to the size of the country. The multiplication of administrative structures and the double use of available resources don't make it possible to reach the critical mass necessary to follow a research policy under optimal conditions [...] It becomes urgent to rationalize as soon as possible the organization of Luxembourg public research, which necessarily implies to fuse the various national structures of research [...] (Chambre de Commerce 2004:41).

In a similar vein, the national agency for innovation recently wrote:

> public research in Luxembourg can be regarded as relatively little structured; it thus appears convenient to reconsider the structuring of the various bodies which compose research in the public sector (Luxinnovation 2005:156).

Luxembourg research is produced in a 'fragmented space' (Lefebvre 1991:282), a space that lacks homogeneity; a space that contains internal boundaries between competencies, research axes, institutions, people and activities. This is a typical barrier for regional innovation systems, fragmentation, the fact that 'relevant actors may be present without forming a working regional innovation system' (Héraud and Isaksen 2001:10). The lack of co-operation between research actors seems to be a major problem in Luxembourg. While official discourses repeatedly stress the need to construct a critical mass of researchers, geographical and cultural distances between researchers are still substantial. To (re)centre research activities, funding, infrastructures, and students on a few specific domains would be one way to counter the 'surface handicap' of Luxembourg (Ternes 1985), to attain 'institutional thickness' (Amin and Thrift 1994) and to counterbalance the strong diversification trend.

3.6. Conclusion

For a long time, the 'creative class' - among which we can count scientists, university professors, artists and cultural figures (Florida 2002) - had a relatively low institutional and social standing in Luxembourg. Luxembourg's museum, university and research infrastructure seemed, to put it simply, tardy. In particular, science was relatively weakly structured due to: the late institutionalisation and professionalisation of research, the drawing upon amateurs to produce scientific knowledge and the recent structuring of the university. Yet, during the last two decades, there have been significant changes in research structures and policies. Apparent in this development are two ways in which we can understand the term 'culture'. First, that culture involves ruptures in any uniformities of practice and, second, that culture can be associated with a certain richness of ongoing events, a thick growth of variegated patterns piling up on top of one another (Knorr-Cetina 1999:10).

Ruptures have appeared in the shift from amateurism to professionalism, in the reconfiguration of institutions and governmental policies, in the creation of the university, and in increasing evaluation. These shifts in the social architecture of science have been accompanied by the suppression of the permeability that existed between institutions of university vocation and institutions of secondary education. In other words, a new institutional and professional space for public research has been carved out through processes of boundary-work (Gieryn 1983:783).

Yet this permeability is still visible nowadays at the Luxembourg Museum of Natural History, a place where the model of co-production of knowledge prevails. At this museum the practice of science is surrounded with a degree of permeability, involving both professionals and amateurs. In contrast, most of Luxembourg research was affected by the construction of boundaries. The professionalisation of science was, in other words, obtained through the delimitation of a specific bounded space. Through *enclosure*, 'the specification of a place heterogeneous to all others and closed upon itself' (Foucault 1979:141), a distinct space for scientific practice was created within Luxembourg. At the same time, there has been an opening up of Luxembourg science to meet international standards and criteria, a strive, for example, to collaborate to European projects. Luxembourg research is moving from a rather fluid and uncoordinated space towards a 'space of prescription' (Callon and Law 1989; see also Murdoch 1998), a space which is strongly prescribed by a centre as norms circulate, imposing fairly rigid and predictable forms of behaviour.

'Culture' can also be seen as the thick growth of variegated patterns piling up - although a 'thin growth' better depicts the development of Luxembourg science during the major part of its history. For a long time, Luxembourg had an uncoordinated research system. The 'surface handicap' and the related 'history handicap' of Luxembourg help to explain the relatively weak research tradition and the rather recent professionalisation of science. But today, Luxembourg is allegedly 'on the road to Lisbon', aiming to meet the goals of the Lisbon Agenda to turn Europe into the world's leading knowledge economy. To reach that goal, human and financial resources will probably be increased - such as spending one percent of the GDP into public research. But alongside this material reality, efforts

will probably be deployed to further improve the social reality of research: to refine politics and identities and to increase the quality and visibility of science in Luxembourg.

These historical, societal and cultural contingencies are key elements to keep in mind before we focus in the next chapters on how amateurs and professionals are related in the Luxembourg Museum of Natural History. Simply put, amateurs have always been involved in science in Luxembourg. Not only is their presence today understandable, their presence further reflects the missing institutionalisation or sheer absence of professional knowledge-producing sites. But today, scientific institutions do exist and the process of structuring and stabilising the actor-network that produces scientific knowledge will probably continue. In a sense, it will become more mature. For scientists this means that they will increasingly have to meet international standards and that they will occupy a better-defined career with increased societal and institutional visibility. For amateurs this means that their place within scientific research in Luxembourg is changing in front of the shifts in the 'topography of the production of scientific knowledge' (Forgan 1994:156). Finally, for politicians this means that they will be a step closer regarding their aim to put Luxembourg on the map of international science.

Chapter 4:

Centralising and Decentralising the Luxembourg Museum of Natural History

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Museums are commonly imagined as fixed structures. As solid buildings in which objects are displayed, often in glass cases, museums produce stable arrangements. These arrangements, which order objects and knowledge in space, seem to persist over time. Museums and their displays *endure*. But museums are not inert, rigid institutions. Belying this apparent stability, museums are places full of movements. The emergence of museums entailed creating a space to which objects and subjects travel. Inside the museum, these objects and subjects move around. The purpose of museums can be seen in sustaining and facilitating different kinds of movements: museums collect objects, construct identities, educate and entertain people, represent the absent, etc. In doing so, museums 'draw things together' as much as they try to 'get something across'.

In this chapter I examine the Luxembourg Museum of Natural History by focusing on two movements: centralisation and decentralisation. After analysing how the Museum and its scientific research centre were discursively, materially, and architecturally made (parts 4.1. and 4.2.) I will theorise this making of a museum with walls (part 4.3.). Then, in the next part, I will explore the means to create a museum without walls through Museum buses, the Science Festival and other means. In part 4.5. I will introduce the scientific collaborators of the Museum before, in the last part, reconsidering the centralisation and decentralisation of the Museum more theoretically.

4.1. From a cabinet of natural history to the National Museum of Natural History

In the middle of the 19th century, shortly after Luxembourg became an independent nation, the first museums emerged in Luxembourg.[19] The origins of the Luxembourg museum of natural history are to be found in 1850, when the Society of Natural Sciences in the Grand-Duchy of Luxembourg was created. The mission of this society was to '[...]

[19] 'After the declaration of the independence of Luxembourg in 1839 [...] the intellectual class has not ceased to promote the idea of the national identity and to claim for a national institute [museum]' (Yegles-Becker 2002:111). According to Eischen (2003) the first museum of Luxembourg city was the Jean-Pierre Pescatore Museum, inaugurated in 1872. Mousset (2003) argues that the Museum of Antiques, the ancestor of the actual National Museum of History and Art was the first museum in Luxembourg. He argues that the word museum appeared in 1843 whereas Christophory (1989) and Yegles-Becker (2002:111) argue that this museum was established in 1839. Another candidate to the 'first museum in Luxembourg' title could be the Medal Museum created by Alexandre de Wiltheim in the 17th century (Neyen 1861). See Meyer (2004) for a review on this topic.

contribute to the progress and propagation of natural sciences, physics and mathematics in the Grand-Duchy' (Massard 2000:13). One aim of the society was to create a cabinet of natural history. It made therefore a call for people to donate objects to the society that might be of interest for the study of the natural history of the Grand-Duchy. A couple of years later, in 1854, the cabinet of natural history opened its doors to the public (Massard 2000, see also Massard and Geimer 2004).[20] At that time, the cabinet consisted of four rooms housed in Luxembourg's only high school. Two curators were in charge of conservation, surveillance and ordering the collections: one for mineralogy and geology, the other for zoology and botany. Both were professors at the high school (the majority of the other members of the society, however, were not learned scientists).

Two denominations, 'museum' and 'cabinet', were used interchangeably during that time - just as the world's first museums had numerous denominations like 'galleria', 'cabinet', 'museo', 'studio', etc. (Findlen 1994). Thus, to say that the Museum is 152 years old today is a bit problematic, even though the Museum celebrated its 150th birthday in 2004. It has become a museum, rather than being one - discursively, materially and legally - since its beginning. Moreover, the Museum was not very accessible during most of the 19th century as it was open only on Thursday afternoons. Then, from the end of the 19th century until the beginning of the 20th century, after the Museum had moved out of the city centre into a former military hospital in 1892, it became even less accessible for visitors and scholars.

Around thirty years later, in 1924, the collections were handed over to the state. At around the same time, the state had acquired a building in the city centre to house the Museum. The collections were transported back once again to a more central location. But over the next years the project to create the Museum of State (that was to house two sections: natural history and history and art) did not take on at the speed some had hoped for. Although the natural history section of the future Museum had gathered a collection with the help of 'disinterested' foreign specialists and 'some naturalist amateurs-collectors' (Ferrant 1934:27), the Museum remained closed to the public until 1949. It is thus difficult to call it a Museum since without visitors, we should rather refer to a

[20] However, the opening of the cabinet is not mentioned at all in the press of that period (Massard 2004,

collection because a museum, *définition oblige*, needs visitors. Not only were visitors absent, also, in the beginning of the 1940s, the Museum was described as 'administratively and materially in an embryonic state', characterised by 'management without a precise form, a building about to be finished and collections in the process of installation' (Heuertz 1949:130). Everything that made up the Museum – its administrative structure, its material structure and its collections – was yet to be stabilised.

During World War II, the German *Wehrmacht* requisitioned the Museum to serve as *Frontsammelstelle*, a place where soldiers gathered when coming back from the front. During that period, the internal order and the protective function of the Museum walls were under threat. The Museum curator complained about dirt and disorder and after the Germans left he was happy that nothing was harmed: 'After 4 years of anxiety, I am happy to know all the collections conserved and the building intact' (Heuertz u.d.:69, underlined in original). A couple of years later, in 1946, the natural history section of the museum organised its first exhibition.[21] But it took another three years before the Museum was officially inaugurated (Heuertz 1949).

The Museum being finally open, the next years saw some efforts to improve its legal makeup. In 1960 a first law was passed that turned the Museum into an official institution:

> This date corresponds to the official birth of a Museum of Natural History within the Museums of State, created through the law of the 17th of August 1960 (Le Brun-Ricalens 2002:79).

Two sections now officially existed at the Museums of State: natural history and history and art. The museum(s) had then its own walls, its law, its personnel, its collections and its visitors. But the two museums that were housed within this single building longed for more space. On the website of the Natural History Museum we can read today:

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personal communication).

[21] The exhibition was dedicated to two persons that had provided many specimens to the museum, one of whom was an adventurer. In his speech, the director thanked them and referred to 'good patriots'.

> their collections and their activities diversified and multiplied so that only a separation of the two museums seemed to solve the lack of space.[22]

But behind this seemingly inevitable separation due to a lack of space laid some bitter disputes, according to the first director of the Natural History Museum. He recounts:

> natural history was, vis-à-vis the other [history and art section], pushed into the corner. Everything broke down. There were only two or three persons anymore. [... The history and art section], who had spread over 80, 90% of the surface, had 90% of the personnel and approximately 90% of the budget [...] It only could go via a collision course. I had to face it; you had to quarrel with them. Politicians said: 'It is time now that we separate them' (NS2).

The natural history section of the museum demanded adequate infrastructures for its personnel, research activities, and for the conservation of its collections. The public's increasing interest in collections and educational activities were mobilised as key arguments.

To the Museum's contentment, the National Museum of Natural History was officially created in1988, which separated it from the history and art museum. From then on, the Museum of State officially ceased to exist and two separate institutions - the National Museum of History and Art and the National Museum of Natural History - emerged. In 1996 the Natural history museum reopened its doors to the public in a new location, at about 10 minutes walking distance from its former place. It had moved, once again, away from the city centre to its periphery.

The Museum building

The Museum's walls reveal a rich and interesting history. In fact, throughout its history, the Museum's collections were housed in four different locations: first, in a high school; then, in a former military hospital; third, in the building of the Museum of State; and, finally, in a

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[22] Quoted from the museum's web pages www.mnhn.lu [last accessed: 3/7/2005]

building that used to be a hospice and then a prison. The Museum's materiality was threatened only once during this eventful history: by German bombs during World War II. Fortunately, the bomb that was dropped closest to the Museum detonated in a nearby treetop (Heuertz u.d.:69). Throughout time, then, the Museum's walls have been rather robust, fulfilling their role in protecting collections from the weather and troublesome individuals.

The building the Museum is now housed in used to be a prison for women. After the prisoners were transferred to another location, the building's architecture had to be stabilised, transformed, renovated, and 're-disciplined'. First of all, it had to be stabilised since it was in a pitiful condition. Apparently, the structural problems of the building were initially underestimated and builders were confronted with walls about to crumble down and foundations washed out by the adjacent river (Stomp and Faber 1995). Restoration was even trickier since the building was listed, and, as such, its external appearance could not be altered.

Apart from external physical and material exigencies the inside of the building had to be transformed too. The architecture of a prison does not lend itself well to museum display since it is a rather enclosed, unfriendly, segmented, and door-dense space. A museum, however, calls for bigger, friendlier and more open spaces. The government agreed to:

> the demolition of all the appendices and disparate constructions, the lowering of the surrounding walls, the partial disassembling of the grids of the windows and the rebuilding of the removed attic windows. [...] The transformation of the building into a penal establishment obviously strongly marked the current provision of the buildings. The conversion into a museum will make it possible to find larger spaces thanks to the demolition of many superfluous partitions. [...] The whole will be carried out by taking into account the requirements imposed by the new educational and cultural function of the unit (Unnamed 1986).

While the outside of the building still looks ancient and church-like today (see pictures 1 and 2), its interior has been significantly transformed.

Picture 1: The Museum building

Picture 2: The entrance of the Museum

The ground floor and the first floor now contain the Museum's permanent exhibition whereas the second floor is reserved to temporary exhibitions. The Museum's permanent exhibition is organised around six themes, each of which are exhibited in a separate room (see pictures 3-9):

Who am I? In this room visitors are shown through models that they are one out of six billion people, a cluster of cells, a genetic code, etc.

Our landscapes. This room contains a large-format projection on the typical vegetation of Luxembourg and stuffed fauna and flora typical of its main biotopes.

Our regions. The four regions of Luxembourg (Centre, *Oesling, Minette* and East) are represented by means 'of typical and various objects that recall the influence of the abiotic and biotic factors on the life and culture of inhabitants'.[23]

Our ancestors. This room is concerned with evolution: 'the visitor will thus meet the most ancient Luxembourger, the famous man of Loschbour' as well as a mammoth, dinosaurs, fossils, etc.

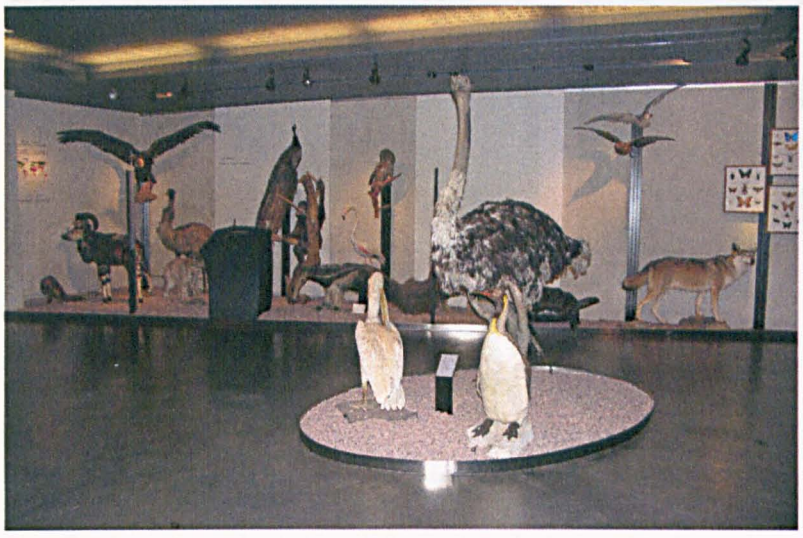
Our earth. This room revolves around planet earth: its history, tectonics, volcanoes, and so forth.

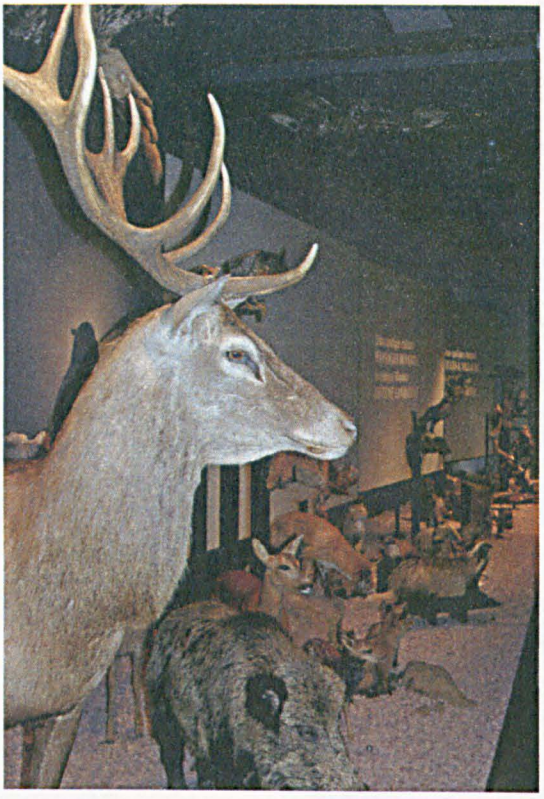
Our planets. Includes models of planets and the solar system and explains the chronology from the Big Bang to the emergence of the earth.

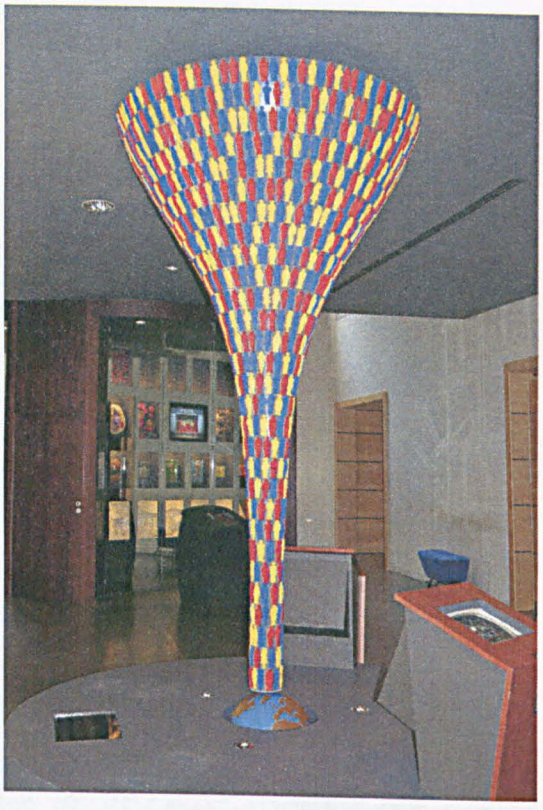
Apart from these six rooms there is also a 'discovery room' where 'the visitor becomes actor, a researcher [... through] the observation of vivariums, by the study of samples under binocular magnifying glasses, [...]'. Hands-on science takes place in this room.

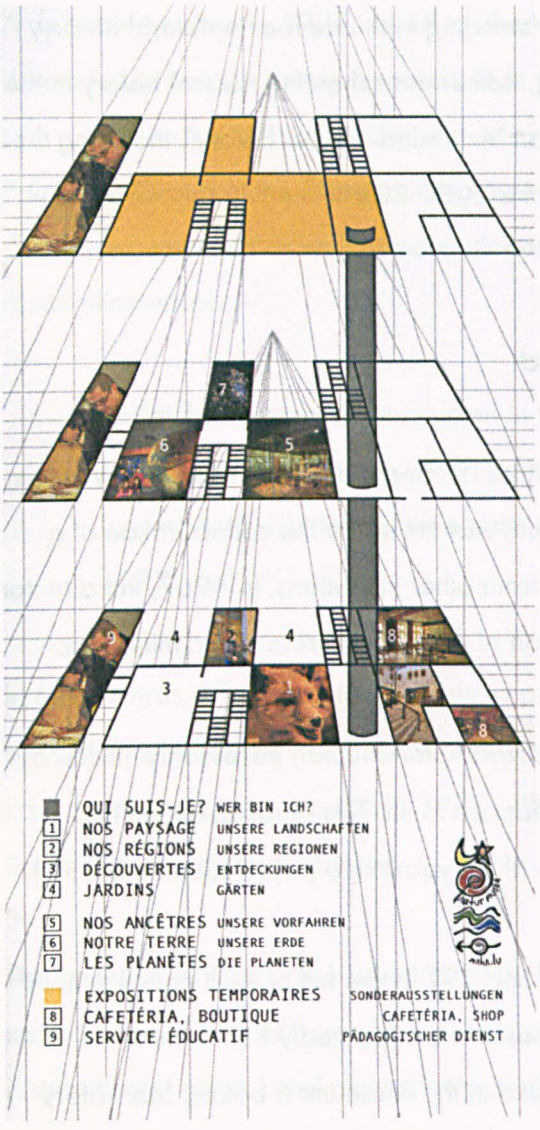
Pictures 3-9: Inside the Museum

[23] This quote as well as the others in this section are taken from the museum's website www.mnhn.lu [last checked 8/7/2005].









Plan of the Museum

With the move into a building on its own, the modes of display in the Museum have changed. As in many science museums elsewhere, glass cases have left the way to more interactive and less object-dense exhibits (see Macdonald 2003). Also, the nature of the exhibits has slightly changed. Although the Museum still exhibits the natural world, human artefacts are also put on display. The place humans occupy in relation to nature is developed throughout the Museum's permanent exhibits: in the Museum's 'our regions' room, for example, products such as butter and wine are displayed and their relationship with the soil is highlighted. Temporary exhibitions often display human makings: there have been exhibitions about perfume, about gold, about time, etc. More so, the Museum sometimes displays artworks, crossing the boundary between science and art every now

and then, or as the director once commented: 'standing with one foot in natural history, standing with the other in the art domain' (GB). Rather than showing natural history in the strict sense of the word, the Museum displays an 'extended natural history', including the creations both of people and of nature (Pickstone 2000:20).

4.2. The Museum's Centre of Scientific Research

Let us turn now to the Museum's research activities. To begin our short historical synopsis, let's start at a time when research at the Museum was limited to the collaboration of a few high school professors partially detached from other institutions. In 1967, the director of the Museum bemoaned that the collaboration of these professors was diminishing:

> Every research, apart from that of the conservator-director, necessarily limited by his administrative functions, is almost reduced to nil. This situation draws the Museum back to its scientific efficiency of 30 years ago [...] (Heuertz u.d.:197).

The Museum's activity reports from the 1960s and 1970s are full of such complaints. A lack of personnel and space for research purposes was repeatedly bemoaned. Nonetheless, at that time two laboratories existed in the Museum: a botany laboratory (the scientific support of the Service of the Vegetation Group Map created in 1949) and a mineralogy laboratory, which was assembled from 1962 onwards. In 1966, the first electronic microscope of the country was installed in the mineralogy laboratory, turning it into a kind of proto-laboratory in Luxembourg. In this laboratory research projects were carried out in collaboration with the steel industry. Very utilitarian research was conducted, for example on industrial dust in relation to different kinds of material. That it was in a natural history museum where these research activities took place says something about the research landscape at that time. As seen in the previous chapter, during the 1960s and 1970s, there were only very few institutions in the country where scientific research took place. The Museum was presumably the only place the industry could turn to in order to carry out collaborative research projects. But most of these activities didn't have anything to do with natural history.

A document written in 1977 aimed to change the lack of funding, efforts and attention dedicated to natural history research. That year, the Association of Luxembourg Biologists sent a letter to the Minister of Cultural Affairs pleading for the creation of a Research and Education Group at the Museum. Two main goals were put forward in this letter: developing research as well as educational activities. Concerning research the association wrote:

> [...] at this moment of time, when in the neighbouring countries faunistic, floristic and biocenotic research is carried out by specialised teams working full-time, Luxembourg risks to remain a terra incognita amongst European countries (Stomp 1977:upd).

In other words, efforts had to be made to put Luxembourg on the map of European scientific research. The association's request was successful and the 'Research and Education' group was created in 1977 - the same year the Luxembourg Council for Scientific Research was created.

The research mission of the group foresaw faunistic, floristic and biocenotic studies of the natural environment of Luxembourg to be protected, as well as studies about the distribution of animal and plant species, with a view to collaborating in international cartography efforts (Stomp 1977). Furthermore, the group strove to stimulate and coordinate the publication of scientific work executed in the Museum. While initially four detached biology professors carried out research work within this group, increasingly more people came to collaborate with - and work in - the Museum over the following years.

In 1981, then, the 'Research and Education' group was split into a Centre of Scientific Research and a *Service Educatif* (Education Service).[24] The following year a grand-ducal *règlement* created the legal basis for the Centre of Scientific Research. Seven scientific sections now officially existed within this centre: anthropology/human biology, botany, ecology, geology/mineralogy, geophysics/astrophysics, palaeontology and zoology.

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[24] On the *Service Educatif* see Bley (2004) and Hoffmann (2004).

The *règlement* further normalised detachments of high school professors to the centre, stressed that funding was assured by the department of cultural affairs of the ministry in charge, and created the status of 'scientific collaborator' of the Museum. In parallel, the Museum launched its own publications series entitled *Travaux Scientifiques du Musée National d'Histoire Naturelle* (Scientific Works of the National Museum of Natural History).

A couple of years later, in 1984, a laboratory of geodynamics was attached to the Museum. This laboratory, installed in a former gyps mine at around 10 kilometres away from the Museum, conducts research in gravimetry, seismology and geodynamics. It is the scientific basis of the European Centre for Geodynamics and Seismology, created in 1988, which organises the Luxembourgish Geodynamic Days (Stomp 2002, Ruymbeke 2002). Apart from this latter laboratory located outside of the Museum, research infrastructures were regrouped in a single building (see picture 10).

Picture 10: The Museum's Centre of Scientific Research

During the last two decades, increasingly more personnel, technical equipment and budgets flowed into this building. Laboratories dedicated to botany, ecology, palaeontology, geology/mineralogy, anthropology/human biology and zoology were set up. The most recent laboratory, dedicated to molecular biology and genetics, was created in the year 2000. This latter laboratory is attached to the Service of Population Biology and Databases, created the same year.

Today, 16 of the Museum's staff members are involved in research activities and the Museum spends nearly a third of its budget for research.[25] Over time, the aforementioned laboratories also became research facilities for doctoral students and post-doctoral researchers. Since the launch of governmental research grants in 1987, a dozen persons have been involved in research projects in the Museum benefiting from these financial aids.

4.3. 'Walling' and centralising the Museum

So far, I have portrayed the creation of the Natural History Museum and its research centre in a rather straightforward way. I have discussed how the Museum as an institution came materially, legally and structurally into being. In other words, I portrayed how the cabinet of natural history became a 'museum with walls' through the creation and delimitation of an official and architectural space for collections of natural history. Within this space, the Centre of Scientific Research was built in a similar way: through the making of material, legal and structural walls.

Throughout this 'walling', a building that used to be a prison for women had to be 're-disciplined' to house a natural history museum. In other words, a space formerly worked by discipline had to allow more interactivity. Whereas before, well-disciplined bodies, persistent codes, and injunctions like 'You must!' were common, room had to be made for undisciplined bodies and guidance, and injunctions like 'You may!' (see Barry 2001).

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[25] Whereas in 1981 the budget for research was around 20.000 euros, in 2002 the amount was around 450.000 euros.

Throughout this 're-disciplining', however, the building's walls still retained their protective function: while the prison walls used to protect the rest of Luxembourg from inmates, today's 'inmates' are protected from the natural and social threats of the outside. Still today, walls and doors turn the Museum into an enclosed and protected space.

'Heterogeneous design' (Law 1986, Gieryn 2002) was necessary to build the Museum. Physical exigencies, regulations of listed building, museum architecture requirements, state funding, attractiveness for visitors and much more had to be taken into account. Only after successfully meeting all these requirements, the walls (and doors) of the Museum came to belong to the Museum. And, only after these transformations, inscriptions emerged on these walls and doors such as the logo of the Museum, the name of the Museum, or the name of staff members. And, finally, only after all these efforts, the name of the Museum could be written beneath pictures of a building: the correspondence between materiality - a building with walls, doors, windows, a roof, etc. - and an organization - an institution with a name, staff members, a legal basis, etc. - was finally made. Yet, as we will see in the next part, the consolidated physical location of the Museum that shelters collections at the same time makes these inaccessible by anyone unable to visit (see Hedstrom and King 2003:29).

Before the Museum opened its doors to the public in December 1996, its legal space had already been stabilised. As noted previously, through a law passed in 1988, the Museum of Natural History had officially emerged. The material and social walling of the Museum almost happened hand in hand. In other words, turning a former prison into an 'exhibitionary complex', where objects and bodies now evolved in a more open and public arena (Bennett 1995:60-1), happened through the re-definition of the materiality, the legality and the discursive frame of that space.

Yet, apart from these material and legal elements, another element had to be mobilised too: people who worked in and for the Museum. A long term collaborator of the Museum recounts that in the 1960s and 1970s the Museum 'was a little bit more than a one man organization' (LR). Until the early 1980s, the Museum didn't employ many people: only one conservator, some part-time professors who were detached for research and educational activities, and a couple of technical assistants. Efforts were thus deployed to

increase the number of staff members of the – at that time – natural history section of the Museum of State. In addition, the first Museum director-to-be used another strategy to 'create a critical mass of people around the Museum' in order 'to just get started' (NS2). The creation of the status of 'scientific collaborator' didn't follow a scientific purpose only, but was quite a strategic move too. The director aimed to 'attract as many as possible to the Museum' because 'this represents more'. He argued:

> If you regroup many people around you, then you actually become, with time, the centre of reference.

The current Museum director reiterated this vision when he explained the future ambitions of the Museum:

> We want to [...] remain, in the future, one of those institutions in the country, that when it comes down to the natural heritage, there is in fact no way leading around [us]. [...] We want to stay the place, the centre of reference for the natural heritage (GB).

In other words, the Museum wants to remain an obligatory passage point (Latour 1987). It wishes to continue to be a place of privilege, a centre of accumulation (Law 1999b) of knowledge about nature in Luxembourg – the centre of the network. To quote the Museum director again:

> And from that side, we are of course interested to bind all those who are directly occupied by the natural heritage [...] to the [Museum] (GB).

The strategy to enrol people interested in natural history has succeeded, as today there are around 200 scientific collaborators at the Museum. Thus, the Museum enjoys a more authoritative position than in the beginning of the 1980s. Today the Museum 'represents more': a museum with walls, some 100 staff members, a legal basis, around 200 scientific collaborators, in short, a centralised, structured and informed space dedicated to natural history knowledge. Yet, this depiction of the Museum must be complemented

with some critical comments. The Museum's research activities, for instance, were assessed quite poorly by some interviewees:

> In the Museum, I would say, apart from very rare places, [...] there is no research going on (AN1).

> The denomination centre of scientific research [...] is a bit disproportionate to the existing human means (AN2).

> Nothing gets written, there is no structure, there are no programs, there is no *fil conducteur*, there is no line (AN3).

The Museum's research activities resemble to the situation of research in Luxembourg some 25 years ago: individualistic, uncoordinated, and not very productive. As I see it, the ministry of research is - unofficially - not very approving of the Museum's research activities. The lack of scientific publications and reports by the Museum staff, the missing relevance of the Museum's research for Luxembourg's economy, and the research centre's deficient mission statement and orientation seem to be the major contention points. Although the Museum has managed to create, over the years, a material, legal, financial, and staff-based basis for its scientific research, the production, coordination and diffusion of knowledge still need improvement, according to some staff members and scientific collaborators.

4.4. Decentralising the Museum

While so far I have been concerned with the creation of a 'museum with walls', in what follows I want to examine the ways in which some of these walls can be surpassed. To start with, we might replace the common image of a museum as an immobile and solid building, by that of a complex of mobilities, a nexus of in and out conduits (Lefebvre 1991:93). In the light of this image, the museum building already appears as permeated by streams of energy which run in and out of it: water, gas, electricity, telephone lines, and so on (Lefebvre 1991:93). At another level, funding and knowledge also run

through it, as well as objects and people, both of which are crucially important to transform a building into a museum.

Yet, the Museum of natural history does not only consist of a single building with walls, permeated by material and social flows. As the public relations officer of the Museum stressed, the Museum's philosophy is not limited to a central building only:

> You can let people come here. But that's perhaps too easy, when you get to the point. When, in fact, the aim would be [...] to put the Museum building above everything, then decentralisation would do no good. But if you say we want [to promote] the interest for science [...] then you have to take care that science effectively comes to the people. [...] And this happens by means of buses, by means of science mobiles, clubs, and also one or the other circuit you make (PM).

The idea to make 'science come to the people' seems to have emerged at the end of the 1970s. As seen before, in 1977, a Research and Education group was created at the Museum with two main goals: developing research and educational activities. In the following years, many activities were set up with the aim to bring the Museum closer to the public: a rental service was created (including movies, mounted animals, minerals and fossils); itinerant exhibitions such as 'Nature in School' or 'The Last Whales' travelled around the country; visits to farms were organised; campaigns to raise the public's awareness of environmental issues were set up; and so on.

Increasingly from the end of the 1970s onwards, the Museum's older inwardly focused values - scholarly knowledge and the value of researching and learning from collections - were complemented

> by more outwardly oriented values that are leading to creative partnerships with audiences, sponsors, and other cultural agencies and providers (Hooper-Greenhill 2000b:180).

Today, many things and places outside the Museum's walls belong to the Museum as well. In other words, a 'museum without walls' emerged through what the former director

called 'a decentralisation and democratisation of culture' (NS2). Let's have a look now at the different means to take the Museum out of its walls in an active way: the Museum buses, the Science Festival, and the Arboretum and nature trails.

The Museum buses

On the Museum's web pages, under the section Museum Bus, we can read the following lines:

> **The problem**
>
> The Museum being located at the centre of the capital of Luxembourg-city, this requires an often long and painful journey for visitors. Many are those who never take the step of the first visit. More so, educational activities, especially in the field of sciences, call today upon specialized equipment or rare or unique collection pieces, which are missing in many regions. Thus the areas furthest away from the centre town are only very little touched by the educational programs offered by the National Museum of natural history.

According to this excerpt, two factors limit the potential outreach of the Museum. First, the distance that separates the Museum from its visitors sometimes requires 'long and painful' journeys. Second, some specimens or objects are relatively rare and mainly available in the capital of the country. Either objects or persons will have to travel a certain distance to increase the outreach of the Museum. The solution to this problem is given as follows:

> **The solution**
>
> A *muséobus*, moving cultural space, which travels through the rural areas. It stops in our cities and villages, on a public place, in front of the church, it spreads and, after a few minutes, it is at the service of the population, for one day, one week, according to the needs. Familiar and secure environment, motivating common visit, easy accessibility, treatment of subjects of local interest; the advantages seem innumerable. In short, culture within reach.

The Museum bus, called Museum Bus 2000, can travel to many places and stay there for a variable amount of time. In other words, it is a vehicle that adds to the spatial and temporal flexibility of the Museum. In fact, since its launch in 1994, the bus has travelled almost across the entire country and even sometimes across Luxembourg's borders.[26]

In addition to the added mobility of the Museum, the bus enables flexibility concerning themes and forms of presentation. The appearance of the bus can vary. It can be set up as an animation room, a mobile laboratory, an exhibition room or a conference room. When it is converted into a mobile laboratory for example, microscopes and magnifying glasses prevail in the bus - even though this 'laboratory' primarily communicates existing knowledge rather than producing novel knowledge.

The themes proposed in the bus vary from mineralogy to zoology to botany and can be tailored onto the age, interests and scholarly curriculum of children. And, as the activities in the bus are usually combined with a tour in outdoor nature (except when it rains heavily), themes also vary according to the seasons. Outside its walls, the museum has to adapt to natural, individual and local contexts.

The Museum bus is very successful: it is fully booked year after year and lots of requests have to be denied. This success was one of the reasons to launch a second Museum bus, the Galileo Science Mobil, in 2002. The idea behind this bus is similar to the one behind the Museum's first bus. According to the person in charge:

> Galileo allows the Museum to offer scientific activities outside [it's] four walls. [...] We take the Galileo unit, we put it on a truck, and we drive around with it. So that we can offer these activities in a decentralised way, not centred in the Museum, but all around the country. [...] We try to bring the Museum to the people (AF2).

The stated advantages of this mobile space are:

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[26] Yet, as the person in charge of the museum bus explained, not all of Luxembourg's regions are covered: 'We just said that we wouldn't take Luxembourg-city, because they should come to the museum. [...] This was like an unwritten rule' (SR).

> decentralisation of scientific and technological culture, big radius of action, proximity to the public, facility of access, presentations on measure and variable according to target groups, flexibility in the choice of the themes (Galileo science mobil u.d.:upd).

Like its predecessor, the aim of this bus is to decentralise the Museum and bring nature and science closer to the public.[27]

There are, nonetheless, quite a few differences between both buses. First, on a very technical level, the first bus was a prototype and has problems of isolation today whereas the second one is more state-of-the-art - at least for the moment. Second, whereas the fist bus foremost educates about natural history, the second bus is rather concerned with science and technology. Third, the underpinning agenda is more explicit in the newer bus. As the person in charge explained, the bus aims for children to 'maybe loose a bit the fear vis-à-vis chemistry and physics' (AF2) through bringing them in contact with sciences in a more entertaining manner. In addition to being educated and entertained, children should even adopt a certain partisan attitude: 'That, maybe in the future, they should support solar energy' (AF2). The last major difference is the presence of sponsors. While the first bus has no sponsors at all, the second one was sponsored (by a car brand and a petroleum producer) for its exhibition on alternative energies (in the second bus exhibitions usually last two years: the first one was entitled 'Alternative and renewable energies' and the current one 'Trick-box nature').

Although both Museum buses aim to decentralise 'culture', two slightly different kinds of culture are decentralised. Whereas the first bus is object-centred, the second one is knowledge-centred. On the one side, through the Museum Bus 2000, natural history knowledge is transported - knowledge *per se* of the natural world, concerned with *what* things are. On the other side, through the Galileo Science Mobil, technoscience is transported - a kind of knowledge a bit more distant from nature, concerned with *how* things work, and a bit closer to politics and economics. In a sense, these two Museum

buses embody the evolution of science in Luxembourg. As seen in the previous chapter, Luxembourg science was formerly a rather descriptive endeavour, where natural history was relatively central. But over the years, a different kind of science emerged - more technological, utilitarian, economical, and applied. Roughly put, the two Museum buses exemplify these two different, co-existing currents.

The Science Festival

One year after the launch of the first Museum bus, the Museum organised its first science festival. The event, initially inspired by the Edinburgh Science Festival, took place in 1995, 1997, 2000, 2003 and 2005. The next festival is due to take place in 2007.

At the 2003 Science Festival, more than 60 activities were organised. During the 10 days of the festival there were: workshops to build batteries, to determine plants; conferences about sleep, about the relation between art and science; exhibitions about science and industry, as well as two very spectacular shows about chemistry and physics, etc. Altogether, these different activities aimed

> to favour the drawing together of sciences and society and to awake the interest and curiosity of the greater public (Musée National d'Histoire Naturelle 2003:34).

Whereas the first three festivals took place mostly in a single location, the 2003 edition happened in many places outside of the Museum too: the University of Luxembourg, other museums, a high school, etc. (see map x). The multi-sitedness of the festival created some difficulties. The coordinator of the festival acknowledged:

> Because it was so big, it simply wasn't possible for me to mark a presence everywhere and to look if it works or not (MK).

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[27] Nonetheless, both buses can't travel everywhere: they require macadam, a relatively even ground, and electricity to be able to function effectively.

She stated that it was difficult to gain 'an overview' of the event. The major problems related to the multitude of the sites were a lack of communication and insufficient signposting. As the Science Festival happened in so many places at once – and in most of them for the first time – it seemed difficult to invest all these places with a 'museum feel'. It proved difficult to tie all the different places together into a coherent event.

Apart from these organisational problems, what was new for the 2003 edition was its funding by the National Research Fund. For the coordinator of the festival,

> These are two things, you know, we want to achieve [and] they want to achieve on a political basis (MK).

The Museum was primarily concerned of getting people interested in science, of educating them, and showing practical utilisations of science in an interactive, hands-on and entertaining manner. But there were also political aims in that it was seen as a means for solving the problem of the lack of researchers in Luxembourg, by trying to attract more youngsters into scientific careers. In the call for proposals of the 2005 edition, the aims of the festival are stated as follows:

> [...] to present and support scientific culture and research in Luxembourg and to awaken the interest and the curiosity of young people and the general public to sciences and technologies.

Like in the Galileo Science Mobil, science and technology rather than natural history are the central focus point. During the Science Festival, the Museum not only materially decentralises scientific culture, by bringing scientists, objects, and practices closer to the public, the Museum also shifts its thematic focus on science and technology rather than natural history. During the Science Festival, the Museum's activities resemble more to that of science centres than to traditional natural history museums – the former of which usually strive to awaken curiosity and to inculcate a spirit of scientific inquiry in their visitors (see Hein 2000:26).

Nature trails, arboretum and other means

Besides the Museum buses and the Science Festival, the Museum manages various other places outside its walls: four geological trails, an astronomical trail and an arboretum. The Arboretum, a collection of trees and shrubs, is situated in the most European district of Luxembourg city (see Helminger 2004).[28] Apart from the Arboretum the Museum is in charge of two other living collections: a small garden in the Museum, where Luxembourg is represented in miniature with plants and soil from its four main regions, as well as a planned botanical garden. The geological trails cover various subjects: mining, mineralogy, geomorphology, etc.

Finally, to complete our tour of the Museum's means to surpass its own walls some more elements need to be mentioned: the annual Museum Night during which the Museum is open until 1 am instead of 6 pm and during which alcohol is served; the Museum's involvement in radio and television programs; its regular presence in the press; its publication of a journal and books; its website; its support of local and regional museums. These elements too add to the making of a 'museum without walls'. As one consequence, people other than the Museum staff sometimes come to collaborate with the Museum. For example, the Museum sometimes works together with researchers from other institutions, teachers, local *syndicats d'initiatives*, people from the Ministry of Research, and so on. In what follows I want to focus on one particular group of such collaborators, namely the scientific collaborators of the Museum. At the same time, we leave the material vehicles underpinning the construction of a museum without walls, to focus on people.

4.5. On the periphery: the scientific collaborators of the Museum

Centralisation and decentralisation are both at stake concerning the Museum's relation with its scientific collaborators.

[28] The panels next to the plants include names in four different languages (Luxembourgish, German, French and English) whereas in the museum only German and French is used on the panels.

Inscriptions

As noted before, the creation of the status of scientific collaborator followed the logics of drawing things together: to amass people around the Museum in order to 'represent more' and to develop scientific activities at the Museum at the same time. The status of scientific collaborator emerged in parallel to the official creation of the Museum's Centre of Scientific Research in 1982. In the grand-ducal *règlement* that defined its legal basis, it is stated that scientific collaborators can be closely associated to the scientific research programmes carried out by the centre and that they work on a voluntary basis. Their number has increased ever since: from 41 in 1983 to 139 in 1991 to 182 in 2004.

Every person who wants to become a scientific collaborator goes through a standard procedure. All applicants have to hand in a written request, which they sign to agree to four principles. They engage themselves to: inform those in charge in the Museum of the results of their research; submit their findings for scientific examination; reserve to the Museum the right of priority in the case of an eventual selling of pieces of scientific or museal interest; and give the Museum a copy of their data for the database of the natural heritage.

The title of scientific collaborator is conferred, after proposal of a conservator, by the minister of cultural affairs and is limited to three years, although it is usually renewed after that. After a person is nominated scientific collaborator, his or her name is published in the official legal journal of Luxembourg, the *Mémorial*. There, two lists appear: one with newly added names and one with those collaborators whose mandate has been renewed. In these two lists the collaborators' names are written down in alphabetical order and next to each name their address appears along with the scientific section of the Museum they are affiliated to (see picture 11). Apart from these lists, their names also appear individually in a document every collaborator receives - collaborators are given a 'legitimisation card' that authorises them to use in the scope of their research roads and ways prohibited to the public (see next chapter).

Arrêté ministériel du 4 juillet 2003 renouvelant le mandat de «collaborateurs scientifiques» au Musée national d'histoire naturelle de certaines personnes.

La Ministre de la Culture, de l'Enseignement supérieur et de la Recherche

Vu l'article 6 du règlement grand-ducal du 10 novembre 1982 portant création de Centres de Recherche Scientifique auprès du Musée national d'histoire naturelle et auprès du Musée national d'histoire et d'art;

Sur proposition de Monsieur le Directeur du Musée national d'histoire naturelle;

Arrête:

Art. 1er. Le mandat de «collaborateur scientifique» au Musée national d'histoire naturelle est renouvelé pour les personnes suivantes:

Nom et prénom:	adresse :	section scientifique:
AENDEKERK Raymond	48 rue d'Olingen L-6832 BETZDORF	écologie
ANTUN Paul	Hoscheiderhof L-9458 BRANDENBOURG	géologie/minéralogie
ARENDT Alexandra	31 route de Wasserbillig L-6490 ECHTERNACH	anthropologie
BAGHLI Adil	27 rue l'Usine L-4490 BELVAUX	anthropologie
BARAL Hans-Otto	Blaihofstr. 42 D-72074 TÜBINGEN	botanique
BORNAIN Steve	13B rue de la Reconnais. Nationale L-4936 BASCHARAGE	géologie/minéralogie
BRAUNERT Carlo	31 route de Wasserbillig L-6490 ECHTERNACH	zoologie
CARRIERES Evelyne	27 rue de Lentilles F-54800 SPONVILLE	zoologie
CEPEDA FUENTES José Manuel	7 rue Léandre Lacroix L-1913 LUXEMBOURG	botanique
CLEMENT Raymond	1 rue du Cimetière L-6161 BOURGLINSTER	écologie
CONRARDY Guy	40 rue de la Barrière L-3321 BERCHEM	anthropologie
CONZEMIUS Tom	Kilschtewee 38 L-6113 JUNGLINSTER	anthropologie
CUNGS Jos	47 rue des Genêts L-3482 DUDELANGE	zoologie
DAWSON Matt	66 rue Boch L-1244 LUXEMBOURG	géophysique/astrophysique
DEBBAUT Vincent	15 rue des Carrefours L-8015 STRASSEN	géologie/minéralogie
DELSATE-AKMAN Dominique	5 rue du Quartier B-6792 BATTINCOURT/HALANZY	paléontologie
DIEDERICH Jules	91 cité Paerchen L-3870 SCHIFFLANGE	anthropologie
DIEDERICH-SARDANA Paul	22 rue de la Solidarité L-8020 STRASSEN	botanique
DOHET Alain	14 rue de Lorce B-4920 HARLE	zoologie

Picture 11: Part of the list of the collaborators from the *Mémorial*

The above techniques of writing the collaborators into documents are standardised and centralised. On a technical level, then, 'the scientific collaborator' is a simple and coherent construct, which exists in a legal text, owns a legitimisation card, and is published in alphabetical lists. This is achieved through processes of 'inscription' – the production of durable and mobile traces which can be transported and accumulated and compared at a centre (Latour 1987). By writing the names of the collaborators on a surface, by coding them into diverse documents, the Museum creates a textual space of similarities, of commonalities.

Yet, the very legal basis of the collaborators is very different from that of the Museum staff. Many differences appear when we compare the few lines mentioning the collaborators in the *règlement* from 1982 to the law about the conditions of admission and nomination of the employees of the Museum. The *règlement* extends on two pages in the official legal journal of Luxembourg and contains three articles out of 10 about the

collaborators, whereas the law about Museum staff extends to six pages and contains 26 articles. Concerning recruitment, the collaborators proposed by a conservator are generally nominated by the minister; the personnel, however, has to conform to conditions of admission, to modalities of trial periods and to exams. Being a collaborator is a mandate, whereas being among the personnel of the Museum means having one of eight different careers. Quantitatively and qualitatively collaborators are inscribed very differently into the Museum's legal basis than are Museum staff. They are not bound to strict, explicit and formally validated recruitment criteria. Their role, their identity and their connections to the Museum are less scripted than that of staff members.

Discourse

The collaborators' enrolment depends on rather variable, implicit and rather vaguely defined criteria. According to one staff member, they only have to respect some minimal requirements:

> He must at least collect something and transmit any piece of data. That is, I would say, the minimal requirement (GC).

However, some collaborators are inactive and/or don't even collect or transmit data to the Museum. Some of them have been active when they became collaborators but have become, over time, rather inactive. Others don't seem to even fulfil these minimal requirements, which seem rather to be 'minimal expectations'.

Another staff member explained that:

> it is precisely checked if the thing they do is pro-Museum, that their work also brings something to the Museum in some way or the other (MM).

He further argued:

> The scientific collaborator does not need an *Unbedenklichkeitsbescheinigung* [document certifying that one has no taxes, loans, etc. outstanding] from the

> secret service. But he must only be able to behave normally and decently in his civic life [laughs] that he doesn't stand out negatively (MM).

Not only should collaborators' activities be in the Museum's interest, collaborators have to be socially apt as well. In other words, they have to conform to scientific and moral requirements. These moral requirements were most clearly expressed when a staff member put forward the reasons for not accepting one particular person as a collaborator:

> Well, in his approach to people he [...] sometimes is, let's say, a bit too hassling in my view. [...] With him, it is the form that counts. [... A collaborator] must also be able to deal with people (AF).

The above-mentioned expectations are not written down anywhere. As the responses reveal, staff members often articulated different requirements, which puts to question the existence of any commonly agreed criteria. Conservators, who take the decision to accept someone as a collaborator or not, seem to have their own criteria. Yet, a commonality that runs throughout staff members' responses was that collaborators were all equally important to the Museum, no matter their background or interests:

> If it is a person who understands a lot about it, then it doesn't matter if he is a biologist or whatsoever [...] (EE).

> Concerning their importance, the Museum actually makes no difference. I think somebody who prepares pieces for the Museum is actually as important to the Museum as somebody who makes a classification (AF).

> A collaborator who [...] has not been to university can as well find his place at ours than somebody who has been to university (AF).

Whatever their education and scientific interests are, all collaborators seem equally important to the Museum.

But centralising the collaborators, their data, and their interests within the Museum is difficult for many reasons. First, their nationality and country of residence are diverse. Besides a great majority of Luxembourgish nationals, there are also non-natives, mainly French, Belgian and German nationals. Most live in Luxembourg, whereas the others live in the neighbouring countries.[29] Second, the professional activity of the collaborators varies. Amongst them there are persons affiliated to other scientific institutions, teachers, students, retired persons, people who work in domains which have nothing to do with science (bankers, engine drivers...), etc. Third, they do not necessarily collaborate with the Museum to the same degree. As the former Museum director explained, there are some *Pabeierläichen* (literal translation: paper corpses), people who have collaborated to only one project and who exist, for the Museum, only on paper. At the other extreme, there are some very active, productive and internationally renowned collaborators. These three differences - country of residence/nationality, professional activity, and degree of activity - turn the collaborators into a very heterogeneous group. Compared to the Museum staff members, all of whom actively work in the Museum for a fixed and regulated amount of time, the collaborators are dispersed across geography, across professional activities and across variable intensities of work.

Moreover, the collaborators' research subjects are diverse, ranging from mushrooms to mammals to fossils. The governmental counsellor for research at the Ministry of Culture, Higher Education and Research had the following view on this situation:

> The scientific collaborators [...] work in very heterogeneous fields. There is a dispersion on very many domains, too many perhaps, in my eyes. And it would have been useful to the whole thing if one could concentrate on a few domains. [...] One person works on mosses, another on frogs, a third one works on, I don't know, on a specific bird species, and a fourth one on a wood disease. Everything, every research project has its own importance. But it doesn't attain an impact because there is no work in *Verbundenschaft* [cooperation]. Or then, why

> not tackle works in a multidisciplinary way, [...] work together, towards a common goal? It seems to me [...] that it is relatively mosaic-like, out of separate pieces. And it is difficult to bring those pieces together (PD2).

The alleged difficulty to bring these 'mosaic-like' pieces together reveals that the different scientific interests of the collaborators don't seem to add up. To rebalance this diversity, the interviewee articulated different means: 'concentrating', '*Verbundenschaft*', working towards 'a common goal', in other words, centring and connecting the collaborators' practices. Related to this, it seems difficult to prioritise research projects and to take one as being representative since each project seems to have its own importance. For instance, one conservator wrote in the book redrawing the 150 years of the Museum about some of the collaborators' works stressing, a few lines later, that

> the works cited above are only very telling examples. The same goes for the numerous other helping hands one cannot enumerate [...] (Faber 2004:130).[30]

But the term 'example' is problematic, since it entails notions of commonality, of some shared standards. An example of a collaborator would be an individual which is representative for the whole group. But what if the collaborators do not form a group? What if it is a 'community without unity' (Corlett 1989)?

As one staff member put it: 'it is not a group, it is [...] a list of people' (MM). He further argued:

> [The scientific collaborators] are something like a wild bunch. So it isn't a group, where one would say that it stands together and heads towards the same goal. Goals are individual. And we then have to look to what extent we can bring that in resonance with what we really want.

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[29] Only two of the 182 collaborators don't live in the neighbouring countries: one collaborator lives in the Netherlands, another in Poland.

[30] Paradoxically, in the same book there is a list of all the scientific collaborators of the Museum.

Bringing individualistic goals in resonance with institutional goals is an ambition that runs through the development of natural history. Drouhin and Bensuade-Vincent (1996:419) argue that in the early 19th century,

> the cultivators of natural history formed an undisciplined crowd which the professionals would like to keep under their control.

Similarly, the Museum staff members often articulated the difficulty of disciplining the collaborators. In other words, staff members struggle to centralise their decentralised geographies, interests, and practices. The collaborators' situatedness at the periphery of the Museum seems to render integration troublesome.

Classification

Just as staff members experience difficulties in disciplining collaborators, I found it difficult myself to 'keep the collaborators under control'. Collaborators are heterogeneous in many ways: concerning their citizenship, their country of residence, their professional activities, their research interests, their degrees of activity, and so forth. But is there any single 'natural history' of the collaborators to favour? Or are these classifications and taxonomies equally important, and all to be taken into consideration?

I often betrayed the complexity of the collaborators, when I used some 'examples' of collaborators while explaining my project to others. Guy the engine driver who is knowledgeable in mushrooms, Matt the bank employee who is interested in astronomy and collaborates with NASA - these are the examples I used over and over again when I described the subject of my PhD to academics, friends or family. But to take these examples as representative of all the collaborators is problematic. More so, it is difficult to attribute any singular characteristic – other than the purely technical, inscriptional ones noted above – to the whole group of collaborators. In a similar vein, staff members described the collaborators as follows: 'it is a multicoloured heap' (AW), 'as there are all kinds [of collaborators], in fact, it is thus difficult to put all of them into the same box' (CW). It is difficult to order, classify and prioritise collaborators. Especially if we

generalise the principal of symmetry (Callon 1986) to our case, not one category is central but it is this multiplicity of categories which has to be taken into account.

We can, however, let the actors speak (and classify). One staff member ordered the collaborators as follows:

> The Museum has always different levels in research, I would say. One question is: What do we have? And if we put the collaborators on this question [...] then there are surely some who deliver more data than others. That's one thing. And then there is the second category of those who write something and publish. That's already a much smaller group. And then there is a third category - how should I say? - Formula One. These are those who also [publish in] international [journals ...]. They are only a few (GC).

Does it make sense to classify the collaborators according to these three categories (data deliverers, writers, international writers)? On the one hand, there surely are differences in the productivity of the collaborators - in terms of quantity of delivered data and in terms of quality of publications. And it does make sense to classify the collaborators according to their 'contributory expertise' (Collins and Evans 2002) rather than classifying them according to academic degrees since Museum staff members themselves have very rarely used degrees when classifying them. But on the other hand, we could easily make four out of these three categories. Or, we could state that collaborators are distributed along a continuum ranging from inactivity to great activity. Whichever classification we chose a difficulty remains: the difficulty to 'lock' collaborators into clear, distinct and unproblematic positions. As much as it is difficult to write sociologically about the collaborators without betraying their complexity, it is difficult for the Museum to centralise and standardise their data, as we will see in the following chapters.

4.6. Centring and decentring the Museum

At least two stories run throughout the evolution of the Museum. One is about efforts of drawing things together, about concentrating artefacts, knowledge and people in a

single place; the other is about efforts of decentralising the Museum, of taking knowledge and objects of natural history pass the Museum walls.

The first story is about the space of the Natural History Museum that gradually took shape to become a 'mature' space. Throughout time, the Museum became a 'museum with walls' through the creation and delimitation of an official and architectural space. In this space natural history collections were assembled, mostly through gifts when the Museum was managed by a savant society, and then more and more through acquisitions after the Museum became state owned. Besides gathering material artefacts, the Museum is a place where data about natural history is centralised, and where members of the public are supposed to come to if they want to learn something about the natural history of Luxembourg. In this sense, the Museum aims to be an obligatory point of passage by taking care that certain things flow to and from the Museum - objects, funding, people, and knowledge. Differences are created, since an obligatory passage point is a unique place, with a distinct social order - materially, legally and structurally.

The second story that runs throughout the evolution of the Museum is about decentralisation. In fact, the Museum is 'decentereing the object' (see Law 2002) in at least three ways. It takes, first, objects out of a central location, the building of the Museum, and transports them to other places. The two Museum buses nicely illustrate this movement. Second, at the same time, the Museum often shifts its focus from natural history to science and technology. Third, understanding the objects *per se* is often not the main goal, but it is through those objects that messages are to be conveyed - messages to awake the interest of youngsters for science or to make people respectful towards the environment. The aim is not only to teach by means of object lessons but also to reveal the meanings and values attached to objects, thus turning 'natural objects' into 'working objects' (Dias 1997:44), that is, objects that are not raw nature but the materials from which concepts are formed and stories told.

By decentering the object, many objects and places outside the walls of the Museum have been invested with its scientific discourse. A 'museum without walls' is created through the surpassing of physical walls via buses, nature trails, the Science Festival, etc. At the same time, the socio-cultural walls also become permeable through the

involvement of different actors into the making of common things. The Science Festival, for example, is infused with a clear political agenda of improving the research culture in Luxembourg.

It should not be overlooked that the construction of decentralised museum spaces is preceded by the creation of discursive spaces. Some reminders. To support the launch of research and educational activities at the end of the 1970s, Luxembourg was portrayed as 'a terra incognita amongst European countries'. To explain the background of the creation of the first Museum bus, people that never come to the Museum are mentioned because of long distances and because of objects that are quite rare outside the capital. The Science Festival is seen to bridge the gap between society and science. The politics of decentralisation are, in other words, co-existent with the construction of rhetorical absences. To create a museum without walls, a reflexive process emphasizes quite strongly its current walls. Doing so, arguments are mobilised that set out the limits of the Museum and put some things clearly beyond those limits. Nonetheless, connecting these 'outside' things to the Museum can't be achieved without material vehicles at hand. So many things travel in and out of the Museum that drawing a list seems almost futile. To name but a few: cars, buses, specimens, books, and panels.

The Museum's research activities are characterised by similar movements as described so far. On the one hand, efforts have been deployed to turn the Museum into a node, to centralise scientific data, knowledge and persons related to natural history. The many scientific collaborators attached to the Museum are an important element in this endeavour. By writing the collaborators into documents, the Museum creates a centralised textual space of similarities and commonalities. Bringing the pieces together, assuring that the 'wild bunch' of these collaborators delivers data, specimens and texts 'in resonance' with the Museum is, then, striven for. There is a logic of exchange: the collaborators are nominated and they receive logistic and scientific support; and, in return, they provide the Museum with specimens and data. Yet, creating a centre, a homogeneous and disciplined place where the collaborators would have (at least some) things in common is a difficult task since their research interests and their socio-cultural backgrounds are heterogeneous. As we will see in the next chapters, since collaborators are situated on the periphery of the Museum, disciplining them, aligning their practices

and interests and standardising their data is not easy. As we move from the centre to the periphery, attachment to the central value system becomes attenuated (Shils 1975:10).

Chapter 5:

Boundary encounters

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A museum of natural history creates and manages places where different social worlds meet. In an obvious sense, it is a place where scientists and visitors meet – though rarely in a direct encounter. Generally speaking, a museum of natural history provides a common stage, a place of encounter, for science and society. In this chapter I want to look at one particular kind of encounter, that between the staff members and the scientific collaborators of the Museum. In doing so, I explore how science is made and how boundaries are articulated at these 'boundary encounters' (Wenger 1998:112).

Why is it important to look at the places in which people meet to produce scientific knowledge? This is so because science is marked by the local and spatial circumstances of its making; scientific knowledge is made by mundane and locally varying modes of social and cultural interaction (Shapin 1998:6). These are the main tenets since the 'localist' turn of science studies (ibid., see also Watson-Verran and Trunbull 1995). In order to look at how science is made we have, then, to look at where science is made. Where do people make science? Where do they come together when they produce scientific knowledge?

In this chapter I will consider four encounters between the Museum staff and the collaborators. First, I analyse a card which both possess in order to do fieldwork. Then, I will examine two events: an annual meeting of the collaborators and a biodiversity weekend. After this, I analyse the Museum's journal, a textual medium where both collaborators and staff members can publish their findings. In the last part of this chapter, I explore the commonalities across these four meeting places.

In the previous chapter we have seen that there are many differences among collaborators concerning their country of residence, their nationality, their professional activity, their degree of activity, their scientific interest, etc. This dispersion across geography, socio-cultural background and practice makes the taming of the 'wild bunch' complicated. But to co-produce knowledge people have to work in *Verbundenschaft* (alliance) – heterogeneity has to be managed. To do so, spaces where people come together have to be created and managed. At the same time, these encounters have to be in places where people can communicate through common languages. As we will see, as many as four different languages are used at the Museum's encounters: French,

German, English and Luxembourgish. How is this linguistic heterogeneity managed? How does language come into play in the production of knowledge and the crossing or maintenance of boundaries?

Apart from making and communicating knowledge in different languages these encounters are also places where things other than science are exchanged. I will look at how both the exchange of scientific knowledge and of more symbolic things are important in shaping the encounters and negotiating the relationships between the Museum and the collaborators.

5.1. The legitimisation card

To do fieldwork collaborators and Museum staff members possess a legitimisation card (see pictures 12a and 12b). This card, green in colour, is about the size of a UK driving licence. It is made of thick paper, the kind of paper used for official certificates. The following is written on this card:

> The Prime Minister, Minister of State, the Ministry of Culture, the Ministry of Transports, the Ministry of Environment, the Ministry of Agriculture, Viticulture and Waters and Forests, the Ministry of Interior authorize hereby the signed holder to take, at his or her risks and perils, in the scope of his or her ecological, botanical, zoological, paleontological and geological research, roads and ways prohibited for public circulation.
>
> Every person and holder of public authority is invited not to constrain the action of the holder of the present card, acting in the interest of scientific research of the National museum of natural history; in particular to permit him or her access to ditches, careers, excavations, forests, meadows and shores.


Durée de validité:

Prolongation:

Prolongation:

Prolongation:

Observation:

1510015

Grand-Duché de Luxembourg

Ministère de la Culture

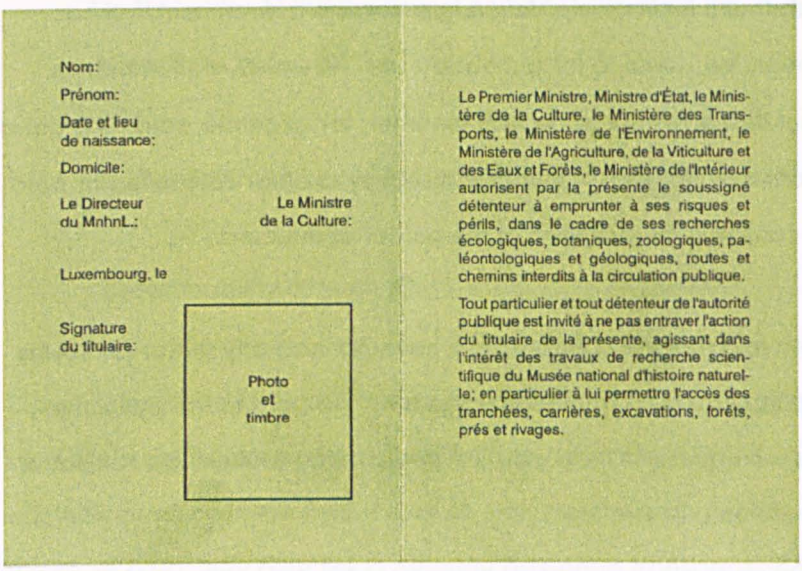
CARTE DE LÉGITIMATION

N°

Collaborateur scientifique
(régl. grand-ducal du 10 novembre 1982)

Personnel scientifique du
Musée national d'histoire naturelle

Picture 12a: The Carte de Légitimation


Nom:

Prénom:

Date et lieu
de naissance:

Domicile:

Le Directeur
du MnhnL.:

Le Ministre
de la Culture:

Luxembourg, le

Signature
du titulaire:

Photo
et
timbre

Le Premier Ministre, Ministre d'État, le Ministère de la Culture, le Ministère des Transports, le Ministère de l'Environnement, le Ministère de l'Agriculture, de la Viticulture et des Eaux et Forêts, le Ministère de l'Intérieur autorisent par la présente le soussigné détenteur à emprunter à ses risques et périls, dans le cadre de ses recherches écologiques, botaniques, zoologiques, paléontologiques et géologiques, routes et chemins interdits à la circulation publique.

Tout particulier et tout détenteur de l'autorité publique est invité à ne pas entraver l'action du titulaire de la présente, agissant dans l'intérêt des travaux de recherche scientifique du Musée national d'histoire naturelle; en particulier à lui permettre l'accès des tranchées, carrières, excavations, forêts, prés et rivages.

Picture 12b: The Carte de Légitimation (inside)

This card visualises and materialises an actor-network. It links four actors: ministries, the holder of the card (either the collaborators or the Museum staff members), holders of public authority (policemen or forest wardens for example), and the Museum. It defines a space: prohibited space. And it describes an activity: natural history research. What the card does, then, is to relate these actors in precise ways. First, ministries *authorise* holders of the card to use prohibited space. Second, holders of public authority are *invited not to constrain* the activities of the holders of the card and to *permit* them access to different places. Third, access is authorised when the holder of the card *acts in the scope* of his or

her research, which is also *in the interest* of the Museum. In short, ministries authorise, holders of public authority are invited not to constrain and to permit, when holders of the card are acting in the scope and in the interest of others to permit this one thing: the use of space prohibited to the public.

We see that space is neither a person nor a thing, that it is neither subject nor object, but a set of relations between these (Lefebvre 1991:82-3, 92, 116). Space 'commands bodies, prescribing or proscribing gestures, routes and distances to be covered' (Lefebvre 1991:143). This card *produces* a space for collaborators and staff members. To use ANT vocabulary, the space produced by this card is relational and it delegates, translates, and mediates actions between the collaborators or the Museum staff, the Museum, the government and the sometimes grumpy holders of public authority. The text on this card further reveals that it is an official *laissez-passer*: it is a document that has legal authority and that might be useful to let somebody see. However, staff members argued that this card is not that powerful. One staff member, for example, said: 'it is not a call to order [...] People can say no' (AF) – this, because they are not *compelled* or *held to* but only *invited* not to constrain the activities of the holder of the card.

In spite of this, I have been told that the collaborators have not had any major problems with access to restricted places so far. Usually after showing this card to the policemen, forest wardens or hunters who question them about their activities, matters are settled. In these cases the card is useful to:

> have an ID from the Museum [...] to say: look, we don't work only for ourselves here to collect fossils or to search for plants. We are here to gather information about the terrain and we are transmitting them to the Museum (AF).

One collaborator held:

> [An] advantage of being a scientific collaborator, from my view, is that one has this card with which one can identify oneself (PL).

In other words, this 'ID' permits the collaborators to make their linkage to the Museum clear and to do so in a tangible and legally endorsed manner. Like an ID card, the legitimisation card establishes the identity of its bearer for purposes of state administration and of gaining access to benefits distributed by the state (Torpey 1998:250). The importance the collaborators attribute to this card is revealed through it being frequently mentioned when I asked them in interviews or in the questionnaire about the benefits of being a scientific collaborator. In the questionnaire they wrote things like:

> biggest advantage for me is the *carte de légitimation* which permits and/or facilitates me the possibilities of approach for my field studies in ornithology, which render a meaningful gain of time possible
>
> to be holder of the legitimisation card to explain the 'funny' actions one is sometimes doing on the field
>
> authorisation to accede to prohibited places for the public
>
> better mobility through ID

Given its ability to authorise access to restricted areas, the legitimisation card can increase mobility, save up time and help to explicate 'funny activities', in short, it can ease scientific practice in the field. Museum staff members and collaborators possess exactly the same card; they only tick a different box on its front-page. Both can thus access exactly the same space, which means that there is no divide between staff members and collaborators, but between those who hold such a card and those who do not. In terms of boundary-work, the legitimisation card does two things at the same time. On the one hand, it does not introduce a boundary between collaborators and staff members by allowing both the same legal access to natural space. On the other, as both tick a different box on its front-page, it maintains a boundary – in terms of name and identity – between both.

5. 2. Speaking, listening, thanking and eating – the annual meetings of the collaborators

In this section I focus on the annual meeting of the scientific collaborators. I will explore its history prior to focussing on the 2004 event.

Each year in March, the scientific collaborators and the staff members of the Museum get together for a one-day conference. The first meeting of this kind took place in 1983, one year after the creation of the Museum's Centre of Scientific Research. That same year, and for some years after, the meeting was organised in the Museum. Because the number of collaborators increased over time, the Museum became too small to accommodate all the participants and the conference had to move outside of the Museum. In more recent years the meetings have been organised in quite elegant locations: in a castle, in a spa and in a recently opened cultural institution.

Apart from becoming an increasingly bigger event in terms of number of participants, the conference has changed in other ways too. According to its current organiser, in early days there were many talks on what he called 'the flowers in the prairie' (PM). Talks were rather descriptive, a naturalist's account on species present in one particular region. But talks seem to have become increasingly analytic and scientific. In 2005 a scientific committee has been set up to select speakers, a procedure that wasn't common before. The conference has also changed in terms of more mundane things – the introduction of badges for the participants in 2004, the increased use of computers for the presentations – which point towards the same trend: a professionalisation of the conference. It has become a space which is well organised and which now resembles a professional international conference.

I attended two of these annual meetings: the one in 2004 and the one in 2005. I now examine the 2004 meeting in more detail, starting with the beginning of the conference.

Picture 13: The conference room

The 2004 conference started at 2 pm. Talks took place in a large room (see picture 13) and the setting was relatively formal. The Museum director gave the first speech. He started off by thanking the collaborators for the work they did over the last year. Then he talked about various subjects: the recent science festival, the new law about the reorganisation of the cultural institutes, the new law about the protection of nature and, finally, the University of Luxembourg. After the director's speech, the governmental counsellor for research matters came to the stage. He presented his apologies for the absent minister - who is usually present at the meetings - and thanked and congratulated the responsible from the Museum and all of the participants. Then he briefly presented the current state of national research: that monetary resources had augmented fourfold in recent years, that the new university will bring new impulses, and that the Museum's Science Festival is an important event to connect research to the man/woman on the street. Stressing that he talked as a spokesperson for the minister, he declared that the ministry would continue to be supportive in the future. He finished his talk by thanking the collaborators for their efforts and works and wished them success for the future.

Both the speech of the Museum director and that of the governmental counsellor were in some respects very similar. First of all, both were dressed very formally - in suit and tie - while most of the conference participants were not. Both also talked about national issues

related to science even if they performed a slightly different version of science. And, finally, both thanked the collaborators a couple of times during their speeches. The two talks, I would argue, set a certain frame: this was to be an event about science; it was an official, important and relatively formal event; and it was strongly related to Luxembourg.

After these introductory talks, the collaborators hit the stage. There were 11 talks: six were held in Luxembourgish, three in French and two in German. A lot of the talks dealt with palaeontology but all in all the subjects covered a very broad range: fossils, meteorites, trees, mussels, climate, etc. Collaborators presented what they had been doing over the past years.

The meeting is an institutionalised space where findings are communicated, where knowledge is exchanged. In general, conferences are one of the traditional channels – with journals – for scientific dissemination of knowledge (Lamb and Davidson 2005). They provide forums where findings become publicly available information (Garvey and Griffith 1972). Yet, although most speakers focussed on their findings, some used their time to make other statements too. One collaborator put forward the need to set up a centre to tag birds at the Museum. Another collaborator mentioned the need of a national observatory on biodiversity with a standardised protocol and made a call to send him data. Between two talks, the chair mentioned a forthcoming atlas on reptiles and asked people to report any relevant data to the Museum. The conference is thus not only a place where findings are communicated in a linear and unidirectional way, where an active speaker transmits a message to a passive audience (see Hooper-Greenhill 2000b, Keith 1997). Rather, the conference is marked by a two way communication process, a place where calls are issued, questions asked, information sought, ideas suggested, responses and criticisms given. The annual meeting is a place for conversation, for dialogue.

When presenting their findings, those collaborators who give talks perform 'spatial turn takings'. They use a specific space: they stand at a lectern, they talk through a microphone and they use a computer. And they do so one after the other. The order of the talks is visible on printed programs and the limited time slots are policed by the chair of the event. When their speech is over, the space is 'tidied up': the speaker leaves the

lectern, takes away his or her notes, and closes his or her PowerPoint presentation. These turn takings are common for any conference; they are a basic device when more than one speaker talks in front of an audience. Yet, the meetings of the collaborators reveal a kind of turn taking which seems quite uncommon for scientific conferences.

At academic conferences speakers are usually selected according to the content of their proposed talk. Yet, for the annual meeting of the collaborators, talks are selected according to the person who gives the talk rather than in relation to the content and topic. In fact, not everybody who submits an abstract is selected. The organiser of the conference explained:

> It is a bit like this, that there also should be an alternation [...] If there are too many demands, I think you also have to take those who haven't been there before (PM).

Those collaborators who gave a talk at a recent meeting are sometimes rejected in favour of those who haven't done so. Conversely, it is those collaborators who have not given a talk at a recent meeting who are privileged. The annual meeting thus reveals a 'temporal turn taking' (Lewis and Weigart 1990:81) between the collaborators. This temporal turn taking is something uncommon for communicating science. While a similar practice has been observed in communities of high energy physics, where 'taking turns' is one criterion for choosing speakers (Knorr-Cetina 1999:169), in most scientific communities this is not common practice.

Apart from this turn taking, the meeting is peculiar in other ways as well. It is an inherently heterogeneous forum. At the 2004 event three different languages were used (German, French and Luxembourgish), while at the 2005 event some of the speakers also talked in English. In fact, the collaborators are free to choose in which language to present their findings. According to the organiser of the conference:

> We leave the collaborators free choice on this matter. Most participants are Luxembourg nationals, the majority of the presentations are and were held in Luxembourgish. A French, English or German speaker will of course speak his

> own language (a feature which is increasing). There have been internal discussions in the Museum to hold the events in one language only, but this hasn't succeeded yet [...] Were it to be a congress where only professional researchers would present their results, it would probably have to be held in English. But this is not the case. During this meeting, people who do not speak other languages well enough should also have a voice (PM).

Apart from this linguistic diversity, the event was heterogeneous in three other ways. First, the professional background of the conference participants was diverse. For the organiser of the conference:

> The whole thing reflects a bit what the corpus of the collaborators finally is. It is extremely heterogeneous. There are people who work professionally, there are people who do this as a hobby, and there are also people amongst them who have no scientific education (PM).

Second, the subjects and the styles of the presentations were diverse. Collaborators covered a large range of topics related to natural history, and they used different styles to present their findings.

Third, comments made about the conference during the following days were varied. Some said that talks should be more popular and more accessible for a wide audience, whilst others said quite the opposite: that they should be more scientific. But most agreed that it was a 'healthy mixture'. One collaborator has put it like this:

> I find this initiative immensely valuable, that one has the possibility [...] at least once a year, for one afternoon, to listen to things that are so very far away as to the theme [...] It is fascinating to simply listen to this (AF4).

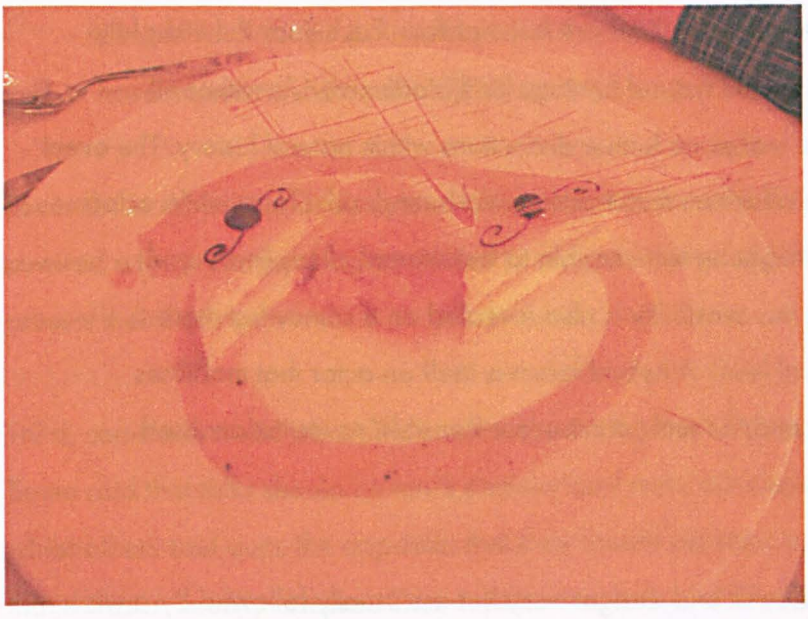
Another commented: 'It is not bad, I say. It has a good standing. Ok, it is not always a very academic level' (AB).

In sum, the conference is marked by heterogeneity in terms of languages used, participants' multiple backgrounds, interests and styles of presentations, and attitudes towards the event. As these are amalgamated at one single event, we can talk about them as 'hybrid forums'. For Callon and Rip (1992:147-153) 'hybrid forums' are characterised by the multiplicity and diversity of actors that interact; they are hybrid since the actors, the problems they formulate and the resources they mobilise are heterogeneous. More so, hybrid forums are not only made up by actors whose interests or projects are different but these actors are less sensible than others to institutional boundaries.

Yet, concerning the annual meetings, it is not the actors alone who seem to be less sensible to institutional boundaries. The meetings themselves happen in places other than the Museum and they attract people other than Museum staff members. Also, the boundaries between scientific disciplines are not manifest. Each year the title of the conference is 'Colloquia on the natural heritage and biodiversity', a rather all-encompassing theme that makes no further distinctions within natural history. The event itself is organised in such a fashion that it cuts across boundaries. Thus, while actors present in hybrid forums might be less sensible to institutional, disciplinary and professional boundaries, this sensibility is also revealed on a somewhat more organisational or structural level. A hybrid forum is itself an actor that mobilises heterogeneous elements and, as such, can be less 'sensible' to institutionalised boundaries.

After the talks, meetings do not end straightaway but are usually followed by a wine reception and a dinner in a restaurant. At both the 2004 and 2005 events quite luxurious dinners were served at the expense of the Museum including three courses, wine and coffee (see pictures 14 and 15). 'This is where I like collaborating' one collaborator amusingly commented about the annual dinner (LR).

Picture 14: The dinner



Picture 15: A luxurious dessert

Thus, apart from being forums for the exchange of knowledge, the annual meetings are an occasion for the Museum to show its appreciation by offering the collaborators something in return, a kind of gift (see Mauss 1990). The importance of gift making in science has been examined by Hagstrom (1982) who argued that the link that unites scientists to their community is based upon transactions; that it consists of an exchange of information against credit. This gift economy helps to organise relationships between people in a certain way in that it implies a recognition of the status of the donor and the

existence of reciprocal rights, which might be to return a gift or to show appropriate sentiments of gratitude (Hagstrom 1982:21).

Not only have the collaborators no conference fee to pay, they are also offered a dinner and free issues of the Museum's journal. But gift giving does not only consist of exchanging objects: in virtual communities, for example, it is through acknowledgement that the giver can receive a certain amount of fame and respect (Bergquist and Ljungberg 2001:313). In a similar way, the annual meetings are places were the Museum shows its recognition towards the collaborators by thanking them in speeches. What might seem a mundane and banal thing to say, 'thank you', is endowed with much significance. At the meetings gift exchange takes many forms: verbal (showing gratitude by saying 'thank you'), organisational (providing a pleasant forum for exchange) and material-culinary (offering a dinner, wine, etc.). Gift exchange is a heterogeneous practice.

5. 3. Celebrating nature - the 2004 Biodiversity Weekend

The second biggest event in terms of numbers of collaborators participating is the biannual Biodiversity Weekend. The idea of this event stems from the USA where it initiated in 1998 as a citizen-based effort to monitor biodiversity (Stevenson and Morris u.d.). The GEO magazine then introduced the event into Germany. In Luxembourg, the first Biodiversity Weekend took place in the year 2000. Compared to the annual meeting during which mainly findings are presented and discussed, this weekend is an event where the collaborators are actively observing, collecting, and determining animal and plant species.

The event takes place every two years in a different location. The 2004 edition was organised in a nature-park (the *Naturpark Öewersauer*) in the north of the country, from June 18th to June 20th. The reasons put forward to hold the event in this nature-park were fourfold: it had already taken place twice in the south of Luxembourg, thus it had somehow to be organised in the north of the country this time; the presence of a school was relevant since one of the weekend's aims was to educate children; it is a biologically interesting area; and it is a manageable territory.

The chief organisers of the event were two collaborators who work in the nature-park and the *chef de service* of the Museum's service of databases and population biology. Before focussing on the event itself, let's have a look at some of the efforts that went into organising it. First of all, the event was advertised: a poster was put up at the annual conference, a presentation was made at a meeting of the botany working group, flyers were distributed, etc. Then, the collaborators were contacted via a letter, which explained the event in more detail. It was tried, more actively now, to enrol some of the collaborators. The most 'difficult' collaborators to enrol were even sent personalised letters stating their name on top of the letter whereas others were addressed as 'dear colleague'.

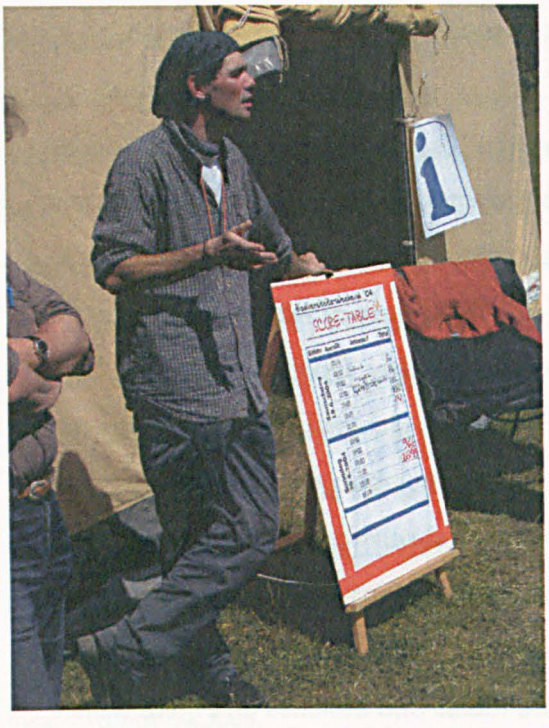
Those who had agreed to take part in the event received some further documents to 'familiarise yourself with the municipality', as was stated in one letter. Throughout these documents participants could get a sense of what was going to happen during the weekend. Various things were communicated: meeting places, maps with the territory to be inventoried, the time and place of different activities, the names of the supporting organisers, their logos, etc. Participants also had to choose a menu for the dinner that was to be offered to the participants on Saturday evening. The weekend was, in a sense, rehearsed - people were familiarised with the place, the time and the scope of the event. Moreover, the goals of the event were repeated many times. In this sense, the weekend was quite different from the annual conference. Unlike the conference, it had to be framed, explained, and visualised beforehand. Most collaborators had yet to become familiar with the event. Also, compared to the conference where the implicit goal was to share knowledge, the Biodiversity Weekend pursued multiple goals and these were made rather explicit.

One of these goals was to determine at least 1000 species and while doing so to draw an inventory of the environment. On the other hand, the aim was also to render people attentive to biodiversity issues and to catch children's interest in sciences. The distinction of a category of activities only for 'experts' was made in the letters and programmes that were sent to the participants (most of whom were collaborators or staff members of the Museum) and to the press and the public. Altogether three different groups were defined:

'children', the 'general public', and 'experts'. The activities organised on Friday were tailored onto the children from the regional primary school. Workshops were organised in the natural surroundings of the school. Children were also present at the two other days when members of the Museum's Science Club aged between 11 and 18 accompanied the 'experts' to observe them at work. A similar agenda as at the Museum's Science Festival was manifest – to educate children and to attract them to scientific careers. For the 'general public' two activities were organised. Nightlife biodiversity was explored on Saturday evening during a promenade during which bats, butterflies and mice were observed. And on Sunday afternoon, a promenade was organised to discover the local fauna and flora as well as the methods used by 'experts'. But it is on the latter of the aforementioned groups, the 'experts', who gathered on Saturday and Sunday, on which I will focus now, as it is amongst them that there were many collaborators of the Museum. Except for three persons, everybody of the 40 'expert' participants was either a collaborator or a staff member of the Museum.

Over the weekend these experts frequently met in a 'central meeting point', the location of which was communicated through letters beforehand. This centre consisted of a tent that was installed on a flat piece of land, close to the regional primary school. Next to it stood the museum bus, which was set up as a mobile laboratory with microscopes. The tent functioned as a meeting place on Saturday and Sunday. On the mornings of both days, people met in front of it for a general briefing (see picture 16). By the entrance of the tent there was a score table on which intermediary numbers were noted down (see picture 17). Inside the tent there were BSO (biophysical soil occupation) maps, enabling people to differentiate between different types of land such as wet lands which the mycologists would favour or dry fields were the botanists would go to. But the tent also functioned as a social meeting place: there were tables and benches and there were things to eat and drink like coffee, beer and croissants.

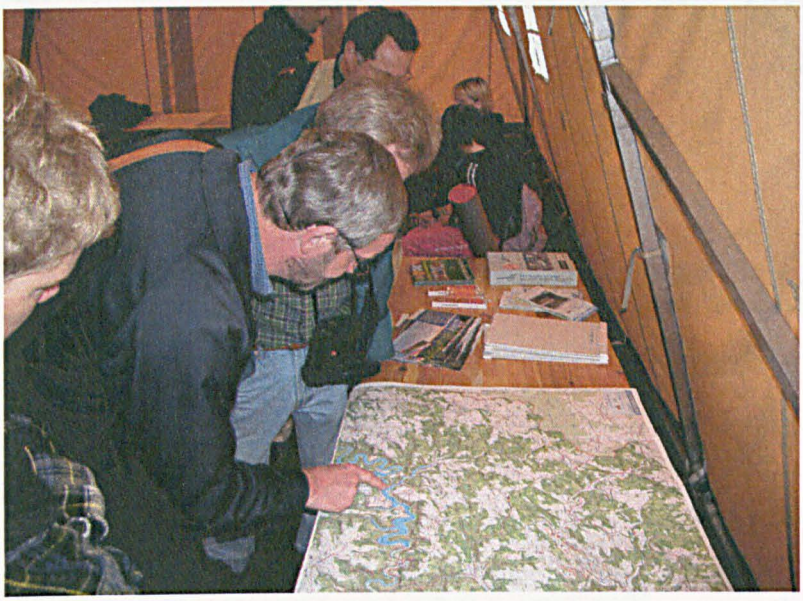
Picture 16: The main organiser of the weekend (first from left, back turned to camera), a collaborator of the Museum, gives a briefing



Picture 17: A meeting by the tent, close to the score-table

The 'experts' formed smaller groups before departing into the field (see picture 18). During the weekend I followed three groups: some botanists on Saturday morning, entomologists on Saturday afternoon and mycologists on Sunday morning. The modus operandi of these three groups was similar: looking for species, determining or trying to determine the species, noting names down and, at a later point, transmitting numbers and

names to others. The practical procedures to do this varied slightly. The botany group possessed a prefabricated list of abbreviated plants names. Each plant the 'experts' saw and determined was crossed on that list (see picture 19). The mycologists I followed showed a slightly different practice (see picture 20). Neither of them had a prefabricated list but noted down the names of their determinations on a piece of paper. Finally, the entomologists I observed tried to catch insects and put them – often without being able to identify them – into glass recipients.

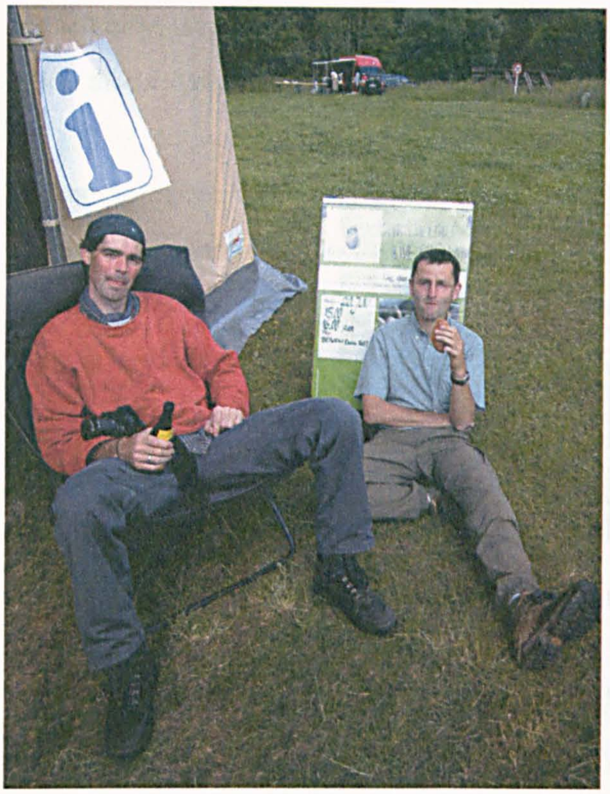


Picture 18: Two staff members interested in birds looking at a map and deciding where to go

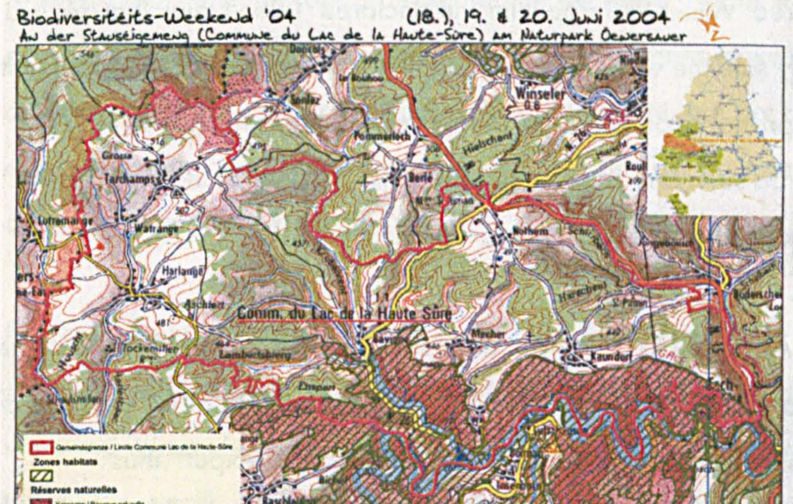
Picture 19: Collaborator determining plant and staff member crossing plant name on a list

Picture 20: Two collaborators from the mycology group looking at mushrooms

Back in the tent, the participants did various things. Some had a short break, and while doing so, ate or drank something (see picture 21). They also communicated their estimated number of species to the ones in charge. At several times during the day intermediary figures were calculated and noted down on the scoreboard. Many participants also exchanged specimens in the tent. Some, for example, had collected an insect that they knew would interest another collaborator. Sometimes the exchange was that of information about an interesting location for another collaborator. A lot of 'heterogeneous couplings' were performed in this tent: borrowing books and nets, receiving specimens, eating croissants, reading maps, writing on etiquettes, etc. Elementary forms of (social) life took place between human and nonhuman agents; intimate relationships between scientists and objects constituting elementary microcommunities were realised (Tchalakof 2004). And, humans and nonhumans travelled to and from this tent up to the very confines of the set territory. The thickened border on the maps that were sent or given to the participants made these limits very clear (see picture 22a and 22b).



Picture 21: Two exhausted collaborators consuming beer and a croissant



Picture 22a: One of the maps that was sent to the participants

Picture 22b: Another map that was sent to the participants

While most of the data was presumably collected from within these confines, the spatial and temporal confines of the event were not clear-cut. During the weekend, I observed four small incidents where boundaries were clearly articulated and to some extent renegotiated. First, one staff member had collected a specimen at about 100 meters outside of the delimited area. When told about this he declared 'I don't think that this is a problem'. He then jokingly said the wasp he had caught was a 'wander-wasp' and that it had a rucksack and was probably on holidays. During another incident, however, a mycologist said that she had seen one species outside of the confines of the municipality. She explained that consequently she couldn't note it down.

The mobility of an insect was taken to be reason enough to include it in the inventory. The assumption was made that even if the wasp had been collected outside of the confines of the set territory, it could still belong to the inside of this territory. The 'expert' thus renegotiated the set border of the weekend. The immobility of mushrooms made this deductive and figurative move probably more difficult. Here, the border was not renegotiated but clearly maintained.

For natural scientists, the crossing of such spatial boundaries probably makes more or less 'scientific sense' depending on the kind of species. Yet, this shows two other things. First, even predetermined borders, such as the political and geographic borders of a

municipality, are not immutable but can be renegotiated. Second, this renegotiation is made discursively, selectively, and argumentatively.

The temporal boundaries were also renegotiated during two instances. One entomologist left traps for a longer period, stating in a slightly humorous way that it was an 'extended weekend'. Finally, during the fourth incident one collaborator said that he had seen a plant at least three times but he hadn't seen it during the weekend itself. 'Don't be so strict with yourself' somebody replied. After having said this, somebody pointed out my presence and a couple of jokes were made about getting rid of me and trying to divert my attention (i.e. 'is that your mobile phone ringing?', 'is that your car that is being stolen?').

In these two latter cases, it was the stability over time that was taken to be reason enough to include both species in the inventory. Here the assumption was made that even if both species had been sighted/collected outside of the given temporal confines, they could still be counted as belonging to the inside of this time frame. The 'experts' thus renegotiated the temporal border set for the weekend. Again, natural scientists would probably argue that the crossing of temporal boundaries makes 'scientific sense': plants and animals don't just appear or disappear overnight. But, yet again, this also shows that even predetermined temporal borders are not immutable but can be renegotiated discursively and argumentatively.

Interestingly, humour was used when the spatial or temporal frame of the weekend was renegotiated. On the one hand, I have to say that the general atmosphere during the weekend was quite joyful and jokes were often made. Yet, I think that making fun about these very specific issues tells us something more. It can be seen as a way of dealing with a slightly uncomfortable situation by taking the 'breach' in a light-hearted manner to render relative the immovability of given borders.

When the weekend was over, the target of drawing a list of more than 1000 species had been reached. 1060 different species had been sighted and noted down on the scoreboard (see picture 23). Yet, during the weekend, numbers were constantly changing. Similarly to the French *Téléthon* (a TV fund-raising show) numbers were never

right, never fixed; the scoreboard was not a tool for pinning things down but to 'echo' a process of indefinite multiplication and proliferation (Callon and Law 2005:729-730). Even after the weekend, it took some time to fix the number of sighted species. The number 1060 was a first estimate. All those who worked on very small species – such as entomologists – had months of determination work ahead of them. However, some figures that made up this number were definite. Those who determined birds and plants had their list of species completed by the end of the weekend. What all of them were asked to do then was to hand data to those in charge 'if possible in Excel or Access format' under the following form: 'list of species (if possible in Latin and German), place of discovery, Gauss-Luxembourg coordinates, date, the author of the observation' (comment from a letter). In other words, participants were asked to produce stable relations now: to provide fixed numbers and names of species; to attach these to author, time and location; and to do so in a standardised way. They were asked to provide information that could now be centralised, juxtaposed, listed, and calculated in more certain ways.

Picture 23: The score on Sunday evening: 1060 species

A couple of months later, two booklets were published. Both contained an easily understandable part about biodiversity and about the area in which the event had taken place. They were published in French and German using a large font size and many colours and pictures – they looked very different from scholarly publications. One of the brochures contained two further elements: a list with all of the participants of the weekend, including their email address and their area of interest and a second list with all the determined species (see pictures 24a and 24b).

Picture 24a: List of the participants of the weekend

Picture 24b: Part of the list of the determined species

Two things can be said about the journey that led from the determination of species to the publication of these brochures. First, we have witnessed an initial transformation of nature; translating the natural world into numbers on a scoreboard and a list of species. This translation makes specific natural nonhuman entities converge with universalistic taxonomies,

> a convergence [which] only occurs through an interaction of diverse actors, involving mundane technologies like pencils [...] and notebooks, as well as the surveyor, the taxonomies, reference texts, and the organisms themselves (Watson 2003:153-4).

The 'nature-cultures' (see Latour 2004a, Law 2004) thus produced can be more easily transported: a number can be calculated and communicated, pictures can be published, brochures can be sent around.

Second, nature was to be discovered and documented in all its richness. As the program of the weekend stressed: 'biodiversity can be found not only in the rainforest but also in our "normal-landscape"'. The implication is that the exotic and surprising is present right next to our door. Throughout the weekend, nature was presented as something quite attractive. The set limit of 1000 species was to be reached almost like in a game or sporting event. In the brochures there were some 'highlights' (rare species) mentioned and the overall tone of the event was one of wonder and marvel at nature's diversity. Nature was celebrated in a similar way as science is often celebrated in museums.

It has been argued that the expectations of the four communities critical to science museums - scientists, public visitors, funders, and the museum staff - overlap in the fact that they expect a 'celebration' of science (Friedman 2000:45). Most scientific museum exhibits are celebrations of objects or phenomena, thus able to please each of these four communities to some extent. Similarly, the common ground for the different actors involved in the Biodiversity Weekend - 'experts', 'children', 'the general public', and Museum staff - was a celebration of nature. The diversity and discovery of nature was something to celebrate.

5. 4. 'Not that severe' - the Museum's journal

Apart from meeting in physical encounters, the Museum staff and the collaborators also meet in texts: articles, books, reports, letters, etc. In this section I examine the Museum's journal.

Since 1981, the Museum issues a journal where both staff members and collaborators can publish their findings. While some staff members have published their works in this journal it is, however, mainly designed for publications from the collaborators. The journal was initially called *Travaux Scientifiques du Musée National d'Histoire Naturelle* (Scientific works of the National Museum of Natural History). It appears irregularly: some years there have been no issues (in 1982, 1983, 1995, 2000 and 2001), and some years there were up to four (in 2003) or five issues (in 1987). Apart from this variability in terms of issues per year, the length of the monographs varies considerably. In fact, except for a technical limit there is no set page limit and contributions can be between 20 and 200 pages long. Shorter pieces by the collaborators often end up in the *Bulletin* of the Society of Luxembourg Naturalists, a journal which appears annually and has a set limit of 20 pages per contribution and is seen as a good complement to the Museum's journal. In contrast to the *Bulletin* and to most international science journals, the *Travaux Scientifiques du Musée National d'Histoire Naturelle* series is quite variable and heterogeneous.[31]

Most of the issues of the journal are single-authored monographs. Yet, if we shift our focus from authorship to contributorship (Cronin 2001), the many helping hands and thoughts that go into a single-authored text become apparent. Especially the acknowledgment sections reveal the collaborative nature of the pieces: people are thanked for the data they have sent to the author(s), for providing them information about the location of some species, for proofreading, etc. Acknowledgments sections repay intellectual debts by

[31] For some, low frequency is one of the characteristics of scientific journals from small countries (Marusic and Marusic 1999:509).

giving credit to contributors and, by reconciling individual achievement with collective ownership of knowledge, they reveal the dialogic and interpersonal nature of science (Giannoni 2002:8-9). In other words, these sections show to what extent texts are *co-produced* by more than one person rather than *produced* by a single author.

In 2002 the journal was given a 'new face': it was renamed *Ferrantia*, the format was enlarged and the articles are now in a two-column layout to create a 'more dynamic look' (as explained on the Museum web site). An editorial board consisting of five staff members was set up whereas before only one staff member was in charge of editing the journal. At the same time, instructions to authors have been established.
In the instructions to the authors we can read:

> FERRANTIA is a series of monographic works [...] dealing with life and earth sciences, preferably related in some way or other to the Grand-Duchy of Luxembourg. It publishes original results of botanical, zoological, ecological, geological, mineralogical, palaeontological, geophysical and astrophysical research and related fields.

Interesting here is the fact that the national scope is clearly emphasized. Works have to deal 'preferably' with science related to Luxembourg. As the current chief editor pointed out: 'There should always be, somehow, in one way or the other, a link with Luxembourg' (TH). Research has to be done within national borders. In this sense, Luxembourg becomes a delimitable 'laboratory in the field' giving research questions a national focus by 'taxonomizing the region' (Naylor 2002). The above quoted extract also states that the journal publishes works from different scientific fields dealing with life and earth sciences. On the one hand, the boundaries between scientific disciplines are confirmed by naming them. Yet, on the other hand, like the annual conference, the journal is a hybrid forum that mobilises heterogeneous elements (disciplines) and that it is less 'sensible' to institutionalised boundaries. By encompassing different scientific disciplines, the journal, taken as a whole, permits to cross the boundaries between these at the same time. In parallel, linguistic borders are articulated along the same lines. One of the instructions to authors holds: 'Papers are to be written in simple, correct and concise French, German or English'.

While the national scope and the linguistic and disciplinary heterogeneity have been some of its constant features, the journal has nevertheless qualitatively changed over the years. As already noted, instructions to authors have been included from 2002 onwards. Concerning the review process it is stated:

> Articles submitted for publication are reviewed by the editorial board and by one or two referees. The final decision on acceptance or rejection of the manuscript is taken by the editorial board. Manuscripts not prepared according to the following instructions to authors will be returned for correction prior to review.

The journal has become firmer – and more like international journals - concerning reviewing. The former editor of the journal explained:

> Since it is called *Ferrantia* [...] we have become more strict and tell people to follow the instructions [...] In the beginning we had on purpose a style [...], a simplified procedure that allowed people to present the thing more naturalistically. Now we want to have a certain scientific standing (MM).

The journal has shifted from a more 'naturalistic' to a more 'scientific' style. It has undergone a change that is noticeable for scientific articles in general in the 20th century – becoming less personal and varied and presentation more formalised (Gross et al. 2002:118). Revealingly, the current chief editor explained,

> We try [...] mainly through reviewers and also through the criteria we have fixed, to keep [...] amateurism out a bit (TH).

Yet, this shift from amateurism towards a more scientific standing is not absolute. In the above quotes, terms like 'a bit' and 'a certain' reveal that the move towards professionalism is not total. According to the chief editor of the journal:

> Of course, we are not that severe [...] it is more important for us that it is scientifically correct and that the method is okay [...]. This means that we do want

> to raise the standard a bit, but we do not pretend to be the *Journal of Biology* (TH).

Another member of the editorial board stressed,

> If we now set the bar very high - though it should be correct - but I think then we would perhaps scare young people and others (GC).

The journal's quality is articulated and defined in relation to two elements. First, the scientific quality of the journal has allegedly risen and it is through a review system and through instructions to authors that publications should be 'scientifically correct'. Second, the journal is 'not that severe', however. It does not aim to achieve the same standard as international journals. The journal's standing is, then, situated between a more amateuristic, naturalistic and 'less severe' standing, situated in the past, and a high international standing, mobilised as a means for comparison but not as a goal per se. This position has also been articulated in terms of intended audience. According to its former editor, the journal is 'not only for specialists, [but] also for naturalists or people interested in nature' (MM).

The review process of the journal can, then, be situated between two extremes: between double-blind anonymized review and very 'light' review (see Cronin 2001). Often collaborators already know who will referee their article beforehand and frequently they themselves select a referee of their own choice, for example a foreign scientist they collaborate with. Yet, even though editors claim to be 'not that severe' this does not mean that the review process is an easy task. Some have argued that the reviewing process is a balancing act requiring *Fingerspitzengefuehl* (literal translation: feeling on the fingertips) - often, also, because the manuscripts submitted by the collaborators are their 'lifework' and therefore difficult to reject. It seems that negotiating a space between amateurism and professionalism and between critically reviewing collaborators' work and not refusing their work straightaway is difficult, probably more so than managing publications in professional journals. One could even argue that the Museum cannot refuse collaborators' contributions since publishing in the journal constitutes an important motivation for them. In a sense, even if the journal is reviewed and published *by the*

Museum, it is foremost so *for the collaborators*, to provide them a textual forum. Thus, refusing collaborators' contributions could be scientifically correct, but it would be politically incorrect. The journal is not only a space through which scientific knowledge is made and exchanged. Something more than science gets done. The need for *Fingerspitzengefühl* reveals the very human, social and political dimensions of the relationships played out. The journal negotiates a space between gentleness and strictness, between ('warm') human feelings and ('cold') scientific standards.

5. 5. Commonalities in boundary encounters

The places of encounter between the Museum and its collaborators I have examined so far do differ. They enable different processes to take place: the legitimisation card permits its holders to navigate in space, the conference facilitates the exchange of findings in an oral form, throughout the Biodiversity Weekend observations are transformed into numbers and lists, and the Museum's journal enables the diffusion of knowledge in written form. We can imagine collaborators relating to these spaces sequentially – they first receive a card, then they do fieldwork, after which they present their findings in oral form before, as a final stage, publishing an article – although the practice of science rarely coincides with such a linear and clear sequence of steps.

While the boundary encounters I have examined are dissimilar, there are, nevertheless, many commonalities between them in terms of knowledge production and boundary-work. It is these commonalities I now want to tease out by further exploring the following matters: naming, language, localness, professionalisation, flexibilities, gift-exchange, and boundary-work.

Naming. At the encounters examined in this chapter, the names of the collaborators were clearly visible. They were inscribed on surfaces: on the legitimisation card, on lists of participants who attend an event, as speakers on programs, on badges, as authors beneath the titles of publications, and so on. Their names appeared intimately linked to them being a scientific collaborator. Since the 'process of naming also sets in place the concept of borders to a community' (Molyneux-Hodgson and Facer 2003:156), naming

the scientific collaborator means, then, delimiting the community of collaborators. Further, as naming creates an order (see Hetherington 1997:191), to designate scientific collaborators as such creates an ordered space of commonalities. It produces a homogenous and bounded discursive space. This is one level through which the Museum manages heterogeneity: by simply naming some people scientific collaborators of the Museum.

Language. A multitude of languages is used at these encounters: at the annual meeting people talk up to four languages, the brochure from the Biodiversity Weekend is published in two languages and publications for the Museum's journal are written in three languages. The specific languages used and the constant switch between them is one of the socio-geographic characteristics of Luxembourg. At first sight, the encounters seem to mirror the distinct linguistic situation in Luxembourg. In Luxembourg, French, German, and Luxembourgish are extensively used at an individual level (trilingualism) and at a societal level (triglossia) (Knowles 1980). While each of these languages has its distinctiveness in Luxembourg's multilingual society, Luxembourgish is allegedly 'what keeps it apart' (Weber cited in Fehlen 2002:92). Nonetheless, although this seems true for society at large, the situation is slightly different in the encounters we have explored in this chapter. First, Luxembourgish is not necessarily what keeps science apart. Rather, it is the flexibility within a multi-linguistic repertoire that sets these places of encounter apart from those where only one language is used. Although Luxembourgish is often used for oral communications, it is not used as a written scientific language: '[to publish] in Luxembourgish: this is just pointless', the chief editor of the Museum's journal commented (TH). While Lëtzebuergesch is used in the national press, in newspapers, in books, etc. it is not so for communicating science. In this sense, it is the absence of written Luxembourgish that demarcates these encounters from other cultural encounters in Luxembourg. Second, apart from not publishing in Lëtzebuergesch, the use of English is also noteworthy. While linguists usually portray Luxembourg as a trilingual society, the Museums' knowledge spaces tend to be tetralingual. The Museum's boundary encounters seem somehow to be both: very local in that three languages are used, but tending to be a bit global as there seems to be a recent trend to use English too.

Localness. The localness of the encounters surfaced throughout this chapter. The legitimisation card, a sort of identity card, allows people to access restricted places within Luxembourg. The former director of the Museum has expressed his regret that the annual conference is not wider in scope. Each year, almost every talk is related to Luxembourg. In the Museum's journal this link is even made more explicit as it is stated in the instructions to the authors. At the Biodiversity Weekend a certain framing was going on as well. While there have been renegotiations of temporal or spatial boundaries - discursively, selectively, and argumentatively; revealing of the flexible and contextually contingent borders of science (Gieryn 1995:405) - in general the exploration of the fauna and flora was limited in space and time. In short, the local character of the knowledge produced and diffused at all these encounters becomes apparent. It is natural history *of Luxembourg,* the exploration of a bounded space, the study of the fauna and flora of a limited territory surrounded by national frontiers. Also, to a large extent this knowledge remains *inside Luxembourg.* It does not travel very far as the journal's circulation is very limited, readers/audiences are mostly local, and visibility is low. At the same time, as being natural history *of Luxembourg* it is also natural history *in Luxembourg,* the production and communication of knowledge in a multilingual country where science has only recently been institutionalised and is on the way of becoming more professional. Professionalisation, as we see, is historically and geographically contingent (Alberti 2001:141).

Professionalisation. The professionalisation of the Museum's places of encounter has become manifest in recent years. To repeat but two examples: the Museum journal has introduced an editorial board in 2002 and in 2005 a scientific committee has been brought together to select the talks for the annual conference. There has been a shift from amateurism towards professionalism in terms of content, review process, 'look', etc. The Museum's forums resemble more closely now to international conferences and journals than some years ago. Boundaries have been drawn over time in demarcating present from past practice and thereby introducing ruptures in some uniformities of practice (Knorr-Cetina 1999). The past, amateurism, naturalistic approaches, 'less severe' ways of doing have been demarcated and given way to a practice that is more prescribed, more codified, more professional, more scientific. Yet these changes are not absolute; the encounters are not totally professional. There still is an element of amateurism and

flexibility present. What we see is a 'proto-professionalisation' (Rip 2003:425) or 'semi-professionalisation' or 'partial professionalisation'.

Flexibilities. Collaborators can choose. They can write and talk about topics within a range of disciplines and they can do so in various languages. The content and the format of the conference presentations leave some room of manoeuvre and the review process of the journal is 'not that severe'. Both these forums are not very restrictive, but quite flexible and relaxed. Compared to conventional meetings and journals, they appear less professional, or at least very different. In 'professional' places of encounter mostly one language is used – and English is the lingua franca of international science (Garfield 1989) – and there is a stronger focus on specific disciplines, themes, or domains. However, the places of encounter between the Museum staff and the collaborators are less rigid and allow more freedom. Including amateurs in the co-production of scientific knowledge necessitates the inclusion of flexibilities, that is, the exclusion of rigidities.

Symbolic gift-exchange. At the places of encounter, not only scientific knowledge was exchanged but also was there an exchange of things of a more symbolic order. At the bodily encounters we have seen food and drinks that were provided and thanks that were expressed. The textual encounters are, we can argue, places where the collaborators gain visibility, recognition and prestige. The need for *Fingerspitzengefühl* shows the very human, social and political dimensions of the relationships played out. These are places where the Museum is *giving*; where an exchange with a relational dimension is performed with an intention to maintain and develop the relationship with the collaborators. On the other side, the collaborators are also *giving*, their presence and their activities at these encounters is rendered possible through volunteering, in other words, through 'the gift of time' (Prouteau and Wolff 2004). All these gifts exchanged remain strongly attached to the giver. Mauss (1990:31) writes that the object is indissolubly tied to the giver: 'the objects are never completely separated from the men [sic] who exchange them'. Because of this bond between giver and gift, the act of giving creates a social bond with an obligation to reciprocate on part of the recipient. Every gift demands a return gift. Gift exchange is, then, a practice that cuts across boundaries: between giver and gift, between subjects and objects, between giving and creating a

demand, and between verbal, organisational and material-culinary forms. Gift exchange is boundary practice.

Boundary-work in boundary encounters. The encounters bring together actors from different professional backgrounds, discussing on a variety of themes in different languages, formats and styles. While the encounters are characterised by homogeneity in terms of national scope, they reveal heterogeneity in terms of language, disciplines and gift exchange. The encounters cut across many boundaries: institutional, disciplinary and professional. They are hybrid forums that mobilise heterogeneous elements and that are less sensitive to institutionalised boundaries. Three final remarks. First, managing such encounters is not an easy task. Negotiating a space between amateurism and professionalism, between inclusion and exclusion, between the local and the global, between flexibility and rigidity, is difficult. Second, while co-producing scientific knowledge, the collaborators and the Museum staff also co-produce spaces for the exchange of information, objects and gifts. Third, in doing so, different kinds of boundary-work take place: some boundaries are crossed, such as linguistic, professional, and disciplinary ones; some boundaries, such as geographic boundaries, are maintained; others, still, are made *de novo* – through professionalisation. Boundary encounters are places where heterogeneous boundary-work takes place.

Chapter 6:

Databasing the world - the world of databases

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So far we have seen the Museum staff and the collaborators coming together at conferences, during fieldwork, and through publications. At these boundary encounters, they produced inscriptions and discourses and they exchanged symbolic things. Yet, the production of science in a natural history museum relies increasingly on digitised information. Besides managing collections of artefacts, the Museum also puts efforts into creating and managing databases. These databases differ from the Museum's more conventional means to produce and order knowledge. First, things collected are no longer physical and weighty objects. Instead, things are reassembled as digitised data through information pathways, which allegedly eases the gathering and accessing of information across space and time. Second, another difference is that the database is a collective medium, as opposed to the more author-centred medium discussed in the previous chapter. Consequently databases bring about new questions about authorship, accessibility and knowledge since

> electronic distribution of information potentially challenges existing practices of knowledge work, boundaries of knowledge communities, and practices of trust and credibility (Van House 2003:15).

This chapter is entirely devoted to the Museum's databases as a site of encounter between the collaborators and the Museum staff. The structure is as follows. First I focus on the history and the aims of the Museum databases and the underpinning dreams of those who try to represent the natural world through collecting digital biological records. Then I will list the difficulties that make the construction of databases a complicated endeavour. These difficulties will shed light on the politics of databases and the underpinning differences between amateurs and professionals, and between the individual and the collective. The last part of the chapter discusses databases as 'boundary objects'.

6.1. Systems, procedures and grey-zones

In 1983 the Museum started to store information in electronic form and initiated the project to build a computerised database on the natural heritage called LUXNAT. The

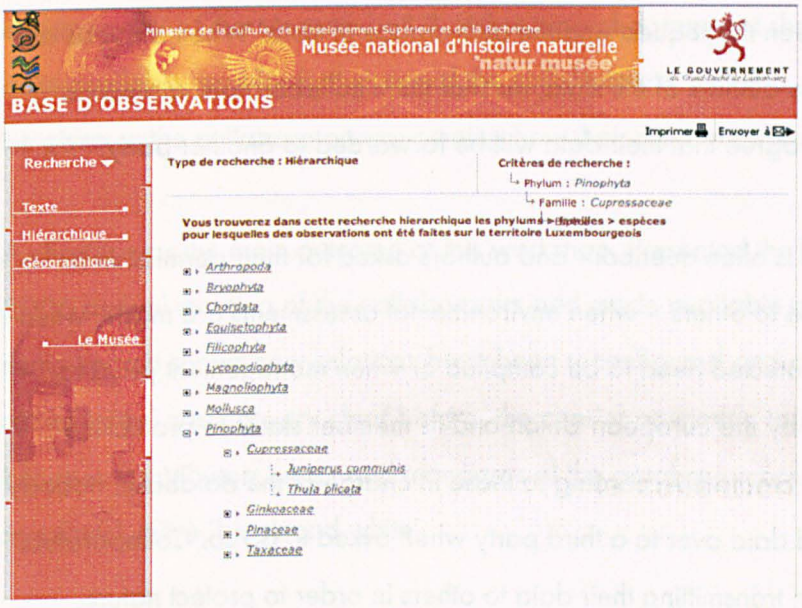
idea was to bring together all data relative to the natural heritage of Luxembourg in a unique and standardised system. The database was set up to permit applications ranging from statistical analysis on the abundance of certain species to the spatialisation of data through digitised maps. A couple of years later, in 1988, the database got a legal endorsement by a grand-ducal *règlement* which 'authorise[d] the creation and exploitation of a database on the natural heritage' at the Museum. In the law from 2004 about the cultural institutes of Luxembourg, this legal mission of the Museum was further defined as follows: to 'unify [...] scientific data relevant to the natural heritage' and to 'centralise and standardise observation data from the field'.

Currently, however, LUXNAT is being replaced by another database system called Recorder, which has been introduced into the Museum in the year 2000 – the same year the Museum's service for databases was created. This new system is based on a model developed by the Joint nature conservation council (UK). It is described by its builders as:

> a powerful piece of biological recording software based on Access 97 [...] It has been developed by JNCC for the National Biodiversity Network as a tool to encourage the collection, collation and sharing of good biological data and is built on a variety of standards that facilitate the storage and exchange of information between organisations and individuals (JNCC 2006).

The Recorder system has been adapted to Luxembourg in terms of spatial references, specific taxonomic dictionaries and relevant lists of localities. Since March 2005, data from Recorder are available online through a 'gate of the natural heritage of Luxembourg', still called LUXNAT (http://luxnat.mnhn.lu). Through this online gate, information can be accessed in three ways: by entering the name of a species, by navigating through the tree of life or by selecting a specific region (see pictures 25 and 26).

Picture 25: Home page of LUXNAT, the gate of the natural heritage of Luxembourg, available from http://luxnat.mnhn.lu. There are three ways to navigate through the database: textual, hierarchical and geographical.



Picture 26: Hierarchic search. The tree of life contains phylum, family and species.

Apart from Recorder and LUXNAT there are three other databases in the Museum: BG-BASE which contains 30,000 records about the Museum's herbarium and 6,000 records about the Museum's living collections; GEOBASE containing 10,000 pieces of data about the Museum's collections of mineralogy, palaeontology and geology; and

SPECIES with 5000 phytosociological records. In addition, the Museum is the national node for the BIOCASE project (Biological Collection Access Service for Europe) - a device through which the location of collections (sometimes the private home address of a collaborator) is made accessible. The Museum aims to merge all five databases into the Recorder system.

According to the ones in charge of the databases, the success of the databases is evident since: around 540,000 pieces of data are included in the LUXNAT and the Recorder systems; many people are transferring their data to the Museum, or intend to do so in the near future; and the German *Bundesforschungsamt* will take over the Museum's Recorder model for the collection of plants in Germany.

Deontology

The Museum's deontology foresees asking authors for agreement before making their data available to others via the database. A certain waiting time has therefore to be taken into account between the request and the deliverance of data. When requests are made to receive data, the authors of the data, most of them collaborators, are sent a letter asking them if they agree that their data will be forwarded to another party.

The Museum's database is often queried - and authors asked for their permission to make their data available to others - when environmental assessments are made, when listings of zones to be protected need to be compiled or when management plans for Natura 2000 (a project by the European Union and its member states to protect the environment) have to be created. According to those in charge of the database, nobody has ever refused to hand data over to a third party when asked to do so. Collaborators seem to be quite keen on transmitting their data to others in order to protect nature.

Those who receive datasets from the Museum's database are asked to indicate that data originate from the Museum's database. Furthermore, the author and the date of data collection must be mentioned. However, the Museum 'declines every responsibility in the case of eventual mistakes in the data' (Museum website).

For the Museum there are some drawbacks with this procedure, namely the delay between the request and the sending of data, which takes an average 76 days. However, decisions about the environment, it is argued, have often to be made as quickly as possible. To improve this state of affairs and to rethink the Museum's relationship with its collaborators in terms of data transfer, a workshop had been organised at the 2005 annual meeting of the collaborators.

Up to that date the self-perceived roles of the Museum were: to facilitate access to data, to enhance data, to back up data and to avoid that data are lost. Yet, some elements remained undefined concerning the Museum's handling of data, some things remained unspoken. The Museum staff and the collaborators often used the term 'grey zones' to describe the spaces in-between well defined areas. It was not clear what the exact roles of the Museum were, what the feedback to the collaborators should look like, how to deal with authorship issues, how to differentiate between paid and unpaid data, and so forth. The workshop was organised in order to resolve some of these problems. The participants were asked to reflect upon the three following questions: 1. What should the engagements of the Museum be? 2. What type of agreement should there be to make data available, and for further use of the collaborators' data? 3. What feedback should be given to the collaborators concerning how their data are used?

A charter was the main outcome of this workshop. Presented the following year at the 2006 annual meeting of the collaborators and made available online in March that same year, it shows how relations have been reconfigured and codified. Whereas being rather 'grey' and not very clear before, the charter attempted to make the relationships between contributors, users and managers of the database more formalised, more regulated, more 'black and white'.

6.2. Mapping nature

One of the functions of the Museum's database is to produce maps showing the distribution of animal and plant species, so-called distribution maps. On the online version of the database, data can be visualised in two ways. A species can be selected by

entering its name or by navigating through the tree of life to find its name and then shown on an adjacent map. The other possibility is to click on a region of a map of Luxembourg after which a list with the species present in this region appears. Through the offline version, more complex visualisations can be made, by representing: more than one species; protected species; species determined by a specific author, etc. In short, almost any possible recombination of data sets can be represented on a map. Whatever data are chosen to be visualised the result is shown as dots on a map of Luxembourg.

On the webpage of the online database the following can be read:

> **Important note**: The data presented correspond to the current state of the database *Recorder 2000*. For certain groups of organisms our current knowledge is not very complete or the existing data were not seized yet in the database. For these reasons the distribution maps do not necessarily correspond to the potential distribution of the species in Luxembourg (emphasis in original).

The mapped distribution and the potential distribution of species are not necessarily matching. Put in another way, representation and reality are divergent. This discrepancy is not only due to a lack of data in the database. In fact, maps are selective altogether: they do not, and cannot, display all there is to know about any given piece of the environment (Turnbull 1993). Maps are partially connected to the environment.

Furthermore, the distribution maps displayed through the database inevitably represent the species that have been sighted by the collaborators or the Museum staff. In the somewhat humorous words of one collaborator: 'The distribution maps of plants and animals are the distribution maps of the [collaborators]' (LR). In other words,

> What is on the map is determined not simply by what is in the environment but also by the human agent that produced it (Turnbull 1993:5).

The maps produced by the database not only show the occurrence of certain species, they also map the collaborators, their preferred area of fieldwork and their favourite

species. Knowledge is inherently spatial and imbedded in practical action (Turnbull 1993, Shapin 1998). Moreover, the geography of authorship is revealed.

Despite the spatially distributed provenance of data, the maps on which species are represented have to be superimposable. Maps have to be 'combinable in one central place, enabling the accumulation of both power and knowledge at that centre' (Turnbull 1993:26). As one of the objects produced when professionals and amateurs collaborate to produce representations of nature, maps have to inhabit multiple worlds simultaneously without losing integrity and robustness (Star and Griesemer 1989).

6.3. Panoptical dreams

The Museum hopes that those people who possess biological records - mainly the scientific collaborators - will transfer them to the Museum. For the Museum the stakes are high. The more data the database contains about the biodiversity of Luxembourg, the better it can represent nature. And the better it can represent nature, the better choices can be made about protecting nature, so the common argument. In sum, the aims of the database are double: to represent and to be able to intervene.

A key endeavour is thus to collect as much data as possible on as many species as possible. The amount of information in the databases has constantly increased and will probably continue to do so in the future. But not only are large amounts of information stored in these databases. Information can also be rearranged, compared, and linked in a multitude of ways.

> A database is a way of holding information in a structured way; of chopping it up into small, defined pieces that can be recombined in different ways (Keene 1998:119).

As we have seen before, through the online version of the Recorder database, data can be accessed by name, by region, or by the tree of life. Those who have access to the offline database, can sort data in many more ways: data can be sorted alphabetically,

by author, by project, by date of collection, in short, by any kind of imaginable relation that can be thought of between bits of information. This is so because the Museum's database is a relational database – relational databases being systems where information is subdivided into elements as small and simple as possible and each element of data is entered in a separate field that can be related to one another (Keene 1998:46-7).

But to be able to use data in recombined ways, data have to be fed into the database in a unified, standardised, and centralised way. On the Museum webpage we can read the following about Recorder:

> The [Museum] wants to use Recorder as a basic tool for putting in common data on the natural heritage and the establishment of a national network on the natural heritage of Luxembourg.

Common models have thus to be used (the same languages, the same taxonomies, the same geographic coordinate systems) and data have to travel to a single point.

But where does the information centralised in the Museum's database come from? First of all, the database software is based upon a British model. Through travelling to Luxembourg, the Recorder system has gained some Luxembourgish characteristics. On the Museum's webpage we can read that Recorder has been 'modified in order to fulfil the specific requirements of the Luxembourg scientists and researchers':

- The maps in use represent the territory of Luxembourg only. On the online version of Recorder, for example, the territory of Luxembourg is represented in brown colour whereas the surrounding countries (Germany, France and Belgium) are absent.
- The names of localities are all places within the national borders of Luxembourg.
- The taxonomic dictionaries have been adapted to fit the fauna and flora of the country.
- To represent the three-dimensional morphology of the country as a flat surface on screen a specific coefficient is used for calculation. This number is specific for

each country and thus transforms, as one staff member said, the real shape of Luxembourg, like a banana, into a flat map (GC).

The second thing to note is that data put into the database come from a limited area only: from within Luxembourg. Similarly to the boundary encounters examined in the preceding chapter, the database reveals a certain localness. The name of the online gate and of one of the databases (LUXNAT, standing for Luxembourg Nature), the maps on which dots are shown, and the names of the localities clearly indicate that nature of Luxembourg is represented.

From the database, then, data travel to a limited area. Those who retrieve data are mostly people from Luxembourg: the Museum, *Bureaux d'études*, the Ministry of environment, the *Administration des Eaux et Forêts*, and collaborators.

Yet, collecting and retrieving data is not an end per se. By reassembling all those data the Museum becomes an important player for environmental politics. It does so because it can claim by being able to draw on a vast resource of records about nature, to know nature. The Museum thus becomes a powerful spokes-person for nature. The database is, then, a device that reveals two inter-linked dreams: the dreams of scientists to know nature, to put names to plants and animals and order them in space, and, on the other hand, the panoptical dream to better govern the natural empire by cataloguing it completely through 'biodiversity panopticons' (Bowker 2000a:645, 2004:11). Through databases scientists wish to create a central point from which the distribution of plants and animals can be monitored. Species are classified, regions are defined, knowledge is fragmented, all in order to be able to be accumulated at a centre, the database. The dream is, then, that of all the nonhuman inhabitants of Luxembourg becoming visible in detail, at all times, from a single point.

With such a panopticon being set up, the Museum sets out protocols, or codes of behaviour - what the Museum calls its 'deontology'. Embedded in the database system are thus issues of authorship, scientific conventions (to reference others' work), different levels of access, and so on. Also embedded in the system is its user. He or she gets defined or 'configured' (see Woolgar 1991) through all kinds of work: software design,

workshops, the charter, informal discussions, etc. However, there is not a single, well-defined database user. We can differentiate between at least three different types of users. First, some Museum staff members have access to all of the data, to funds, and to technical knowledge. Second, the collaborators can access the database in a slightly more reduced way. While they don't have access to the entirety of the database, they do possess the database software. Third, members of the public can navigate through the online version of Recorder, but are not permitted to access the exact location of endangered or protected species, nor to feed data into the database. The most visible boundary is drawn between, on the one hand, the public who is excluded from giving any input into the database, and, on the other, the Museum staff and the collaborators who are both actively producing knowledge to be fed into the database.

The Museum staff and the collaborators are written into Recorder in similar ways. Both have to be knowledgeable in English, they have to possess some computer skills, and some scientific knowledge to make observations and to seize them into the database. In other words, the database contains a script that its users enact while working with it. Thus, in the preceding paragraphs we have not only done a description of the database by telling details of its technical nature. We have also done a 'de-scription' of a technology (Akrich 1992) by retrieving the script that is embedded in the database.

Yet, people do not always adhere to this script. They do not necessarily share the Museum's panoptical dream to fully catalogue the natural world. As we will see in the next section, the building of a coherent and representative database is a complex task.

6.4. Difficulties

Hereafter are the difficulties reported by the staff members and the collaborators I interviewed.

1. Some collaborators seem to be 'computer resistant', as one person in charge of the databases has lamented (GC). Many of those who possess biological records have no computer or don't seem to be wanting to work with computers. Those who use computers,

on the other hand, often found the LUXNAT system too complicated. One collaborator held:

> It never became user-friendly [...] You didn't really have access. It was too high. It was too difficult. You weren't cared enough for (RG).

2. According to some collaborators, the Museum isn't even trying to transfer their data. One of them explained:

> It has always been said since more than 15 years 'you will seize your data into LUXNAT, don't you?' Then I said yes. And that was it. Nobody in the Museum has even made one further step than this (PD).

Similarly another collaborator commented:

> Of course I would like to give them [the data]. It means a terrible lot of work for me. The profit is for them. But if they don't make the effort to show me how I should do this, to make them get it. I won't run after them forever (NS).

In front of such arguments, those in charge of the database respond that they will eventually transfer the data in the near future.

3. The reasons for the lack of transfer of data are often claimed to be a lack of time and human resources in the Museum. From the mid-1980s until the year 2000, only one staff member was in charge of the database. Apart from working on the database, this staff member was also a *conservateur* and as such he was responsible for managing collections and undertaking research and educational activities. He argued that:

> The problem with the database was that [the former director] never recognized what work it is to seize data (MM).

He further commented:

> and then I never got personnel. And when I got some, it was temporary, or it was unsuited, or students, or something else (MM).

For a long time, there was nobody in the Museum who was specialised in databases and working full-time for this task. In 2000, then, two persons were appointed with a specific mission to develop the databases. But the argument of a lack of personnel has not disappeared since. Often the situation in Luxembourg where two persons are working on the database is contrasted with the UK where about 20 persons are doing the same job. Especially in Luxembourg, the panoptical dream runs up against the problem that someone has to sit there and do the necessary seizure which takes a considerable amount of time and resources (Bowker 2004:7).

4. Another source of trouble is the number of different database systems in use. As seen before, there are not one but five databases in the Museum, which renders the centralisation and standardisation of data difficult. In addition to these different systems in the Museum, there are some collaborators who have their personal system at home. One collaborator has described the situation as follows:

> They have LUXNAT, Recorder, another system [...] then also people who have their private one. [...] There are so many [systems], that this does not bring anything (NS).

Here is a comment from one collaborator with a private database system:

> I needed a database for my work. And LUXNAT was never available. [...] So I wrote my own software (PD).

Problems with incompatibility are therefore recurrent. Transfer programs have to be set up in order to merge data. A related difficulty is the need to learn how to use a new database system. This seems particularly difficult the more one is accustomed to another (older) database system, that is, as one collaborator has put it, the more one's brain is 'formatted' in LUXNAT for example (EC).

5. There seem to be lots of mistakes in the databases. Especially LUXNAT is 'full, full, full of mistakes' according to one collaborator (Anonymous). There are various causes for the occurrence of errors. First, the determination of species is not always straightforward. Putting the right name to a species can be complicated if the species itself is hard to determine. In addition, from the moment of determination to the moment of sending data to the Museum, descriptions might be messed up. Second, errors can occur when data are seized into the database. Part of the reason for this is that very often data are not seized directly by those who did the observations. Instead, the scenario is often as follows. The collaborators make their observations that they write down and then pass onto the Museum. Then, data are seized by students or by disabled persons working in the Museum. Although this procedure relives the collaborators from what they perceive to be a boring task and it permits them to reinvest their time elsewhere, more mistakes creep in. Staff members estimate that there are fewer than one percent of mistakes in the databases.

6. A sixth reason for not transferring data is linked to the risk of making data about rare species available to the public. The argument is that the location of endangered, rare or protected plants, for example, should be kept secret to avoid unscrupulous collection, vandalism or wilful destruction (for a discussion see Bowker 2000a). To prevent such problems, there are currently discussions to build a differentiated access into the database. One for Museum staff and collaborators, with a password, where all the data are visible exactly and one for the public, without a password, where the distribution of sensible species is shown on a 10 by 10 kilometre grid only. Yet, keeping the location of sensible data secret might be counter-productive, as people might unknowingly destroy rare species. To prevent this from happening, the Museum is planning to give forest wardens an indication of sensible species (such as an insect or a plant) without, however, divulging the exact species itself.

7. Some scientific collaborators might not be willing to hand their data onto the Museum for reasons like the following:

> Simply because I cannot agree with handing over all those data on the first hand - that is already painful enough - and having to do the work [of transferring the

> data]. And that wouldn't be remunerated. And I would be hindered to do my research during that time (Anonymous).

Linked to this argument is the fear of somebody else evaluating the collaborators' data:

> Somebody else could do the evaluation, although he would be faulty [...]. It's *my* role to evaluate my own data [...] I like to do this myself (Anonymous, emphasis in original).

Data might be considered as one's own property, as something personal. Transferring it to the Museum would mean loosing authorship, loosing grip of the thing that the collaborators might see as their own. It would mean relinquishing control over parts of their knowledge (Ellis and Waterton 2004b:99).

8. Finally, data about birds do only rarely end up in the Museum database but in a database hosted by the League for the Protection of Nature and Birds. To my question if this data are transferred to the Museum's database, I got the following response by the person in charge of the League's database (who is also a collaborator):

> There once was a query from the Museum to receive all the data. But on the other side it is clear that if the League for the Protection of Birds would simply give the data to the Museum, [it] would of course loose very strong arguments. Because of this database we have a convention with the Ministry of environment, to keep the ornithology centre running and to get money [...] *For us these data are a very important survival argument* and give us a position of a relative importance (PL, emphasis mine).

Apart from the Museum and the League for the Protection of Nature and Birds, there are other institutions aiming to bring together biodiversity records. One research team of the Public Research Centre Gabriel Lippmann, for example, is aiming to collect data about species that serve as indicators to climate change. When the person in charge of this project expressed his wish at the 2004 annual meeting of the collaborators to collect biodiversity data, the Museum was perplexed. Especially his call to the collaborators to

send him data annoyed the Museum staff. Staff members saw it as a potential rivalry on a topic (biodiversity) and a means (collecting data from collaborators) where the Museum had a kind of monopoly so far. The person in charge of the project held in an interview – in which he was very evasive – that he was referring to data in relation to climate change only (LH).

In front of this situation, where there are not one but a couple of institutions that collect data about biodiversity, some critics have been raised about the difficulty of mapping and protecting nature when competing systems are used. The Museum is trying to maintain its position. As the Museum director declared at the 2005 annual meeting of the collaborators, the Museum wants to remain the sole player concerning databases about the biodiversity of Luxembourg. Yet, the Museum's central role in this domain is open to debate. Since the Museum has had enduring troubles to develop an efficient and representative database we can imagine another institution overtaking that role from the Museum. Especially the Museum's limited possibilities to hire more personnel and to mobilise more funding for the development of the databases could put other institutions into a better position as collector of digital data.

As we see, there are many reasons that make the building of a representative database difficult. Here is a quote from the conservator who was formerly in charge of the database, which summarizes some of them:

> There are also others who simply don't want a computer, because they don't want to invest time and efforts to make something available *for other* people. This is what the problem is about. There are reclusive people who just work for themselves and hardly ever show their collection, or shelter their data like a hen nurtures its hatchling (MM, emphasis his).

Missing computers or computer skills, lack of time and efforts, personal attachment to the data – these are some of the reasons that make the building of a representative database difficult. The other reasons we have seen so far are, to sum up, complexity of the system, missing efforts from the Museum, incompatible systems, mistakes, sensitive data, and 'survival arguments'.

There is a relative scarcity of the 'ideal' collaborator for the Museum, a collaborator who would be very disciplined, who would have enough computer skills to use the database, who would transfer every piece of data to the Museum, and who would be willing to spend time and efforts to do so. Although a significant part of the collaborators are already quite eagerly transferring data to the Museum, and although in the future an even greater part of the collaborators will probably possess the technical capabilities to do so, not all collaborators fit this scheme. There are many collaborators who are complicated to deal with: they might be undisciplined, computer resistant, keeping data, short of time, etc. They might want, as one of them said, that

> the Museum staff should, via the notes from the amateurs, seize scientific data from the field into the computer instead of forcing them to do it on their own (comment from the questionnaire).

6.5. Decomplexifying and decentring the database

To improve the flow of data, the licences of the Recorder software are distributed for free to those interested: scientific collaborators, but also *bureaux d'études, stations biologiques,* foundations. A kind of 'open source' movement (a movement which is proclaiming the democratic and liberatory value of freely sharing software (see Bowker 2004:6)) is visible. Staff members sometimes even visit collaborators at home to help them to install the program and assist them with specific problems. These personalised visits are 'worth the investment' as one staff member involved in the databases held (GC). Investing time and efforts to travel to a collaborator's home to help them with Recorder is worth the outcome (data) that a fully computerised collaborator might produce. Furthermore, workshops for using the Recorder software have been organised at the Museum, which around a dozen collaborators attended.

One collaborator has suggested the 'construction of an efficient database with access from home' to improve the relationship with the Museum (comment from the questionnaire). This is one of the things the Museum aims to do: to create a 'light' version

of Recorder, which collaborators could access via the web. In other words, collaborators wouldn't have to install the whole database system on their home computer anymore, but could seize or visualise data directly through the web.

Another way for the Museum to increase the flow of data is to promote a more flexible use of the database software. In fact, Recorder can be adapted to personal use, as people can configure their own 'rucksack' (term used in the software). Into their 'rucksacks' they can put the maps and the lists of species they specifically require. During one of the workshops about Recorder one of the collaborators, for example, only installed the maps he punctually needed for his work on the computer. The possibility to add languages other than English is also being discussed since 2004. Collaborators can also 'blind' functions they do not use, or add 'Add-Ins' to the system. While retaining the robustness of the database, it also becomes more flexible through a personal use, by the possibility of configuring it to one's own interests. The designers of Recorder describe this feature in the following terms:

> This flexibility can also assist the individual volunteer naturalist, especially if they want to undertake recording within more than one taxonomic group. [Recorder] allows naturalists to manage and use their own data effectively without investing in a number of different packages for analysis and presentation. The mapping functionality in particular gives slimmed down GIS functionality without the need for an expensive GIS package or extensive training (NBN 2006).

The plans to build a 'light' version of the database and to have a system with 'slimmed down' functionalities, without 'the need for [...] extensive training' show that directions for collaborators cannot be made too complicated. It reveals that 'allies enrolled by the scientist must be disciplined, but cannot be overly-disciplined' (Star and Griesemer 1989:407). Put in a more managerial parlance, the Museum can better manage the complexity of coordinating the collaborators' logically and geographically distributed activities by a flexible adherence to coordinative procedures and protocols (Spasser 2000).

Curiously, the person formerly in charge of the databases held that the new Recorder system is 'not easier but it is decentralised' (MM). In other words, whereas the seizure or retrieval of data from LUXNAT could only be done in the Museum, today, the users of Recorder can work at home. Yet, I would argue that this is precisely what makes it easier. By decentralising the database, by permitting personal configurations and by blinding some functions, the database becomes easier for the collaborators to use, although *at the Museum*, at the (former) centre, things might look as complex as they did before. Decentralising and decomplexifying the database increases its mobility, its usability and its plasticity, all of which are crucial for its use by the collaborators, all of which *do* make it easier to use than the LUXNAT system.

Compared to LUXNAT, the Recorder system has eased the seizure and transfer of data for the collaborators. Recorder is described as being more accessible and more flexible, and the workshops organised at the Museum have helped the collaborators to familiarise themselves with the software; whereas before the collaborators were 'alone in the desert' as one of them affirmed (RG). While heavily criticizing LUXNAT, the latter collaborator was nevertheless much in favour of using Recorder:

> I am now more in favour of Recorder and support it in this sense, because I see: one gets support, there is a feed-back. So you are also motivated to seize data (RG).

Despite acknowledging a more flexible adherence to the database, the Museum does not stop from hoping that in the future the collaborators will by 'fully computerised'. For one of the persons in charge of Recorder, the ideal collaborator would be as follows:

> Our vision [...] of the collaborator of the future [laughs] is [... someone] equipped with a GPS while on the field, eventually even with a little hand-toy, a computer, where you enter data [...] Then, at home, [he would enter] his material on the computer, in a standardized format and send it to us. And the next step would probably be - that would be possible today already, but not in the complexity we want - that he logs in via Internet right away [and doesn't keep] anything at

> home. That would be the next step. But then one has to check whether the collaborator is satisfied when not having his own data (GC).

Yet, the 'collaborator of the future', as s/he is depicted in this account, is nothing like the present collaborator using the database. There still are many differences between the projected user and the real user of the database, between the world inscribed in the database and the world described by its displacement (see Akrich 1992).

6.6. The politics of databases

Is there anyone to blame for the partial failure of the database? Must we blame the Museum since it hasn't yet set up a single and representative database, although it has spent around 20 years in trying to do so? Or those who refuse to give their data to the Museum for whatever reason? Or must we blame the system for being far too complex? All these reasons are related through the database, all these elements partially determine how good the database is to represent the biodiversity of Luxembourg. Yet, there are some more elements – at least three - that come into play.

First, it must be stressed that articulation work is difficult: meshing, aligning, integrating, coordinating is not an easy task (see Spasser 2000). Data have to be entered into the database in a standardised format whereas the collaborators deliver their data on all kinds of supports (Excel format, Access format, Word documents, hand-written notes, etc.). Making sure that collaborators are able to work with the Recorder software forces the staff to deal with individual problems and to sometimes travel to their homes.

Second, maybe the aim of the database is wide of the mark. As one collaborator has argued:

> I think that a critique can be made towards the objective to collect all data [...] They simply want too much [...] For me the aim is too ambitious. And it costs tax money to rework all this data that, for some part, doesn't interest anybody (Anonymous).

A similar point has been made by Bowker and Star (1998:233) who do not demand that classification systems be complete. Concerning databases, translation and enrolment are never finished (Van House 2002:238). Building a database is an ongoing task since any

> modes of ordering are never complete, closed totalities: they always generate uncertainties, ambivalences, transgressions and resistances (Murdoch 1998:364).

Third, maybe the database was thought to be what it never can be: a neutral and objective technological system. Very often I have been told that the technology itself is to blame, that it was and partly still is too complex. Yet, a database is much more than a technological device that just represents nature as it is. We have seen that different interests and powers come into play as well. Differences surfaced between attachments to the data, between people's eagerness to use computers and to spend time and efforts for data transfer. A power struggle was clearly visible in the lack of transfer of data about birds due to a 'survival argument' and a 'position [of] relative importance'.

Data which is collected is being integrated into two discourses - a scientific and a political one (Bowker 2000b:748). In fact, every technological system, be it an electric light system or a nuclear power system bares with it a political agenda (Winner 1996). Edison's electric light system, for example, was not only the invention of a light bulb, it was a blend of economics, technology and science (Hughes 1985:47-8). Technologies are always shaped by social and cultural factors (Bijker and Law 1992b). Databases too are a site of political as well as technical work (Bowker and Star 1999). They are not only producing representations of the world, but they also map out the people who design and use them. In other words, databases map the natural *and* the social world (Bowker 2000a:645).

The different social worlds databases map are quite complex and sometimes conflicting. The transfer of data, which the Museum so much strives for, can mean for the collaborators an additional effort, an arduous investment of time, learning how to deal with computers, in short, things to avoid - especially during leisure time.

Thus, the partial failure of the database to represent nature is not due to a difference between the Museum's aim to collect data in a 'disinterested' database and the personal – and, it could seem, irrational – refusal of some to transfer their records. Both parties have their agenda, their reasons for action. But none is more right than the other, none is irrational. Both put forward their understandable arguments. On the one hand, the Museum wants to be a truthful and thus powerful spokesperson for Luxembourgish nature; the collaborators, on their hand, might claim their ownership right on their data and their self-determination of their leisure time.

So what do we make of a statement like the following: 'It is a kind of boycott when you don't hand in data', a comment from the person formerly in charge of the database (MM)? Are the collaborators really boycotting the Museum? Are the notions of betrayal or dissidence adequate to describe this state of affairs? Partly. On the one hand, *for the scientist*, some collaborators betray. For scientists the dream to construct a sound database is boycotted by those who do not want to align to these goals. The situation in the Museum seems similar to that of the fisherman of the Saint-Brieuc bay who refused to act according to scientists' recommendations (Callon 1986). But in Callon's story (1986), the adopted perspective revealed an unsymmetrical account, a portrayal that showed that the prime movers, the scientists, were too closely followed at the expense of the other actors (see Wynne 1992). To use a less situated and more symmetrical vocabulary, instead of betrayal, we could also use terms like 'sticking to one's ideals' or 'being consistent and truthful to one's principles and beliefs' to describe the same action.

Nonetheless, in the case of the scientific collaborators, a contract is breached. Those collaborators who do not transfer their data to the Museum do not fulfil one of the principles they have agreed upon when signing the application letter to become a scientific collaborator, which is to 'give the Museum a copy of their data for the database of the natural heritage'.

These conflicting views might be internalised in a single person, in a single interview. One collaborator, for example, discussed transferring data to the Museum, which 'would be

objectively right' (Anonymous) as he stated, but at the same time he gave reasons for not doing so. Another collaborator, reflecting upon the same problem thought:

> as a total outsider the situation is of course not ideal, because the Museum should, as a rule, fulfil the role of collecting data about nature covering the whole surface of the country [...] So I see this wearing two different caps. One time I see it from my professional point of view. The [other time] I would say it is not ideal, this is of course as a scientific collaborator (PL).

Some collaborators have to cope with two conflicting stances. One that says the data are their property and the other that urges them to deliver it to the Museum in order to protect nature, to care for a higher good.

The motivations of the collaborators are sometimes divergent from those of the Museum which wants to centralise data in a standardised format, to be a node where 'things are drawn together' (see Latour 1990). The collaborators, on their side, have reasons for not transferring data. Doing so, they actively construct and protect their identity, their individuality, and their independence. Transferring data to the Museum, and, at the same time, accepting the disciplined role the Museum would like to prescribe can conflict with their sense of ownership of data, with their use of 'their' time, with their privilege not to be bothered by computers, with their right, simply put, to do what they want.

While refusing to transfer data to the Museum they deploy a resistive agency: 'the capacity to act back, granting or refusing translation' (Fox 2000:863, see also Star 1991:30). Resistance is then one mode of power (Few 2002) developed by the collaborators, which seems almost opposite to the Museum's efforts to enrol them, to link them to a more rigid frame, to a common scientific endeavour. They resist being fully enrolled, which would mean for them giving up some of their freedom to choose what to do and when and how to do it.

Foucault (1982:211) asks us to take resistance as a starting point

> to bring to light power relations, locate their position, find out their point of application and the methods used.

In their relation with the Museum, the collaborators are not merely submissive actors who can be told by the Museum how to act; they possess a desired good, data, that they can withhold from the Museum – a manoeuvre which reveals their control of their data and their active deployment of power.

Through the database we can thus perceive a tension due to the entangling of the collective and the individual. The Museum wants to collect and centralise data and, while doing so, to attach the collaborators to a common enterprise. A database stands for all sorts of collective work: it is a common project, a collective technological device; it requires shared computer knowledge, shared taxonomies, shared languages, etc. A database is also a device that has been developed and that must be fed and maintained by more than one person. It is difficult to discern individual authors. One reason for refusing translation is thus due to the risk of the 'erasure of the individual as an epistemic subject' (Knorr-Cetina 1999:166). When transferring their data to the Museum database, collaborators loose a bit of their individuality, the 'heroic myth of the individual researcher' (Bowker 2004) weakens. For individual Museum staff, this might not be overly problematic since in science museums they are generally subsumed to a 'floating signature' both present and absent simultaneously: the museum itself (Macdonald 2002:110). But for the scientific collaborators, naming seems to be much more important.

Whereas I have not encountered Museum staff complaining about the work that collaborators put into publications or conferences, the database is the origin of much more concern. A crucial difference between publications and conferences, on the one hand, and databases, on the other, seems to be that the former are much more centred on the individual than the latter. The person formerly in charge of the database held: 'regarding databases, individualism is much more revealed' (MM). Indeed, individualism is more revealed because it might, at times, clash with the collective ideologies inscribed in the database.

'Friction' is the term used by one of the staff members currently in charge of the database to describe this state of affairs (GC). But this friction is not only due to the sociologically classical tension between the individual and the collective order. It is also due to the fact that through the database distinct social worlds are entwined. It has been argued that networked information can cross the social and technical boundaries across epistemic communities (Van House 2002:232-2). Not only might individual and collective interests conflict; as we have seen, the differences between work and leisure, between public and private, as well as a 'digital divide' are articulated through the Museum's databases.

What also gets articulated is a difference between 'science' and 'people' which Wynne (1992:299) has put in the following terms:

> [T]he implicit moral imperative driving science is to reorganise and control the world so as to iron out contradiction and ambiguity, [...] a moral prescription which may be legitimately rejected, or at least limited, by people. They may opt instead for a less dominatory, more flexible and adaptive relationship with their physical and social worlds. In this orientation, ambiguity and contradiction are not so much of a threat, because control and manipulation are not being sought or expected.

In this sense, the Museum's database is a boundary object, since it has slightly different meanings to different people. For the Museum it means control, it represents a dream to be able to represent nature truthfully and holistically, and it plays a central role in environmental assessments. For the collaborators the database is less central to their activities (only 8 out of 90 questionnaires mentioned the database as a benefit in the collaboration with the Museum), and they require a flexible relationship with it. There are, nonetheless, shared understandings and practices regarding the database without which there could be no co-production of knowledge. Since the database contains many data stemming from the collaborators, it has become to a certain extent a boundary object where information from different epistemic communities can be brought together. Doing so, data crosses certain boundaries: from private to public, from leisure to work, from individual to collective. As we have seen, these boundary crossings are not always smooth.

6.7. Databases as boundary objects

Amongst the four types of boundary objects described by Star and Griesemer (1989:410-1) two resemble databases. First 'repositories', which they see as ordered piles of objects which are indexed in a standardized fashion such as a museum or a library. Second 'standardized forms', the methods of common communication across dispersed work groups. Since digital libraries have been theorised as boundary objects (Van House 2003) what about the Museum's databases? To what extent are they boundary objects?

Most of the collaborators do not use the database software and many of them are in the process of learning how to use it. Although the Museum has distributed 50 copies of the Recorder software to the collaborators, many of them haven't installed the database on their computers yet. Amongst those who have, many still have problems using it. At the time of writing this PhD, there were around 20 collaborators actually using Recorder to transfer data to the Museum. Thus, the database is not fully a boundary object. It only partially functions as a boundary object: it is partially used, partially understood, partially distributed.

Maybe the database is *not yet* a fully effective boundary object. The database might become such an object in the future when all the collaborators possess the technical skills to use it, or when there will be a 'light' version available online; in brief, when the database evolves in a way that is suited for both parties. But it could also not become a functional boundary object.

A point to make here is about the temporalities and complexities of boundary objects. Although Star and Griesemer offer a useful concept to think about the collaboration between amateurs and professionals, their boundary objects exist a bit too easily and are too readily available for use. Star and Griesemer look at boundary objects as a product, but do not analyse the processes of making them. There is no mention of all the efforts,

conflicts and socio-technical difficulties that shape the making of such objects. But a database is something made as much as it is something in the making.

Aligning and integrating datasets is not an easy task. One way to manage some of the complexities of coordinating the collaborators' activities, is to allow a more flexible adherence to coordinative procedures and protocols (Spasser 2000). Decentralising, decomplexifying and personalising the database are means to increase its mobility, usability and plasticity. Still, building and managing a database is an ongoing and ambivalent task, not a process that can be 'closed' - translation and enrolment are never finished (see Van House 2002:238). This - especially, if actors refuse translation, if they resist, withhold - might look, *to some*, like a form of betrayal.

Another point to make is about the distributed character of boundary objects. When analysing the Museum's database we cannot consider the database in isolation, nor the ensemble database-plus-user only. We have seen that there are some more intermediaries between the collaborators and the database as those who seize data are not necessarily the collaborators themselves. Also, there are scripts, which can be more or less codified. The Museum's charter is one device that turned a rather 'grey' script into a more regulated, more 'black and white' script, allowing more formalised relationships between contributors, users and managers of the database. A database is an ensemble which combines technologies, objects, people, and scripts. By bringing these elements into a relation, databases cut across many kinds of boundaries.

Science studies, especially ANT, have told us to do away with dichotomies such as social/technical and human/nonhuman. Databases are, then, not to be conceived as merely technical devices but as hybrid collectives where humans and nonhumans are drawn together. For Haraway (1997:12), a cyborg figure such as the database is 'the offspring of implosions of subjects and objects and of the natural and the artificial'. Others have argued that 'as far as the *computer database* is concerned, there is very little difference between humans and nonhumans' (Bowker and Leigh Star u.d.:29, emphasis theirs). Thus a database is a boundary object as much as is a boundary subject. A database is an amalgam of the technical and the social, where designer and user,

scientific dreams and messy realties, discourses and materialities, complexifications and simplifications are woven together.

Chapter 7:

The texture of boundaries and the nature of partial connections

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In Gieryn's *Cultural Boundaries of Science* (1999) boundary-work is performed as rhetorical and discursive practices – a very restrictive use of the term cultural. However, Gieryn's notion of boundary-work does not deal with heterogeneity and materiality (Michael 2002:370) and it is this shortcoming I want to address in this chapter. I will unpack the notion of 'cultural' boundaries by breaking it up into more meaningful and manageable parts. In order to reveal the 'stuff' of boundaries and boundary-work, I focus in particular on how the boundaries between amateurs and professionals are constructed, made, blurred and crossed through temporal, spatial, and material practices. In this way, the heterogeneity and materiality of boundary-work can be made visible.

Breaking boundaries into such distinct elements – time, space, and materiality – is somehow artificial. This typology is constructed in the sense that I subdivide boundaries into elements that we should not (keep) separate. Time and space, for instance, are not separate from each other but are fused into a time-space (Murdoch 1998, Urry 2000). Nonetheless, breaking up boundaries into these constituents makes sense for three reasons. First, the actors themselves have articulated them in these different terms. Second, it will allow me to get a better sense of the substance of boundaries. Third, while reassembling these different elements at a later point in the chapter, we are still left with a more fine-grained understanding of boundary-work consisting of different yet interconnected processes. Similarly to the 'cloth of globalisation' (Cohen and Kennedy 2000:139), we can talk about the 'cloth of boundaries', consisting of component strands or threads woven into a multicoloured fabric.

This penultimate chapter is structured as follows. In the first three sections I will look in turn at the temporal, spatial and material features of boundary-work. Finally, I will explore a theoretical space between boundary-work as expansion, expulsion and protection of autonomy, that is, between exclusion and inclusion. In order to do so, I particularly theorise how actors are partially connected (Strathern 1991).

7.1. Temporalities

In this section, I examine how the boundaries of science are articulated as temporal matters.

Amateurs have less time, professionals have more time to do science – this is, in essence, the common answer from the Museum's collaborators. One collaborator argued that the difference between amateurs and professionals has to do with

> the frequency, with the reality of that collaboration. Where one could now say, well, amateurs are the ones who only sometimes have to do with the Museum, professionals are those who have much more often something to do with it (AF4).

In their own words, collaborators possess less time because they must 'sacrifice leisure' (comment from the questionnaire). A 'voluntary has to nibble on his residual and spare time' (comment from the questionnaire). That amateurs have less time than professionals and/or that collaborators have less time than Museum staff is due to a difference between leisure and work. Roughly put, amateurs spend leisure-time and professionals spend working-time. As a consequence amateurs don't possess as much time for their scientific activities as professionals do. The difference is thus a quantitative one of clock times but also a qualitative one of social times (see Sorokin and Merton 1937): different quantities of time are available to collaborators and staff members, as well as different kinds of time (leisure-time and working-time).

Yet in practice, the boundary between leisure and work is not always clear-cut. One collaborator explained:

> But I also do it during my leisure time – the little I have got. I continue straightforward. I don't actually see when I do work and when I don't work (JC).

Likewise, one collaborator explained that he did his field observations before or after work or during weekends yet

> the evaluation of those things is then done during working time. So that it is partly during my leisure time and partly during my working time (PL).

One collaborator wrote about the time he invested into research activities: 'cannot estimate, because it overlaps with my job' (comment from the questionnaire).

Thus, we should not regard as an absolute the opposition between amateur/leisure-time versus professional/working-time. For those collaborators whose scientific activities relate more directly to their work, the difference between leisure and work seems to be more difficult to make.

For two collaborators in particular, temporal boundary-work was problematic. Since the beginning of the 1980s, these two collaborators were detached from a high school to the Museum to do research. In other words, their home institution (the high school) allowed them to undertake research activities at the Museum for a few hours every week while still paying them as usual. But in 2002 the government decided to abolish these detachments (see Chapter Three) and the two got quite frustrated. The time they could dedicate to research was reallocated to teaching activities at their home institution. If they now wish to carry out research, they have to do this on top of their workload as a teacher, that is, during their leisure time. On the one hand, they could earn more money since they would get paid for their teaching load as well as for their research activities, but on the other, they complain about a lack of time for research since they have to liberate time from elsewhere. 'We want time, not money', one of them complained (CM). The other explained: 'I now receive a pay I never asked for and that I don't need. I asked for one thing only, and that's the only thing I need: time' (PD).

The story of these two collaborators reveals two issues. First, what the Luxembourg government primarily decided in order to demarcate the emerging university from high schools also affected research at the Museum. The professionalisation of research and the reconfiguration of the boundaries of science in Luxembourg manifest themselves at the level of the Museum. The temporal boundaries of science have shifted to generate a reconfigured time frame for scientific practice in Luxembourg. Second, time represents an important resource and also operates as a medium of social meaning for science (see

Hassard 1990:5, 14). It is a resource since practicing science necessitates time and a lack of time impedes work and creates frustration. Time also functions as a medium of social meaning since it informs the collaborators' and the staff members' definitions of scientific work, identity and activity. The collaborators' and staff members' way of referring to time indicates the kind of attachment collaborators hold to the Museum. It has been argued that the partiality of one's involvement in a role is very often defined in temporal terms (Zerubavel 1990:169). In this sense, having less time and only sometimes doing science means being partially connected to science.

Apart from the formerly detached collaborators, the Museum works - to simplify a bit - with two kinds of actors: collaborators who are unpaid, and contractors who are paid for their work. Among the contractors there are organisations such as *bureaux d'études* which carry out environmental assessments for the Museum. Contractors and collaborators are handled differently. As one staff member explained:

> Somebody who is a contractor of mine has got a date. The 31st of December everything has to be in. This he knows right from the start. But with a voluntary this is not the case (EE).

For contractors, deadlines are written down, sums of money are agreed upon, and the exact work to be done is defined. Here is an extract from the standard contract which both parties (the Museum and the contractor) sign:

> **Article 2: duration**
> The present temporally limited contract starts the ..... and ends the 31.12.200.....

When the blanks in these lines are filled in, the temporal frame of the activity is established. In total eight articles clearly define the temporality, the cost, and the kind of work to be undertaken. But my aim is not to analyse this contract in more detail. My point is that concerning collaborators there are no such formal and restrictive bonds. In a sense, I 'observed' an absence of such written and standard contracts between the collaborators and the Museum staff during my fieldwork. Only a couple of collaborators

- about five - do contractual work for the Museum. The vast majority of them, however, don't rely on such agreements. In general, collaborators are not paid nor given any deadlines by the Museum. Without temporal frames being agreed upon in black and white, the collaborators' activities take place in a more flexible timeframe. One collaborator underlined that he would even refuse projects with time limits:

> [to be independent], well, that's important for me. That's important for me. I just received from [the *Naturmuseum*] *Senckenberg* - I am a scientific collaborator there too - they sent me a huge package with [animals] from a nature-park reserve. And then I immediately said: 'Yes, if you give me the freedom to do things just like I did until now. Which means: no deadline. That I can do it at my pace, with my priorities. And that it is fun for me, the way I want to do it, and then, then I will do it for you' [...] If someone tells me 'Here, you have to do this in one year's time' then I say 'No, find someone else' (NS).

In a similar vein, another collaborator argued that being an amateur permits oneself to be 'less constrained by time' than professionals (DD). One staff member characterised amateurs as people with 'less time but without "dead-line"' (TH). A crucial difference between amateurs and professionals appears to be the different time constraints each are confronted with. Professionals are usually governed by schedules - a strong coordinating force that paces, phrases and slates work (Knorr-Cetina 1999:190-1).[32] They are bound to deadlines, that is 'socially instituted temporal boundar[ies]' (Palen 1998:25). The collaborators, on the other hand, work within a weakly structured timeframe; they have their own rhythms and their self-determined time constraints - 'Only the deadlines that one sets oneself' (AW), as one said.

Apart from disliking deadlines, it appeared that collaborators sometimes do not take advantage of all the financial support they could benefit from:

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[32] Yet, this does not mean that professionals necessarily stick to deadlines. During my fieldwork, I have seen staff members missing deadlines. To take but one example: the contributions to the Museum's book for its 150th birthday were handed in so late that the official celebration had to be delayed by six months. Neither the *conservateurs* (exept but one) nor the Museum director managed to finish their manuscript by the initially set up deadline.

> But as I said, it's not my intention, I don't want any money. Maybe I am some kind of an anarchist. I love my freedom. And I don't want to be [hassled] by administrative stuff [...] I renounce on certain things (Anonymous).

The collaborator who made this comment further added: 'Paperwork [...] No, I've had enough of that during my life. I'd rather renounce on money' (Anonymous). Another collaborator said: 'I feel ashamed to be remunerated [...] I cannot exploit the system' (DD).

Collaborators who say no to deadlines and money refuse devices that bond them to the Museum. In a sense, by not 'exploiting the system' they make sure that the 'system' cannot exploit them either. Doing so, they maintain their individuality and their independence in front of the constraints an institution might impose upon them.

For the Museum, money and deadlines are enabling inasmuch they help to produce a certain kind of work during a certain amount of time. More so, both time and money are essential constituents of the definition of professionals; in theory

> professionals are seen [...] as people who spend the majority of their working hours enacting their professional roles, roles from which they receive the bulk of their livelihood (Stebbins 1992:21).

For collaborators, however, deadlines and money can signify different things. For the collaborators they can be disabling since they limit or at least clearly frame their activities. This is why they might refuse them; to avoid to be fully enrolled. This refusal of deadlines and money bears some resemblance to the refusal of data transfer seen in the previous chapter. It shows, again, how collaborators might use resistive agency (Fox 2000) to maintain their independence and individuality and to avoid their freedom of action being hampered (see Rip 2003:431). Not only is the allotting of time to activities a demonstration of control over one's time and commitments (Palen 1998:27), it also manifests, I would argue, the making of 'collaborator time' and an articulation of this kind of time in opposition to 'professional time' or 'museum time'. (The same also counts for

money: by deciding whether they receive money or not and by articulating a certain indifference towards payment, collaborators retain control over their activities.)

Staff members and collaborators inhabit, then, different time regimes. Staff members have to stick to deadlines, their time is remunerated, and their future research is already shaped to some extent. They are confined by time, money and contracts. On the contrary, collaborators are not and cannot be forced to stick to deadlines, they mostly spend leisure time and they can plan their future research activities at will. Working partially outside of the Museum relieves of some temporal constraints the professional has to adhere to. In this sense, the museum without walls liberates from time (Malraux 1965:256) or, to be more precise, it liberates from 'museum time'. This liberation from time (constraints) increases the collaborators' feeling of freedom. As one of them declared:

> I do what I want [...] The only thing one can do against me, is steal my time. But during my leisure time, during *my* time, I do what I want (PD, emphasis in original).

The link between '*my* time' or 'collaborator time' and self-determination ('what I want') seems essential and has often been made by the collaborators in the interviews. Time belongs to the collaborators, not to the Museum. One of the drawbacks for the Museum is thus the difficulty to coordinate research projects when collaborators are enrolled:

> The disadvantage is that the work of a collaborator, compared to the work of a permanent staff member, can be planned less (PD2).

The presence of collaborators means that projects can only advance at reduced pace. As one staff member acknowledged: 'Then one simply has to ride at their speed and to ask for less than from someone who does it professionally' (JM). However, as another staff member argued, 'If we sometimes want to get forward a bit, we have to spend money and to buy people's time [laughs]. In this sense: time is money [laughs]' (GC). If the Museum wants outcomes to be produced faster, it has to pay people for their work. In other words, it has to 'own' people's time by turning it into working-time (see Zerubavel

1990:171). Doing so, the Museum produces a time, common and commodified, for science, for *its* science. This kind of institutionalised and objectified time stands in stark contrast to the collaborators' time, which tends to be more individual, gratuitous and subjective. To sum up, temporal boundary-work revolves around several but related issues: different adherences to deadlines, leisure/work distinctions, different quantities of clock time, commodification/gratuity of time, and different predominating orders (self/institutional). It is through temporal boundary-work that collaborators and Museum staff members produce, enact, and protect different kinds of times. And it is through these kinds of boundary-work that distinctions between amateurs and professionals are maintained or blurred.

7.2. Spatialities

As noted in Chapter Four, walls and doors turn the Museum into an enclosed and protected space. While the Museum's walls are robust and protect collections from natural and social threats, doors permit a selective access to buildings. They permit scientists, knowledge, and papers to be accumulated inside and order and information to be increased locally (Latour 1988:299). By being open, closed and/or locked, doors permit the distribution of knowledge, people, and objects differentially in a museum. For visitors the difference between the Museum's front stage and its back stage is clearly noticeable as they are only permitted access into some areas of the Museum. In the Museum they can walk through the spaces where knowledge is exhibited, where objects are displayed. The doors that would lead them to the spaces where knowledge is produced, where objects are stored and where things look a bit messier are closed to them.

Collaborators, however, have far greater access to the Museum than visitors. Some have swipe cards to enter the Museum's research building and one collaborator even has a personal office space in this building. With access to the Museum's 'private' space, collaborators can use photocopy machines, study collections, use instruments, make use of the Museum's postal service, etc. Moreover, they have access to the Museum's

computer network. Some have e-mail addresses and a homepage hosted by the Museum's computer server.

Collaborators can, more or less freely, move through the Museum and use its infrastructure. Also, in the boundary encounters described so far - the annual meetings, the biodiversity weekend, the publications, and the database - knowledge can be produced and integrated almost simultaneously by both collaborators and staff members. Because the aim of these spaces is to collect, accumulate, integrate, and compare data in order to create representations of nature, the collaborators' knowledge, bodies, and specimens are, then, found in the same spaces that Museum staff use.

Yet, in practice there are spatial differences between the Museum staff and the collaborators. These differences do not lie in which spaces the collaborators can access in theory - both possess a legitimisation card that gives them the same 'spatial rights' and both can come to the Museum - but in which space they use in practice, which space they produce, or enact. In the last chapter we already heard one collaborator who argued that the distribution map of species is in fact the distribution map of the collaborators. This means that the collaborators' fieldwork takes place close to where they live. As the following quote reveals, sometimes the private home garden is explored in great detail:

> There are places in the country that are badly explored and there are others that are rather thoroughly explored. The place that is most completely explored is my own garden [laughs]. Where I [...] found 141 species of bugs during the last years [laughs]. There I was most often (LR).

In this collaborator's garden even a new bug species was discovered. We see that fieldwork does not necessarily have to entail practice in nature 'out there'. It can take place in the bounded, private, and encultured space of the garden, 'right here'. Fieldwork is a placed activity (Kohler 2002), an activity strongly connected to certain places.

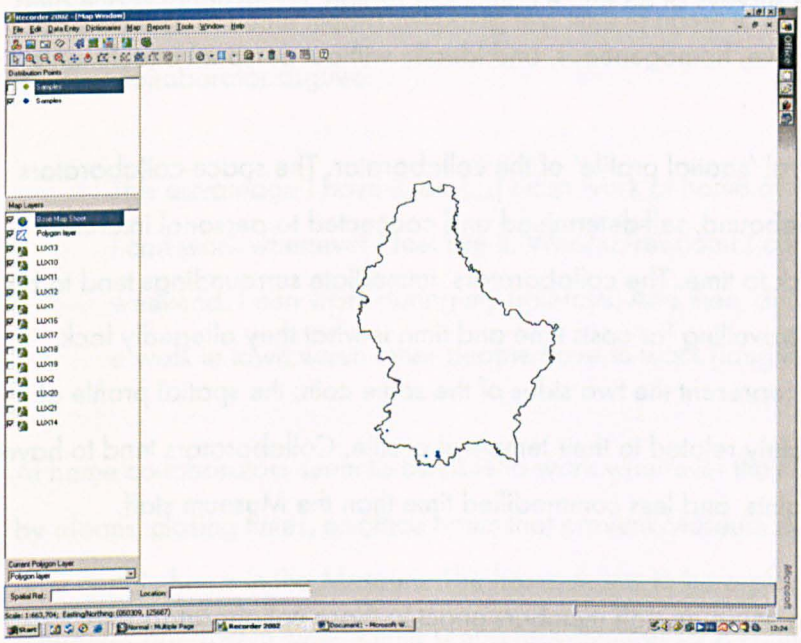
Two rather funny stories further illustrate this point. One collaborator told me that he always travelled by bus, by train, or by bicycle to get to his fieldwork sites because he doesn't own a driving licence. The taxa he is most interested in (Psocoptera, Hymenoptera, galls) are therefore best studied in areas where the public transportation system is best developed. Conversely, those regions in Luxembourg that are not easily accessible through public transport are not well documented concerning these species. Second story. As most collaborators do, one of them used to deliver lots of data to the Museum from an area close to his home. Strangely from one day on, he gathered data from another area too, quite remote from the one he usually went to. What had happened? The answer is quite straightforward. He was seeing somebody in this area: his future wife. As we see, the collection of data might even be connected to love!

These two stories reveal that the gathering of data, as any practice dedicated to produce knowledge, is situated, located and embodied (Haraway 1997). Data is not just collected. It is collected by human agents. Biological records not only reflect how species are distributed in space but also how the actors who produce those records are distributed – where they are situated on the map of knowledge, where they live, where they dwell, where they love. In this sense, every piece of data is in fact an assemblage of knowledge, emotions, interests, and capabilities; every point on a map is a map of its own. In other words, beneath every 'biological' point on a map a whole socio-technical map unfolds.

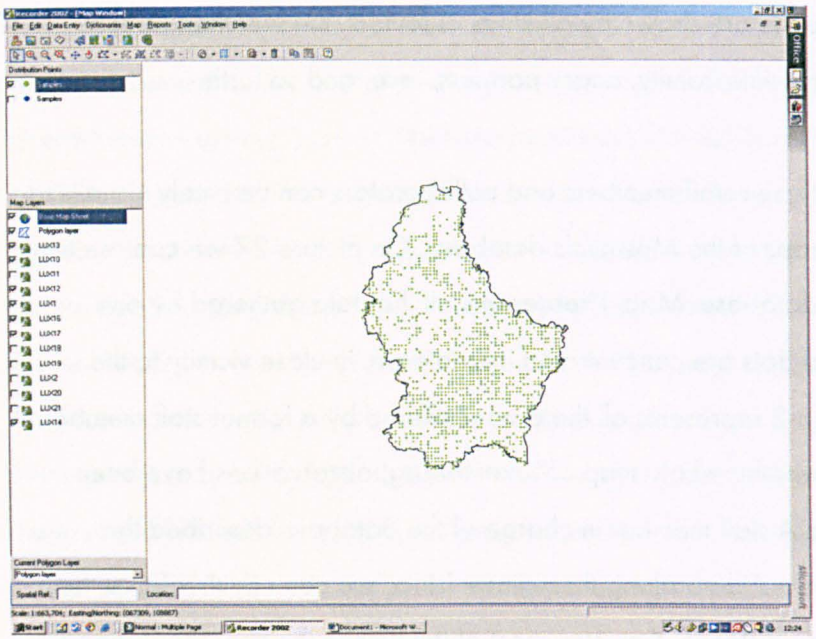
Is it true, then, that the knowledge-producing practices of fieldwork sciences are not bound to any delimited space (Golinski quoted in Withers and Finnegan 2003:336)? In theory the collaborators can do fieldwork whenever and wherever they want. *A priori* their field does not seem to be delimited – at least not by the Museum. However, national borders come into play when collaborators work together with the Museum. The articles they publish in *Ferrantia*, the data they transfer to the database, and the observations they make during biodiversity weekends are all framed geographically. In addition, fieldwork might be delimited by available time, by personal interests, by the weather, by the potential occurrence of species, and by the closeness to other interesting things. Thus, the knowledge-producing practices of natural history are very much bound to a delimited

space. The limits of this space are drawn by all kinds of factors: the weather, public transport, national borders, time, family, angry partners, love, and so forth.

The spatial difference between staff members and collaborators can be nicely visualised through the distribution maps of the Museum's database. On picture 27 we can see two maps from the Recorder database. Map 1 represents all the data gathered by one collaborator. On this map dots are concentrated in one area, in close vicinity to the collaborator's home. Map 2 represents all the data gathered by a former staff member. Here we see that dots cover the whole map of Luxembourg; observations have been done all over the country. A staff member in charge of the database described the localness of the collaborators' data along these lines: 'since we can't neither force them to do it, nor to drive a long distance, they stay most of the time in their neighbourhood' (GC). On another occasion he stated that collaborators have a 'limited radius of action' (GC).



Picture 27a: Map 1 (taken from Recorder)



Picture 27b: Map 2 (taken from Recorder)

Collaborator space is located, concentrated, homebound. Professionals, on the other hand, like to do complete surveys of an entire country or region. Professional space aims to be national, representative, homogeneous, and ideally without personal attachments.

We can establish a general 'spatial profile' of the collaborator. The space collaborators produce tends to be homebound, self-determined and connected to personal interests. This inevitably relates back to time. The collaborators' immediate surroundings tend to be better explored because travelling far costs time and time is what they allegedly lack. (Lack of) time and space represent the two sides of the same coin; the spatial profile of the collaborators is intimately related to their temporal profile. Collaborators tend to have less time, less time constraints, and less commodified time than the Museum staff.

If we compare the spaces Museum staff members enact to those collaborators enact there appears to be a difference between them – in terms of scale, density, and subjectivity. Staff members and collaborators tend to have different spatial and temporal profiles. Yet, these profiles cannot be opposed firmly, they rather represent 'ideal types'. These are tendencies rather than clearly distinct categories into which collaborators or staff members easily fit. Somewhat in-between the two there are all kinds of configurations: instances where the collaborators' activities are not restricted to leisure-

time only, where they receive money, where they are given deadlines (i.e. giving a talk at a conference), where they travel far to gather data. The same counts for professionals: they also often claim to be amateurs in some domains, they too do scientific activities in their leisure time.

Doing science at home

The majority of the collaborators who returned the questionnaire declared that they spend most of their time at home for their scientific activities - more than in the field and in the Museum. Working at home has several benefits. Quite obviously, collaborators can save time. As one of them explained:

> Like this it's easier. It takes me five minutes to go there [instead of] losing every day one hour for return (EC).

Doing science at home means spending less time to travel to the Museum. Second, as another collaborator argued:

> This advantage I have it too [...] I can work at home at 10 o'clock in the evening. I can work whenever I feel like it. What a freedom! I can [work] during my whole weekend. I can work during my holidays. And then, during the day, I can go for a walk in town when other people have to work [laughs] (PD).

At home collaborators seem to be able to work whenever they want. They are not limited by alarms, closing times, or office hours that prevent Museum staff members from working after certain hours in the Museum. The home seems to be a place where official schedules are absent, where time seems less dictated by a collective order. In other words, home is not a very institutionalised space and time; it therefore allows actions to take place in a more autonomous manner. According to Hinchliffe (1997:205) the home is 'a place where order prevails and time can stand still'. The home is delimited from the public, from the institution, and from temporal constraints. Home is where we can be ourselves: where there exists a centered subject (Hetherington 1997:192). This also because there simply are less people at home than in the Museum; as one collaborator

said 'At home [...] I am not distracted. In the Museum there are many people' (PD). At home, it might be easier to concentrate. Due to the rareness of interactions with the institution and its ordered times and spaces, the home is a place which permits a more or less own articulation of time and rather self-determined actions.

In the Museum, time, space and practice unfold in almost opposite ways: there are many time constraints; there are places to which staff members have to go to when they do surveys; and there are laws, contracts, notifications, codes of behaviours and agreements which regulate their activities. When the Museum and the collaborators work together, different spaces, times and practices are also brought together. The following quote points to the difficulty of aligning these three elements:

> That's also a problem I have got with [one staff member]. Because he never wants to understand. He says: 'you can do this in the evening'. He thinks that I do the things the way he does them. He always wants the things done his way [But] everybody has his own pace. I get home in the evening, [...] around seven or eight. I also have a family, and I cannot just sit down and seize thousands of data every day (Anonymous).

Since home is the place where collaborators can disconnect from certain daytime activities or duties, they might not necessarily want to do science in the evening. In the above quote the order and temporal autonomy of the home are disrupted (in theory) by the practice of seizing data into a computer. The contrast between spaces (Museum and home), times (on duty and off duty) and activities (scientific practice and family life) is articulated. On the one hand, the Museum wants certain attachments – to science, to computers, to time. For the Museum, the ideal collaborator would be someone who eagerly collects data in a standardised and computerised way, and who would do so almost permanently. But, on the other hand, collaborators can choose to detach themselves from time and computers and choose different attachments, such as spending

time with their family. In the above quote the conflict is resolved by the assertion of the interviewee that 'you can't' do it.[33]

'You can't do it' has probably been asserted by many of the collaborators' partners as well. On several occasions collaborators told me that their partners were sometimes frustrated because of their unusual and late activities at home. There seems to be a certain myth about the amateur who gets divorced because of his or her unconventional activities. One collaborator declared that this conflict resulted in a divorce, explaining:

> Until 2 1/2 years I was a very intense collaborator of the Museum. Unfortunately my divorce from my spouse got in-between, partly caused through this intensive work (Anonymous).

Like amateur theatre practitioners who also sometimes divorce because of their hobbies (Stebbins 2004:110), the collaborators' scientific activities at home sometimes create conflicts with their partners that might lead to split-ups. One staff member acknowledged:

> The family starts to grumble when you disappear behind the magnifying glass every day and evening. This can even lead to serious family problems [laughs] [...] And the wife or the husband will say somehow 'hey, I also still exist'. And there we have reached again the limit (GC).

We see that the home is not a totally unconnected space where time and actions are independent. Although actions might be unconnected to work, they are connected to other things such as the family.[34] The home is a place where one reconnects.

Nonetheless, most of the collaborators' homes are somehow connected, let's say partially connected, to the Museum and to science. The degree of connectivity is variable, ranging

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[33] Nevertheless, this does not mean that data stemming from this collaborator does not end up in digital form in the Museum. The problem has been resolved in two ways: in the past, the collaborator hired someone at his own costs to seize data and, second, the Museum has entered his data with the help of a temporally employed person more recently.

[34] In Luxembourg, the principal limits for volunteers to practice their hobby is 'the difficulty of finding time' due to professional or family reasons (Lejealle 2003:8).

from those collaborators who are rather unconnected to the Museum - the so-called 'papercorpses' - to those who frequently and closely collaborate with the Museum. To describe some of the most active collaborators the former Museum director used the term 'private scientists'. One of these 'private scientists' described his working-space at home as follows:

> At home I have a laboratory which takes a whole floor in a big house. [I have] a library few foreign universities can compete with, people come from abroad to make photocopies. I have collections of which there are only a few in the world in this domain (PD).

The use of the term laboratory as well as the books, collections, and instruments that populate this collaborator's home turn it into a very professional space, not very different from any other laboratory. For two weeks, the quoted collaborator even turned his home into a kind of university space. He lectured two PhD students registered at a Belgian University both of whom slept, lived, ate, and studied at his home for two weeks. This remarkable story shows that the private home and the more public spaces of science, universities and teaching can be brought together in a single place. The collaborators' homes can be turned into 'border spaces' where 'local and distant contexts, amateur and professional identities, [...] and private and public spheres of life intermingl[e] in important ways' (Opitz 2002).[35]

This 'border space' is a remaining feature from scientific practice since its beginnings. In 17th century England, private residences of gentlemen were the most significant venues of scientific work (Shapin 1988:378). Also, in the early 19th century much knowledge about nature was produced by gentlemanly and gentlewomanly amateurs working in largely domestic environments (Outram 1996:249).[36] However, while Shapin (1988:404)

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[35] Opitz's (2002) analysis of Victorian country houses is useful here. In another sense, the home is also a border place in the history and sociology of science, since it has not been well explored in comparison to the laboratory or the museum. According to Outram (1996:253) little attention has been paid to domestic spaces in science.

[36] Although women were involved, the production of scientific knowledge was nevertheless gendered in that women were often excluded from scientific practice. At the Museum, around a third of the collaborators are women. However, no notable gender differences between male and female collaborators emerged in the data I gathered.

claims that the distinction between places of residence and places where scientific knowledge is made is almost absolute today, this is far from being the case for the collaborators. For collaborators, the place where they perform most of their scientific activities is their home. Moreover, they tend to actively protect their home by demarcating it from a collective order – an order that tends to prescribe spaces, times, and actions in an institutionalised manner. Collaborators produce scientific knowledge in a space that enables the unfolding of a more autonomous and centered subject; a subject con*centrating* time, space and action.

7.3. Materialities

Even when collaborators are not physically inside the Museum and work at home, even when they are not in direct contact with other people, they are still connected to the Museum. There is a connection made through objects. It is these technical and material connections that we also have to consider in addition to the spatial, temporal and merely 'social' aspects of relationships. The apparent remoteness between the collaborators and the Museum can be partly overcome by objects that travel from and towards the Museum.

The moving of tools and objects was clearly visible at the biodiversity weekend. At this event, when the collaborators came together, different materialities were brought together at the same time. Some of these were brought in by the collaborators themselves: many had their personal enhancing glasses around their necks, their pencils and notebooks in their pockets, their field guides in their rucksacks. Other things, like maps, were sent or given to them. Still other things were transported directly from the Museum to the nature-park in which the biodiversity event took place, such as the Museum bus, microscopes, books, nets, traps and so on (see picture 28).

Picture 28: Some of the material entomologists carry around in a car of the Museum (alcohol, traps, boxes, nets, ...)

By travelling across time and space, these objects can bring amateurs and professionals, institutions and individuals closer. Objects create and maintain sociotechnical links that connect disparate entities. Many objects are exchanged between collaborators and Museum staff including specimens, maps, books, magnifying glasses and boxes. The co-production of science across different epistemic cultures necessitates the simultaneous production of such 'immutable mobiles' - objects which are mobile but also immutable, presentable, readable and combinable with one another (Latour 1990). These immutable mobiles permit the collection of data to take place in an enlarged laboratory. The mobility of these objects enables 'action at distance' (Law 1986), thus permitting scientific discipline to be exported beyond the museum walls.

However, if objects cannot be turned into such mobiles, the co-production of scientific knowledge becomes difficult. The Museum's European Centre for Gravimetrics and Seismology, the only research unit of the Museum without scientific collaborators, can serve to illustrate this point. In this laboratory, experiments take place in an underground facility sheltered from temperature variations and vibrations. Even people are kept out when staff members perform

> experiments not allowing you to enter the laboratory. [...] There are certain types of instruments that need to be left alone and visits induct perturbations that will be a problem for these kinds of instruments and these types of measurements (NO).

This laboratory is packed with delicate, costly and heavy instruments, for some of which there are only six across the whole world. These complex machines differ from the kind of materialities used by the collaborators. The complexity of these machines, so it is argued, keeps amateurs away:

> The projects which are treated here are perhaps too technical, needing perhaps too significant means (NO).

A seismology laboratory, just like a laboratory dedicated to high-energy physics or genetics, seems to be out of reach for amateurs because of the technicality and the inherent complexity of the science produced in these spaces - arguments are frequently articulated along these lines.

But to explain the absence of amateurs we can put forward another argument. Some objects, such as weighty, expensive and delicate instruments, cannot travel very far. Their mobility is clearly limited compared to the rather small and widespread tools amateurs frequently use. As a consequence, some objects cannot populate the boundary space in which both amateurs and professionals could co-produce knowledge. Yet, the co-production of science necessitates translational efforts to produce boundary objects. If no such boundary objects can be created, only 'non-boundary objects' or 'immutable immobiles' populate the laboratory, that is, objects that don't have the abstractability and mobility of boundary objects. This, in turn, clearly maintains a boundary between the inside and the outside of a laboratory.

On the other side, natural history practices can more easily take place outside the laboratory and the Museum. Here is the Museum director talking:

> A botanist, an ornithologist, a mammalogist, a herpetologist, needs in principle [...] a binocular, a magnifying glass, a couple of field guides, a wind jacket and boots to go to the field. And then he can in principle start working (GB).

Similarly, a staff member argued:

> An amateur in population genetics does not exist. [...] An amateur cancerologist does not exist either. That's it. However, an amateur entomologist does exist. And what does he need? A magnifying glass. And alcohol [laughs]. We can provide him with that (GC).

Natural history fieldwork requires only a few tools. And these tools are cheap, handy, and rather ubiquitous, in short, quite mobile. Especially 'proto-instruments' (Callon and Rabeharisoa 2003) such as cameras, written accounts, and letters, are easily available and transposable and enable formalising and publicising knowledge. These objects and their significant mobility enables the crossing of the boundaries of science. In other words, the transportability of scientific tools and material is one factor helping to explain why some scientific fields are more open to amateurs than others.

Seismology is not only more 'technical' than other scientific domains. A seismology laboratory is a repository of knowledge (Knorr-Cetina 1999) where knowledge is highly embodied in particular skills and connected to specific techniques which are, to put it simply, heavy. The money, degrees, techniques, knowledge and scientists that are put inside a seismology laboratory to perform experiments remain strongly attached to this very confined space. The history of most sciences is that of an extreme confinement that sets laboratories and instruments out of reach of the amateur and the layperson (Callon et al. 2001:65). In a sense, the technical and spatial distance between laypersons and science increased through the history of science. The strength of the laboratory - its precision of measurement and the elimination of interferences - has necessitated that experiments be done in a private and confined space (Callon et al. 2001:72).

Such a confined space makes it difficult for amateurs to be enrolled into the co-prodution of science. On the one hand, it is difficult to enter and move within such a confined, dense

and weighty space and, on the other hand, the materialities and sociabilities of this space cannot be transported outside. This is quite contrary to entomology or ornithology, for example. In such fields scientific practice is related to 'lighter' materialities. A field guide or a magnifying glass can easily be transported and the vision of the collaborators can easily be 'disciplined' through experience. The field or the home can be turned into a scientific space without much money, degrees, and technical equipment. Amateurs can quite easily co-produce scientific knowledge if, on the one hand, the spaces in which they do so are 'light' and 'movable' – if, in a sense, they themselves can move these spaces around – and, on the other hand, if the materialities and sociabilities of the inside of the Museum can be transported outside without too much cost and loss.

Apart from technologies and objects, there are also persons moving between the collaborators and the Museum. A collaborator from the Museum's palaeontology section explained:

> Collaborator: This is also partly my role, to act as an intermediary between the institution, the state, the Museum, the scientific interest to deposit the specimen and, then, the collector, the discoverer [...] It is true that there are two worlds: the Museum and palaeontology and the amateurs and palaeontology [...] Knowing that I am not official, they are more open and they will tell more about what they expect and what they do not expect on this matter.
>
> Morgan: Have you been asked, the people from the Museum asking you: 'Could you not try to approach this person?' where they have some troubles.
>
> Collaborator: Yes, yes, there sometimes is this kind of rapport. It does happen. In both ways, as there are collectors who have put themselves on bad terms with the Museum for issues about specimens, points of view, or competition and where you try to recreate links [...] Sometimes he has broken some links. And there I can play a little bit on this [...] It is a bit a role of intermediary (DD).

There are also persons travelling between the amateur and the professional worlds, persons who act as bridges across boundaries. Collaborators can be in a privileged

position. They can be trusted from both sides, facilitate exchanges and recreate broken links. In short, collaborators can act as '"brokers" between amateurs and professionals' (Lankford 1981:299). In the above quote, the interviewee's brokering was similar to that of 'roamers', of 'people going from place to place, creating connections, moving knowledge' (Wenger 2000:235, see also Wenger 1998:108-110). There are thus not only boundary objects travelling between the amateur world and the professional world but also 'boundary subjects' (Helmreich 2003:438). One staff member held: 'there is no limit. There are precisely these persons who make the frontier between the two' (SP). More than this, it is persons and objects that make and unmake the boundary between the world of amateurs and the world of professionals.

7.4. On partial connections

So far, this chapter has discussed how the boundaries of science are maintained and crossed through temporal, spatial, and material practices. Empirically and analytically we can subdivide boundaries into constituents like these, not least since the actors themselves have articulated them as such. This has enabled us to shed light on the substance of boundaries. The boundaries of science are not only negotiated in discourse but also through material processes. Boundary-work is interconnected with objects, tools, bodies, and specific places. As science is a material practice 'in action' – a cultural and embodied activity practiced in space, in time, and through the use of objects –, the cultural boundaries of science are revealed through spatial, temporal and material processes. As these processes are intrinsically interconnected, boundary-work is a heterogeneous process.

Collaborators 'in action' are temporally, spatially and materially partially connected to science. When they do science, where they do science, how they do science and with what tools they do science is what might differentiate collaborators from Museum staff members and amateurs from professionals. Time, space and materiality come into play when identities are constructed, when they are compared, opposed, and entangled. Identities are, then, located in space, articulated in discourse, related to materialities, expressed as temporalities, and situated as practices. Defining oneself as an amateur or

a professional is not merely a human attribute; it is about situating oneself in relation to these interconnected elements.

Time, space, and materiality come into play when connections are made, maintained, nurtured, or disrupted. What we can see, then, is the nature of the connections linking collaborators and Museum staff, amateurs and professionals. In a sense, collaborators are on the 'periphery of practice' (Wenger 1998:117) and there are, for them, 'multiple, varied, more- or less-engaged and inclusive ways of being located in the fields of participation' (Lave and Wenger 1991:35-6).

The 'broken links' mentioned in the previous section point to a certain fragility of the relation between the Museum and its collaborators. The Museum recognizes the need for carefulness. Staff members explained: 'we have to care for them and heed them' (GC), 'contact-care is very important' (EE), '[one] has to deal with them very tenderly' (SB). One staff member declared that they (the staff members) were paid to 'keep [the collaborators] in the bosom of the Museum' (EE). She further said:

> I think you have to have it on the *Fingerspitzengefühl* how you react there. How you can motivate people and keep them going (EE).

The term *Fingerspitzengefühl* was also used in relation to the review process of the Museum's journal. While the English translation of the term is 'intuitive feeling', the German term suggests a more tactile image: the literal translation would be 'feeling on the fingertips'. In a same order of ideas, staff members often mentioned that they had to put on gloves to deal with some of the collaborators. The vocabulary used to describe this nursing and caring of the collaborators reveals the fragility of the connections. Since the connection is not permanent, nor strong, but rather fragile it has therefore to be nurtured and cultivated with care. More so, *Fingerspitzengefühl* indicates that the relation cannot be rationalised, formalised, or institutionalised but that it has to be dealt with individually and specifically. How Museum staff deal with collaborators requires knowledge of people and sensitivity rather than the ability to handle 'de-humanising' devices such as time-schedules, money, and contracts.

Although the 'weak ties' (Granovetter 1973) between the collaborators and the Museum seem to be rather fragile, they also have strengths. For the collaborators, the advantages are that they are not strongly tied to strict time-schedules, remote spaces and predetermined activities. For the Museum, however, this makes enrolments and alignments difficult. To fully realise its 'panoptical dream', the Museum would have to lock the collaborators into clear positions, discipline them, prescribe their gestures, regulate their times, etc. (see Foucault 1979). The Museum would have to make sure that disciplinary techniques would control bodies, practices, materialities and times in strict ways. But for the collaborators this would be, in a sense, a 'panoptical nightmare'. The difficulties to align, to enrol and to manage some of the heterogeneities that result from the co-production of science by amateurs and professionals can shed light on how science, in general, is made.

Science is made by bringing actors into a relationship. Science is made by connecting humans, technologies, times, spaces, materialities, identities, etc. When the connections between these elements are strong, the production of knowledge becomes relatively easy. Professional science could be seen, in this sense, as the mutual disciplining of specific actors and their relationships, that is, of actor-networks. However, partial connections between actors make it more difficult to produce science.

To study amateurs, one needs, then, to move beyond the simple *a priori* assertion that there is one group of persons that can be called amateurs. Instead, it is through the study of connectivities, through the examination of how humans are connected to other actors, that one can get a better grasp of the identities, practices, and roles of amateurs. This means considering amateurism not as an essence, but as relationally defined through connections and demarcations.

Nonetheless, it does make sense to talk about amateurs and amateurism as there are people and practices that fall in this category. But there are many who do not fit into the neat amateur box. There are many who could be called 'amateur-experts', 'lay-experts', or 'partial scientists'. Too often, I would argue, such hybrid identities are taken to be reason enough to argue that the boundary between amateurs and professionals is obsolete. From particular cases where boundaries are crossed, the generalisation is often hastily and simplistically made that there is in fact no boundary at all. But in some

instances, there is still a clear boundary. The picture is more complex than that often made. Neither is there an immutable clear-cut boundary between both nor is there no boundary at all. Neither should we imagine a space that encompasses amateurs and professionals, nor should we imagine two different realms. The fact that two spaces intersect is not reason enough for claming that amateurs and professionals belong to the same social space. Still, the fact that two spaces can be distinguished does not mean that they should be totally separated. But how, then, can we think about such complex cases? What else can we say other than that we are faced with complexity? I face my own conceptual boundaries when trying to answer these questions. This is where I struggle most, where I feel most vulnerable, where I feel that I cannot really picture the solution. But I'll try to do so, starting from a common phrase found it science studies: 'more than one but less than many'. Yes, amateurs and professionals belong to more than one space but less than many spaces.

Let's try a short detour. Let's think about the relationship between the 'thing' I study and this study, between the actual situation in the Museum and my written account. While examining and writing about how amateurs and professionals co-produce science, I have done many translations: I write about them; I transcribed and translated accounts; I did so in only 75,000 words; I now disseminate my research on sheets of paper. Doing this, I selectively write about some of the events I was faced with – all the events could not fit. (These were all necessary translations to be able to produce an informed, academic, coherent, sensible, defamiliarising and analytic account.) Nonetheless, the thing I study is not identical to this study. Put crudely, 'reality' does not fit into a thesis. However, this does not mean that this thesis or any other thesis is unreal. It is, in a sense, a different reality; a sociological, analytical, experience-distant reality. Luckily, a sociological reality can fit into a thesis. My argument is, then, the following one. If someone would compare this thesis to what he or she sees as the 'real' or the 'full' picture, he or she would probably see two things: differences and similarities. It would be difficult to say that this thesis does not relate to 'reality' and, equally, it would be difficult to say that this thesis is 'reality'. In other words, if we would compare this thesis to the 'real' world, we could say that these are more than one thing, but, still, less than two things.

In a very similar way, I would argue, amateurs and professionals belong to more than one and less than many spaces. Another way to put this, is to think of amateurs and

professionals as being partially connected. But when using ANT, it is difficult to make sense of such partial connections. Connections and processes of networking – especially what we could perhaps call 'partial networking' – are relatively weakly theorised within ANT. Yes, actors are networked and enrolled, and humans and non-humans, for example, can be connected to each other in very similar ways. Yet, actors can be differently networked, differently connected. There are, to say the least, weak and strong connections. Some of the collaborators I examined are weakly linked to the Museum, whereas others are very strongly attached to it. As the collaborators are not connected to the Museum in the same way, it is problematic to call them 'one' or 'an' actor. The picture is even more complex as between strong and weak ties there are potentially all kinds of ties. When using ANT one risks treating connections equally and overseeing their variability. There is not much room for questions like the following ones: How well are people connected? What leeway is there, how much room is there for resistance? If people are connected, how much room is there to remain disconnected? In what sense is betraying a positive thing? At the margins of a network, how are relations played out?

One of the benefits of ANT is that it can draw our attention to the performativity of connections: that it is through relations with others that actors come about. But ANT might blind us to see differences between connections, between types and degrees of attachments. More than flattening and obscuring distinctions between the entities which comprise networks – one of the critiques commonly made to ANT –, ANT also flattens the distinctions between connectivities, between different types of attachments. While we might start by assuming a priori that all connections are equal, this should not stop us from recognising and analysing how much these connections might differ. The partiality or strength of connections, which inevitably shape the construction of scientific knowledge, is important to take into account. As we have seen, collaborators are rarely fully enrolled, connected, linked, assimilated; but neither are they completely disconnected, nor do they betray. It is this ambivalence, marginality, and partiality of attachments that is hard to grasp when using ANT.

Yet, how can we think about such ambiguities, about people partially connected, about people on the periphery, on the margins? How can we make sense of different degrees of partiality? I feel that I have to leave the principle of generalised symmetry but I don't

know what principle to embrace instead. I have to leave this one space of similarities, but I don't want to arrive at another space. I cannot come up with an alternative. I cannot come up with a second space, a space of only differences between actors and connectivities. I am caught in-between.

Or am I not stuck at all? And maybe this is one of my findings? What would that finding be? Maybe the finding is how I see things now. While I started off my PhD rather embracing ANT, now I can see nothing but partial connections. I am partially connected to ANT, this thesis is partially connected to the 'real' world, ethnographers are partially connected to their field, amateurs are partially connected to science, translation is not to betray but to partially betray. And, outside this thesis, I see PhD students as being partially connected to their department and to a scientific community. And all these connections are partially different.

Chapter 8:

A museum without walls?

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At the end of an interview with a scientific collaborator I was asked a nice question. The interviewee wondered how I was going to make something 'big' out of the rather 'small' case I was examining; what I would have to add to my study of the relation between the Museum and its collaborators to elevate it to the level of a PhD thesis (interview with BS).

8.1. Pre-conclusion

Before coming to the conclusion itself, I want to briefly reflect on this question and expose how I have connected a 'small' case study to the 'big' world of, say, academia. I have tried to do so - and will be doing so in this conclusion - in various ways:

- I formulated research questions at the beginning of this thesis. How is science co-produced at the Museum? How are the boundaries between professionals and amateurs made and unmade? These questions were already theory-laden and relatively abstract and they permitted me to explore more generalisable issues. Focusing on boundaries, for instance, may generate theoretical insights into a range of general processes (Lamont and Molnar 2002:168).

- The methods I used in this thesis are based upon a pre-existing body of methods. I have emphasized the messiness of methods and their interconnectedness with theory, personal experience, and requirements imposed by the field.

- While referencing the works of scholars, I mobilised the past, the known, the already written. I make this work bigger through relating, connecting, and comparing it to other work. This thesis does not float in an empty space, but it is connected to sociology and to STS.

- Throughout this thesis I did not only present material, I was also 'speaking from' the material (see Macdonald 2002:246-7). I did so by analysing and theorising the material. Theory involves going beyond what we can see and measure (Jenkins 2002:31-8). I defamiliarised the familiar by 'adding theory', decontextualising, transforming a portion of the world. In addition, I would argue

that I also defamiliarised existing theories by testing them in new contexts, by suggesting connections with other theories, by criticising them, by moving beyond them.

- While producing this thesis, I presented drafts to my supervisors, papers to academics at conferences, and I had stimulating discussions with peer students. Great parts of this thesis have been presented and discussed in an academic environment and have been constructively criticised.

- Representation is another key element to make a case travel beyond its local contingencies. By reducing information to more manageable dimensions, representation can be eased (Lynch 1990:181). All the 'presents' I observed have been represented, or translated. They have been put into words, they have been translated into another language, they have been connected to theories, etc. What I present in this thesis is only partially connected to the 'presents' and 'heres' I was faced with.[37]

- In this concluding chapter I will also be suggesting routes for further investigations – another strategy to make academic work even bigger. By asking further questions and by pointing to areas that need clarification and rethinking, scholarly work tries to increase its frame. It suggests the yet-to-be-written. It connects 'here' and 'there', the known and the unknown, the past and the future, that is, it mobilises something more than present in a text.

Questioning, framing, methodologically inquiring, referencing, analysing and theorising, representing, suggesting further work – these are all strategies to make a case bigger. These are all strategies that enable the crossing of boundaries. And, especially the final chapter of a thesis, usually a conclusion, tries to do so by making many generalisations.

[37] A better word for 'representing' might be 'transpresenting', since I do not present again, but do so differently.

Yet, concluding and generalising is always problematic because, if we simplify a bit, it is based on two kinds of movements: scaling down and scaling up. A conclusion is based on research that has been done on the world scaled down: the world simplified, de-complexified, a portion of the world. This is an absolute necessary move. Scientists – be they social or natural – have to set out a certain frame, they have to investigate a topic within spatial, temporal and cultural limits. For framing enables a better exploration, handling, manipulation, and questioning of this topic.

Framing eases concentration.

But a conclusion also tries to scale up. It tries to move beyond a specific context. A conclusion aims at being able to say 'in general, we can argue that... '. A conclusion has, then, to do a balancing act. On the one hand, it has to acknowledge that some things remain local, typical, national, confined. Some things cannot be transported but they stay within a case, they remain *a* case. On the other hand, the format of a conclusion pushes one to make findings travel. The essence of a conclusion can be nicely expressed in actor-network theory language: a conclusion has to draw things together and it has to press translations; it mobilises and stabilises heterogeneous sets of ideas and, after that, tries to increase their mobility. While translating, a conclusion always risks (partially) betraying; a conclusion is ever partially connected, fragile, and hybrid.

In this conclusion I draw things together around four themes. I first consider the 'Luxembourgishness' of the science produced at the Museum and discuss the usefulness of the 'cross-border worker' image to further explore science. Second, I review and theorise the different practices of boundary crossings that have emerged throughout this thesis. Third, I examine the materiality, permeability and complexity of boundary-work. In the final part I reflect on natural history as a democratic model for producing scientific knowledge and on the place natural history museums can occupy in the contemporary and future topography of science.

8.2. Articulating Luxembourgishness

Throughout this thesis Luxembourg surfaced in many forms. Luxembourg materialised as historical features, geographical borders, multilingual forums, and societal characteristics. These national boundaries and characteristics influenced and framed scientific practice.

The historical contingency of science in Luxembourg surfaced as three related features: the late professionalisation of scientific research, the drawing upon amateurs to produce scientific knowledge and the absence, until recently, of a proper academic environment. This history still frames scientific practice in Luxembourg today – boundaries are, after all, the product of history (Vandermotten et al. 1996:267). We have also seen that knowledge produced at the Museum is geographically framed. National borders confine talks at annual conferences, contributions to the Museum's journal, data gathered at the biodiversity weekend and data transferred to the database. What the Museum and its collaborators produce is a natural history *of Luxembourg*, they create knowledge about nature within a bounded space.

While being bounded by geographical limits, the making of natural history knowledge of Luxembourg also happens in a distinct linguistic environment. The 'Luxembourgishness' of knowledge is not only visible through the place where it is made, but also through the languages in which it is expressed. The history, social organisation, and linguistic repertoire of science are certainly distinct in Luxembourg. In particular, the flexibility within a multi-linguistic repertoire is a commonality of the places of encounter between amateurs and professionals, which sets them apart from other (monolingual) countries and also from professional – most of the time English-speaking – sites of knowledge production. The specific languages used and the switch between them in conversations, meetings, and journals is one of the characteristics of the Museum in particular, and Luxembourg in general. History, geography, and languages set out some of the boundaries in which the production of science takes place.

We have seen in the third chapter that the import of foreign elements was necessary for Luxembourg to prosper the way it did. Still today, the society of Luxembourg depends very much on external human resources, the so-called 'cross-border workers'.

Luxembourg is thus frequently portrayed as an open economy. Interestingly, these characteristics also materialised at the level of the Museum. In fact, the situation at the Museum and the situation in the whole of Luxembourg look very similar:

1. The kind of science made within the Museum is an 'open science'. Just as Luxembourg has an open economy, sustained through flows of money and people across national borders, the Museum's production of knowledge crosses cultural boundaries.

2. We can call those collaborators who live in territories distant from the territory of science 'cross-border workers of science' (or *frontaliers de la science* in French or *wissenschaftliche Grenzpendler* in German). Like cross-border workers who cross national borders and enable the economy of a small country to flourish and to compensate for a limited indigenous workforce, amateurs can travel across cultural boundaries and compensate for limited scientific capacities.

Generally speaking, cross-border workers are one of three types of migrants, the other two being permanent migrants, who decide to migrate permanently, and temporary migrants, who cross the border for a certain period only (Kondoh 1999:467-8). Cross-border workers, however, work in a neighbouring country but return home periodically. Amateurs also often leave their home country ('amateur-land') to enter the territory of professionals. The cross-border worker image can be useful to further explore scientific practice and communities of scientists, for two reasons. First, it has been argued that science amateurs can be especially interesting as their serious leisure informs us about work itself (Stebbins 2004:49) and that we better learn about 'science' through contrasts to 'non-science' (Gieryn 1983:791). It can thus be revealing to study science by focussing on boundary crossings. For an examination of the boundaries of science enables us, at the same time, to analyse science itself. Second, I would argue that scientific cross-border workers disserve more attention. In science studies the main focus has been on either 'permanent migrants' or 'nationals' but not on those who cross borders more regularly and more ambiguously.

Apart from the cross-border worker image I want to discuss another common image of Luxembourg, namely its situation at a crossroads. Historically, this image takes us back to

the time when the first settlements appeared in the country: Luxembourg emerged at the crossroads of three roman roads (see Kreins 2003:7). Even today, the country is frequently portrayed as being at the crossroads between Romania and Germania (Christophory 1994, Fehlen 2002). To this common way of depicting Luxembourg, I would add the various ways in which Luxembourg is 'caught in-between'. Not only is it geographically caught in-between two political and cultural heavyweights in Europe (France and Germany), Luxembourg is also historically caught in-between two moments. Its cultural landscape is caught in-between, on the one hand, a recent past with very few museums, an almost non-existent cultural policy, and low budgets dedicated to culture and, on the other, a clear will to 'put Luxembourg on the map'. And, now more than ever, Luxembourg is faced with a legacy of science producing amateurs, an uncoordinated and underdeveloped epistemic infrastructure through its aims to professionalise and internationalise its scientific landscape.

For the Luxembourg Museum of Natural History, one of the challenges is the management of crossroads; in particular, the management of places of encounter between amateurs and professionals. The challenge is that of organising spaces where roads can be crossed relatively easily, where knowledge can be co-produced quite smoothly by heterogeneous actors. Yet, this is not an easy task. On the one hand, the Museum is a museum with walls. Over time, the cabinet of natural history became a 'museum with walls' through the creation and delimitation of an official and architectural space for collections of natural history. The Museum was built through the making of material, legal and structural walls. And, it produces knowledge by drawing things together: by concentrating artefacts, knowledge and people in a singular place. In this place careers are regulated, collective work is scheduled and people are remunerated. The Museum is a place closed off by walls, by rules – it is a framed place. Although the making of a museum without walls is possible through common interests, collective projects, the making of boundary encounters, and/or the mobility of objects and knowledge, at the same time, the different objectives, resistive agency, and different articulations of space and time make this difficult.

For a social scientist this might prove challenging. It is the intersection of two social worlds, resulting from the interrelationships between amateurs and professionals, that can

be so puzzling. Maybe crossroads are difficult to make sense of since they stand for two things: they allow people to meet at a single time and place, yet those who meet can come from and go to different places. In this sense, aligning, centralising, and disciplining actors becomes difficult if the number of intersecting roads increase and if those who meet come from remote places. Centralisation is difficult as those who produce science are dispersed across geography and professions, their intensities of work are variable, and they have varying research interests. There are certain distances between the Museum and its collaborators and, moreover, these distances are not equal. While it is possible to create textual spaces of similarities through a standardised and centralised inscription of the collaborators into documents, it is difficult to centralise things beyond these mere 'flat subjectivities'. In particular, it can be complicated to make collaborators follow a certain path since they might want to remain amateurs and are thus keen on concentrating time, space and practice around their own subjectivities.

8.3. Strategies for crossing boundaries

We can further subdivide what Gieryn broadly termed the 'cultural boundaries of science' (Gieryn 1999). The boundaries between professionals and amateurs might be linguistic, institutional, disciplinary, professional, temporal, spatial, and/or material. How are these boundaries crossed? What general strategies are there to manage some of the heterogeneities that emerge when amateurs and professionals work together? Throughout this thesis, a number of strategies emerged, most notably the management of boundary objects, boundary spaces, and loose connections, and practices of brokering and decentralisation.

Boundary objects. The co-production of science necessitates translational efforts to produce boundary objects – objects with a high degree of abstractability and mobility. These objects can turn the field and the home into a scientific space where data can be produced and fed back to the Museum. The collaborator's physical proximity to the private habitat and their evident ability of self-governance does not necessarily signify theoretical and methodological remoteness to science. Although the field and the home in natural history is external to the museum and the lecture hall, it is defined by these latter

(Naylor 2002:497). Doing science outside in the field or at home means being partially connected to the Museum through objects. Yet, the spaces outside the Museum are not overly disciplined: being materially and literally outside of the Museum means being partially outside an institutional order. This relieves the collaborators of some of the constraints, the rigidities and the disciplinary mechanisms of professional science.

Boundary spaces. Boundary objects are closely entangled with the making of boundary spaces, that is, spaces in which heterogeneous actors can co-produce knowledge while crossing certain boundaries. The Museum and outdoor nature are boundary spaces as both Museum staff members and collaborators use them, but do so differently. While both have access to these spaces and are able to freely move within them, they enact these spaces differently, they are differently connected to them. The home, as well, is a border space in the sense that local and distant contexts, amateur and professional identities, private and public spheres can intermingle (Opitz 2002). Still, the home is demarcated from institutionalised spaces, times, and actions; home enables subjects to concentrate time, space and action. In all these spaces, disciplinary time (Foucault 1979:149-152) gives room to less disciplined, more flexible time-frames. The Museum's journal and its annual meetings are spaces, or boundary encounters, that are not very restrictive nor rigid, but quite flexible and relaxed. These boundary spaces cannot be densely 'populated' by time, money, qualifications, rules, and immobile technical equipment but need to be rather 'light' and flexible. They must be places where the co-production of science happens hand in hand with the building of bridges across different epistemic cultures. This, in turn, calls for heterogeneous gift exchanges and the nurturing of loose connections.

Loose connections. Boundary encounters are not only important for enabling boundaries to be crossed but also to nurture certain kinds of connectivities. In general, meetings are important for the maintenance of 'weak ties' (Granovetter 1973:1373). It is precisely those loose attachments that seem to get cultivated through boundary encounters. The encounters discussed in the previous chapters are not places where an already fixed and coherent community assembles. Rather, these are places where we encounter the 'collective' which is 'not a thing in the world, a being with fixed and definitive borders, but a movement of establishing provisional cohesion that will have to be started all over

again every single day' (Latour 2004a:147). We could argue that building and managing a database, for example, means assembling a hybrid collective through connecting humans and non-humans, subjects and objects; it is as much a boundary object as it is a boundary subject. Boundary encounters are places where boundaries are crossed, where connections are provisionally made, thus enabling the co-production of science.

Brokering. The exchange of objects, knowledge and gifts is crucial when amateurs and professionals co-produce scientific knowledge. These exchanges call for practices of brokering which do two things at the same time: the crossing of boundaries and the maintenance of boundaries (Wenger 1998, 2000). One the one hand, objects, people, gifts and knowledge cross boundaries. Science brokers (or boundary subjects), for instance, can (re)create connections and move knowledge between amateurs and professionals. On the other hand, the fact that objects, people and knowledge do not effortlessy flow and the very need for brokering, reveals that there still are different worlds that are sometimes difficult to connect. Maybe symbolic exchanges, involving gifts, prestige, and status, are particularly important when amateurs are involved in the production of science.

Decentralisation. The Museum decentres objects by taking them out of the central building of the Museum and transporting them to other places. It thereby invests places outside the Museum walls with its scientific discourse and practice to create a museum without walls. This happens for visitors - through buses, trails, festivals - as much as it happens for the collaborators who can benefit from tools, specimens and other objects that travel from the Museum to their homes and fieldwork sites. The database is an object that can be used in a decentralised manner. Decentralising, but also decomplexifying and personalising the database, are means to increase its mobility, usability and plasticity. An opposite force to decentralisation is not necessarily centralisation but also what could be called concentralisation - a force that centres objects, times, and spaces around a subject and keeps these demarcated, clearly locked into subjective, local, personal and individual configurations.

8.4. Boundary-work revisited

In the past 20 years Luxembourg's science has witnessed many changes: the creation of scientific institutions and professional careers, the increase of budgets, the reconfiguration of policies, and the construction of buildings dedicated to research activities. All these elements have created a demarcated space for the production of scientific knowledge, a space occupied by but a few. Boundary-work has taken place and the boundaries of science have been reconfigured.

The Museum, however, reveals another evolution. Whereas Luxembourg's recently created scientific institutions have distanced themselves from amateurs, the Museum has always actively attracted them. It has created places of encounter where both can join hands and exchange knowledge. It has not excluded some on the basis of their academic or professional credentials, but, on the contrary, included all those who are interested in nature and who are knowledgeable in a given field of natural history. Ever since scientific activities at the Museum were instigated through the creation of a centre of scientific research and the launch of a publication series, amateurs were intimately linked to its research activities. The boundaries of science that were constructed and envisioned were permeable right from the start. So, what kind of boundary-work is taking place in the Museum?

In order to answer this question, let us quickly review the different kinds of boundary-work. According to Gieryn (1999) three kinds of boundary-work characterise science: expulsion, where science is demarcated from posers; expansion, where an authority squares off for control over a domain; and protection of autonomy during which scientists protect their professional authority. There is, however, another kind of boundary-work at work between the Museum and its collaborators. Amateurs and professionals keep each other 'at arms length'. Neither are amateurs expulsed since their knowledge and specimens flow into the Museum and into activities of professionals; nor is the professional world expanded over the amateur one, as amateurs resist to be fully enrolled and since professionals are unable and unwilling to fully discipline them. Amateurs both belong and do not belong to science. Amateurs are provisionally and partially connected to science (see Strathern 1991, Munro 2005). More so, protection

of autonomy is not only a strategy used by scientists to protect their professional territory, as Gieryn assumed. Amateurs also protect themselves from, say, 'panoptical nightmares'. Amateurs protect their identity as practitioners of science who are able to choose what to do, when to do it, and where to do it. Understandably, they happen to refuse money, say no to deadlines, and withhold data. Accepting these things would mean bonding themselves more strongly to the Museum, which most of them do not want.

The boundary between amateurs and professionals is partly the making of amateurs. We explored in Chapter Seven the making of 'collaborator time' – an individual, gratuitous and subjective time, much different to the scheduled, commodified, and disciplined time of professionals. Also, we analysed the making of 'collaborator space' – a located, concentrated, homebound and self-determined space very different from the national, representative, and homogeneous spaces of professionals. There are many places where amateurism is made.

The kinds of boundary-work manifest in the Museum are less radical and more subtle than the ones described by Gieryn. It does not expulse, expand, or protect but it allows certain elements to flow while retaining others from doing so. Some things do flow quite easily: prestige, scientific and logistic support, the nomination as collaborator, food, some pieces of data, etc. However, we have also seen that deadlines, money, and some pieces of data do not flow as easy. Two kinds of boundary-work take place at the same time: 'boundary maintenance' (Barnes and Edge 1982b:241) and boundary crossing. In other words, the semi-permeability of the boundaries (Mol 2003) between amateurs and professionals is evident.

Resistance is boundary-work. In a sense, it is work against one kind of boundary-work: expansion. Amateurs might resist against the expansion of scientific values, schedules and practices over their own practices. Rather than being assimilated to science, some collaborators preferred to protect their own territory. They refused the temporalities, materialities and sociabilities that would turn them into professionals, and, at the same time, would refrain them from being amateurs. Amateurs might not want to become professionals and therefore clearly demarcated their identity as being 'non-professionals'. I would thus argue that what has been historically described as professionalisation and

amateurisation (Alberti 2001:117) also happens on a more horizontal level. The making of amateurs and professionals also happens in everyday practices, through the reinforcement of identities in any kind of encounters. Professionalisation and amateurisation are processes that both happened in history and that still happen today.

Another difference to common boundary-work appears. This kind of boundary-work operates through negativity, through denial. It surfaces through things that are not done, data which is not exchanged, words which are not spoken, enrolments that do not take place. Yet, the use of the term 'betrayal' would be inadequate to describe this state of affairs as it would provide a very asymmetric explanation. These 'non-actions' are also positively meaningful. For some it is a deliberate choice not to connect. In a sense, it is an 'active passivity'. Being enrolled in the construction of a network means participating in this construction and accepting and agreeing to certain disciplining mechanisms. Not letting oneself be enrolled is a way to counteract networking activities, it is a means to protect identities and to deploy power.

The kind of boundary-work performed between amateurs and professionals does not purely safeguard a border, but it also enables the crossing of that border. Boundary maintenance and boundary crossing take place at the same time. This is a process through which the 'other' (either the cultural space of amateurs, or the cultural space of professionals) is simultaneously included and excluded, in other words, a process through which the 'other' is 'folded' (Mol and Law 2005:640). Through the complex interrelationships between amateurs and professionals the boundaries of science are folded.

8.5. A democratic model

Roughly put, two modes of knowledge production exist in Luxembourg today. The model of co-production of knowledge, predominant in the Museum, and characterised by a practice of science surrounded with a degree of permeability involves both professionals and amateurs. The other model of knowledge production prevails in most other research institutions in Luxembourg; it is a mode with a reduced openness towards non-

professionals and a stronger focus on economic utility and international and professional standards. Over time, this latter model has come to dominate within Luxembourg science and has fostered an increasingly delimited, enclosed and distinct scientific practice. That the older, more open model is marginalized seems a bit paradoxical, especially at a time when policies in Luxembourg and Europe constantly advocate closing the gap between science and society and promote more participative forms of governance.

There surely is a kind of doubletalk from political powers. While stressing the importance of bridging the knowledge gap, political leaders also underline the importance of producing professional, cutting-edge knowledge that drives the economy, a kind of knowledge which is very often produced in places that are out of reach to non-professionals. Scientists, on their side, seem to want it all: that political and social actors understand, value, accept and support their work, but that they restrain from interfering, protesting, regulating, and undermining it. For Gieryn (1995:435),

> [t]he cartographic challenge for scientists is to draw science near enough to politics (ideally, as adjacent cultural territories) without risking spillover of one space into the other or creating ambiguity.

Scientists want to open up their territory (for financing, recruitment, credibility) while demarcating it at the same time (from politics, public opinion, etc.). They want to safeguard the boundaries around science *and* leave them porous.

The Museum's reliance on amateurs has, then, some relevance for further debates about democratic and participatory models for science. First, the Museum's reliance on amateurs is necessary. Amateur knowledge might become indispensable when professional expertise is lacking, but when informed decisions about nature have to be made. Second, it is a representative model. In the Museum, actors from different backgrounds are involved in the making of knowledge. The principle of including lay people to contribute directly and co-produce expert knowledge holds the promise of plurality and promises better social outcomes (Pestre 2003:260). Third, it works. Although people might refuse to collaborate, there are many who do so. The result is a paper given at a conference, a piece of data in a database, a list of species of a region,

an article in the Museum's journal. With the help of the scientific and logistic support of the Museum, amateurs do produce knowledge that leaves their home. The Museum's model is needed, representative, useful, and effective and produces a fairly 'open science'.

This openness seems to be a constant feature of natural history. In the past, natural history was viewed 'as a science close to the public domain, open and egalitarian' (Drouin and Bensaude-Vincent 1996:408). While in Europe it was after the first half of the 19th century that the status of natural history declined with the rise of laboratory biology, this shift happened by the end of the 20th century in Luxembourg. So what about natural history and museums of natural history today? Where is their place in the contemporary topography of science? Is natural history a useful model for participation and for a more democratic science?

A natural history museum can still play a significant role today, not only to promote public understanding of science. According to Wagensberg (2000:137) a modern science museum can still play an important role, because,

> [s]cientific questions affect not only the scientific community. In a democratic society, the possibility of taking part in decision making should be open to everyone. [...] Where can science for citizens be done? The museum, as an institution at the same time scientific and civic, would seem to be a good place.

To connect science and society, amateurs and professionals, museums seem to be ideal meeting grounds or 'contact zones' (Clifford 1999) since they provide participative and accessible environments (Cossons 2000:8).

This openness has benefits and downsides. One of the downsides, compared to universities and laboratories, is missing closure. The 'success' of the university, we could argue, is based on the making of a rather enclosed space. The strength of the laboratory lies in its reconfigured setting, a closed off and confined space where scientists can 'rehearse' the world (Eagleton 2006:18). As a consequence, 'universities and research institutes are atmospheres where anyone, except scientists, feels inhibited' (Wagensberg

2000:138). Conversely, the weakness of the museum lies in a different power balance. To paraphrase Latour (1984), the museum cannot raise the world (anymore) because it has opened up itself too much to the world, because it must make sure that nobody feels inhibited.

But the openness of the museum is also its greatest strength. After all, a museum seems to be a much more democratic institution than a laboratory. Natural history, as a kind of science open to amateurs, might therefore be a good model for rethinking the relations between science and the public. According to Pickstone (2000:216):

> If we can focus on natural history as the accessible heart of many public debates, then the prospects for democratic outcomes would seem better than those derived from other models of science-public relations.

Natural history seems best placed to produce democratic, egalitarian outcomes, where different actors can have their say. For some, natural history has recently been revitalised (Rip 2003:423, 427). Pickstone (2000:214) goes even so far to suggest that

> [m]aybe the twenty-first century will come to resemble the eighteenth in the proliferation of the 'descriptive sciences' and a relatively open flow of knowledge between professionals and others.

If Pickstone's prediction is right, then natural history might play a renewed role, not only as a 'way of knowing', but also as a model through which people are brought together. The museum of natural history might become a central place in the topography of the production of scientific knowledge for it provides a place where natural history, as a 'way of doing' and connecting, takes place. While this might be an overly optimistic vision for museums, we should not underestimate the potentialities museums might still hold. Even if museums have been displaced concerning the production of knowledge, even if there has been a shift away from an object-based epistemology (Conn 1998), museums might be of principal importance in one particular area. They can still produce and manage places of encounter. They can still provide a space where professionals and amateurs, scientists and children, experts and lay can meet.

Maybe, the renewed importance of museums is not so much of their own making. It could be due, primarily, to the increasing complexity, distance and technicality of the science that researchers produce in other places of knowing. The role of museums might thus increasingly be defined in opposition to laboratories, research centres, and universities. Their role might be defined in terms of what they can do that other institutions cannot. Museums seem to be good at bringing together heterogeneous actors. They seem to be well placed to manage flexible and hybrid forums where multiple identities, languages, cultures, and views can be drawn together. Museums might thus not only continue to act as information brokers through the possibility of retrieval of information (Keene 1998:17), but also do brokering in a much broader sense, by negotiating a place between multiple epistemologies and ontologies.

Appendix

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B. Interviewee list

Name	Occupation	Date of interview
Museum staff		
Backes, Simone (SB)	In charge of the Service graphique	31/10/2003
Bechet, Georges (GB)	Museum director	9/12/2003
Biel, Claudine	Musée-Bus 2000	28/11/2003
Bley, Marc	In charge of visits	22/12/2004
Buttini, Eric	Conservateur of geophysics and astrophysics	24/11/2003
Colling, Guy (GC)	Chef de service of population biology	23/10/2003
Engel, Edmée (EE)	Conservatrice of zoology of vertebrates	12/3/2004
Faber, Alain (AF)	Conservateur of palaeontology	15/1/2004
Fischer, Aline (AF2)	In charge of Galileo Science-Mobil	12/12/2003
Helminger, Thierry (TH)	Chef de service of vegetal collections	10/2/2004
Herr, Jérôme	Computer administrator	30/1/2004
Kirsch, Monique (MK)	In charge of the Science Festival	18/12/2003
Meisch, Jim (JM)	Conservateur of botany	22/10/2003
Meyer, Marc (MM)	Conservateur of invertebrate zoology	4/3/2004 and 14/10/2004
Michaely, Patrick (PM)	Communication and public relations	8/6/2004
d'Oreye, Nicolas (NO)	Geophysicist	13/9/2004
Philippo, Simon (SP)	Conservateur of geology and mineralogy	18/3/2004
Ries, Christian	Conservateur of ecology	10/12/2003
Risch, Stéphane (SR)	In charge of the Musée-Bus 2000	11/5/2004
Soisson, Veronique	Temporary assistant for the 2003 Science Festival	27/11/2003
Walisch, Tania	Conservator of databases	25/6/2004
Collaborators		
Baghli, Adil (AB)	Scientific interests: mammalogy and environmental ecology, Occupation: independent biologist, Degree: PhD in science	28/7/2004
Carrières, Evelyne (EC)	Scientific interests: entomology, Occupation: independent biologist, Degree: diploma in science	14/4/2004
Cungs, Josy (JC)	Scientific interests: ecology and entomology, Occupation: employee at the Water and Forests	4/6/2004

	Administration, Degree: no diploma in science	
Delsate, Dominique (DD)	Scientific interests: palaeontology and geology, Occupation: dentist, Degree: doing a PhD in palaeontology	6/5/2004
Diederich, Paul (PD)	Scientific interests: botany and lichenology, Occupation: mathematics teacher, Degree: PhD in botany	28/6/2004
Frantz, Alain (AF3)	Scientific interests: molecular ecology and ecology, Occupation: researcher, Degree: PhD in science	15-16/4/2004
Friesing, Arno (AF4)	Scientific interests: ecology, Occupation: employee at the Ministry of Agriculture, Viticulture and Rural Development, Degree: PhD in science	23/8/2004
Gerent, Raoul (RG)	Scientific interests: entomology, Occupation: school teacher, Degree: no diploma in science	10/3/2004
Hoffmann, Lucien (LH)	Scientific interests: biology and environmental sciences, Occupation: director of a research laboratory, Degree: PhD in science	24/6/2004
Kronz, Guy	Scientific interests: mineralogy and palaeontology, Occupation: owner of a shop, Degree: no diploma in science	31/3/2004
Lorgé, Patrick (PL)	Scientific interests: ornithology, Occupation: employee of the League for the Protection of Nature and Birds, Degree: Diploma in science	1/6/2004
Marson, Guy	Scientific interests: mycology; Occupation: locomotive driver; Degree: no diploma in science	4/11/2003
Meisch, Claude (CM)	Scientific interests: biology, Occupation: biology teacher, Degree: PhD in science	1/7/2004
Reichling, Léopold (LR)	Scientific interests: botany and zoology, Occupation: retired biology teacher, Degree: Diploma in science	17/5/2004
Schley, Laurent (LS)	Scientific interests: animal biology, Occupation: employee of the Water and Forest Administration, Degree: PhD in science	18/8/2004
Schneider, Nico (NS)	Scientific interests: entomology and cecidology, Occupation: biology teacher, Degree: Diploma in science	15/7/2004
Schultheis, Ben (BS)	Scientific interests: mycology, Occupation: retired mechanic, Degree: no diploma in science	6/7/2004 and 9/7/2004
Walzberg, Claudio (CW)	Scientific interests: biology, Occupation: independent biologist, Degree: Diploma in science	8/3/2004
Welter, Antoinette (AW)	Scientific interests: botany, Occupation: teacher in biology, Degree: Diploma in science	19/8/2004
Werner, Jean	Scientific interests: bryology, Occupation: retired legal advisor, Degree: no diploma in science	17/7/2004
Wolff, Frank (FW)	Scientific interests: biology, Occupation:	25/6/2004

	employee of the Ministry of Environment, Degree: Diploma in science	
Other		
Huyberechts, Gilbert	Architect, Architecture et Environnement	30/10/2003
Decker, Pierre (PD2)	Conseiller de Gouvernement de 1.ère classe (Ministry of Culture, Higher Education and Research; Department of research)	3/11/2003
Dockendorff, Guy (GD)	Conseiller de Gouvernement de 1.ère classe (Ministry of Culture, Higher Education and Research; Department of culture)	21/7/2004
Fayot, Ben	Member of Parliament	21/12/2004
Stomp, Norbert (NS2)	Former Museum director	4/12/2003
Wiltzius, Pierre	Director of the Beckman Institute (USA), Member of the scientific council of the National Research Fund (Luxembourg)	12/6/2004

C. Questionnaire

Last Name:

First Name:

Professional occupation:

Scientific interests:

Do you have a diploma in the field of your scientific work? (please tick)

-Yes

-No

Concerning you scientific work, do you consider yourself as an:

-Amateur

-Professional

-Other

How did you get interested in your area of interest?

Where do you spend most time for your research? (Please number by order of importance: 1 most of the time to 4 least of the time)

-In the museum

-In the field

-At home

-Other places:

What are the benefits for you working in collaboration with the museum?

What are the drawbacks in working in collaboration with the museum?

Do you see a difference between a professional scientist and an amateur, or a scientist who works on a voluntary basis?

Do you have any suggestions to improve the collaboration between you and the museum?

What of the following do you get from the museum?

-A publication in Ferrantia – Les travaux scientifiques du MNHN

-Subsides pour frais de voyage

- Subsides pour frais de séjour

-Borrowing material (enhancing glasses, microscopes, ... please specify):

-Journals and books

-Photocopies

-Service to send letters

-Other:

Do you also collaborate with other museums ?

Any other comments

Thank you for your cooperation