Expertise and the Fractal Model

Communication and Collaboration between Climate-Change Scientists

Tiago Ribeiro Duarte

Supervised by Harry Collins and Rob Evans

Cardiff School of Social Sciences, Cardiff University

Thesis Submitted for the Degree of Doctor in Philosophy

May 2013

DECLARATION

This work has not previously been accepted in substance for any degree and is not concurrently submitted in candidature for any degree.

Signed .... (candidate)
Date 01/08/2013

STATEMENT 1

This thesis is being submitted in partial fulfilment of the requirements for the degree of PhD.

Signed .... (candidate)
Date 01/08/2013

STATEMENT 2

This thesis is the result of my own independent work/investigation, except where otherwise stated.

Other sources are acknowledged by footnotes giving explicit references.

Signed .... (candidate)
Date 01/08/2013

STATEMENT 3

I hereby give consent for my thesis, if accepted, to be available for photocopying and for inter-library loan, and for the title and summary to be made available to outside organisations.

Signed .... (candidate)
Date 01/08/2013

To Camila,

she was there with me and for me all the time.

Abstract

This thesis examines how scientific communities which are heterogeneous among themselves communicate and collaborate to produce knowledge on climate change. Climate-change science is a relatively new field of investigation and it includes experts from virtually all areas of scientific enquiry. This field is not, however, a homogeneous transdisciplinary area of research so that the different scientific communities that compose it have to bridge the gaps among themselves to be able effectively to communicate and collaborate. I use Collins and Evans' (2007) realist theory of expertise combined with other relevant Science and Technology Studies concepts, particularly the notions of trading zones (Galison 1997; Collins et al. 2007) and trust (Giddens 1990, Shackley and Wynne 1995b; Reyes-Galindo 2011) to explain how different groups of experts build bridges between their heterogeneous forms of life. As climate-change science is too broad to be covered in one PhD project, I focus on paleoceanography, a subfield of geology that reconstructs past oceans and their interactions with the climate system. I use the fractal model (Collins 2011) to move through different levels of analysis and examine the different bridge-building mechanisms between expert communities at work at each of them. The main contribution of the present work is to identify and explain how the various mechanisms that mediate communication between expert communities come into play at different levels of analysis.

Acknowledgements

This thesis is the result of a long process that began with my arrival in the UK without funding for my PhD and a long year of struggle doing the course on a part-time scheme until I was finally awarded the grant that made it viable to carry on with the project. A number of people and institutions were extremely helpful in the different stages of this process and without their support this thesis would have never been written.

First and foremost thanks to Camila, my beloved wife, who embarked on a journey to a foreign country with me and went through this whole process always supporting and encouraging me, even when we had to re-use the same Tesco value teabag twice because we had no money. Without her by my side I wouldn't have had the strength to carry on when things looked difficult.

Obrigado Pai e Mãe pela educação que vocês me deram, a qual me possibilitou fazer este Doutorado. Obrigado também pelo apoio financeiro e emocional nas etapas mais difíceis desta empreitada.

Thanks to Daniel for good times in Italy and a mission well accomplished.

Thanks to my supervisors Harry Collins and Rob Evans for continuous encouragement since the time when I applied for a place at Cardiff University and for insightful criticisms and suggestions along the way that helped me mould this thesis into its final shape.

Thanks to Dr. Sara Delamont, my English *madrinha*, who helped me in every possible way, particularly in the latest stages of the PhD by having me as an informal research assistant, by proofreading several chapters of this work, and by helping me turn my methods chapter into something presentable.

Thanks to all the good friends I've made in Wales over the past four and half years. Thanks particularly to Rodrigão for reeeeealy reeeeeeealy being the best friend I could have had in my first year in Cardiff and for making suggestions on how to improve the research proposal that led me to get funding for this project. Thanks also to *el muchacho muy loco* Luis for great friendship and for proofreading more drafts of this thesis and all sorts of proposals, abstracts, and conference presentations than any human being would have liked to.

Thanks to Will Gray for reading the bits of this thesis where I introduce paleoceanographic research and for helping me make them more accurate. Thanks to Sophie for helping me transcribe interviews by deciphering what my interviewees had said when I had already lost hope.

Thanks to Professor Steven Yearley and to Professor Ian Rees Jones for kindly accepting to be my examiners and for a very enjoyable viva voce examination.

Thanks to Professor Susan Baker for making me read more of the literature and think harder about my research design during my yearly progress reviews. Thanks also for chairing my viva voce examination.

Thanks to Professor Renan Springer de Freitas for loads of support since the time I was still applying for a place at Cardiff University until the end of the PhD by reading and commenting on research proposals, writing recommendation letters, and writing reports on my progress for my sponsor.

Thanks to all the scientists who accepted my interview requests for sharing with me their linguistic worlds. A massive thank you to those who replied to emails following up on the interviews and to those who kindly showed me their labs.

Thanks to a number of institutions who supported this project. Capes Foundation awarded me the scholarship that enabled me to carry out this project. Cardiff University provided me with additional funding for fieldwork through the *'125 for 125'* award and through departmental funding. Thanks to the Waterworlds research group at the University of Copenhagen for receiving me as a visiting researcher for a delightful couple of weeks in 2011. Thanks in particular to Martin Skrydstrup for encouraging me to go to Denmark. Thanks to the organisers and to the participants of the two summer schools in which I took part during the PhD: The Brown University Advanced Research Institute in Climate Change (BIARI) and the Urbino Summer School in Paleoclimatology. The lectures, debates, and the informal chats during these events helped me improve a great deal my understanding of climate-change science and of paleoclimatology.

It goes without saying that any eventual errors that might have remained in this thesis are my sole responsibility.

Table of Contents

Introduction

This thesis is on knowledge production in climate-change science. This field is remarkably heterogeneous as over the past few decades virtually all fields of science have become somehow involved in it. Knowledge is produced across disciplinary and national borders, which entails a high degree of coordination between scientists and the formation of multi and interdisciplinary research projects (Jasanoff and Wynne 1998; Edwards 2001, 2010). Climate-change science, however, is not a homogenous transdisciplinary field. Rather it is a conglomeration of scientific fields revolving around the same topic and creating links among themselves to address particular research questions (Jasanoff and Wynne 1998; Edwards 2001, 2010). The Science and Technology Studies (STS) literature has shown that communication and collaboration between scientists belonging to different expert communities is not straightforward (e.g. Fleck 1935; Kuhn 1962; Galison 1997; Collins et al. 2007) and studies have shown that this is also the case in climate-change science (Shackley and Wynne 1995b; Sundberg 2006, 2007). The present work seeks to work out how scientists bridge the gaps between their different fields of expertise in order to produce knowledge on climate change. It focuses on communication, which is understood here in a broad sense including face-to-face, telephone, webcam, and email conversations as well as the reading of scientific papers or the use of secondary data. I will use Collins and Evans' realist theory of expertise (Collins and Evans 2002, 2007; Collins 2010, 2011) associated with other STS concepts, especially the notions of trading zones (Galison 1996, 1997; Collins et al. 2007) and trust (Giddens 1990), to identify how experts from different communities communicate and collaborate to produce knowledge on climate change.

The problem of communication between different expert communities has a long history in STS as well as among some of its predecessors. In what could be called the prehistory of STS, Fleck (1935) and Kuhn (1962) examined the difficulties of communication between individuals belonging to different social groups (members of different thought collectives and different traditions of normal science, respectively). Both authors pointed out that different social groups hold different worldviews which prevent their members agreeing on matters such as whether an experiment really proves something or whether a fact is really a fact, etc. This issue is not difficult to grasp if we take social groups with radically different beliefs and ontological commitments as examples, such as the debates between creationists and evolutionists. Whereas biblical passages are legitimate pieces of evidence from the perspective of creationists, for an evolutionist they are only religious mysticism. Conversely, any empirical evidence put forward by evolutionists against creationism will not be accepted by creationists as more relevant than their religious beliefs. Communication between these groups therefore is bound to be difficult.

Within science itself there are also issues in communication. Kuhn (1962) offered several example of this. Proponents of the Ptolemaic system, for instance, classified the moon and the sun as planets. Within the Copernican paradigm, on the other hand, the moon is seen as a satellite, which is a concept that did not exist within the Ptolemaic system, and the sun is regarded as a star. Scientific vocabulary changes and defenders of different theories might end up talking about the same entities but attributing different meanings to them. More importantly, these meaning changes also present ontological and cognitive issues. To understand this point it is necessary to introduce a sociological theory of knowledge and language based on the philosophical work of Wittgenstein, which was initially set out by Kuhn (1962, 1974) and then further developed by sociologists of science (e.g. Barnes 1982; Collins 1985)[1]. According to this theory, the empirical world can be classified and understood in several different ways by different social groups. Language mediates the contact of human beings with the empirical world in the sense that it groups certain empirical entities under similar concepts while it differentiates others by using different concepts to talk about them. In other words, the way we use words establishes certain relations of similarity and difference between

[1] I acknowledge that there are differences between Kuhn and the sociologists of knowledge inspired by his work (e.g. Bloor 1976; Barnes 1982; Collins 1985). Whereas Kuhn hesitated in accepting all the sociological consequences of his idea of paradigm and incommensurability, these sociologists embraced his sociological insights and developed a whole new field of investigation: the sociology of scientific knowledge. An example of Kuhn's hesitation was him defending the idea that epistemic values could be used by scientists to choose between competing theories. This would be a way of finding some 'rational' criteria for theory choice. His own sociological insights, however, undermine this point as in different communities theses values are evaluated and weighed differently so that they cannot resolve issues of theory choice when it comes to controversial issues.

empirical phenomena. What is considered a planet within one community is regarded as a satellite in another.

These worldviews carry with them not only a system of classification of the world, but definitions of what is real and what is not, or, in other worlds, ontological commitments. Whereas according to Priestley there was a substance called phlogiston which was associated with combustion, according to Lavoisier, phlogiston did not exist (Kuhn 1962). According to Lavoisier's theory, combustion would need oxygen, an entity that did not exist within the phlogiston theory. Kuhn (1962) has phrased this idea in an influential way when he stated that proponents of competing scientific theories lived in 'different worlds'.

The major sociological point in this theory of language and knowledge is that these systems of classification are only learned and transmitted within collectivities (Barnes 1982; Collins 1985; Bloor 1997; Collins and Evans 2007; Collins 2010). This means that one cannot fully understand a particular worldview if one is not socialised within it. This is because using concepts 'correctly', that is, according to the agreed way of using them in a collectivity, depends on rule-following. However, as we know, following Wittgenstein's *Philosophical Investigations* (1953), rules do not contain the rules for their own application. It is necessary to be immersed in a domain so as to acquire tacit knowledge to follow its rules according to the standards of the community (Collins 1985; Collins and Evans 2007; Collins 2010). As a result, individuals belonging to different social groups who have not been exposed to each other's language are, in principle, expected to have problems in communication.

The problem of communication between different social groups is therefore seen as an issue related to their *heterogeneity.* If religious groups and scientists, for instance, held similar worldviews, they would be able to communicate effectively. However, because their worldviews are conflicting, they are not able to fully understand and agree with each other. Similarly, competing expert communities are also bound to have issues in communication.

When it comes to communication and collaboration between different expert communities producing knowledge on similar topics issues in communication are less related to competing worldviews and more to a lack of understanding of the process of knowledge production in the other community as well as of their interests and tacit rules[2]. The distinction I am drawing here is between *competing communities* and *heterogeneous collaborative communities.* The former have opposite positions in a scientific controversy, for example, creationists and evolutionists. The latter consist of communities that have different expertises, use different methods and instruments, speak different technical languages, but do not have opposite worldviews and are looking to communicate and collaborate. I am interested in the latter. I am more interested in paleoceanographers and paleo-modellers or in computer modellers and meteorologists willing to collaborate and produce knowledge together than in sceptics on climate change disputing the reality of anthropogenic global warming. Because of the disunity of science (e.g. Galison and Stump 1996), or, in other words, because scientific domains are heterogeneous, experts from different communities might experience a feeling of estrangement or a lack of understanding when communicating with experts from other fields.

Heterogeneity between Expert Communities

Collins and Evans' (2002, 2007) theory of expertise is useful to understand the issue of heterogeneity between different expert communities. They argue that expertise consists of the tacit knowledge shared within a domain of practice. In this sense, expertise is not defined by the subject, but by the acquisition of tacit knowledge. One can be an expert in things as diverse as speaking a language, science, engineering, gardening, music, arts, building, sports, astrology, and palm reading. To understand this point let us return to the idea set out above that to learn a language it is necessary to have immersion in the form of life of its speakers. This is because using concepts correctly depends on rule-

[2] Problems of communication are also sometimes associated with the incommensurability thesis (see Kuhn 1962 and Feyerabend 1993[1975] for the original discussion on this thesis). I decided not to use this term as it has generated a great deal of confusion in terms of its meaning and I believe it is more helpful for examining competing scientific communities rather than different ones looking to work on similar issues.

following. One cannot become proficient in a language solely through reading dictionaries, stories and grammar books or through listening to broadcasts. A child learning his or her mother tongue, for example, learns it through immersion in his or her own society. Only later in life he or she might learn to read and the formal rules of the language. Most people never study grammar and still speak their mother tongue fluently.

The same need for immersion applies to other domains of practice. Scientists, for instance, go through a long process of immersion in the relevant scientific community to acquire expertise. This process includes activities such as attending conferences, conducting research under supervision of experienced researchers, informal conversations in the corridors of universities, etc. All these activities give an individual a sense of what the main issues within a scientific field are; which theories should be taken into account when researching; which variables are really important in research; which scientific works are outstanding and which are not; and so on. In sum, through these activities students acquire the tacit knowledge of the field (Collins 2010). The acquisition of tacit knowledge enables experts to make informed judgements within domains of practices according to the standards of relevant the community.

High-level specialist expertise can be divided into two types (Collins and Evans 2007): *contributory expertise*, which consists of the skills necessary to contribute to a domain by engaging with its practices; and *interactional expertise*, which consist of the mastery of the language of a domain. Scientific domains are heterogeneous because they have different languages and different practices. With regards to language, each scientific community has their technical vocabulary and the meaning of concepts used by a group of scientists is not always straightforward to members of other research areas (Galison 1997; Collins 2011). As pointed out above, learning to use a language correctly requires a long socialisation process in the relevant community. The contrast between cultural anthropology and quantum mechanics illustrates this point. Scientists from these communities have very little shared technical vocabulary that they can use to communicate about their domains. Furthermore, as argued above, language classifies the world and attributes meaning to it, providing individuals with worldviews. When it comes to heterogeneous collaborative communities, it is not expected that scientists will experience major disagreements as in the case of scientific controversies. Yet, scientists who speak different languages sometimes see and classify objects differently. A micropaleontologist, for instance, when looking at microfossils under the microscope has a number of concepts with which he or she can classify the different species. A sociologist, and I am a proof of this, when looking at the same microfossils sees only white balls and has no technical concepts that he or she can use to classify them. It is worth noting that even in closer domains, however, there might be problems of mutual comprehension (Galison 1997, pp. 652-653).

With regards to contributory expertise, each scientific domain has their own research practices that can only be competently carried out by fully socialised individuals. Scientists, however, cannot be socialised in all scientific domains at once as it takes a long time to become a full-blown expert in a single area of research. Scientists' expertise is therefore limited to narrow fields of investigation. As science has been increasingly becoming more divided into specialised fields of research, it has also become more heterogeneous.

Furthermore, research practices are associated with particular instruments. Hacking (1992) and Pickering (1995) have put forward the idea of instrumental or machinic incommensurability, which means that groups of scientists using different instrumentation sometimes produce no common measurements so that their data sets are not comparable. This thesis, similarly to the incommensurability thesis set out by Kuhn (1962) and Feyerabend (1993[1975]), was originally applied to competing expert communities, which is not the focus of the present work. However, the idea that different groups of experts use different instruments and techniques points to another aspect of the heterogeneity of science. This does not mean that all different expert communities use different instruments and produce incompatible data, but that this frequently happens, especially in a diverse field such as climate-change science. This may give rise to some issues. Firstly, it creates difficulties for scientists trying to interpret and integrate data sets produced by other communities into their own work if they have not been socialised on how these data were produced and on what the caveats for using them are (Collins and Evans 2007; Edwards et al. 2011). Secondly, if scientists want to integrate data produced by other communities into their work but the data sets have different resolutions, levels of precision and accuracy, i.e. they are not compatible, the data will need to be processed or transformed in some way (Edwards 2010). Even in the case of fields of science that are not that far apart, such as modelling future climate change and modelling the impacts of future climate change, there are incompatibilities between data sets that generate difficulties for these communities to use each other's data (Shackley and Wynne 1995b).

Different expert communities also have different rules on how knowledge is produced and legitimised, or, as Knorr-Cetina (1999) has put it, they have different epistemic cultures[3]. This means that for a scientist to be an accomplished contributory expert he or she has to internalise the epistemic culture of his or her domain and carry out his or her research according to its rules. An example of this is the contrast between high-energy physics and molecular biology (Knorr-Cetina 1999). Both fields are regarded as experimental sciences, but have very distinct research practices. Whereas in high-energy physics scientists have no direct contact with the minuscule particles they try to detect, in molecular biology, the objects of research (e.g. animals, cells, etc.) are continually present in the laboratory. This has several impacts in their research practices. In high-energy physics, for example, scientists struggle to separate noise from signal as any minimal change in the instrumentation might interfere in the experiment result. When an experiment is not successful a great deal of effort is made towards understanding the experimental setup and what could have possibly gone wrong. In molecular biology, in contrast, when experiments are unsuccessful scientists re-run them with slightly different setups until they obtain satisfactory results. There is no major effort to work out what went wrong with the original experiment.

[3] Epistemic culture is the concept Knorr-Cetina uses to talk about different social groups that compose science. It plays a similar role in her theoretical framework as the ideas of form of life (Wittgenstein 1953; Collins 1985; Collins and Evans 2007), tradition of normal science (Kuhn 1962), thought collective (Fleck, 1935), or social worlds (Clarke and Leigh Star 2008) play in different STS traditions.

Finally, heterogeneity in science also relates to the distinct interests of different expert communities. There is a wide literature, particularly from the early days of STS, focusing on the diverging interests of competing communities, and how these interests explained how scientific controversies would play out and eventually settle (e.g. Bloor 1976; MacKenzie 1978; Shapin 1979; Barnes 1982). Although in this work as I am not focusing on controversies, interests are still an important issue in communication and collaboration between different expert communities and there have been cases in climate-change science of scientists from different areas working on similar phenomena but not communicating due to diverging interests (Sundberg 2006, 2007).

Science therefore is not homogeneous. Different expert communities speak different technical languages, have different contributory expertises, use different instrumentation, have different epistemic cultures, and have different interests. Communication and collaboration between them requires some effort towards bridging these gaps.

Two Mechanisms for Dealing with Heterogeneity in Science

There are two main sociological mechanisms that may improve communication between heterogeneous expert communities: the *homogenisation of expert communities* and the *building of bridges between expert communities* (see figure 1). There are two main mechanisms for homogenising expert communities: translation of interests (Callon 1986; Latour 1987) and standardisation (Latour 1987; Jasanoff and Wynne 1998; Lampland and Star 2009; Edwards 2010). The former consist of particular groups of experts taking control of a network through making the interests of other actors converge with theirs. Translation is a particularly effective way to reduce interest heterogeneity. Standardisation consists of efforts to standardise procedures to collect and to process data. This is a helpful way to reduce issues related to instrumental heterogeneity particularly when different expert communities produce incompatible data sets. Although I acknowledge the importance of these mechanisms to understand how climate-change science works, they will not be the focus of the present work. I will discuss them in chapter 3 as they are also part of the story to be told about climate-change science. However, I am much more interested in mechanisms that make communication between heterogeneous groups possible rather than in mechanisms that reduce diversity. For this reason, I will focus on efforts towards building bridges between those communities.

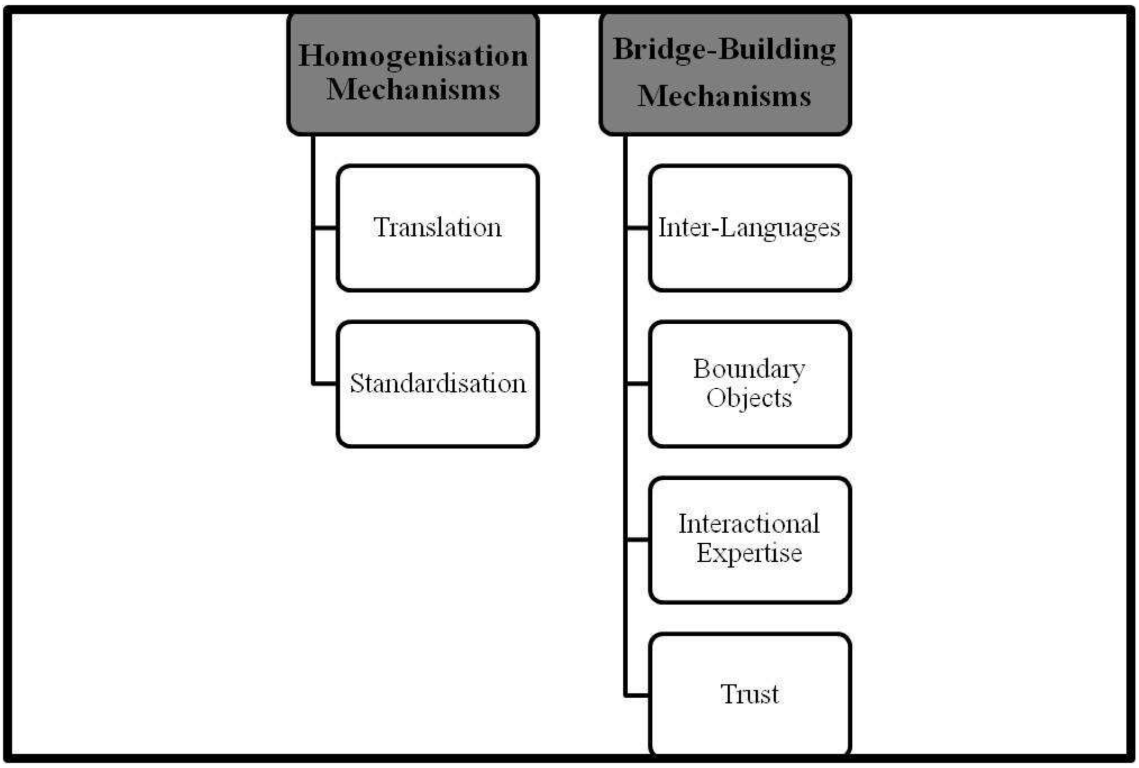

Figure 1: Mechanisms for dealing with heterogeneity in science.

Three main mechanisms for building bridges between expert communities have been described by STS scholars: inter-languages (Galison 1989, 1996, 1997), boundary objects (Star and Griesemer 1989), and interactional expertise (Collins and Evans 2002, 2007). These mechanisms are not aimed to create convergence of interests, rather they only operate in contexts where heterogeneous expert communities are *willing to communicate and collaborate*. Furthermore, they do not standardise research practices, although they may work alongside standardisation efforts (Star and Griesemer 1989; Fujimura 1992). They facilitate communication and collaboration between heterogeneous groups of scientists. I will argue in this work that trust is also a mechanism that mediates communication between expert communities. Trust has been the focus of a great deal of STS research (e.g.Wynne 1989; Yearley 1999; Collins 2001; Shrum et al. 2001; Brown 2009; Hedgecoe 2012), but few authors have identified it as a mechanism for bridging the gaps between heterogeneous communities (e.g. Shackley and Wynne 1995b; Reyes-Galindo 2011). I will now examine the three main mechanisms set out above and return to trust afterwards.

Building Bridges between Expert Communities

As it has been pointed out above, three main mechanisms for building bridges between expert communities have been described by STS scholars: inter-languages, boundary objects, and interactional expertise. Collins et al. (2007) set out a trading zone model that encompasses these three mechanisms of communication and I will use it to guide us through this section. I will begin by introducing the original concept of trading zones, which was introduced to STS by Galison (1989, 1996, 1997).

Galison (1997) argued against the historiographical depiction of monolithic traditions of research within physics that would comprise theoretical and experimental physics. He first argued that physics is divided into several subcultures, such as instrument makers, theorists, and experimenters. Although these subcultures might deal with the same or similar phenomena, they have different expertise and have their own domain language. Furthermore, despite their being connected they do not always evolve or change simultaneously. Progress in experimentation, for instance, might take place at a barren period in theory development. Similarly, instrumental progress might be accompanied by a period of lack of progress in theoretical physics. Galison (1996, 1997) developed the concept of *trading zones* to explain how heterogeneous scientific subcultures could communicate and effectively collaborate. The idea of trading zones is an attempt to show that different subcultures of science find ways of locally bridging the gaps between them even if at a general level they attribute different meanings to the world and to the objects involved in their 'trade'/interaction. Galison (1996, p. 119) defined trading zone as "an arena in which radically different activities could be *locally*, but not globally, coordinated". In trading zones, experts would develop *inter-languages* that would mediate their communication. They would begin by developing pidgins. At this stage, a number of concepts from different subcultures of science or sometimes from different scientific disciplines would be abstracted from their original context. These would only be very rudimentary inter-languages which would have only a few words. They are too simple to be the native tongue of groups of people and are useful only for basic communication between different social groups. In the case of continued interaction these pidgins would become more complex, constituting an autonomous creole language. At this stage a new field of science would emerge with its own experts, research problems, journals, etc.

Collins et al. (2007) argued that inter-languages were only one type of trading zones. Interactions between different forms of life take place through other mechanisms for bridging the gaps between them. They classified trading zones according to two dimensions: whether they are collaborative or coercive and whether the interactions may result in the development of a new homogeneous culture or not. According to this model there are four types of trading zones: inter-language, which is collaborative and might become a new homogeneous culture; fractionated, which is collaborative and might not become a new homogeneous culture; subversive, which is coercive and might become a new homogeneous culture; and enforced, which is coercive and might not become a new homogeneous culture (see figure 2). I will not go into the details of the coercive types of trading zones as in the present work I am only interested in collaborative work.

Inter-language trading zones are those in which interactions take place through the use of pidgins and creoles, i.e. through new trade languages that are developed to mediate communication between heterogeneous social groups. When they reach the stage when they are homogeneous, i.e. communication is mediated by a full-blown creole around which a whole new culture develops, they stop being trading zones. An example of this is biochemistry. This is an autonomous domain with a fully-fledged creole spoken by its members and not a trading zone where heterogeneous social groups interact through inter-languages.

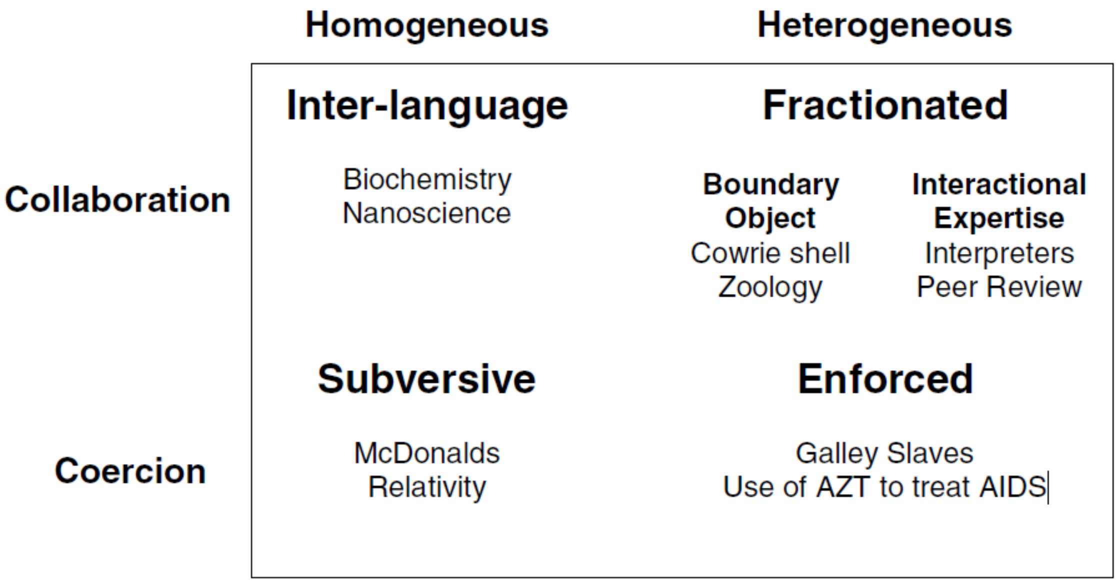

Figure 2: A general model of trading zones (Collins et al. 2007, p. 659).

Fractionated trading zones, on the other hand, do not lead to the development of new homogeneous domains. Collins et al. (2007) pointed out that there are two mechanisms that mediate interactions in this type of trading zones: boundary objects and interactional expertise.

In the case of *boundary-object trading zones*, interactions take place around objects, be they abstract or concrete. Boundary objects are a concept developed by Star and Griesemer (1989) to explain how a range of actors belonging to different and heterogeneous domains, such as zoologists, amateur collectors, university administrators, curators, clerical staff, taxidermists, etc., interacted in a zoology museum. All these groups had their own interests and they had to be reconciled in a way that worked for all of them. Star and Griesemer’s main argument is that there were some objects that mediated the interaction between these groups, such as specimens, field notes, museums, and maps of relevant territories for the museum activities. Each group would attribute different meanings to them so that their interactions would take place without any group imposing coercion over the others:

> [A boundary object] is an analytic concept of 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 individual- site 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, p. 393).

The second type of fractionated trading zones is that whose main mechanism mediating interactions is interactional expertise. In this case experts from a domain of practice learn the language of another domain without acquiring their practical skills. As pointed out above, within Collins and Evans' framework, high-level specialist expertise can be divided into two types: interactional expertise and contributory expertise (Collins and Evans 2002, 2007). It is possible to acquire interactional expertise in fields in which we do not have contributory expertise and become a *special interactional expert* (Collins 2011). Sometimes scientists acquire a linguistic understanding of the practices of a field which is not their own and become able to effectively communicate with its members (Collins 2011; Reyes-Galindo 2011). Interactional expertise in this case works as a mediating mechanism between heterogeneous expert communities and facilitate interactions in fractionated trading zones.

I have pointed out above that trust is also a mechanism that bridges the gaps between heterogeneous expert communities. Trust however does not fit that easily within the trading zone model set out above because it plays different roles in different social configurations. There are several definitions of trust in the sociological literature (see chapter 7 for more on this). In this work I use the definition set out by Giddens (1990), which has more explanatory power to deal with the issue of trust in contexts of interactions between heterogeneous expert communities. According to Giddens, trust is a crucial sociological mechanism in modern societies in which individuals frequently deal with 'expert systems' that they have very little knowledge about. An example of this is traffic lights. Most people do not understand the complicated system underpinning the working of these devices but trust that they will work properly. With regards to heterogeneous scientific communities, trust comes into play when one community cannot make informed expert judgements about knowledge produced by other communities. In this case, they can only trust or distrust the theories, experiments, data, etc., produced by these other groups of experts (e.g. Shackley and Wynne 1995b; Reyes-Galindo 2011). As I will explain further in chapter 7, trust works here as 'suspension of doubt'.

Having thus far described the reasons for the difficulties in communication between different expert communities and having presented different models for explaining how these social groups bridge the gaps between their heterogeneous domains, it is now time to develop more fully the objective of the present work. I will seek to shed light on the sociological mechanisms that enable experts from distinct fields of expertise to communicate and collaborate to produce knowledge on climate change. I will provide further details below on the high level of heterogeneity of climate-change science, but for the moment it is enough to state that this is one of the most heterogeneous areas of science in that it comprises experts and contributions from virtually all areas of contemporary science. For this reason, climate-change science is an interesting case for STS. It presents several sociological challenges related to the interactions and to the exchange of knowledge between its members.

Studying the entirety of the interactions between all kinds of experts in climate-change science would be an impossible task for an individual given the complexity of the field, much less a realistic goal for a doctoral research project. I will therefore use paleoceanography as a case study - the reasons for selecting this field will be fully explained below - and an analytical model entitled the 'fractal model', developed by Collins (2011) which will help identify different patterns of communication and collaboration within climate-change science. By doing so, I will deliberately leave boundary objects out of the scope of this work. I acknowledge that boundary objects play a relevant role in mediating interactions between different groups of experts in climate-change science as it has been pointed out in the STS literature (Jasanoff and Wynne 1998, pp. 36-37; van der Sluijs et al. 1998; Edwards 2001, pp. 53-54; Kwa 2005). I will focus however on the notion of interactional expertise in combination with the notions of trading zones and trust. The main reason for this is that in my fieldwork on paleoceanography I found much more evidence of language, particularly interactional expertise, facilitating communication and collaboration between experts than boundary objects. In addition, the notion of boundary objects is a well-established concept in STS so that it has already been applied to different contexts and several new theoretical concepts have stemmed from it (e.g. Fujimura 1992; Shackley and Wynne 1996; van der Sluijs et al. 1998). Collins and Evans' theory of expertise and its link to the rest of the STS literature, on the other hand, are still under development. For this reason, there is much more fresh ground to explore when working with this theory than with the notion of boundary objects.

The Fractal Model

The fractal model was developed by Collins (2011) and it is an important part of the theoretical framework of this thesis. I will use it to identify how expert groups communicate and collaborate taking into consideration how far apart they are in terms of their expertise and of their technical language.

The fractal model conveys the idea that a field of science (or any domain of practice) is itself part of wider domains and also comprises narrower sub-communities. For example, geology is a subfield of the Earth sciences and has itself also some subareas, such as paleoclimatology and geochemistry. This idea is not new (e.g. Kuhn 1977, pp. 296-297). What is new about the fractal model is that it implies that there is a similar structure in all levels of analysis. At all levels of analysis there are domains of practice, which can be narrower or wider, that are composed of different types of contributory experts who have their own practical skills. Communication between them is mediated by a shared language that is spoken by all of them. Figure 3 illustrates this point by representing a single domain of practice. The stick figures holding hammers and working on anvils represent different types of contributory experts. The bundles of waves represent the language of the field, which is spoken by all experts that are part of it. There is also a special interactional expert represented in this picture (Collins 2011), who has none of the practical skills of the domain but speaks its language. An example of this is a sociologist of science who spent so much time immersed in a field that he or she became fluent in its language. The special interactional expert is represented by the stick figure that is not working with a hammer and an anvil, but still speaks the domain's language. The main idea behind this figure is that in a domain there are different types of experts who do not have the same contributory expertise. Their work is linked through the language of the field.

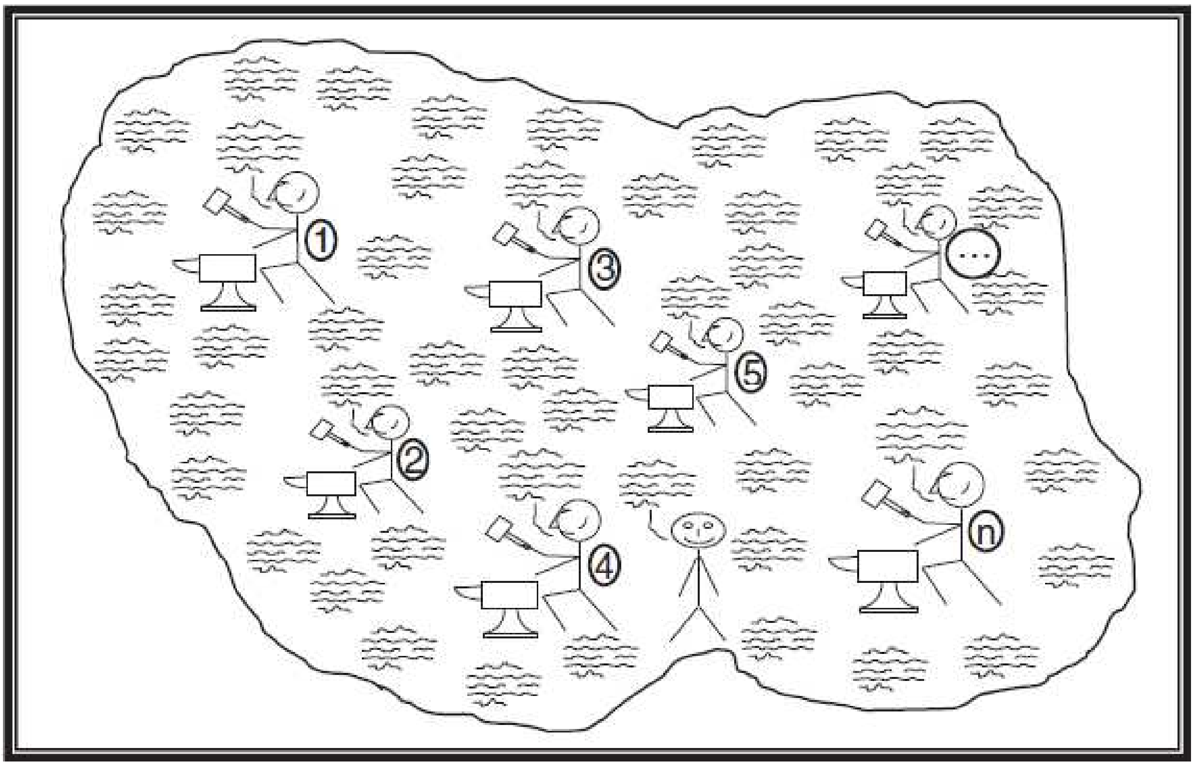

Figure 3: A domain of practice (Collins 2011, p. 276).

This representation of domains of practice is linked to the idea of interactional expertise. Each of the stick figures has access to the practices of the other contributory experts who are members of the domain through interactional expertise in that they only carry out their own practices and have access to the practices of their peers through the language of the domain (Collins 2011). Domain languages, therefore, are not inter-languages. They fulfil a similar role to inter-languages as both facilitate communication, but they are distinct mechanisms. Inter-languages consist of trade languages that are developed to mediate communication between heterogeneous social groups in contexts in which meaning is not fully shared. They are not, however, the languages of autonomous collectivities. Domain languages, on the other hand, are autonomous languages and relatively stable institutions revolve around them. They work on the basis of interactional expertise. If an inter-language reaches the stage in which they are autonomous from their parental languages and have a number of institutions revolving around them, it will have become domain language. This is the case, for example, of biochemistry.

Figure 4 represents the fractal model. It shows that if we zoom in on a stick figure that makes up a domain of practice we find that this particular field of contributory expertise is composed of narrower subspecialties. Conversely, each domain is also part of wider domains of practice. Paleoclimatology, for instance, is a subfield of geology, which, in turn, is a subfield of the Earth sciences. If we keep moving upwards to wider levels, we find that the Earth sciences are a subfield of Western science as a whole, which in turn, is part of the Western culture. Paleoclimatology also comprises narrower fields, such as paleoceanography or dendrochronology[4].

It is important to bear in mind that this description of a domain of practices only makes sense at the societal level. The unity of analysis here is collectivities, not individuals. Individuals immersed in a given domain of practices usually have contributory expertise in more than one of its subspecialties. Collins (2011, pp. 289-290) developed a metaphor that is useful to understand the relation between individuals and contributory expertise. Collectivities are similar to atoms in that they are the basic matter of which the social life is made up of. Individuals can be compared to molecules. They are composed of a number of collectivity-atoms. At the individual level, therefore, scientists usually have the skills of a few different groups of contributory experts.

To present more fully the objectives of the present work: I will seek to identify social mechanisms that mediate communication and collaboration between different expert

[4] These are not definite subdivisions. There are different ways in which scientific fields can be subdivided and the fractal model can be used to represent these different types of subdivisions. In this sense, it is essentially a model and its purpose is to elucidate different features of society rather than being a final description of it.

communities in climate-change science. I will use the fractal model to 'navigate' through different levels of analysis and examine how the gaps between different scientific communities are bridged in the case of higher and lower fractal levels. Because language is denser or more technical in lower fractal levels than in higher fractal levels, different patterns of communication are expected to take place at different levels. I will associate the notion of trading zones to the fractal model and identify the fractal levels at which trading zones are formed. I will also examine the relevance of trust at different fractal levels in mediating communication between different groups of experts. *The main contribution of this thesis to the STS literature therefore consists of identifying how different bridge-building mechanisms work at different fractal levels.*

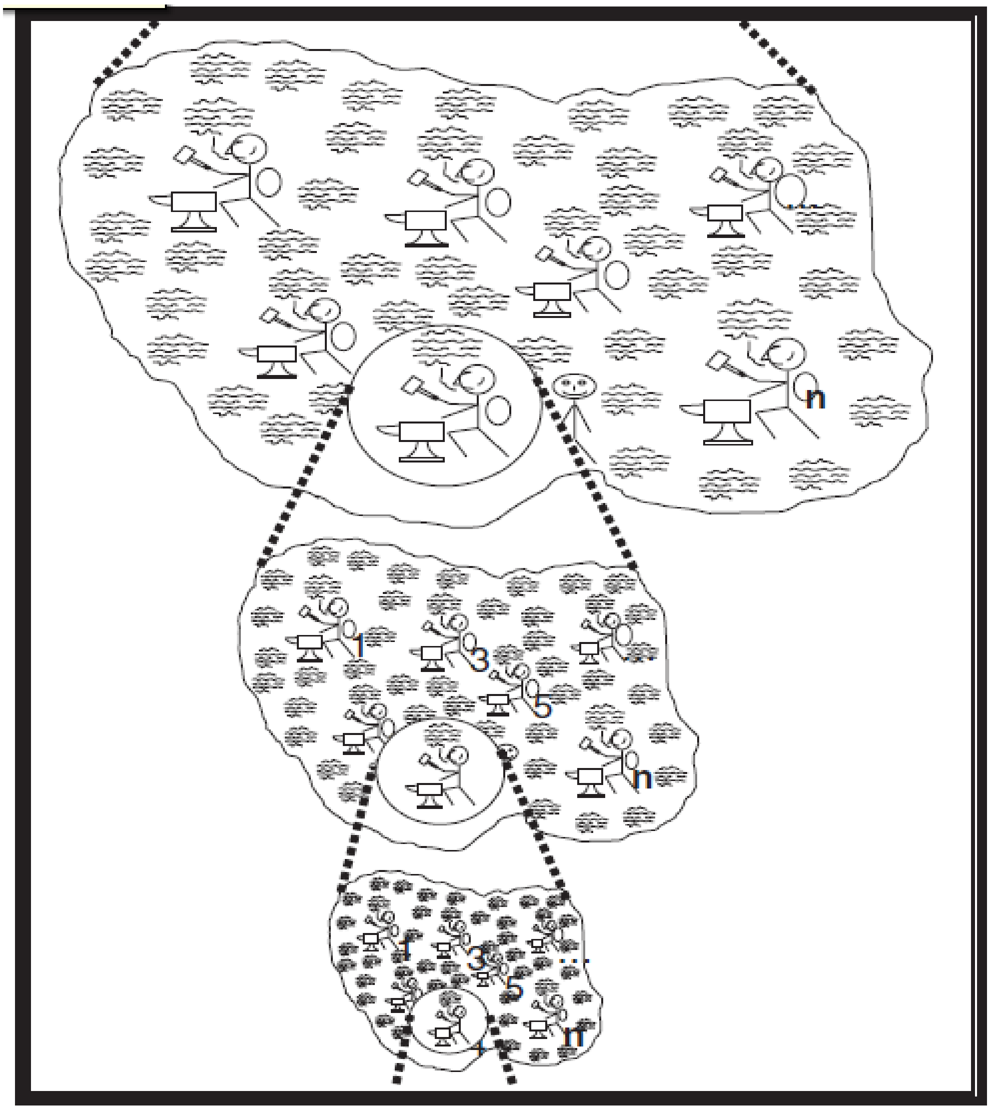

Figure 4: The fractal model (Collins 2011).

Having defined my research question in details I will now introduce the issue of climate change and climate-change science to provide a better sense of what this field of investigation is about.

Climate Change and Climate-Change Science

The international concern over climate change began to grow in the late 1980s (Ungar 1992; Weart 2003). Although scientists had been considering the possibility of anthropogenic global warming for over a century it was only in the 1980s that climate change became a public and political issue (Ungar 1992; O'Riordan and Jäger 1996; Weart 2003). Scientists, policy-makers, and members of the public became interested in knowing how warm the planet could become and what the impacts of climate change would be. As a result, scientific efforts to research climate change were intensified resulting in the creation of the Intergovernmental Panel on Climate Change (IPCC). The IPCC is a United Nations body that reviews the scientific literature twice a decade with a view to providing policy-makers with scientific advice (Ungar 1992; O'Riordan and Jäger 1996; Weart 2003).

The dominant theory of anthropogenic global warming, which is championed by the IPCC, goes as follows (IPCC 2007a)[5]. There are greenhouse gases in the atmosphere, such as carbon dioxide, water vapour, methane, etc. These gases have always been a natural part of the climate system. They play an important role in that they retain part of the solar radiation that gets through the atmosphere and do not let it return to the outer space. Were it not for these gases, the global average temperature, which is around 14°C, would be several degrees below freezing. Their influence on the climate keeps the

[5] This theory, although widely accepted in the scientific community (Oreskes 2004), has been disputed by some scientists, the so-called climate sceptics. A number of claims are usually associated with climate scepticism and they in general refer to disagreements with the main thesis defended by the IPCC. Some of these claims are: Global warming is not happening. Rather, temperatures have been cooling down (e.g. Robinson et al. 1998); global warming is happening but it is not caused by human activities (e.g. Robinson et al. 2007; Idso and Singer 2009); and global warming is happening, but it is not a serious issue. Rather, it will be beneficial to our societies (e.g. Robinson et al. 2007; Idso and Singer 2009).

planet a habitable place for human beings and many other living things. Since the Industrial Revolution, however, human activities, particularly the burning of fossil fuels, has resulted in the emission of larger amounts of greenhouse gases, especially carbon dioxide (see figure 5). According to the theory of anthropogenic global warming, these greenhouse gases would be trapping more solar radiation in the atmosphere and, consequently, making the planet warmer. As a result, global temperature will rise significantly in the next decades disrupting the equilibrium of the climate system and have consequences for the environment. Some will adversely affect human societies, including the rise of sea level, the increased likelihood of extreme weather events (heat waves, droughts, storms, hurricanes, etc.), the loss of biodiversity, and outbreaks of tropical diseases.

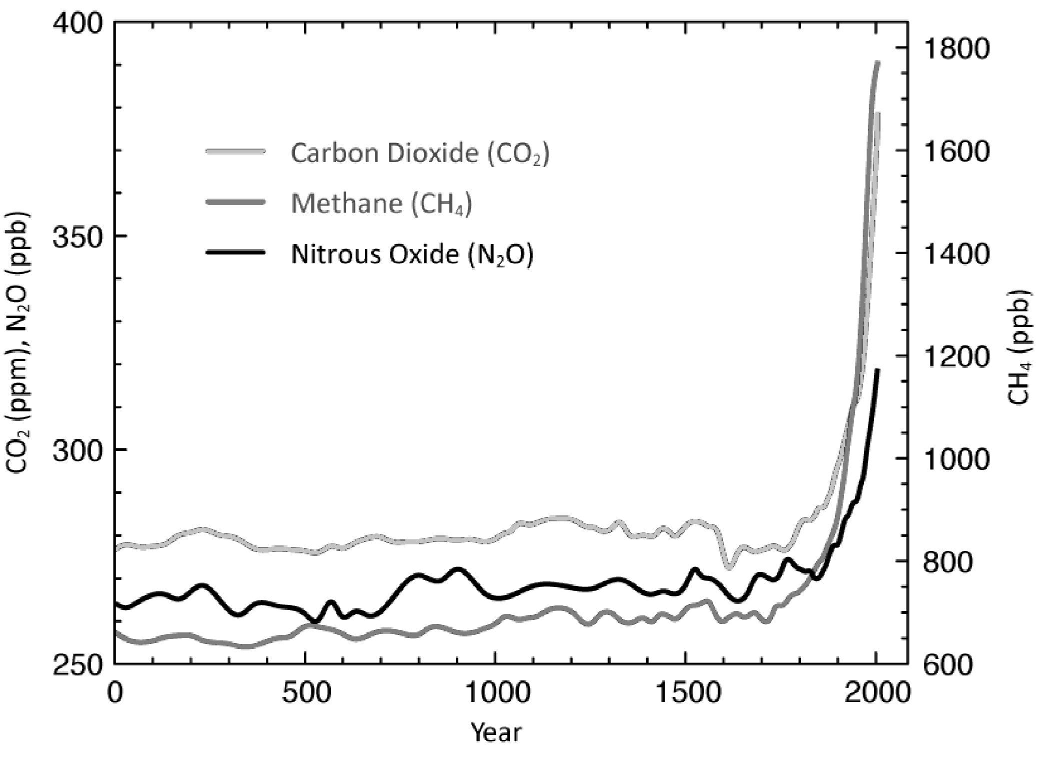

Figure 5: Concentration of greenhouse gases in the atmosphere from year 0 to 2005 (IPCC 2007a, p. 135).

Although the theory of anthropogenic climate change is largely accepted, researching climate change is not a simple task. It involves gathering data from the whole climate system, which is composed of five subsystems (see figure 6): atmosphere, cryosphere, land surfaces, the oceans, and vegetation (Ruddiman 2008). All these subsystems interact in complex ways so that changes in one of them result in changes in the others. On the left-hand side of figure 6 is shown external sources of change in the climate system: the climate *forcings*. On the right-hand side is shown how the climate system may respond to these forcings with variations in any of its subsystems[6].

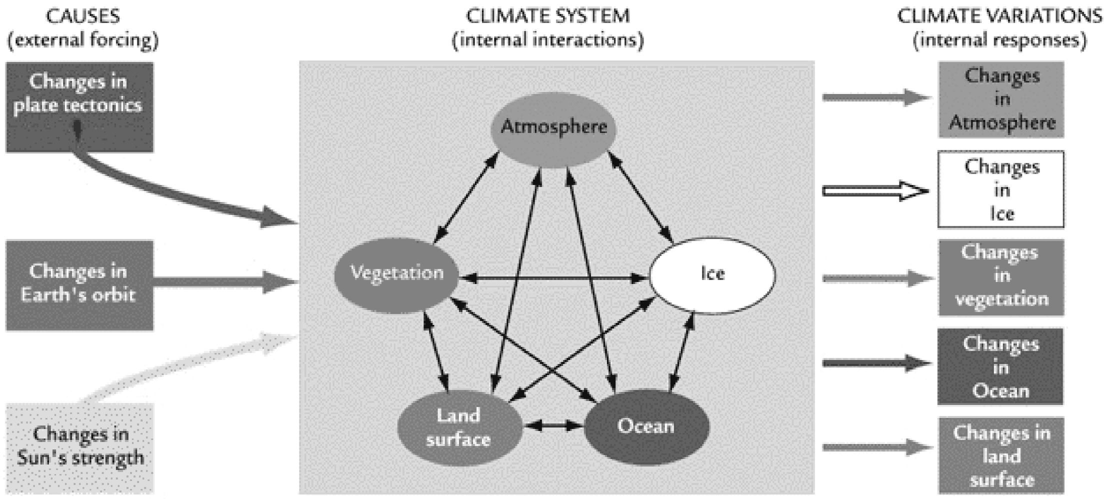

Figure 6: The climate system (Ruddiman 2008, p. 9) .

These subsystems have different response times (Ruddiman 2008, pp. 10-14). This means that the different elements of the climate take different lengths of time to react to the changes. For example, whereas the atmosphere responds quickly to external forcings, going through daily cycles of heating and cooling, it takes from hundreds to thousands of years for ice sheets to respond fully. Scientists have to bring together elements from all subsystems, take into consideration their different response rates, and work out how those combined responses will affect the climate.

[6] Without human interference on the climate, there are three major types of forcings. Firstly, tectonic processes, such as changes in the configuration of the continents - which can open or close ocean basins - and the uplift of mountain ranges. The time-scale of these changes is extremely slow, taking millions of years for changes to occur. Secondly, there are also changes in the Earth's orbit. These alterations take place over tens of thousands of years and change the amount of solar radiation received in the planet by season and by latitude. Finally, changes in the Sun's strength also affect the amount of solar radiation that reaches the planet. Sunspots cycles, for example, last 11 years. There are also longer solar processes. For instance, over the last 4,5 billion years the Sun's strength has slowly diminished.

There are also *feedbacks* which intensify and hugely complicate the changes in the system (IPCC 2007a, p. 97; Ruddiman 2008, pp. 15-16). For instance, if the climate system is warmed by an external forcing ice will melt and uncover land surface (IPCC 2007a, pp. 96-97). Areas which were covered in ice and reflected solar radiation back to the space will begin to absorb radiation. As a result, temperatures will increase more and cause further feedbacks. The opposite can also happen. If the external forcing has a cooling effect, more ice will accumulate on land and contribute to make the climate even colder. Feedbacks make it difficult for climate-change scientists to identify what phenomena are causes and what phenomena are consequences in a process of change. They also generate a kind of 'snowball' effect in the system, which makes it necessary for scientists to assemble data from all parts of it to understand the processes.

In order to understand this very complicated system, climate-change science has grown become an extremely heterogeneous field of science. In the late 20th century scientists began to bring together knowledge from several disciplines, such as meteorology, atmospheric physics, oceanography, biology, glaciology, paleoclimatology, computer modelling, etc. (Jasanoff and Wynne 1998; Edwards 2001) and input them into the analysis. This involved collecting data on the past, on the present, and simulating the future. A number of studies of the impacts of global warming also emerged. These resulted in an even wider range of experts from environmental sciences, economics, and social sciences researching climate-related topics. Finally, experts began to study the best responses to these impacts. Two new fields of investigation then arose: adaptation and mitigation studies. Experts from several different areas started to look for strategies to minimise the impacts of warmer temperatures and to prevent the planet from becoming warmer. Areas of science as diverse as engineering, architecture, ecology, social sciences, demography, economics, etc., became involved in the search for the best responses to global warming. The result was a very complicated mosaic of experts addressing questions such as: what are the main mechanisms of climate change? What climatic changes will happen in the future? What impacts will climate change bring about? How can human societies adapt to these impacts? How can we mitigate climate change so as to minimise its effects?

Narrowing Down to Paleoceanography

Climate-change science is a huge field of science. It would not be possible in a single doctorate research to investigate all groups of experts in this area, how they interact, and how they bridge the gaps between their expertises. I had therefore to focus on a specific subfield of climate-change science to make my research feasible. Making a decision on what field would be the focus of my research was not easy. I decided to research a field that was part of the studies of the mechanisms of climate change – what I call below studies of causes and processes of climate change. This was because the study of these mechanisms is the heart of climate-change science. If it were concluded that the climate is not changing at all, there would be no point, for example, in studying the impacts of climate change. However, this criterion still left me with too many options, including fields such as atmospheric physics, oceanography, glaciology, biology, geology, paleoclimatology, climate modelling, etc. Two other criteria were used to reach a final decision.

Firstly, I decided to study a field that interacts closely with climate modellers. Climate models, as it will be shown in chapter 3, are at the centre of climate-change science. They assemble data from several empirical fields and produce global simulations of how the climate has changed in the past and of how it may change in the future. Secondly, it would be important to study a field that is strongly represented in my university, Cardiff University. As I will argue in chapter 2, the methodology underpinning the present work is based on participant comprehension, which means that I had to immerse myself in the field of science under study. The reason for this was the need to acquire an in-depth understanding of how this field works, what skills are shared by all its members and what skills are not, how difficult members of this field find it to communicate with other scientists, etc. Researching a field of climate-change science that has a strong group at Cardiff University would make it possible for me to have this immersion.

Taking these two criteria into consideration, I decided to focus on paleoceanography. Paleoceanography is a subfield of paleoclimatology. Paleoclimatology is the scientific study of past climates before the emergence of consistent instrumental measurements of climatic variables, which took place approximately in the middle of the 19^{th} century. Paleoceanography consists of reconstructing past climates, but it focuses on the oceans and on its interactions with other parts of the climate system. Paleoceanographers interact closely with computer modellers. Moreover, among the disciplines that are part of the studies of causes and processes of climate change paleoceanography is the only one that has an internationally renowned group at Cardiff University.

Finally, there is very little STS research into paleo-sciences or related geological fields and the few papers published on these areas of science (e.g. Law 1980; Yearley 1990; Skrydstrup 2012) do not help us understand how scientists communicate and collaborate to reconstruct past climates. This made it even more exciting to research this field as I would be exploring unknown territory. It is therefore a secondary goal of this work to shed light on the process of production of knowledge on past climates and to give more visibility in STS to these fascinating fields of investigation.

Thesis Structure

In chapter 1, I describe in detail the realist theory of expertise that underpins the entire argument of this thesis. This theory is based on a sociological interpretation of the Wittgensteinian problem of rule-following. The main argument put forward in this chapter is that there are institutionalised ways of following rules within domains. Only by immersing oneself in the relevant domain it is possible to learn how to follow these rules according to the community's standards. In chapter 2, I present the methods used in the present work. I will introduce the notion of participant comprehension and describe the different steps I took to immerse myself in paleoceanography. Chapter 3 consists of a review of the STS literature on climate-change science. I give a 'big-picture description' of climate-change science and identify the homogenisation mechanisms at work within this field. I argue that although these homogenisation mechanisms are relevant to understand what climate-change science is, they do not produce definite solutions for the problem of communication between expert communities as they have not transformed climate-change science in a homogeneous transdisciplinary field.

In chapter 4, I give a general description of paleoceanography to prepare the ground for the sociological analysis presented in the following chapters. In chapter 5, I argue that paleoceanography is a low fractal level and communication between the different contributory experts that make up this domain is mediated by its domain language, which is rich in technical details. In chapter 6, I examine the interactions between paleo-modellers and the empirically-oriented scientists who are members of the paleoceanographic community. I argue that these expert communities form a fractionated trading zone in which interactional expertise plays a major role in facilitating communication. In chapter 7, I examine the role of trust in mediating the interactions of experts at different fractal levels. I begin by examining paleoceanography and move upwards in the fractal model examining how trust comes into play in different sociological configurations, i.e. in fractionated trading zones or in communication at high fractal levels where there are no bridge-building mechanisms at work. In the concluding chapter I summarise the main findings of the present work and the areas where there is still further research to be done.

Definitions

Before moving on to the first chapter it is necessary to make some conceptual definitions to avoid confusions. Firstly, it is important to clarify what it is meant when the concepts of global warming and climate change are used. Climate change is an umbrella term which refers to any climatic change that has taken place during the Earth history. It includes periods of cold climate as well as periods of warm climate. Global warming, therefore, is a type of climatic change.

It is also necessary to distinguish between *climate science* and *climate-change science*. Although in STS some scholars (e.g. Sundberg 2007) have avoided defining climate-change science as they are mostly interested in how scientists build the boundaries around their fields, in this work it is essential to define this concept. This is because one can only be an expert *in something*. There is no way to carry out research based on a realist notion of expertise without clearly defining what the subject of a community's expertise is. My definition goes as follows: Climate science refers to basic scientific research into the climate system. It focuses on the causes and processes of climate change. Climate-change science has a broader meaning. It refers to all scientific research related to climate change, not only the basic research. It also comprises scientists working on the impacts, adaptation, and mitigation of climate change.

Chapter 1 - Theoretical Framework: Studies of Expertise and Experience

In this chapter I introduce the theoretical framework of this thesis, i.e. Studies of Expertise and Experience (SEE), which has been developed by Collins and Evans (2002; 2007) since the early 2000s. I will set out the realist concept of expertise that underpins the entire argument developed in the thesis. I will argue that expertise is the tacit knowledge shared by members of a domain of practices. I will also examine a criticism of this realist notion of expertise set out by STS scholars who believe that the correct approach to research expertise is the use of attributional theories. I examine this criticism because if expertise cannot be researched by using a realist theory the entire argument of the present work is flawed. I will argue that a realist concept of expertise is useful for addressing STS issues as well as wider sociological phenomena. I will also argue that a realist notion of expertise is not at odds with a constructivist notion of expertise and that a realist notion may sometimes help understand some aspects of the processes of social construction of expertise.

Science and Technology Studies

STS emerged in the early 1970s in the United Kingdom. A number of sociological studies of science began to be carried out at the University of Edinburgh and at the University of Bath. In Edinburgh a group of researchers based at the Science Studies Unit, including David Bloor, Barry Barnes, David Edge, Steve Shapin, and Donald MacKenzie, developed the Strong Programme (e.g. Bloor 1976; MacKenzie 1978; Shapin 1979; Barnes 1982). At the same time the Bath School emerged around the work of Harry Collins at the University of Bath (e.g. Collins 1974, 1975, 1985; Pinch 1986). Inspired by the work of Kuhn (1962) and Wittgenstein (1953), these researchers sought to explain the formation of consensus around scientific theories by using sociological variables, such as negotiation, interest, power, prestige, status, etc[7]. The

[7] Although to a large extent both 'schools' were in agreement, there were two main differences between them. Both groups focused on studying scientific controversies and on showing how social contingencies, particularly interest, status, and power, influenced the settlement of scientific controversies. Research carried out in Edinburgh, however, tended to emphasise macro variables by linking the interests of scientists involved in controversies with the interests of larger social groups. In Bath, on the other hand, research tended to focus on micro-social aspects of scientific controversies. Collins argued that linking controversies in the scientific community with the wider social structure was part of his research

most important rupture brought about by these scholars was with a traditional social division of labour between philosophers and sociologists (Bloor 1976). While philosophers would typically seek to work out the criteria of rationality or truth that would justify theoretical changes in science (e.g. Popper 1934; Neurath et al. 1973), sociologists would typically investigate science as an institution, focusing on its norms, values, and on its relations with other institutions (e.g. Merton 1938; Ben-David 1960). The new generation of sociologists, however, sought to explain the very content of scientific theories, or, in other worlds, show that the acceptance or rejection of scientific theories was not decided on the basis of rational or logical arguments, but by social processes of negotiation (Bloor 1976). The work of this first generation of STS scholars is known as SSK (sociology of scientific knowledge).

In the following years, the field flourished and spread across most developed countries. A number of new approaches emerged. Scholars went to laboratories and carried out ethnographies of science (e.g. Latour and Woolgar 1979; Knorr-Cetina 1981; Charlesworth et al. 1989); sociologists investigated how scientists demarcate science from non-science, the so-called boundary-work (e.g. Gieryn 1983); a sociology of technology emerged (e.g. Bijker et al. 1987); the reflexive school was founded and sociologists applied sociology of science to its own discipline (e.g. Ashmore 1989); feminist and postcolonial STS studies sought to show power relations within science and technology (e.g. Anderson and Adams 2007; Suchman 2007); and actor network theory arose proposing an ontological turn in the humanities that questioned the distinction between nature and society (Callon 1986; Law 1986; Latour 1991, 2005). More recently STS has reached all continents becoming a truly global field of investigation (Fu 2007; Kreimer 2007; Urama et al. 2010).

programme (1981), but this has never been a major topic within his own work. Furthermore, there was a philosophical discussion between both groups. The scholars based in Edinburgh assumed that an independent material and empirical world existed and influenced the content of scientific theories (Bloor 1976; Bloor and Edge 2000). Collins, in contrast, insisted that the sociology of knowledge should adopt as a methodological principle the idea that the empirical or material reality had no influence in the content of scientific theories (Collins 1985). This led members of the Edinburgh School to argue that Collins' programme was idealistic (Barnes, Bloor and Henry 1996).

Since its early days expertise has been a topic of interest in STS. Most researchers have looked at expertise from a constructivist viewpoint and investigated how experts gain/lose credibility or build up trust around their knowledge/expertise. The idea underlying these studies was summarised by Barnes and Edge (1982), who argued that experts' credibility was not based on logical or rational reasons, but on sociological factors. The concept of boundary-work, set out by Gieryn (1983), is a central notion in this type of research. Gieryn (1983) argued that social scientists, instead of looking for the inherent characteristics of science that would distinguish it from other activities (religion, engineering, and so on), i.e. for demarcation criteria, should examine how scientists construct the ideologies that distinguish science from other intellectual activities. In other words, social scientists should look at how science is described by scientists in ways that legitimise it and empower it as an institution. According to Gieryn (1983), scientists use different rhetorical styles and select particular characteristics of science when presenting it to different interlocutors in different contexts. When distinguishing science from religion, for example, scientists tend to emphasise that science is empirical, whereas religions are dogmatic and cannot be refuted by empirical evidence. However, when distinguishing science from engineering, the theoretical side of science is highlighted and contrasted with the hands-on activities of engineers. In other cases, particular groups of scientists label competing groups as 'pseudo', 'deviant', or 'amateur', in order to monopolise professional authority and resources and exclude the competing groups from their domain. In other words, they do so to legitimise their own expertise as genuine scientific expertise. Finally, scientists sometimes also seek to maintain science as an autonomous institution by dissociating their work from consequences they might have (ex. Nuclear physics and the atomic bomb).

In a similar vein, a number of studies have investigated empirically how experts' credibility is constructed or undermined in different settings. STS researchers have examined, for example, how the expertise of scientists and the authority of science is constructed or deconstructed in court (e.g. Oteri et al. 1982; Jasanoff 1997; Lynch and Cole 2005); how lay people constructed their credibility as legitimate speakers of scientific languages in debates surrounding healthcare treatment (e.g. Epstein 1995); how different fractions of a society, ranging from scientists, members of NGOs, stakeholders, schools pupils, etc., define expertise and scientific authority in debates related to science and technology (e.g. Tutton et al. 2005); etc.

A first effort was made in STS to develop a non-constructivist theory of expertise only in the early 2000s, when Collins and Evans developed an approach they call SEE.

The Emergence of Studies of Expertise and Experience

Collins and Evans set out to develop a sociological theory of expertise with a view to contributing to policy-making processes. They argued that there had been too much focus in STS on what they call the problem of legitimacy, but little focus on the problem of extension (Collins and Evans 2002). The former refers to attempts to provide more legitimacy to decision-making processes related to science and technology by including more public participation. The latter consists of establishing boundaries for laypeople participating in decision-making related to science and technology so that the distinction between experts and laypeople does not disappear. Their idea was that although experts were not always right, they were the best source of information available because experts 'know what they are talking about'. They should therefore have a privileged voice in terms of providing technical advice for policy-making.

Although SEE was initially developed to contribute to policy-making related to science and technology, it is also a very useful theoretical framework for investigating how knowledge and technology are produced and transmitted in human societies. Collins and Evans developed a Periodic Table of Expertise in which they categorise different types of expertise, which offers a theoretical framework that can be used to investigate a wide range of sociological problems (Collins and Evans 2007). A number of studies have been carried out in the past ten years applying this framework to a range of different sociological problems, including communication and knowledge exchange in physics (Reyes-Galindo 2011); the formation of trading zones (Gorman 2002; Collins et al. 2007); the transmission of technology and tacit knowledge in the steel industry (Ribeiro 2007a); and the general problem of social division of labour in human societies

(Collins 2011). A new method, the Imitation Game, was developed to investigate the level of understanding that different social groups have of each other (Collins et al. 2006; Collins and Evans 2007, pp. 91-112; forthcoming)[8].

Expertise and Tacit Knowledge

> The realist approach adopted here is different. It starts from the view that expertise is the real and substantive possession of groups of experts and that individuals acquire real and substantive expertise through their membership of those groups. Acquiring expertise is, therefore, a social process – a matter of socialisation into the practices of an expert group – and expertise can be lost if time is spent away from the group (Collins and Evans 2007, pp. 2-3).

According to Collins and Evans (2007), experts are individuals who master the tacit knowledge of a domain of practices. Expertise is the tacit knowledge shared by the members of a domain of practices. These definitions call for an in-depth examination of the meaning of tacit knowledge.

The idea of tacit knowledge was first set out in the philosophical literature by Polanyi (1966). He pointed out that all knowledge is either tacit or, if explicit, it has to be rooted in tacit knowledge (Polanyi 1966, p. 195). This point is crucial because it means that the entirety of our social lives depends on tacit knowledge. For this reason tacit knowledge is (or should be) a central topic for sociology of knowledge.

Collins defined tacit knowledge as follows. “Tacit knowledge is knowledge that is not explicated” (Collins 2010, p. 1). According to Collins (2010), there are three types of tacit knowledge: relational tacit knowledge (RTK), somatic tacit knowledge (STK), and collective tacit knowledge (CTK). This categorisation is based on the reasons why knowledge is not explicated. CTK is knowledge that is tacit because of the very nature of the social. STK is tacit knowledge that could in principle be made explicit, but

[8] Further applications of SEE to a wide range of problems can be found on the special issue of *Studies in History and Philosophy of Science* on Studies of Expertise and Experience published in 2007 and on the website of SEESHOP, the yearly international workshop that brings together researchers from several parts of the world interested in developing and applying Collins and Evans’ realistic theory of expertise to a range of sociological, philosophical, and policy-making problems: http://www.cf.ac.uk/socsi/contactsandpeople/harrycollins/expertise-project/seeshophome.html

because of the nature of our bodies, it is very difficult to do so. Finally, RTK is knowledge that could have been explicated and made explicit, but has remained tacit because of contingent reasons related to the organisation of social groups. I will now examine each of these types of tacit knowledge separately.

Relational Tacit Knowledge

RTK, which is also called weak tacit knowledge, could in principle be made explicit, but it happens that it is not made explicit in social interactions (Collins 2010, pp. 85-98). There are different reasons for this. In some social groups, for example, knowledge is kept secret. This might be due to competition between different groups or because knowledge is believed to be sacred and therefore not supposed to be shared with non-initiates (e.g. Hess 1994; Johnson 2002). Collins (2010, p. 91), for example, found in his early work on scientists building TEA-lasers that these scientists were not completely open to competing groups about how they built these devices. Scientists who had already successfully built these lasers used a number of strategies to withhold information while giving the impression of being open about their discoveries. For instance, when their laboratories were visited by other scientists they would strictly answer what the visitors had asked but would avoid providing any further information that could facilitate the work of other groups.

In other situations, members of a collectivity take for granted that individuals who are not members of their social group share with them knowledge that they actually do not share. This is what Collins (2010, p. 95) call mismatched saliences. During the four years I have been living in the UK, for example, I experienced several occasions in which British people told jokes whose meaning depended on knowing British television shows' characters that I had never heard of and, for this reason, the joke sounded meaningless to me. These awkward situations were usually overcome by me asking what the joke was about, which triggered puzzled facial expressions, or by a change in the topic of the conversation, which left the meaning of the joke mysterious.

Somatic Tacit Knowledge

STK relates to the limitations and potentialities of human bodies. Polanyi (1966) provided what is probably the most well-known example of tacit knowledge: bicycle riding. One cannot learn how to cycle just by listening to someone explaining how he or she does it. Learning to cycle is a process of skill embodiment that requires some time until the body becomes used to moving in a way that keeps the bicycle balanced. Having other people giving advice on how to do it certainly helps, but verbal explanations by themselves are not enough for enabling someone to cycle. It is not possible to write a manual on balancing on a bicycle that would make an individual, just by reading it, become able to cycle.

Yet, Polanyi pointed out that it is possible to write the rules that describe in terms of physics concepts how to balance on a bike (Collins 2010, pp. 99-101). However, one does not learn to cycle just by reading these rules. This is because when cycling decisions have to be made in fractions of seconds to keep the bicycle balanced. Collins (2010, p.100) took this point further. He pointed out that if our brains and other parts of our physiological systems were quicker, or if we cycled in a small asteroid where gravity was close to zero, we would be able to follow Polanyi's rules and, by doing so, keep the bicycle balanced. Whenever the bicycle started to fall over we would have enough time to check the instructions and correct its balance. In this sense, it is due to the nature of our bodies and of our physiology that one needs to acquire tacit knowledge to balance on a bike and not because the rules underpinning cycling cannot be made explicit.

In other cases STK is tacit not because of the speed in which decisions have to be made, but because it is necessary to adjust our bodies to doing things they are not used to. Let us take playing the guitar as an example. When beginners first learn bar chords, they cannot do them effectively. They usually press the strings really hardly and still do not extract a nice sound from the instrument. It takes them at least a few weeks, and usually some months, to adjust their bodies to playing these chords. Experienced guitar players, on the other hand, press the strings softly and produce a beautiful sound. I have taught guitar playing for years and whenever I tried to explain to my students how they should position their fingers on the strings to play bar chords so as to produce the 'right' sound they could never do it immediately. It always took them some time until their hands 'learned' how to press the strings with the appropriate strength.

It is worth noting that the positioning of fingers on the guitar and the amount of pressure that has to be put on the strings to play bar chords could be described in terms of rules. In other words, the knowledge necessary to play these chords could be made explicit. However, this description would be useless for a beginner as their fingers are just not used to being utilised in certain ways[9].

Collective Tacit Knowledge and Rule-following

CTK, as Collins (2010, p. 119) pointed out, "is the irreducible heartland of the concept [of tacit knowledge]", at least from a sociological perspective. This is because the concept refers to knowledge that is tacit due to the very nature of the social. CTK is the property of collectivities, not of individuals. It can only be acquired through immersion in the community in which it is shared.

CTK consists of knowledge that depends on rule-following. Rule-following here is understood according to a specific interpretation of the late work of Wittgenstein (1953) initially developed by the philosopher Winch (1958) and then transformed into a strong theoretical pillar of SSK by authors such as Collins (1985), Collins and Kusch (1998), Bloor (1983, 1997), and Barnes (1982)[10]. According to this interpretation of Wittgenstein, rules organise the social world and, consequently, social actions can be assessed as to whether or not they were performed 'correctly'. In other words, it is

[9] These examples emphasise the limits of our bodies, which Collins (2010) call somatic-limit tacit knowledge. STK, however, also refers to the potentialities of our bodies, the so called somatic-affordance tacit knowledge (Collins 2010). If human beings did not have opposable thumbs, for example, it would be much harder (although not impossible) for us to play bar chords as we would not have a thumb to press against the neck of the instrument.

[10] There are alternative interpretations of Wittgenstein in STS, such as the one put forward by ethnomethodologists (e.g. Lynch 1992). See Bloor (1992) for a defence of SSK's interpretation of Wittgenstein against ethnomethodology.

possible to make judgements on whether social actions conform to the rules of a form of life.

Although rule-following is trivial for someone who is fully immersed in the relevant form of life, for a non-socialised person it is not straightforward. This is because rules do not contain the rules for their own application. The following example that has been widely used in the STS literature (e.g. Collins 1985; Bloor 1997) illustrates this point.

If we take the sequence 2, 4, 6, 8 and ask any individual who had a basic mathematical education to continue it *the same way* it is very likely that he or she will automatically continue with 10, 12, 14, 16, 18, and so on. However, there are infinite different ways of continuing this sequence, such as 2, 4, 6, 8, 2, 2, 4, 6, 8, 2, 2, 4, 4, 6, 8, or, 2, 4, 6, 8, 2, 4, 6, 8, 10, or 2, 4, 6, 8, 1, 3, 5, 7, etc. Somehow there is a shared understanding that continuing this sequence means adding 2 to the last number of the sequence *ad infinitum*. Even though there are alternative ways of following this sequence socialised people tend not to consider them. It is as if there were some kind of logical compulsion leading individuals to follow the sequence in a particular way. If socialised people do not continue with the sequence by adding 2 to the last number they feel as if it they were making a mistake – or at least some extra account is needed to justify why it is not being continued in the *usual* way.

The rule 'continue the sequence in the same way' can be followed in several different ways as in the example provided in the paragraph above. However, we feel compelled to follow it in a *specific* way. The reason why we feel this way is because there are *institutionalised* ways of following rules (Bloor 1997). Rules are *institutions* that frame the way people behave within a form of life. In our society there is a shared understanding that there is *one* correct way of following the number sequence, which is by adding 2 to the last number. As Bloor (1997, pp. 15-16) has pointed out,

> […] a tacit *consensus* of actions determines what is counted as a 'right' step, i.e. a genuine and successful piece of rule-following, if it is aligned with the steps everyone else, or nearly everyone else, takes.

To understand these ideas better I will examine the concept of institutions closely.

Rules as Institutions

Institutions are "collective patterns of self-referring activity" (Bloor 1997, p. 33). They are self-referring because "there is no way to justify the pattern of behaviour without circularity" (Bloor 1997, p. 31). Money, for example, is an institution (Bloor 1997). One of the forms in which we exchange money is by using coins. Coins have value because of the social convention that defines them as money. There is nothing intrinsic in the nature of coins that makes them more or less valuable, they are only metal discs.

> Calling something a coin is correct because it is the practice to call it a coin. Although 'coin' doesn't mean 'called a coin by others', ultimately, it is correct to call something a coin because others call it a coin. If there is a reality that matches or corresponds to any given episode of such talk it can only be the totality made up of this and all the other similar episodes. Talk of coins, taken collectively, is not about a reality that is independent of such talk. It is, in a sense, just talk about talk (Bloor 1997, p. 30).

A thought-experiment helps understand this point. In the case of an anthropologist who was walking through the Amazon jungle and found an indigenous tribe who had never been contacted before, he or she would not try to buy food or to pay for accommodation by using coins. This tribe would quite possibly have no concept of money[11].

Institutions are also self-creating (Bloor 1997, pp. 30-35). Whenever we use money or talk about money we are reinforcing its meaning and its usage. Similarly, a bank that is considered sound will continue to be regarded as sound if all clients do not withdraw all

[11] Bloor distinguishes between natural kinds and social kinds. Coins are social kinds, which means that there is no independent reality in their 'coin-ness' that has to be matched by their social meaning. Natural kinds, on the other hand, such as trees, cats, dogs, pebbles, etc., have an external reality independent of human institutions. All institutions that are built around them have to match the possibilities that they afford (Bloor 1997, p. 30).

their money at the same time. Each person who believes that a bank is a sound institution and does not withdraw his or her money from it is also contributing to maintain the soundness of the bank. However, if all clients withdraw their money at the same time the bank will cease to be able to work as a bank. That a bank can remain a bank depends on its users treating it as a bank.

Rule-Following and Social Actions

Going from one instance of application of a rule to another is not necessarily a clear-cut step. To an accomplished rule follower, taking the next step is straightforward, but for apprentices or for an outsider, this might look like a complicated step. They might not be sure whether they are dealing with a situation which is the *same* as previous situations. The issue at stake here is that the very *sameness* of one instance of application of a rule to another instance is defined by its institutionalised usage. In this sense, whether a situation is collectively regarded as the same as another so that certain rules apply to this new situation, depends on a collective agreement about the 'sameness' of the new situation compared to previous situations. In this sense, applying a rule is an open-ended practice, in which, at each new instance of application, the actors have to make a judgement as to whether they are facing a situation that is similar to previous instances or not. This point was summarised by Collins and Kusch (1998, pp. 13-14) who pointed out that future application of rules are underdetermined by past instances[12]:

> Obviously institutions must be bounded and rule-following in some sense, or they would not be recognizable as institutions, but the way they are lived out is continually up for renegotiation – at least at the boundaries. Moving from context to context, actors continually reconstitute the institution through their decisions and actions. Such judgements are unavoidable, but they are underdetermined by past instances.
>
> That judgements of how to act in new instances cannot be arbitrary is ensured by the fact that they must be collectively justifiable within the institution. In this sense, the 'rule-following' is not congruence with a formula, but mutual recognition that no mistake has been made.

[12] This is a crucial point. If rule following were not underdetermined by past instances there would be no possibility in this theoretical framework for social change and for processes of negotiation.

There are two important points in this quotation. Firstly, as mentioned above, rule-following is underdetermined by past instances. Furthermore, it emphasises that the ultimate criteria for assessing whether a rule has been correctly followed is the consensus within the relevant community that no mistake has been made. This point is very important because it shows that rules organise our social lives by generating patterns of social actions[13] and, consequently, of interactions. It also leaves open the possibility of social change, which takes place when social actors agree that new ways of interpreting the rules are legitimate.

Collective Tacit Knowledge and Rule-following

I have argued thus far that that there are institutionalised ways of following rules. These social patterns organise social life and make it to a certain extent predictable. I have not described yet how individuals become accomplished rule followers.

Individuals learn how to follow rules according to institutionalised patterns through immersion in the relevant form of life. This results in the acquisition of CTK, which consists of the ability to apply social rules in new contexts without making mistakes. CTK cannot be transferred solely through formal instructions because it depends on rule-following. If one tried to write the rules on how to follow a given set of rules, this would lead to an infinite rules' regress, where each set of 'meta-rules' would require further meta-rules to elucidate how they should be followed. It is only through socialisation that institutionalised patterns of rule-following can be learned. By being socialised in a form of life individuals are exposed to the institutionalised ways of following the rules of the collectivity, which leads them to acquire CTK.

[13] This particularly applies to actions defined by Collins and Kusch as formative actions (1998, pp. 10-12). Formative actions are actions that constitute a form of life. "Such actions make a society what it is and distinguish it from other societies (Collins and Kusch 1998, pp. 10-11). In a Catholic mass, for example, there are a number of actions carried out by the priest that are formative whereas others are not (Collins and Kusch 1998, p. 11). Whether the priest sings or not along with the congregation is not central to the mass. The action of praying for transubstantiation, in contrast, is an absolutely crucial part of this ceremony. If a priest does not do it, a mass will not have been performed properly. In this sense, transubstantiation is a formative action whereas the priest singing along with the congregation is not. Obviously, in different contexts and historical moments, different actions are formative whereas others are not. Singing along with the congregation, for instance, is a formative action in some sects of the Catholic Church, such as the Charismatic Renovation.

In sum, CTK consists of knowledge that is not explicated not because of social contingencies, as in the case of RTK, nor because of the physiology of our bodies, as in the case of STK[14]. It is not explicated because it is not possible to do so. Any attempt to explain it leads to a rule regress. It can only be developed through processes of socialisation where individuals are continually exposed to the institutionalised ways of following rules.

Expertise

Expertise, as pointed out above, is the tacit knowledge shared by the members of a domain of practices. Although tacit knowledge can be analytically classified according to Collins' typology, in the social world the three types of tacit knowledge are usually entangled. For example, as pointed out above (see footnote 14) cycling has somatic and collective dimensions. Scientific work involves all types of tacit knowledge. It involves RTK when scientists avoid sharing their laboratory procedures with competing groups. It also involves STK, which comes into play when scientists operate instruments in laboratories or in the field, or when they use their intuition (Reyes-Galindo 2011). It also involves CTK, which cuts across the entirety of their social actions. There are social rules, for instance, on how to collect data (methodological rules), on how to present findings (stylistic rules), on how to address peers in conferences, on how to assess the quality of particular publications, etc.

One can be an expert in any activity that requires a minimal amount of skills. Lying in bed, for example, is not a field of expertise as anyone can master it immediately without

[14] Distinguishing actions that can be automated from those that cannot be automated helps differentiating CTK from STK (Collins 2010). If the activity at stake can be reduced to a set of rules that machines can follow, then it is performed on the basis of STK. CTK, on the other hand, depends on social judgements that can only be made by human beings who are socialised in the relevant community of rule-followers, therefore it cannot be automated. An example of this is the difference between bicycle balancing and riding a bicycle in the traffic (Collins 2010, pp. 121-122). The former has been automated and robots that can ride bicycles were created. The latter, on the other hand, cannot be automated because robots cannot understand the social conventions involved in cycling in busy traffic. Cycling in the traffic depends on making social judgements about the behaviour of drivers and of other cyclists. These judgements depend on understanding social rules and following them correctly. These rules are different in different societies and they change through time. It is necessary therefore to be continually immersed in the 'traffic culture' of a given society to be up to date with its institutionalised way of following traffic rules.

the need to acquire any skills (Collins and Evans 2007, p. 17). There is a wide range of activities, however, that require the acquisition of skills to be effectively performed, including farming, sports, arts, music, gardening, engineering, science, and so forth. In this sense, expertise is not defined by the subject matter, but by the process of acquisition of tacit knowledge and consequently the development of 'social fluency'.

Expertise enables individuals to make judgements according to the standards of the relevant community. Ribeiro (forthcoming-b) identified three types of judgements that an expert can make: judgements of similarity/difference, judgements of relevance/irrelevance, and judgements of risk and opportunity. Judgements of similarity and difference are the crux of the idea of CTK as they underpin judgements of sameness:

> The 'judgement of similarity/difference' underlies the ability to identify what is considered 'the same' as well as violations of tolerance (e.g. mistakes, improprieties and problems) in rule-following situations and outcomes. […].This type of judgement also underlies the ability to create contrast (similarities versus specifics) between situations, scenarios or technical proposals, to provide reliable estimations based on past experiences and to make correct 'approximations' (Kuhn 1962) in a field (Ribeiro forthcoming-b, p. 9).

The other types of judgements are also crucial for living within a form of life. Judgements of relevance/irrelevance relate to the ability of attributing value to all elements that are part of our social lives. These judgements are also linked to judgements of risk and opportunity, which refer to assessing the consequences of actions or events:

> The 'judgement of relevance/irrelevance' is the ability to locate and attribute value to events, claims, artefacts and people within the current and past history of a given form of life. This judgement enables enculturated actors to prioritise correctly, to 'retrieve selectively', to evaluate who is who—and who to trust—to identify key changes/tendencies and to weigh the pros and cons between options. In some cases, judging relevance/irrelevance presupposes or encompasses the 'judgement of risk and opportunity', i.e. the ability to evaluate the (short-, medium or long-term) consequences of ongoing or future actions or events within a form of life (Ribeiro forthcoming-b, p. 10).

In the case of science, for example, scientists have to make judgements about the relevance of certain papers and theories are in the scientific literature, and how much they might be risking their careers when putting forward innovative/controversial ideas and concepts.

Knowledge that Depends on Ubiquitous Tacit Knowledge

Collins and Evans (2007) created a Periodic Table of Expertise, in which they describe the different types of expertise. They classify expertise into two main types: ubiquitous expertise and specialist expertise. Human beings have a large amount of ubiquitous expertise that enables them to live in society. This include a wide range of aspects of social life, such as one's ability to speak one's native language(s), one's understanding of what good manners are in different contexts, one's knowledge of how to acquire essential goods for their survival, such as water and food, etc.

Specialist expertises are those that are the property of particular social groups so that they are not spread across a whole society. The simplest forms of them require only ubiquitous tacit knowledge and refer to the acquisition of varying amounts of explicit knowledge. Collins and Evans described three main types of knowledge that fall into this category: beer-mat knowledge, popular understanding of science, and primary source knowledge.

Beer-mat knowledge is propositional knowledge that can be used for answering quiz-type questions. If one knows the date when Christopher Columbus first reached America, this can be used to answer a quiz question correctly, but it does not enable one to go much beyond that. This information by itself does not carry any useful information about the importance of this event, its causes, its consequences for European and American history, and so on. In other words, knowing a historic date does not provide an individual with an accurate understanding of history.

Popular understanding is significantly more sophisticated than beer-mat knowledge, although still limited if compared to higher level specialist expertises. In science it consists of knowledge about scientific theories which is acquired through the mass media or by reading popular-science books. This information is ‘digested’, simplified, and condensed so that the public can make sense of it. Although it might provide people with a general understanding of scientific ideas, it does not convey all the complexity of scientific life and does not enable individuals to make expert judgements. As popular understanding of science does not involve any immersion in the scientific community, the public might end up with highly misguided conceptions of the state of a scientific field. Settled science, for example, may sometimes be taken to be controversial, particularly in cases where there are strong interests at stake (e.g. Oreskes and Conway 2010). The opposite might also happen and members of the public sometimes take seriously theories that experts would not regard as credible (e.g. Boyce 2006). This is because members of the public do not have specialist CTK to weigh the relevance of different theories within the relevant scientific domain.

Primary source knowledge consists of knowledge that is acquired through reading primary literature or quasi-primary literature, but without immersion in the relevant community of experts. Individuals can acquire a great deal of primary source knowledge by searching on the internet, by borrowing books from libraries, reading academic journals, and so on. Being able to understand and to reproduce complex scientific arguments might give individuals and the people around them a sense that “they know what they are talking about”. Primary source knowledge, however, does not enable individuals to make judgements related to the relevance and the level of uncertainty surrounding different scientific theories. For examples, an individual interested in anthropology who went to a library looking for some books on this subject could end up with books written by evolutionists from the late 19th century, who, although regarded as the founding fathers of this field, are considered ethnocentric by contemporary anthropologists. Even within current science it can be hard for outsiders to recognise real/important dissent as opposed to marginal views (e.g. Weinel 2010).

Specialist Tacit Knowledge and Expertise

Specialist expertises are those that require socialisation in the relevant community of experts to acquire them. Collins and Evans (2007) described two main types of specialist expertise: contributory expertise and interactional expertise. *Contributory expertise* consists of the ability to contribute effectively to a domain of practices. An expert in cloud physics, for example, is capable of carrying out research into the physics of clouds that meets the standards of the relevant peer group, i.e. other cloud physicists.

Interactional expertise is the mastery of the language of a domain of practices, which results in an interactional expert being able to engage in an informed conversation with contributory experts. Managers of big-science projects are an example of interactional experts (Collins and Sanders 2007). They have to manage large-scale projects that involve scientists from a wide range of specialties even though they do not have contributory expertise in all these fields. However, because they can talk in an informed way to scientists with different expertises they are able to lead these projects. Some sociologists and anthropologists of science are also examples of interactional experts. They can neither carry out experiments nor write papers in the field of science they research, but they can keep up with a conversation between scientists and ask informed questions (Giles 2006; Collins and Evans 2007).

Contributory experts also have interactional expertise as they have the ability to talk about their own practices. Scientists, for instance, spend much time talking about their research and in certain fields, such as gravitational waves physics, more time talking then carrying out experiments or analysing data (Collins 2011). However, as the examples given above indicate, not everyone who has interactional expertise has contributory expertise. People who have only interactional expertise are *special interactional experts* (Collins 2011).

Criticisms of Collins and Evans: Attributional Theories of Expertise

Collins and Evans' theory on expertise started a great debate in STS (Lynch 2003, p. 325). Some members of the community have used their definition and/or typology of expertise in their studies (e.g. Gorman 2002; Roth 2005; Faulkner 2007; Marie 2008; Sismondo 2009; Rosenberger 2011). Others, however, have criticised their programme. Most criticisms have been directed towards their policy-making ideas (e.g. Jasanoff 2003; Wynne 2003; Tutton et al. 2005; De Vries 2007; Hildebrandt and Gutwirth 2008; Fischer 2009; Moore 2010; Papadopoulos 2011). However, a small number of people have criticised their definition and/or their typology of expertise (e.g. Jasanoff 2003; Rip 2003; Lynch and Cole 2005; Carr 2010). The present work does not deal with policy-making thereby criticisms related to this topic do not affect it. I will therefore only address critiques that have been made against the realist notion of expertise developed by Collins and Evans.

Among those who criticised Collins and Evan's definition of expertise, there is one argument that stands out as the greatest threat to investigating expertise on the basis of a realist definition of this concept: the argument according to which expertise can only be studied by using attributional theories. Addressing this criticism is crucial for my work as it relies on Collins and Evans' realist definition of expertise. I will focus on two scholars who made this point explicitly in the literature, namely Jasanoff (2003) and Carr (2010). I will argue that there is no reason why attributional approaches to expertise *prohibit* a realist approach. I will also argue that a realist approach may help understand relevant aspects of attributional studies.

Sheila Jasanoff believes that social scientists should not deploy a realist definition of expertise. Rather, they should investigate questions such as: how expertise is defined in different social settings; how experts gain or lose credibility; how the boundaries between experts and non-experts are socially constructed, etc:

> Well, to begin with, I have always insisted that expertise is not merely something that is in the heads and hands of skilled persons, constituted through their deep familiarity with the problem in

> question, but rather that it is something acquired, and deployed, within particular historical, political, and cultural contexts. Expertise relevant to public decisions, I have further shown, responds to specific institutional imperatives that vary within and between nation states. Accordingly, who counts as an expert (and what counts as expertise) in UK environmental or public health controversies may not necessarily be who (or what) would count for the same purpose in Germany or India or the USA. […] Finally, what operates as credible expertise in any society corresponds to its distinctive civic epistemology: the criteria by which members of that society systematically evaluate the validity of public knowledge (Jasanoff 2003, pp. 393-394).

Jasanoff's work is clearly focused on instances where expertise is negotiated in decision-making settings. However, her argument is not only that a realist definition of expertise is not adequate for policy-making purposes. She indicates that she does not believe that a realist concept of expertise could be developed at all: "Nor there is an objective Archimedean point from which an all-seeing agent can determine who belongs, and who does not, within the magic rings of expertise" (Jasanoff 2003, p. 394)[15]. She criticises Collins and Evans for essentialising the nature of expertise by not taking into consideration contingent socio-historical factors that influence the power and credibility attributed to experts (Jasanoff 2003, p. 392). According to her, the appropriate role for STS is to describe how different agents define expertise and attribute different levels of credibility to them:

> […] what emerges as most deserving of analysis by our field is how particular claims and attributions of expertise come into being and are sustained, and what the implications are for truth and justice; the intellectually gripping problem is not how to demarcate expert from lay knowledge or science from politics (though reflexive attempts to make such demarcations should be taken seriously). Such demarcations will keep being produced in any case, in the everyday work of scientists, citizens and institutions of governance. Showing what is at stake in the making of such boundaries is another matter. That is a fitting place for critical science studies scholarship (Jasanoff 2003, pp. 398-399).

There are two lines of argumentation in Jasanoff's criticism. Firstly, there's no Archimedean point from where it is possible to determine who is an expert and who is not. Indeed, within a realist theory of expertise sometimes there will be issues in identifying who the experts are. This argument, however, is unsatisfactory as if it is taken seriously it undermines all attempts to do scientific research. As much as there are

[15] It should be noted that at some points she is ambiguous about this: "We need both strong democracy and good expertise to manage the demands of modernity, and we need them continuously" (Jasanoff 2003, p. 398). It is not clear exactly what she means with 'good expertise' if she only accepts an attributional approach to expertise, but taking her whole argument into account it cannot be a realist notion.

issues in operationalising the concept of expertise, there are issues in operationalising all sociological concepts, including concepts such as interests, social class, power, credibility, etc. Yet, sociologists have been working hard for over a century to find the best ways to operationalise their concepts. With regards to the specific concept of expertise, one could object that it is particularly resistant to attempts of operationalisation. This does not appear to be the case. Several recent studies informed by Collins and Evans' framework have successfully deployed their definition and typology of expertise to a range of STS problems and to more general sociological issues (Collins et al. 2007; Ribeiro 2007b, c; Collins 2011; Reyes-Galindo 2011). These studies do not rely on an Archimedean point from which they can assess whether particular actors have expertise or not. Rather they use a sociological criterion to identify who the experts in a give domain are. That is, they are based on the idea that expertise is acquired through immersion in the community of relevant experts. Whether or not a particular actor is an expert depends on whether he or she has had immersion in the relevant domain. This is not a matter of looking at the social reality from an Archimedean point, but of carrying out sociological research, which, as research in whatever scientific domain, will sometimes get things right and sometimes get things wrong.

This is not to say that there are no boundary issues related the usage of concepts. These issues will always emerge due to the very nature of language, which depends on rule-following. However, this cannot be used as an argument against using a realist concept of expertise in research, otherwise all explanatory science is also condemned.

Jasanoff also believes that the role of social scientists is to investigate the different meanings of expertise in different contexts, or, in other words, how individuals acquire credibility and authority as experts. Indeed expertise is defined in different ways by different actors. Investigations on these processes of boundary-work have given an invaluable contribution to STS and to a better sociological understanding of social life. However, I disagree with the idea that expertise can *only* be studied by using attributional theories. As pointed out above, a number of studies based on Collins and Evans' theory of expertise have recently been published that shed light on several sociological and STS issues that cannot be explained by attributional approaches. For example, from the viewpoint of an attributional theory of expertise nothing can be said about how social division of labour is possible. In other words, how can people with different contributory expertise/skills work collaboratively? In contrast, a realist theory of expertise helps understand how different types of contributory expertise build bridges between their practices. Collins (2011), for example, pointed out that social division of labour is only possible because of interactional expertise in that it bridge the gaps between different types of contributory experts and makes communication between them feasible. Gravitational waves physics is an example of this. There are several different types of contributory experts working in this field (e.g. experts in mirror suspension design, laser development, analysis of waveforms, and so forth) and, as Collins (2011, p. 277) pointed out, "they do not do each other's work, so the only way they can gain such understanding is via a shared practice language".

Another example of the value of a realist theory of expertise is in explaining what happens to STS scholars when they carry out their analysis of scientific fields. Collins and Evans (2007, pp. 31-5) pointed out that STS research frequently relies on the acquisition of interactional expertise. STS scholars usually begin their fieldwork without much knowledge about the field under study and end up learning a great deal not only about the interactions between their research subjects but about the language they speak (see chapter 2 for more on this). This does not make them contributory experts in the field of science under study, but accomplished speakers of the domains' language, which enables to them to engage in informed conversations with experts[16]. In other words, they acquire interactional expertise. Again, an attributional theory of expertise cannot explain anything about this process. It could only describe whether the STS scholar acquired status within the community under study as a fluent speaker of the language or not.

[16] An example of this is Collins (Collins and Evans 2007; Giles 2006), who became so fluent in the language of gravitational-waves physics that he could not be distinguished from gravitational-waves physicists in an imitation game.

Furthermore, the very fact that different social groups have different definitions of a concept should not prevent sociologists from formulating their own definitions. Several sociological concepts are also used by non-sociologists and defined in a range of ways. If this is an obstacle for STS, this again would also immobilise all explanatory science.

The Benefits of a Realist Definition of Expertise to Attributional Studies

In a recent review of the anthropological literature on expertise entitled *Enactments of Expertise*, Carr (2010) parallels Jasanoff's argument that expertise cannot be examined in isolation from its social context. Carr's point of departure is the assumption that expertise is not something that people have, but rather something that people do (2010, p. 18). Expertise is enacted by agents and by doing so they acquire their status of experts. Carr's approach to expertise is informed by the idea that expertise cannot be disentangled from the power relations within the networks in which they are enacted. Furthermore, expertise, according to her, entails creating hierarchies and distinctions between actors and objects. In other words, experts perform their expertises and, as a result, participate in the process of constructing the reality.

> After all, to be an expert is not only to be authorized by an institutionalised domain of knowledge or to make determinations about what is true, valid, or valuable within that domain; expertise is also the ability to 'finesse reality and animate evidence through mastery of verbal performance' (Matoesian 1999, p. 518). Accordingly, this review approaches expertise as intensively citational institutional action, rather than as a powerful cache of individual knowledge that is simply expressed in social interaction. To this end, I highlight how expert actors use linguistic and metaliguistic resources – such as jargon and acronyms - and poetically structure real-time interaction. I also address the role of gestures, uniforms, and other visual media in the enactment of expertise (Carr 2010, p. 19).

Jasanoff and Carr are right when they point out that Collins and Evans' approach to expertise do not cover all social dimensions of expertise. The credibility of experts is socially constructed and experts also construct social realities by using their power and prestige. However, this does not undermine Collins and Evans' theory. Both dimensions of expertise can be investigated by STS and they are not mutually exclusive. Rather, a realist approach can bring to light interesting aspects of the social construction of the credibility of experts. Ironically, this is made very clear in Carr's review. She dedicates a whole section of her paper to discuss socialisation processes which lead people to become experts. In this section she provides several examples of the need to acquire expertise first to then be able to enact it. The following quotation is an example of this:

> The social organisation or training has arguably been of enduring interest to anthropologists, at least since Malinowski (1964[1922]) described the range of skills that one must master to initiate a ritually and technically sound canoe – from the selecting, felling, and transporting of trees to the recitation of rites during the piecing together of ribs, poles, planks (Carr 2010, p. 20).

The following quotation also exemplifies the same point:

> Because being socialised as an expert involves establishing a deliberate stance in relation to a set of culturally valued or valuable objects, novices must master a register – that is, a recognizable, if specialised, linguistic repertoire that can include technical terms or acronyms, specific prosodic practices, and non-verbal signs such as facial expressions or gestures (Carr 2010, p. 20).

In short, in cases where experts try to build up credibility around their expertise they have to be able to show that they have actually mastered the practices they claim to be experts in. A relevant variable in this case is whether the people watching them 'performing' their expertise have any expertise in the domain of practices at stake themselves. If they have, then it is essential for the individuals enacting their expertise to master the tacit knowledge of the relevant domain, otherwise, other people will immediately identify them as non-experts. If those around them, on the other hand, do not have expertise in the relevant domains of practice, it is much easier for anyone to enact expertise and pass for an expert, even when they are not experts. This point is closely related to the debate on bogus experts carried out by Collins and Evans (2007, pp. 54-57). As they have pointed out, a bogus solo violinist who had to play, for example, a Paganini piece in an orchestra would be unmasked immediately by the other musicians. On the other hand, a beginner violinist who had learned to play one or two pieces could probably pass for an expert if he played for a community of indigenous people who were not familiar with this instrument[17].

---

[17] A short story from the Brazilian literature entitled "The man who knew Javanese" exemplifies the same point. This story was set in the early 20th century in Rio de Janeiro, where Castelo, an unemployed man, read in a newspaper a job advertisement for a Javanese teacher. He did not know a word of Javanese, but knowing that there would hardly be a Javanese speaker in Rio de Janeiro he decided to

One could object that there are situations, however, where individuals trying to pass for experts end up deceiving real experts. This is the case, for example, of bogus doctors. If there is no need to acquire any expertise in a domain of practices to enact expertise in a believable way, then a realist approach to expertise has no contribution to attributional theories. However, even in these cases a realist approach is useful. The case of bogus doctors illustrates this point (Collins and Pinch 2005, pp. 35-60). They are usually admitted as junior doctors who are still being trained and supervised by senior doctors. Hospital staff are used to having inexperienced novices who make several mistakes before mastering the skills necessary to do their jobs. For this reason, nurses and experienced doctors usually 'fix their mistakes' until they have enough practice to work more autonomously. In other words, the long years of university courses do not enable junior doctors to perform their tasks competently in a hospital. They have to be trained in these settings and acquire tacit knowledge in working there. For this reason, when bogus doctors begin working in hospitals, medical staff usually interpret their mistakes as a result of lack of experience. They then have plenty of time to be socialised into hospital practices. Some of them have long and successful careers and, when unmasked, other members of their teams get very surprised. Bogus doctors in general are only able to construct a successful career in medicine as long as they immerse themselves in the medical culture and acquire enough tacit knowledge to pass for fully-trained doctors.

---

apply for the job. He was then invited to go to Manuel's house, a rich man who wanted to learn Javanese. Beforehand Castelo went to the library and learned the basics of Java's history, the Javanese alphabet, three grammatical rules, and about twenty words of this language.

Manuel was a frail old man who had an old book written in Javanese, which was given to him by his grandfather, who, in turn, had it awarded to him in London by a wise man from Asia. Manuel's grandfather was advised by the wise man to have his grandson read the book, which would make his family keep its good fortune. Manuel, feeling that his last days on earth were approaching, decided to fulfil his mission and learn Javanese. He hired Castelo to teach him Javanese. During the first month of lessons Castelo tried to teach Manuel the Javanese alphabet. However, Manuel's progress was too slow. The old man then decided that if Castelo read the book and translated it for him this would be enough for him to fulfil his task. Castelo began then to pretend he was reading the book and to invent its content. He was so successful at doing this that eventually Manuels' son in law, who was an influential judge and knew many politicians, got him a job in the Ministry of Foreigners.

This story is interesting because Castelo only passed for a person who knew Javanese because there was no speaker of Javanese who could assess his knowledge of the language. If there had been a single Javanese speaker around him he would have immediately been spotted as a fraud. (This story can be found on http://www.releituras.com/limabarreto_javanes.asp. This link was last accessed on 09/10/2012).

In other cases bogus doctors are assigned to work as General Practitioners. General Practitioners, in the UK system, are the first contact for patients with the health care system. They run their own clinics and refer patients to specialists whenever they deem it necessary. In these cases, they usually work isolated from other doctors and, as long as their behaviour is not too unusual, they can easily deceive their patients, who do not have the expertise to assess whether they are competent doctors or not.

In sum, a realist theory of expertise may shed light on interesting issues related to the credibility of experts and enrich attributional approaches.

Chapter Summary

In this chapter I have presented the theoretical framework that underpins the whole argument set out in this work. I have presented the realist approach to expertise developed by Collins and Evans. According to them expertise is the tacit knowledge shared by members of a domain of practices. I presented the different types of tacit knowledge, i.e. relational, somatic, and collective. I pointed out that for sociology collective tacit knowledge is the heart of this concept as it consists of knowledge that is kept tacit due to the very nature of the social. I developed this idea by explaining the problem of rule-following, which is based on a sociological interpretation of the late philosophy of Wittgenstein. Finally, I examined some criticisms of Collins and Evans' theory of expertise. Some critics have argued that expertise must be investigated by using attributional theories. I responded to these critics by arguing that there is no incompatibility between realist and attributional theories of expertise. Rather, an attributional approach can benefit from insights from a realist approach.

Chapter 2 – Methods

This chapter describes the methods used in the present work. I have used a combination of qualitative semi-structured interviews with participatory observation to immerse myself in climate-change science as whole and in paleoceanography in particular. The rationale behind these choices was to acquire interactional expertise in paleoceanography, which underpinned my sociological analysis of mechanisms of communication between different expert communities (Collins 1984; Collins and Evans 2007, pp. 31-35; Collins 2009).

Hess (2001) reviewed the STS literature and argued that there have been two main generations of ethnographic research in this field. It is however more appropriate to talk about qualitative instead of ethnographic research as Hess includes the work of scholars who are not traditional ethnographers, such as Collins (e.g. 1985; 2009). Hess's division of STS into different generations is helpful to situate the methods deployed in this work. The first generation emerged with SSK (see chapter 1 for more on SSK) and concentrated on the problem of how particular scientific theories acquire credibility through a number of social processes (e.g. Bloor 1976; MacKenzie 1978; Latour and Woolgar 1979; Shapin 1979; Barnes 1982; Collins 1985). The second generation emerged around the 1990s and had more political and social engagement than the previous one, including, for example, feminist (Suchman 2007) and postcolonial studies (Anderson and Adams 2007). They sought not only to understand social processes taking place within science and technology but also to reform society so as to make it more inclusive or less unequal. Studies carried out within these traditions went much beyond the realm of science – i.e. the core-set – and also examined activists, social movements, and other social groups linked to science and technology.

Within Hess's classification of STS qualitative methods literature, the present work can be located in the first generation. Although I am interested and concerned about the prospects of rapid climate change, this work has emerged much more from an intellectual interest in how knowledge is produced and communicated in climate-change science than from the idea of producing knowledge that will necessarily have a direct practical application in environmental policy.

According to Hess (2001), within this first generation of ethnographers there have been different traditions approaching qualitative methods in different ways. Laboratory studies (Latour and Woolgar 1979; Knorr-Cetina 1981; Traweek 1988; Knorr-Cetina 1999; Doing 2008), for instance, have been one of the most influential approaches. These studies consisted of long periods of fieldwork usually in a single laboratory, where researchers sought to do something similar to what classical anthropologists (e.g Malinowski 1964[1922]) did when researching indigenous people in remote areas. They spent extended periods of time in laboratories and by observing scientists and 'science in the making' they sought to work out how scientific knowledge was produced and legitimised.

The approach to methods in this work is a different one. It is based on the idea of participant comprehension developed by Collins (1984) and deployed in a number of studies by STS scholars (e.g. Collins 1985; Pinch 1986; Collins 2004a; Stephens 2005; Ribeiro 2007a; Reyes-Galindo 2011). Although this approach to qualitative methods in STS has sometimes also been referred to as laboratory studies (e.g. Knorr-Cetina 1995; Doing 2008), there are important differences between them. Participant comprehension consists of acquiring competence in the domain under study. Instead of carrying out extended fieldwork in a single laboratory, the usual practice is to carry out interviews with researchers working in several different institutions as well as having other types of immersion in the domain by going to conferences and other scientific meetings. In certain cases there has been even engagement with the practices of the scientific community under study (e.g. Collins 1984; 1985).

Before describing in detail the activities that I have used to collect data, I will explain more thoroughly the notions of participant comprehension and introduce the idea of alternation better to explain the methods deployed in this work.

Participant Comprehension and Alternation

In the present work I chose a combination of methods informed by the idea of participant comprehension. The idea of participant comprehension was initially developed by Collins (1984) to explain the methods he deployed in his early work (e.g. Collins 1974, 1975, 1985) and then further elaborated under Collins and Evans's theory of expertise (Collins and Evans 2007, pp. 31-35; Collins 2009). In studies informed by participant comprehension researchers seek to interact as much as possible with the group that is being researched. The main goal is to develop native competence or, to use SEE's conceptual framework, to acquire expertise in the form of life of the research subjects:

> […] the investigator *him/herself* should come to be able to act in the same way as the native members 'as a matter of course' rather than remember or record the details of *their* interactions. […]. The stress is not on recording events (though this may be necessary for other aspects of the project in hand) but on internalising a way of life. Once this has been achieved, observation may as well be done on the investigator as other native members, for he/she should be like a native member. We might call this 'participant introspection'. In this method, then, the distinction between observer and observed is blurred (Collins 1984, p. 61).

Collins' research on parapsychology is an example of full immersion in a form of life (Collins 1984, pp. 61-64). He worked with scientists investigating paranormal phenomena and helped designing, carrying out, and analysing experiments. He even co-authored a paper on the results of parapsychology experiments (Pamplin and Collins 1975). This was an example of a sociologist of science becoming a contributory expert in the field under study. In other cases, contributory experts in fields of science and engineering have made a transition to STS and have used their contributory expertise to underpin their sociological investigations (Ribeiro 2007a; Reyes-Galindo 2011)[18]. Contributory expertise however is not a condition for carrying out research informed by the idea of participant comprehension. As Collins (2011) pointed out most of our knowledge comes from language, particularly in domains with high levels of specialisation. In these cases, most of what individual experts know is learned through language as their own practices are only a minimal part of all the practices that belong to their domain. For this reason, the difference between an individual who has

[18] There are other examples of STS researchers informed by other theoretical frameworks who undergone the same transition from natural sciences or engineering to STS (e.g. Park Doing 2004).

contributory expertise in a domain and an individual who has special interactional expertise is solely the engagement of the contributory expert with a very narrow set of practices. When it comes to the language of the field, there is nearly no difference between them. In this sense, having contributory expertise in a field or special interactional expertise does not make much difference for a STS researcher. Collins, for example, has been over the past 40 years conducting an intensive fieldwork among gravity waves physicists and does not claim to have become a contributory expert in this field (Collins 2009). He has acquired special interactional expertise in this domain, which enables him to carry out an in-depth analysis of this community. The present study is also an example of this. It is informed by the notion of participant comprehension. I sought to acquire a general understanding of the language of climate-change science as a whole and a deeper understanding of the language of paleoceanography, i.e. to become a special interactional expert in this field (I will return to the issue of how deep my immersion in these fields has been below).

Having immersion in the domain of practice under study, however, is not enough for a sociological study. If this were the case, every individual would be a sociologist of the collectivities he or she is immersed in. It is also necessary to be able to examine the experiences involved in the immersion process using a sociological framework. In this sense, it is necessary to alternate between different frames of meaning, i.e. to be able to 'see' the world through the eyes of a sociologist as well as through the eyes of members of the form of life that is being investigated (Collins 2004b). This process is called alternation (Berger 1963; Collins and Yearley 1992). In the present work I have immersed myself in the form of life of climate-change scientists, and particularly of paleoceanographers, to understand what it is to be part of their collectivity and to understand the worldview shared in their domain of practice. When I carried out my sociological analysis, however, I had to alternate back to a sociological framework and produce a sociologically relevant piece of research.

The idea of alternation is useful to distinguish participant comprehension from other methodological approaches in STS. A usual concern in social sciences research is that of going native, which would bias the researcher perception of the domain under study.

In this case, he or she would only reproduce the discourse of their informants instead of producing analytical accounts that question taken for granted social practices. Latour and Woolgar (1979, pp. 27-33), for example, who have produced one of the most well-known works within the laboratory studies tradition, argued that in STS it is important to keep the attitude of anthropological strangeness when doing fieldwork so that the analyst do not take for granted the research activities of the scientists under study. According to them, the high status of science in our society could potentially lead STS scholars to accept that scientific practices are the way they are because this is the most rational way of doing things. For this reason, too much immersion in the culture under study could be risky: "Outsiders largely unfamiliar with technical issues may severely jeopardise their observational acumen by initially submitting themselves to an uncritical adoption of the technical culture" (Latour and Woolgar 1979, pp. 30). From the point of view of participant comprehension, however, this risk does not exist as long as the researcher alternates between the frame of reference of the field under study and the STS framework underpinning his or her analysis. By doing so, it is possible to acquire an in-depth understanding of a domain of science and still carry out a sociological analysis of it.

Methods: An Overview

The idea of participant comprehension was central for my study in that I investigated how scientists communicate, collaborate, and interact among themselves to produce knowledge on climate change. In order to identify where there is shared understanding or communication issues between two communities, it was essential to understand the form of life of my subjects, their practices, and the meaning they attribute to these practices.

To acquire a general linguistic understanding of climate-change science and special interactional expertise in paleoceanography I have deployed a combination of qualitative methods. I have conducted qualitative semi-structured interviews, which were chosen because they are particularly useful for understanding how respondents frame and attribute meaning to phenomena, events, or behaviours (Mason 2002; Warren

2002; Forsey 2012, p. 365). They were an appropriate method to understand the frame of meaning that underpins the worldview of the particular forms of life under study, i.e. climate-change science as a whole and paleoceanography in particular. These interviews were a rich opportunity to immerse myself in the language of these domains.

I have also conducted participatory observation. I am deliberately not using the term participant observation here to distinguish the more traditional laboratory ethnography approach from participatory observation, which consists of observational research with a view to acquiring participant comprehension. Therefore, I did not carry out a traditional ethnographic work, i.e. I did not spend extended periods in a given locality taking careful notes on the events, behaviours, and phenomena taking place there (Delamont 2004, 2012). Rather, I have conducted a number of participatory observations at different sites, including laboratory visits, and attendance at two summer schools and scientific meetings. This is because I was more interested in immersing myself in climate-change science and in acquiring as much special interactional expertise as possible in paleoceanography than in understanding the production of knowledge at a particular site. To do so I did what scientists usually do when they are socialised in their own fields, i.e. attend conferences and scientific meetings, talk to senior researchers, visit laboratories, attend summer schools, and so on (Collins and Evans 2007; Collins 2011).

From a methodological point of view, it is important to emphasise that paleoceanography is a laboratory science and a field science, having also the usual conferences, research seminars, and so on, where scientists meet their peers. Traditional STS ethnography has been carried out in the laboratory (e.g. Latour and Woolgar 1979; Knorr-Cetina 1999; Doing 2004; Hong 2008) and in the field (e.g. Frodeman 1995, pp. 95-116; Goodwin 1995; Almklov and Hepsø 2011). In this work, as pointed out in the above paragraph, I conducted participatory observation in both settings.

Associated with these activities, I have also read textbooks, technical papers, and material available on the internet on climate-change science and on paleoceanography.

This included reading information on websites devoted to promote public understanding of science as well as watching videos available online[19].

Preliminary Stages: Collecting Data on Climate-Change Science

My fieldwork consisted of two stages: Firstly, I conducted a preliminary set of interviews with experts from several areas of knowledge which are part of climate-change science. The main goal was to have a basic linguistic socialisation in this field to 'get a sense' of what climate-change science is and of how scientists interact with each other in this field. When I set out to do these interviews it was not clear to me what my PhD project would be about. Similarly to what Collins (2009) reported about most of his fieldwork, I just 'went out there' looking for something interesting in climate-change science that I could research. I was not sure whether I would study something related to climate policy or whether I would keep my focus on social phenomena that take place within the scientific community; the latter have turned out to be the case. At that point I carried out semi-structured interviews and most of the questions revolved around my interviewees' research projects and their interactions and collaborations with other experts.

These interviews were carried out in 2010 with sixteen scientists. As these preliminary interviews intended only to provide me with a general idea of what climate-change science is I sought to interview scientists with different types of expertise and who were easily reachable. I initially conducted eleven interviews in the first semester of 2010 at my own institution, Cardiff University, where I interviewed scientists in all departments where there is research into any aspect of climate change. My respondents had expertise in the following areas: hydro-environmental engineering, paleoceanography, ecology, and architecture. I also interviewed in September, 2010, Sir John Houghton, a retired atmospheric physicist who is the former chairman of the IPCC working group I and who currently lives in Wales. The IPCC is the most influential institution in climate-change science (see chapter 3 for more on the IPCC). As at that point I was looking to

[19] The videos produced by the International Drilling Programme were particularly relevant for this work. See note 41 on page 111 for further information on this programme.

acquire a general understanding of what climate-change science is, I interviewed Sir John Houghton to collect data on the history and on the goals of the IPCC as well as on how this central institution in climate-change science works. I also carried out three interviews in Brazil in December, 2010, my home country, with scientists working in an international interdisciplinary research programme entitled 'The Large Scale Biosphere-Atmosphere Experiment in Amazon (LBA)[20]. These interviews were carried out by chance as I was then in Manaus, where this research programme is based, and I had a key contact in that city that facilitated my access to climate-change scientists. My interviewees had expertise in biomass, biogeochemistry, and atmospheric physics. The atmospheric physicist was also the manager of this research programme and provided me with a big-picture view of how such a multidisciplinary and interdisciplinary effort worked.

I conducted most of these preliminary interviews in the interviewee's offices. Sir John Houghton was interviewed in his house. These settings were appropriate as my respondents could focus on the interviews without many distractions. The only exception was an interview with a postdoctoral researcher in Brazil, which was carried out in a corridor of the research institute where he worked. This was not an ideal setting as there were people constantly passing by and greeting him. Yet I managed to ask him all the questions I intended to. These interviews were then transcribed. These data, however, are not directly used in the empirical chapters of this thesis (chapters 4 to 8), which deal mostly with paleoceanography. These data were mainly used as background information that helped me frame my research and set the data on paleoceanography into a wider context.

In addition to these interviews, in 2010 I attended a two-week long multidisciplinary summer school at Brown University, US, entitled 'Climate Change and its Impacts'[21]. This was an opportunity to have some initial linguistic immersion in several areas of climate-change science though this was a long way from full interactional expertise. I

[20] Further information on this research programme can be found on http://lba.cptec.inpe.br/lba/index.php?lg=eng.
[21] Further information can be found on their website: http://brown.edu/about/administration/international-affairs/biari/.

attended lectures on a range of different aspects of climate-change science, covering its four main research areas (causes and processes of climate change, impacts, adaptation, and mitigation), which were taught by renowned researchers from institutions across the world. During these two weeks I had several informal conversations with many of the forty participants in the summer school and could learn from them a little about their research areas. Most of the participants were researchers at doctoral level. They had a variety of scientific backgrounds, including climate modelling, biology, economics, city planning, anthropology, demography, engineering, meteorology, etc.

Collecting Data on Paleoceanography: Introductory Remarks

At the end of this period of preliminary fieldwork I began to narrow down my research interests so as to find a domain that could be feasibly researched within the time frame of a British PhD and decided to focus on paleoceanography. As pointed out in the introduction, I chose to focus my research on this field for several reasons, including the fact that it is part of the main area of research in climate-change science, i.e. the study of causes and processes of climate change; the close collaborative ties between paleoceanographers and climate modellers, which is a central research area in climate-change science; and the existence of a world-class paleoceanography group at Cardiff University, which made it a more viable choice for my study, given funding constraints for fieldwork, than fields of science that are not strongly represented in my institution. The interviews and the participatory observation were then carried out between the autumn of 2010 and the autumn of 2011. A number of activities were carried out including qualitative semi-structured interviews, laboratory visits, and attendance at scientific meetings and at a specialised summer school.

Interviewing Paleoceanographers: Recruitment and Sample

Rubin and Rubin (1995) pointed out that there are four key steps in recruiting interviewees: initially finding a knowledgeable informant, getting a range of views, testing emerging themes with new interviewees, and choosing interviewees to extend results. Rapley (2004, p. 17) argued that although these are valuable ideals, recruitment usually does not linearly follow these steps, taking place on an *ad hoc* and chance basis. This was the case in my research. I have never had an initial knowledgeable informant. Once I finished my preliminary interviews, I had already interviewed some paleoceanographers at Cardiff University, and as pointed out above, I selected paleoceanography as the focus of my research. At this point I was already getting a range of views on several relevant themes for my research, such as how knowledge is produced in paleoceanography, how labour is divided in this field, as well as I had already began to be socialised in this area, although this process was still at initial stages. What happened next was trying to get an even wider range of views on how paleoceanographic knowledge is produced by interviewing a larger number of paleoceanographers and scientists from adjacent specialties involved in paleoceanographic research, such as micropaleontologists, geochemists, paleo-modellers, and so on. By doing so, some initial hypothesis about the process of production of knowledge, social division of labour, and communication between experts in this field were tested, revised, and a better understanding was gradually achieved. To do so I carried out a new set of interviews at Cardiff University and at this time I interviewed all members of the paleoceanography group. These were found by searching the university's website. Subsequently, I asked members of this group if I had missed anyone. Three paleoclimatologists from Cardiff University that I had already interviewed in the preliminary stage of my research were re-interviewed at this point.

In order to have a wider and more diverse sample I used the technique of snowball sampling (Biernacki and Waldorf 1981; Atkinson and Flint 2001) to find new interviewees. Snowball sampling consists of asking interviewees to provide suggestions about other actors belonging to the same community who could be interviewed in the future. It is a particularly useful method of recruitment and sampling in research with hidden or hard-to-reach populations, such as the deprived, the socially stigmatised, and elites (Atkinson and Flint 2001). This was an appropriate methodological choice for my research as my interviewees are part of the scientific elite as they have privileged access to scientific knowledge in their field and they enjoy more prestige and power than the average citizen (Stephens 2007). Moreover, what universities have world-class research centres in paleoceanography is not ubiquitous knowledge; therefore I had to rely on the judgements made by the experts in this field. I asked my respondents at Cardiff

University where in the UK there are strong paleoceanography and paleo-modelling groups. I restricted my interviews to the UK due to funding constraints. I identified the following universities as appropriate sites for fieldwork: Cardiff University, Bristol University, the University of Southampton, the University of Cambridge, the University of Oxford, the University of Nottingham, the University of Leeds, the Open University, the University of Edinburgh, the Imperial College, and the University College London. Time and financial constraints, however, did not allow me to carry out interviews in all these British institutions. I excluded Scottish universities from my sample and concentrated only on England and Wales. I then carried out interviews at four universities that, according to the information gathered from my respondents, had some of the most renowned paleoceanography and paleo-modelling groups and at the same time were easily accessible: Bristol University, the University of Southampton, the University of Cambridge, and the University of Oxford.

Identifying experts in paleoceanography at these universities was not a difficult task. Some names were suggested by interviewees. I also searched the websites of these institutions for scientists working in paleoceanography. I then emailed them explaining about my research project and requesting an interview. Afterwards, I arranged interviews with all scientists who replied positively to my emails.

Kvale (1996, p. 102) suggested that the standard number of interviews in qualitative research is 15 ± 10. In my research I carried out many more interviews than this, which is justified by the need to have as much immersion as possible in the language of paleoceanography. In 2011, I carried out interviews with forty two paleoceanographers and scientists from related specialties which are strongly involved with paleoceanographic research, such as geochemistry, micropaleontology, and paleo-modelling. I did not follow any rule of thumb in determining this number such as interviewing a pre-determined minimum of people or interviewing until reaching a point where new interviews would not bring to light ‘fresh’ information. Rather, I tried to maximise the number of interviews considering the funding I had available. I did so because of the major goal of the interviews, i.e. having a linguistic socialisation in paleoceanography, so that the larger the number of interviews, the deeper my immersion in this domain would be. The final number of interviewees, therefore, reflects the number of scientists willing to talk to me at the universities where I carried out interviews.

I only interviewed full-blown experts, which means that I only interviewed scientists who were at postdoctoral level or above, excluding PhD and Masters students, who are not yet full members of the oral community in which interactional expertise is located. I also interviewed technicians and research associates who provide support to paleoceanographers. Most of them had already been granted their PhDs in paleoceanography or in related areas. The only exception was a laboratory manager who held a Masters degree. I interviewed technical and research staff in order to understand the technical dimensions of scientific research and how knowledge and skills were divided between scientists and people providing them with technical or research support.

I interviewed all members of the paleoceanography groups at Cardiff University who fall into the categories described in the last paragraph. At the other universities I interviewed at least half of the scientists involved with paleoceanographic research. Those who were not interviewed either did not reply to my emails or refused to be interviewed arguing that they were too busy or away.

Only one interview stood out as sharply different from the others. I went to the University of Southampton and interviewed a postdoctoral researcher in one of the University refectories. Once the interview finished some colleagues of the interviewee sat around us and started chatting. One of them was a senior researcher who had previously declined my requests for an interview. I introduced myself and he remembered the emails I had sent him. He then told me to start asking him questions. I did so and the interview unexpectedly happened there and then. One third of the interview, which lasted forty five minutes, was carried out in the presence of his postdoctoral assistant who had just been interviewed, and of a lecturer who also worked with him and who I had interviewed earlier on that day. At some point all of them commented on how they divided labour between them, the different skills they had, and how they could contribute to fieldtrips with those skills. Although this unexpected situation could have generated difficulties in that some people could have felt uncomfortable with being interviewed among their colleagues, this interviewee had a strong personality and did not appear to be embarrassed. This unusual situation actually ended up being beneficial as it provided me with the situation to explore the social division of labour within a particular research group.

The Interviews with Paleoceanographers

The interviews usually lasted between one or two hours. I developed a list of topics that I tried to cover in the interviews, but whenever an interviewee had particularly useful information on a particular topic I would dedicate a larger amount of time to it than to other topics (see appendix A for examples of topic lists). As is usual in qualitative research the topic lists changed over time and different topic lists were used depending on the actors being interviewed (Rapley 2004; Forsey 2012). Before the interviews I read the university profile of my interviewees to have a general sense of what their research was about and whether there was a specific topic that would be particularly interesting to explore.

The interviews usually began with me asking about the interviewee's research interests, which were useful to break the ice. The answer to this question usually led to a number of other questions related to the research carried out by my respondents. These questions were useful for my socialisation in this field. I usually also explored some topics that helped me operationalise the contributory expertise of my interviewees: their particular specialisation within paleoceanography; the social division of labour in their research group, in their laboratories, and in their collaborative networks; the different stages of research in paleoceanography and how involved in each of them they were; the research techniques used by them to generate, interpret and/or model data; the types of judgements they had to make during research; the conferences they usually attended; the journals they kept up with and those they published in; and how they attempted to transmit their expertise to their students. The following topics were used to explore what fields my respondents had interactional expertise in: which groups of scientists were particularly easy or difficult to communicate with; how much my interviewees were well informed about paleoceanographic research techniques they did not use; and, again, the conferences they usually attended and the journals they kept up with.

Most interviews were carried out either in the interviewee's office, in their laboratories, or in meeting rooms where we would not be disturbed. In six cases, however, I had to carry out interviews in other settings, particularly when I interviewed postdoctoral researchers or research staff who shared office space. Five interviews were carried out in cafes and one was carried out in a park near the building where my interviewee worked. In these cases I attempted to conduct the interviews in quiet areas to minimise noise and distracting factors as well as to keep the conversation private.

As other researchers have reported when interviewing experts, most interviewees adopted a pedagogical tone when speaking to me (Stephens 2005). At the beginning this was useful as I knew very little about their field of investigation. As I acquired higher levels of interactional expertise, I had to develop strategies to suggest that I already understood the basics of paleoceanography so that I did not waste interview time with explanations of basic information. I would usually make an intervention either mentioning that I had already interviewed someone with a similar expertise or ask a question involving some paleoceanographic technical language showing that I already had a basic understanding of this field.

I have also emailed some interviewees and asked them to clarify points they had made during the interviews that I found obscure and that would be useful to understand better. Information provided in these emails are also used in this thesis.

Interviews: Transcription and Analysis

All interviews at this second stage of my research were recorded on electronic devices and transcribed. The transcribing process was very useful in terms of my linguistic socialisation in paleoceanography in that I heard again the information provided by my interviewees.

There are debates in the methods literature on what epistemological status should be attributed to transcriptions. A constructivist view on transcriptions have emerged over the past decades and questioned the idea that transcriptions are direct representations of the encounter between researchers and interviewees (e.g. Denzin 1995). This has led to experimental ways of transcribing whose extremes are mixtures between qualitative research and arts, such as poetic transcriptions (e.g. Glesne 1997). Indeed transcribing is not a fully straightforward process as it involves a number of judgements that to a certain extent construct what readers will have access to, i.e. whether to represent in the transcription the whole range of sounds, silences, pauses, and overlaps between interviewer and interviewee; where to begin and to end the extracts, and so on (Hammersley 2010). However, as Hammersley (2010) pointed out, transcriptions are not only constructions. They are also based on some stable records that, despite being amenable to multiple types of transcription, are not made up. There is a sharp difference, for instance, between a researcher making decisions on how to transcribe an interview and a novelist creating stories. In this sense, rather than engaging with postmodern forms of transcribing that take the idea that transcriptions are constructions to its extremes, I deployed a more traditional approach to transcribing. I basically sought to transcribe the words uttered during the interviews using standard orthography. Exceptions to this were long pauses which were represented with ellipsis to indicate that the interviewee had to ponder before responding; laughter, to indicate that what the interviewee had just said was a joke; and I also wrote notes indicating interruptions to the interviews, including telephone calls to the interviewees or colleagues and students coming to their offices to ask them questions.

The interviews were then coded using NVivo. Most transcriptions generated word documents with more than 5000 words and in the case of long interviews they reached up to 19000 words. The codes were used to categorise parts of the interviews under labels that would later on help me retrieve information on specific themes and patterns, i.e. I deployed a code and retrieve procedure (Coffey and Atkinson 1996). This strategy was particularly useful because of the large amount of interview data. These codes also established links between the ‘raw data’ and concepts that would inform the interpretation of the data (Coffey and Atkinson 1996). Strauss (1987) pointed out that codes can be sociologically constructed or in vivo codes. The latter are based on the language of the social group under study, whereas the former are based on the sociological literature. I used a mixture of both. The most important sociologically constructed codes were: collaboration, social division of labour, types of specialisation, interaction between empirically-oriented scientists and modellers, and stages of research in paleoceanography. In vivo codes were used to categorise information on specific techniques deployed by my interviewees and on phenomena researched by them, such as anoxic events, carbon compensation depth, carbon isotopes, oxygen isotopes, etc. None of the codes were generated before this stage of the research. I developed them and sometimes re-labelled them throughout the coding process. The whole process was iterative in the sense that I generated codes when reading the transcripts and once new information was read I would either generate new codes, apply codes that I had already developed, or change the codes so that they would be more appropriate for the goals of my research. The codes reflected my interest in understanding the production of knowledge and communication between expert communities in paleoceanography and how I could link these topics to the expertise framework developed by Collins and Evans.

After coding the interviews I began to analyse the coded data. However, it was not at this stage that the data analysis began. My research was informed by the idea that data analysis and data collection should not be separated stages (Delamont 2012). The interviews were transcribed throughout the fieldwork and not only after I finished collecting all the data. This procedure allowed me, throughout the data collection, to identify topics that looked particular interesting and to collect further information on them. By doing so, interviews that had already been transcribed informed the following interviews. Furthermore, during the data collection I presented preliminary results at research seminars at Cardiff University, for research groups based at other universities, and at conferences. In all these presentations I benefited from invaluable feedback from the audience which helped me developing the next steps of my data collection.

With regards to how I have interpreted the coded data, I did not use a traditional inductive ethnographic approach, i.e. find patterns in the data, such as regularities as well as the lack of regularities and then make generalisations (Delamont 1992). Although this is a legitimate method for traditional ethnographic research, if I had used it to interpret interview data it could have led me to epistemological problems. Gilbert and Mulkay (1984) pointed out there are issues related to seeking regularities in interviewee's accounts of phenomena and events relevant to the research. They argue that in interview data there is a multiplicity of different accounts that the social science analysis transforms into a single story by finding regularities and taking them at face value. By doing so, radically diverging accounts provided by different interviewees and even diverging accounts provided by the same interviewee are ignored. According to them, this procedure misrepresents the interviewees' voices and ignores the diversity of accounts of the social world.

The present work is not affected by this criticism because I did not base my analysis on patterns found in the coded data. Rather, I sought to internalise the form of life of paleoceanography and based my analysis of the interview data in the interactional expertise that I acquired in this field. The interview data was used to illustrate points that I learned through my immersion in this field and to convey the feel of relevant situation or phenomena (Collins 1984, p. 64). Therefore, instead of basing my analysis on interviewees' accounts, I based it in my socialisation in this field and used these accounts to exemplify points that are particularly relevant for this thesis.

The Interviewing Process in Retrospect

Overall the interviewing process was successful and I experienced few problems. I went through a fascinating socialisation process. It was noticeable how much more I got to understand of my interviewee's domain language and about their form of life towards the end of my fieldwork if compared to the beginning. Examples of this were the interviews carried out at late stages of my fieldwork, which discussed much deeper scientific issues than the initial ones. The interactional expertise acquired during the research also helped me transcribe the interviews in that I could recognise and accurately transcribe technical concepts that at the beginning of my fieldwork I could not understand. As I will argue below, I did not become a full-blown interactional expert in paleoceanography as my immersion in this field was too short to achieve this. The level of interactional expertise that I acquire was however sufficient for the task of conducting a sociological analysis of area of research.

I experienced some issues related to the policy relevance of climate-change science and the controversies over this topic. Scientific knowledge from this field has been regularly used by politicians, activists, and the media either to argue that global warming is happening and is a serious issue or to deny its existence. This has put a great deal of pressure on climate-change scientists and made some of them wary of possible uses of their work. The glaciergate scandal is an example of this[22]. Although paleoceanography has not been as much as the centre of the global warming debate as other fields of science that are regarded as more relevant for policy-making, a few issues emerged in my study related to getting access to interviewees. Even though most of the paleoceanographers that I contacted were happy to help me with my research, some of them were wary of being interviewed because of the pressures and political issues related to climate-change science. A researcher from the University of Cambridge, for instance, responded my email saying that before deciding to accept to be interviewed she had checked with a senior paleoceanographer from Cardiff University the authenticity of my research. This was motivated by an email attack that her department had received a few weeks before my request in which, according to her, pressure groups

[22] See Grundmann (2013) for a brief description of this scandal and Pearce (2010, pp. 193-209) for a journalistic account of it.

requested data looking to use it in a negative way. Fortunately, I had already interviewed this senior researcher and he was aware of the authenticity of my research project. It cannot be determined how many potential interviewees declined my interview requests because they were suspicious of the legitimacy of my research project, but this might have generated some refusals.

Laboratory Visits: Physical Contiguity with Paleoceanographic Practices

Besides the interviews, I have also carried out participatory observations through some laboratory visits, attendance at scientific meetings and at a summer school in paleoclimatology. I would hesitate to call this ethnography though. In contrast with traditional ethnographic approaches (Delamont 2004; 2012) I did not spent an extended period of time in the field doing participant observation and taking detailed notes of what was going on. Rather, these activities were part of the process of immersing myself into paleoceanography to acquire a participant comprehension of this field.

The laboratory visits were carried out to have physical contiguity with paleoceanographic practices. Physical contiguity is a type of immersion in forms of life that consists of interactions with experts in proximity to the practices of the domain, but without any ‘hands-on’ experience (Ribeiro forthcoming-a). It is a more efficient way of acquiring interactional expertise in a domain than by only talking to experts. It provides individuals with an enhanced linguistic socialisation as it creates opportunities for individuals to ask experts questions that perhaps would not arise were they not close to the site where practices are carried out (Ribeiro forthcoming-a).

I visited paleoceanography laboratories at Cardiff University and at the University of Cambridge. I carried out four visits at Cardiff University between the spring of 2011 and the spring of 2012, having been to all the paleoceanography laboratories of this University. This includes the laboratories where sedimentary cores are processed and where geochemical analysis is carried out (see chapter 4 for further information on paleoceanographic research). At the University of Cambridge I went to their mass spectometry laboratories where a researcher explained to me some details of their geochemical analysis.

Unlike more traditional laboratory studies (e.g. Latour and Woolgar 1979; Knorr-Cetina 1999), in which STS scholars spent extended periods in the laboratory observing the daily routines of scientists, these visits were short, lasting around half an hour each of them. I did not stay in the laboratories watching the scientists working for long hours. They showed me the different parts of the laboratories and they explained to me what they were doing or would ordinarily do there at each particular stage of research.

In these opportunities, I benefited from physical contiguity with experts. I could ask the people who were showing me the laboratory, who included technicians, research staff, and faculty members, about their production of data and understand better how they worked and divided tasks. This provided me with a better linguistic understanding of research in paleoceanography, although not the ability to carry out research in this area.

In addition to these laboratory visits, I had physical contiguity with paleoceanographic research in other occasions. During several interviews experts showed me samples they were working on, fossils they were looking at under the microscope, and graphs they were plotting and interpreting. These occasions prompted further questions about these practices. They provided me with an opportunity for better understanding these activities and for acquiring a wider vocabulary in the paleoceanography language.

During the occasions when I had physical contiguity with paleoceanographic practices I waited until I left the laboratory or the office of my informants to write fieldnotes in my notebook. It was more valuable to focus my attention on what my informants were telling me than to take notes as my main goal was the acquisition of interactional expertise in the language of paleoceanography. Listening was particularly valuable at these occasions. As Forsey (2010) pointed out, although there seems to be a hierarchy in the qualitative methods literature that places sight as the most important sense at the field, all senses are equally important to researchers. Listening is particularly important when it comes to grasping the meaning attributed to actions, events, and phenomena by particular social groups, which was the case in my research.

The fieldnotes were written by using a salience hierarchy strategy (Wolfinger 2002). This means that rather than seeking to write comprehensive notes about ‘everything’ that went on during these laboratory visits, I wrote about events that stood out, such as new information about how to produce paleoceanographic knowledge, for example, information about instruments that I had not heard about before or information about the strengths or weaknesses of that particular laboratory when compared to laboratories based at other universities.

Participatory Observation: Scientific Meetings and a Summer School

In addition to interviews and laboratory visits I had further immersion in paleoceanography by attending scientific meetings and a summer school, which provided me with opportunities for an intense linguistic socialisation. In 2011 I attended three research seminars at Cardiff University and a conference at the Royal Society, in London, whose topic were paleoceanography. In addition, in 2011 I attended a summer school in paleoclimatology, in Urbino, Italy, which lasted three weeks. I was lectured on the whole range of paleoceanographic topics, including the history of the Earth, different paleoceanographic techniques, paleo-modelling, etc. A conference was also held in the second week of the Summer School, at which some of the lecturers presented their most recent research. In Urbino I also spent a great deal of time with the other participants in the summer school and became a friend of some of them. Most of them were PhD students in paleoceanography. This allowed me to ask further questions about this research area in an informal manner.

The benefits of attending these meetings involved deepening my understanding of the vocabulary used by palaoceanographers, which resulted in my being able to follow a conversation between experts and have at least a ‘big-picture’ understanding of what they were talking about. I also learned 'who is who' and 'what is what' in this field. Some researchers' work, some data sets, and some phenomena, were repeatedly mentioned during the lectures in Urbino, during the conferences and seminars I have attended, and by students in informal conversations. I therefore acquired a sense of what the most relevant topics and the most prominent researchers in this field are; this is an essential part of the interactional expertise that glues a field together.

.

During the time I spent in Urbino, I and the other participants went on a fieldtrip to the Apennines, where we visited some rock sections that contain information about the climate from millions of years ago. During the trip we were lectured about these rocks by experts who explained what information was contained in them. Afterwards, the participants were divided into groups of around six people and each group went to different rock sections to make basic field logging, measurements, and classification of the rocks. I went with one of the groups to a rock section made up of sediments from over 90 million years ago and took part in the whole process.

The day after the fieldtrip we were lectured on how to interpret the measurements we had made. Subsequently each group had to interpret the data collected and present it to the lecturers and to the other participants. I and my group spent half the morning and half the afternoon trying to make sense of the data we had collected and preparing a PowerPoint presentation with our results.

My input in this process was little as I had no previous training in collecting and interpreting paleoceanographic data. On the fieldtrip I did not get involved until I had a clear sense of what we had to do. After observing the other members of the group for some minutes I contributed with measuring and logging the rock section. During the interpretation of the data, however, I spent most of the time trying to make sense of what the group was doing. I did not have any significant input in this activity. However, this was enough to give me a general sense of what was going on. Later on, when I returned to Cardiff, I attempted to go through the same steps that the group did when we interpreted the data we collected in the field. I managed to do it after a few hours. I would not describe this experience as having provided me with contributory expertise in paleoceanography, not even in some very narrow paleoceanographic research practices. A single field experience and a single data interpretation however do not cover the whole range of experiences that a paleoceanographer goes through in research. I would be able to repeat exactly what we did then, but this experience did not enable me to make any informed judgement on how to log and measure rock sections and subsequently interpret the data collected in any situation slightly different from that one. This fieldwork situation provided me with a good opportunity to acquire a better linguistic understanding of what paleoceanographers do when they go to the field and what they do with the data afterwards.

During the Urbino Summer School I kept a diary where every day, after the lectures or field activities, I wrote fieldnotes. Similarly to the notes written after the laboratory visits, I used a salience hierarchy strategy to select what I would write about. The diary was continually typed into the same word file.

Once I finished my fieldwork, the fieldnotes were considerably shorter than my interview material, therefore there was no need to code them to make them more easily retrievable. At the time of the writing of particular drafts of this work, whenever I wanted to refer to a particular fieldwork situation I returned to the fieldnotes to be able to provide a more accurate description of relevant events and phenomena that I observed.

Participatory Observation in Retrospect

The observational research strongly contributed to the main goal of my fieldwork, i.e. the acquisition of interactional expertise in paleoceanography. The laboratory visits helped me better understand the production of data, whereas the conferences and the summer schools provided me with a general understanding of ‘who is who’ and ‘what is what’ in this field.

Undoubtedly, the most important of these activities was the attendance at the Urbino Summer School. This course raised considerably my interactional expertise in paleoceanography as I was lectured for three weeks on the most relevant topics of this field by some of its most renowned experts. Afterwards my capacity to transcribe interviews and deal with paleoceanographic technical language was significantly improved. I could also understand better examples provided by my interviewees during the interviews. I would not claim however to have become a full-blown special interactional expert in paleoceanography. A single year of immersion in this research area is not enough for this. I acquired a very good understanding of certain topics whereas a much shallower understanding of others. At conferences and lectures I would oscillate between understanding nearly everything that was being presented and understanding very little, depending on the topic and on the level of technical language used by the presenters. For instance, I would usually not be able to follow lectures that involved the representation of geochemical phenomena by using complicated chemical equations.

I acquired enough interactional expertise to carry out the sociological analysis presented in the present work. If I had had more time and funding to have an even deeper immersion in paleoceanography, I would have reached higher levels of interactional expertise. This would have made my work reach deeper layers of the language of paleoceanography and would have enabled me to bring to light more detailed analysis of the production of knowledge and communication in this field. This however remains as further research to be done in the future.

Research Ethics

Ryen (2004) has pointed out that there is no internationally agreed code of ethics for qualitative research, but there are some issues that are frequently brought up when research ethics is debated: codes and consent, confidentiality, and trust. I collected my data looking to meet these standards although acknowledging that they cannot always be met because of particular contexts in which qualitative research is carried out (Ryen 2004; Warren 2002). With regards to codes and consent, which consists of obtaining informed consent from the research subjects, I informed my interviewees about the goals of my research on the email requesting an interview. During the interviews I requested permission for recording and for using the recorded information in scientific publications. Furthermore, when attending scientific meetings to do participatory observation I always identified myself as a sociologist of science and was open about my goals there.

Regarding confidentiality, it was agreed with my interviewees that all quotations would be unattributed. As Collins has pointed out in his own code of practices[23], sometimes members of a domain can identify the author of particular quotations because of their content or style. To minimise this problem, I have sometimes used more than one pseudonym for the same scientist in cases when they are quoted in different parts of the thesis and one quote might lead other scientists to immediately identify them. An example of this is chapter five, where I quote scientists describing their specialisation within paleoceanography.

Trust in the context of qualitative research means establishing a good rapport with the community under study so that situations that might prevent future researchers to have access to the community are not created. I have experienced no issues related to lack of trust, such as requests from any participant to have the information provided by them withdrawn from this research. Arguably the ethical procedures listed above are one of the reasons for this.

Ryen (2004) pointed out that trust also relates to how data are presented in papers of research reports. In this regard I was very careful not to misrepresent what my interviewees told me. This does not mean that they will always agree with my sociological analysis, but that every effort was made for the data to be accurately transcribed - whenever difficulties emerged in transcribing I requested help from native

[23] http://www.cardiff.ac.uk/socsi/contactsandpeople/harrycollins/code-of-practise.html.

speakers - and presented within its original context. Whenever I was not sure about what an interviewee meant in a particular quotation I have email him/her and asked for clarification.

Furthermore, the present work does not deal with emotionally loaded topics or with any socially or politically sensitive issue so that there were no major ethical concerns related to its conduction. Although the reality of global warming is disputed by some groups of scientists, my research is not directly related to controversial aspects of climate-change science and of climate policy.

Chapter Summary

In this chapter I have described the methods deployed in my fieldwork. My data collection was divided into two stages. Firstly, I sought to acquire a general understanding of what climate-change science is by doing preliminary interviews with experts from a range of scientific fields and by going to a summer school that covered a wide number of aspects of climate-change science. Afterwards, having noticed that climate-change science was too big to be examined as a whole I decided to focus on a single field of investigation: paleoceanography. My immersion in paleoceanography consisted of a number of activities, including interviews, laboratory visits, participation in scientific meetings, and the attendance of a summer school. By having this immersion I was deeply exposed to the language of paleoceanography and to a more limited extent to the practices of paleoceanography.

At the end of my fieldwork I did not consider myself a full-blown special interactional expert in paleoceanography. This would take some more years to be accomplished. However, I acquired enough interactional expertise to analyse this field sociologically and understand how knowledge in this area of science is produced and communicated. Deeper immersion would certainly provide further information that could deepen my sociological understanding of the production of knowledge in paleoceanography. However, this remains as further research to be carried out in the future.

Chapter 3 – Climate-Change Science, the Fractal Model, and Homogenisation Mechanisms

This chapter is a literature review of STS studies on climate-change science and it has two main objectives. Firstly, I work out how climate-change science can be described by using the fractal model. To do so, I will subdivide climate-change science into subareas. I will also locate paleoceanography within the fractal model. This is an essential step for the present work as communication between paleoceanographers and other climate-change scientists will be examined in the following chapters in the light of the fractal model. This description is partly based on the first stage of my fieldwork, in which I sought to acquire a general sense of what climate-change science is and partly on the structure of the IPCC reports. As I argue below, the IPCC to a large extent frames research into climate change. Each of its reports explicitly points out key uncertainties in climate-change research and areas that need further research. These become priority and several groups of scientists tend to focus their research on them. The IPCC also implicitly defines what is more relevant in climate-change research by focusing on certain areas of science, certain time periods, and certain types of scientific approach. Even though the IPCC does not represent the full diversity of climate-change science, it can be used as an entry point to the social organisation of this field.

Secondly, I examine two mechanisms of homogenisation of climate-change science: translation (Callon 1986; Latour 1987) and standardisation (Latour 1987; Jasanoff and Wynne 1998; Lampland and Star 2009; Edwards 2010). I examine these mechanisms of homogenisation and identify their analytical strengths and weaknesses. I also argue that although they are relevant for understanding how knowledge is produced and communicated in climate-change science, they have to be associated with bridge-building mechanisms as climate-change science has not become a homogeneous transdisciplinary field of research.

These mechanisms are examined at a very high fractal level, the entire climate-change science (see figure 7). Halfway through this chapter, once a more detailed description of climate-change science has been made, I will enrich figure 7 with subareas of this field of investigation.

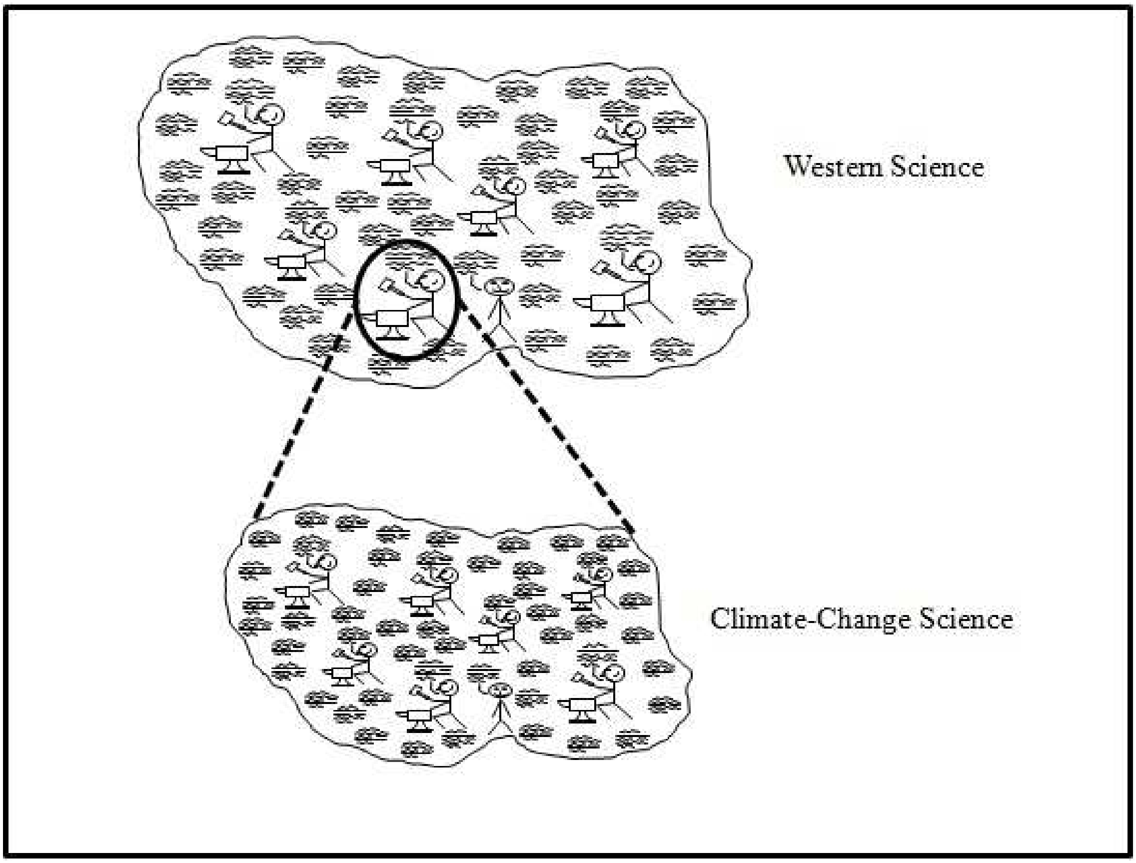

Figure 7: Western science and climate-change science.

Before I move to the first section of this chapter an important point has to be made. A large number of papers have been published on the co-production – or the mutual construction - of climate-change science and climate policy (e.g. Shackley and Wynne 1995a, 1996; Jasanoff and Wynne 1998; Shackley et al. 1998; van der Sluijs et al. 1998; Shackley et al. 1999; Demeritt 2001; Moore 2011). It has been widely documented that policy-makers' expectations regarding the types of information scientists should provide them with and scientists' tacit assumptions about what policy-makers expect from them have framed climate policy as well as research into climate change. Although I acknowledge that interactions between scientists and policy-makers are an important research topic, I will not examine them here in detail as they lie beyond the scope of the present work. I therefore only use this literature as long as it provides me with clues about how scientists interact among themselves.

This chapter begins with an introduction to the IPCC.

The IPCC

Public and governmental concern about global warming emerged in the late 1980s and spread across the globe (Ungar 1992; Weart 2003). As a response to these concerns in 1988 the United Nations Environmental Programme (UNEP) and the World Meteorological Organisation (WMO) founded the IPCC. The IPCC aims to provide policy-makers with assessments of the current state of the climate system, predictions about future global warming, predictions of the expected impacts of warmer temperatures, and strategies to respond effectively to the threat of major climate change. In its most recent report the IPCC describes its role as follows:

> The IPCC does not conduct new research. Instead, its mandate is to make policy-relevant – as opposed to policy-prescriptive – assessments of the existing worldwide literature on the scientific, technical and socio-economic aspects of climate change. Its earlier assessment reports helped to inspire governments to adopt and implement the United Nations Framework Convention on Climate Change and the Kyoto Protocol. The current report will also be highly relevant as Governments consider their options for moving forward together to address the challenge of climate change (IPCC 2007a, p. v).

Approximately every six years the IPCC releases a major synthesis report reviewing the scientific literature on climate change. The IPCC is currently divided into three working groups, which are in charge of assessing the literature on different topics[24]:

[24] The IPCC working groups have had this format since the preparation of the 2001 report. The 1990 and 1995 reports had slightly different subdivisions. In both reports working group I focused on the basic mechanisms of climate change. In the 1990 report working group II was entitled Impacts assessment of climate change and working group III was entitled The IPCC response strategies. In the 1995 report, working group II was called Impacts, adaptations and mitigation of climate change: scientific-technical analyses and working group III was called Economic and social dimensions of climate change.

- Working group I: The physical science basis
- Working group II: Impacts, adaptation, and vulnerability
- Working group III: Mitigation of climate change

The first group looks at the mechanisms of climate change, how climate has changed in the past, what the current state of the climate system is, and how it will change in the near and mid-term future. Working group II examines the impacts of climate change, the populations that are more vulnerable to these impacts, and strategies to adapt to a world changed by hotter temperatures. Working group III examines how to mitigate climate change or, in other words, how to prevent temperatures from growing dramatically in the future.

The IPCC reports are written by a large number of authors, who work on a voluntary basis[25]. In general each chapter has between one and ten lead authors. The lead authors are formally appointed by national governments, but there is a core group of scientists and policy-makers who greatly influence these decisions (Shackley 1997, p. 77). According to the IPCC, each chapter should include lead authors representing a diversity of perspectives, expertises, geographical areas, and gender. The lead authors may invite other experts as contributing authors to help them write the report. The idea is that all the key experts in a given field of science should participate in the drafting of the reports. The chapters are written after a number of informal meetings which are coordinated by the lead authors. Once a first draft is ready, it is sent for review. Experts not included in preparing the drafts and governments review them, and a final version of the report is agreed in a session of the working group[26]. A summary for policy-makers is also prepared and published alongside the technical reports. Those are prepared by experts, but have to be approved, line by line, in sessions including lead authors and government representatives.

---

[25] Further details about the IPCC process can be found on http://www.ipcc.ch/pdf/ipcc-principles/ipcc-principles-appendix-a-final.pdf.

[26] An interesting case study on how IPCC scientists come to conclusions on their reports' content can be found on the paper by O'Reilly et al. (2012) on how the consensus on sea level rise due to the disintegration of the West Antarctic Ice Sheet that prevailed in the third IPCC report collapsed on the writing of the fourth report.

The IPCC's Influence

To a large extent, the IPCC has built up a consensus among governments and policy-makers around climate change and its risks (O'Riordan and Jäger 1996; Weart 2003; Miller 2004). Since its first report, which was released in 1990, the IPCC has stated with increasing levels of confidence that the average global temperature has been growing since the Industrial Revolution and that at least part of this warming is due to human activities. This has given momentum to policy-making, with most governments signing the Kyoto Protocol in 1997, a major agreement seeking to reduce carbon dioxide emissions (Weart 2003; Miller 2004). Its main goal was that developed nations should reduce by 2012 their CO2 emissions to levels below those of 1990[27].

Because climate-change science became strongly intertwined with policy-making (Shackley and Wynne 1995a, 1996; Jasanoff and Wynne 1998; Shackley et al. 1998; van der Sluijs et al. 1998; Shackley et al. 1999; Demeritt 2001; Moore 2011), the IPCC, which plays a role mediating between science and policy-making (van der Sluijs et al. 1998, p. 293), has been also framing scientific research on climate change at least when it comes to research funding[28]. This has been reported in the STS literature. Shackley et al. (1999, pp. 431-436), for instance, in a study of climate modelling (see pages 87-90 for a description of climate modelling) provided evidence of the IPCC framing the research of certain groups of modellers. The authors pointed out that there were two ideal-types of climate modellers (1999, pp. 431-432). Firstly, those they called 'purists'. These modellers were focused on developing and improving state-of-the-art models. They were not very interested in policy-making applications of climate models as they

[27] Although most countries signed this agreement, the US, the greatest emitter of carbon dioxide per capita in the world, failed to ratify it, at least partly due to the influence of climate sceptics in their policymaking processes (McCright and Dunlap 2000, 2003; Jacques et al. 2008).

[28] This is not to say that there are no criticisms to the IPCC. It was criticised for several reasons including: the tacit hierarchy of sciences that underpins its reports gives much less importance to the social sciences than to natural sciences (Demeritt 2001; Yearley 2009; Hulme and Mahony 2010); the privilege that economics has had over other social sciences within the IPCC framework (Shackley 1997; Yearley 2009; Hulme and Mahony 2010); most IPCC authors and reviewers come from developed countries, which has led to criticisms related to the underrepresentation of third world perspectives in its reports (Demeritt 2001; Hulme and Mahony 2010); after the glaciergate scandal (see Grundmann 2013 for a brief description of this scandal and Pearce 2010, pp. 193-209 for a journalistic account of it) the IPCC has been criticised for not being rigorous enough so as to prevent errors to be included in its reports. For this reason a number of reforms were proposed by members of the scientific community (Hulme et al. 2010); finally, the IPCC has been accused by the so-called sceptics of being an ideological institution that has an agenda focused on controlling carbon dioxide emissions (e.g. Idso and Singer 2009, p. iv). However, none of these criticisms have had a large impact and significantly reduced its credibility and influence.

believed it was premature to make policy based on models that needed to be more robust. They also applied their models to study a range of other phenomena. The 'pragmatists', on the other hand, had research goals closely linked to those of policy-makers. These modellers were particularly interested in researching past and future climate change with a view to assessing the extent of anthropogenic climate change that could be useful for policy-making. According to Shackley et al. (1999, p. 435) their link to policy-making was mediated by scientific institutions and funding agencies, particularly by the IPCC, which defined the priorities for future research.

More evidence of the IPCC's influence in climate-change science can be found in Sundberg's (2007) research into meteorologists in Sweden. She pointed out that meteorologists were aware of what the IPCC regarded as the main gaps and uncertainties in climate-change science, particularly those related to climate models (e.g. the effects of aerosols and of clouds on the climate). These scientists argued that they felt it was necessary to connect their research interests to the IPCC priorities to obtain funding for research. As a result, in a number of grant applications they argued that their research would produce data that would help address the uncertainties and gaps in climate modelling. Even though Sundberg (2007) pointed out that in several cases the data produced by these experimentalists were not useful for climate modellers because they were not collected in the appropriate way, this study still shows the influence of the IPCC on funding bodies and, as a result, in directing scientific research towards certain topics.

In addition to research in STS, there is also evidence in the scientific literature of the influence exerted by the IPCC in framing research on climate change. One example of this can be found in the introduction to an issue on paleoclimate and the IPCC published in the *Journal of Quaternary Science* where the editors pointed out that the IPCC had been setting the scientific agenda and influencing what paleoclimatologists were researching (Caseldine et al. 2010, pp. 3-4). The influence was felt through funding agencies, which, across the globe, had been emphasising the need for paleoclimate research to be linked to the issue of climate change, particularly to the priorities set by the IPCC:

> The NSF [the US National Science Foundation] solicitation, for instance, quite closely follows AR4 [the fourth IPCC assessment report], with an emphasis on reducing uncertainties, and the development of paleo datasets as analogous to observational data for model validation, although at times it is not that clear what is being requested (Casedine et al. 2010, p. 3).

Climate-Change Science and the Fractal Model

Having described the IPCC and its influence in climate-change science I will now use the fractal model to represent this field of investigation. To do so, I will break it down into subareas. Climate-change science can be subdivided in several different ways. I will divide it into subareas which are similar to the IPCC working groups. As pointed out above, the IPCC to a large extent frames climate-change science by defining what the priorities for research are. It is also the most influential scientific body that reviews and summarises the literature on climate change. For this reason, it is reasonable to expect that its internal structure is similar to the structure of climate-change science. Furthermore, the preliminary stage of my fieldwork, in which I acquired a general understanding of what climate-change science is, confirmed that this really is the case. There is also evidence in the literature that the structures of the IPCC and of climate-change science are similar. Although this point has not been explicitly made in the literature, there are studies that describe the different subareas of climate-change science and the flow of information between them in a way that closely resembles how the IPCC reports are structured (Shackley and Wynne 1995b; Shackley et al. 1998).

As mentioned above, the IPCC working groups are divided in the following way: Working group I: The physical science basis; working group II: Impacts, adaptation, and vulnerability; and working group III: Mitigation of climate change. The names of the groups and their divisions however are not fully accurate sociological descriptions of the subdivisions of climate-change science. Firstly, the name physical science basis does not do justice to all the different fields of science that make up basic climate science. The IPCC was founded mainly by atmospheric scientists and its focus, especially in its first reports, was primarily on the physics of the atmosphere[29]. Since then, however, it has grown and in subsequent reports has increasingly included researchers from other fields. Furthermore, although the theory of anthropogenic global warming is based on atmospheric physics, without understanding the whole climate system and the wide range of responses and feedbacks taking place in all its subsystems it is not possible to make sense of climate change. For this reason, it is more accurate to talk about the study of causes and process of climate change to define this field of investigation. It is also important to separate the study of the impacts of climate change from research into adaptation to these impacts. Although several scientists involved with the study of impacts also work on adaptation, not everyone working on one of these works on the other. Climate-change science can therefore be divided into four big research areas: causes and processes of climate change; impacts of climate change; adaptation to climate change; and mitigation of climate change.

In figure 8 I use the fractal model to portray climate-change science, its four big research areas, and their subdivisions. This picture will inform the remainder of the present work when I discuss the issue of communication between expert communities. I will now describe each of these research areas before moving to the analysis of mechanisms of homogenisation of forms of life.

Causes and Processes of Climate Change

This area of research comprises a wide range of fields of science that seek to understand the causes and process that drive climate change. As we have seen above, the climate system is composed of five subsystems: the atmosphere, the oceans, the land surfaces, the biosphere, and the cryosphere. Scientists with a range of different backgrounds seek to understand the climatic processes taking place in these subsystems and their interactions in a range of different time scales. The study of the causes and processes of climate change therefore includes scientists with expertise in fields such as meteorology, atmospheric physics, climatology, oceanography, biology, geology,

[29] This information was provided by Sir John Houghton, the former chairman of the IPCC's working group I in an interview carried out in 27/09/2010.

hydrology, computer modelling, paleoclimatology, etc., and several interdisciplinary combinations of these disciplines.

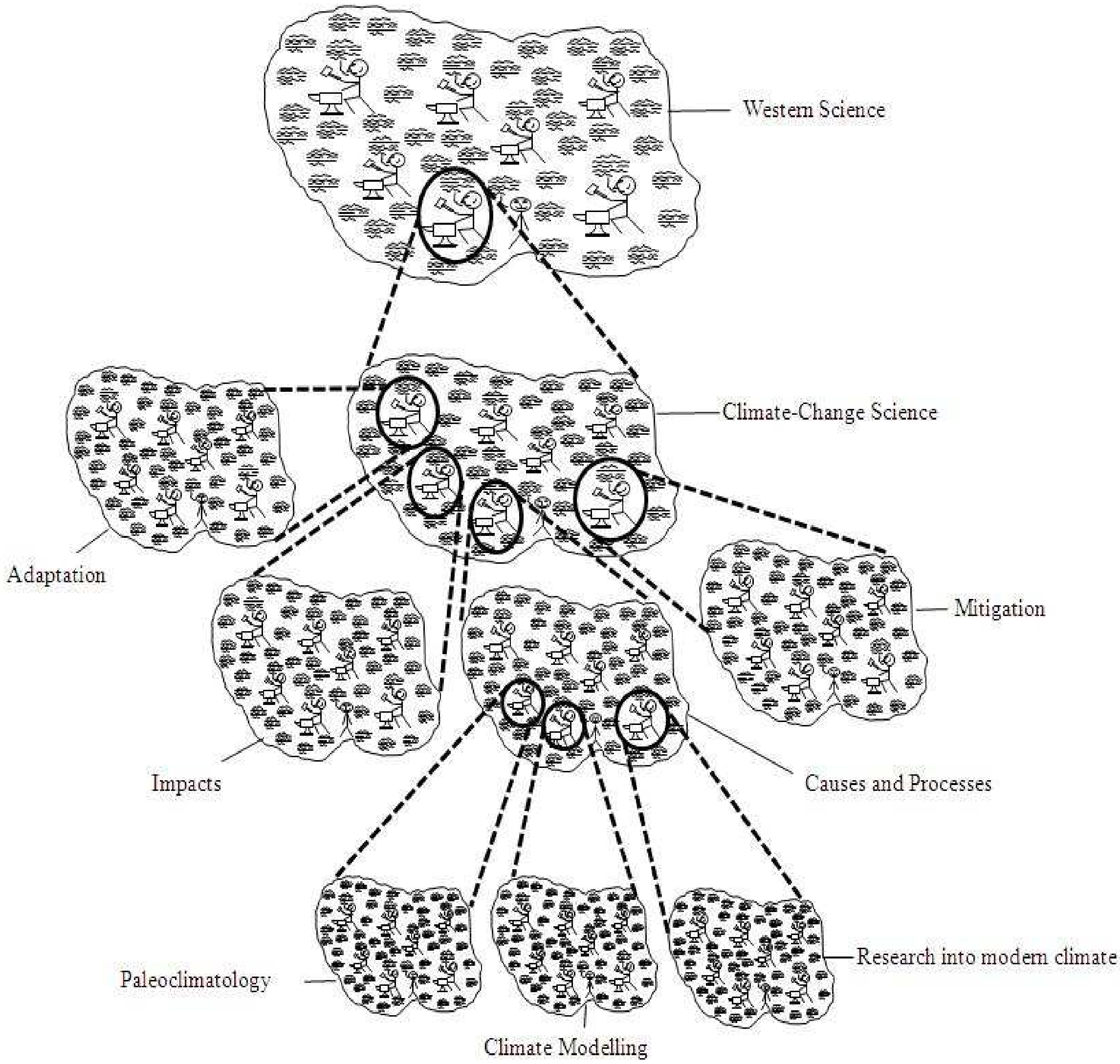

Figure 8: Climate-change science and its subareas of research.

The study of causes and processes of climate change can be subdivided into three main fields: paleoclimatology; research into modern climate; and climate modelling[30]. Each of them has different focuses, expertises, and languages.

[30] Edwards (2001, 2010) pointed out that the boundaries between modelling and data production are 'blurred'. Large-scale climate models, on the one hand, include basic equations describing the physics of the atmosphere as well as 'semi-empirical' parameters. Data, on the other hand, frequently has to be modelled before becoming useful for understanding global phenomena. For example, sometimes the

Paleoclimatology consists of the study of climate before the 19th century, when the first consistent direct measurement of climatic phenomena began to be taken. (A more detailed description of this area of research will be provided in the following chapters.) Paleoclimatologists seek to reconstruct a number of climatic variables, including temperature, the concentration of carbon dioxide in the atmosphere, ocean currents, ocean acidity, orbital forcing, biogeochemical cycles (such as the carbon cycle), the hydrological cycles, and so forth. To do so, they use a number of indirect means called proxies, such as data extracted from ice cores, tree rings, marine sediments, lake sediments, speleothems, corals, outcrops, pollen, etc. To extract climatic data from these archives they deploy a range of techniques, including geochemical measurements, assemblage counts of fossils, and so on.

Research into modern climate consists of empirical scientific endeavours aiming to record present and near past climatic data from all the subsystems that make up the climate system and to identify major modern climatic phenomena and processes. It includes all efforts to deal with data collected since the mid-19th century, when consistent direct measures began to be taken. Some of the specialties involved with this type of research are atmospheric physics, oceanography, biology, glaciology, and various combinations between those disciplines. Data are collected across the globe on all kinds of environments using a wide range of instruments and methods, including satellites, weather balloons, buoys, rockets, measurement towers, among many others.

The last sub-area is *climate modelling.* Whereas the two subareas described above are composed of scientists with an inclination to do empirical research, climate modellers' work is more distant from the empirical world. They model past, present and future climate change to understand better climatic processes and to predict future climate change.

---

coverage of empirical measurements is not as wide as required by models so that interpolation models are deployed to 'fill in the gaps' in data sets. Another example is satellite data, which is collected worldwide, but has to go through statistical modelling so as to separate authentic signals from noise. From a sociological point of view, however, the modelling community is still distinct from the other two subareas in that people carrying out modelling have a different expertise from those collecting and compiling data.

Climate modelling is done on computers. Models have different levels of complexity, which are usually called by modellers hierarchy or spectrum of models (e.g. Shackley and Wynne 1995b; Shackley et al. 1998, pp. 163-165; Edwards 2001, p. 37): they range from very simple zero-dimensional models to extremely complex general circulation models (GCMs). One example of simple models are zero-dimensional energy balance models, which treat the Earth as a point mass and are based on the principle that all the energy that enters into the system has to leave it eventually (Edwards 2001, p. 37). They usually have a fairly simple setup and simulate the interactions between a small number of variables: "Using measured values for such factors as solar radiation and concentrations of the atmosphere's constituent gases, they compute (for example) a single global average temperature […]" (Edwards 2001, p. 37). Energy balance models can be more complex and be either one- or two-dimensional, involving, as a result, more equations in their setup.

Although there are other types of climate models, GCMs, as I will point out below, are widely regarded as the most important tools to bring together climatic data and to simulate the whole climate system. They have been widely described in the STS literature (e.g. Shackley et al. 1998; Edwards 2001; Lahsen 2005; Sundberg 2006). The most modern GCMs model the Earth system in powerful computers. They divide it into three-dimensional grids in which physical interactions among its various components are simulated mathematically[31]. Physical interactions in the atmosphere, for example, are represented as follows:

> Equations of state compute the effect of various forces (radiation, convective heating, and so on) on the air masses and moisture (clouds and water vapour) within each grid box. Equations of motion compute the direction and speed of the air's movement into the surrounding grid boxes (Edwards 2001, p. 37).

[31] GCMs resolution has evolved over time. The sides of the grids of those whose output were used by the IPCC in the 1990's report, for example, were large as 500 kilometres. In the 2007's report the grids' sides had been reduced to approximately 110 kilometres (IPCC 2007, p. 113).

The atmosphere and oceans usually are fully simulated in different models and then coupled (Lahsen 2005, p. 903). The other subsystems (vegetation, the cryosphere, and land surface processes), in contrast, are not fully represented and more or less simplified versions of them are included in these models (Shackley et al. 1998; Edwards 2001; Lahsen 2005)[32]. Not all components of the climate system can be represented by model equations. Some phenomena take place in scales smaller than the grids. These processes are parameterised, which means that their physics is not fully represented (Edwards 2001, pp. 56-57; Lahsen 2005, p. 900; Sundberg 2007, p. 477). Examples of phenomena that are parameterised are clouds and aerosols. Edwards (2001, p. 56) described the parameterisation of cloud formation as follows:

> For example, rather than represent cloud formation in terms of convection columns, cloud condensation nuclei, and other direct causes, a GCM typically calculates the amount of cloud cover within a grid box as some function of temperature and humidity. This approach embodies what is known as the *closure assumption*. This is the postulate that small-scale processes can ultimately be represented accurately *in terms of the large-scale variables available to the models*.

Modellers generate parameterisations by reviewing the meteorological literature and observational data to work out how small-scale processes are linked to large-scale variables (Edwards 2001, p. 57). Parameters generated in this way are called physically based parameters. However, modellers frequently do not find these relations between phenomena in different scales. They then develop *ad hoc* schemes to represent these parameters: "For example, one method of cloud parameterisation represents all the cumulus clouds in a given region as a single 'bulk' cloud" (Edwards 2001, p. 57). There is another type of parameterisation that consists of observed patterns whose physics is not understood, but which have been described mathematically (Edwards 2001, p. 57).

---

[32] Although I am not going into details on GCMs and their users, this is not a totally homogeneous community. Shackley (2001), for instance, pointed out that there are different 'epistemic lifestyles' among general circulation modelers. Some groups (who he calls purists) are more focused on model development whereas others (who he calls pragmatists) are more interested in the application of climate models to address specific research questions. There are also differences between groups that prioritize thermodynamics and others whose models concentrate on dynamics. Similarly, Lahsen (2005) pointed out that there is a social division of labor within the GCM community. In the 90s, when she carried out her study, there were usually three groups involved with the development of a GCM: one would develop the atmosphere model; a second one would develop a simulation of the oceans; and, finally, a third one would couple the two systems.

GCMs are used for a number of different purposes. One of the most popular under the IPCC umbrella is estimating the so-called climate sensitivity, i.e. how the climate system will react to a doubling in the concentration of carbon dioxide in the atmosphere (van der Sluijs et al. 1998, p. 291-293). They are also used, for example, to generate climate scenarios for the future under a number of different assumptions and to carry out ‘experiments’ to assess the influence of different forcings in the climate system (IPCC, 2007a).

Impacts, Adaptation, and Mitigation of Climate Change

Global warming is expected to bring about impacts on the environment and on human societies. Scientists research a wide range of impacts including, for example, the rise of sea levels, extreme weather (e.g. storms, hurricanes, heat waves, droughts, etc.), the melting of glaciers and ice sheets, changes in vegetation cover, the spread of tropical diseases, increases in poverty, water shortages, loss of biodiversity, and so on. A variety of experts is involved with this area of research, such as impact modellers, agronomists, biologists, medical scientists, social scientists, economists, and so on.

The study of impacts is closely related to research into adaptation techniques and adaptation strategies to the impacts of climate change. During the first stage of my fieldwork, in which I interviewed scientists from several areas of climate-change science, I met some scientists involved with both fields of research. Adaptation studies are also very diverse due to the large number of impacts that global warming is expected to bring about. For example, it includes the expertise of engineers for the planning and construction of coastal protection, such as levees and floodgates, to prevent sea level rise inundating inhabited areas. It also includes agronomists seeking to develop irrigation and crop diversification techniques to deal with extreme weather. Social scientists have also been involved with this area of research. Demographers, for instance, look at migration as a mean of adaptation. In addition there is a wide range of interdisciplinary studies, combining social scientists and natural scientists, that seek to develop various techniques of adaptation, such as diversification of livelihood that could help vulnerable populations adapt to economical impacts of a changing environment. Finally, economic modelling is used to estimate the costs and benefits of different adaptation techniques.

The final area of research that makes up climate-change science is mitigation of climate change. There are several proposed ways of doing this. Some of them (IPCC 2007b) are: the deployment of low-carbon energy (e.g. wind or nuclear power, biofuels, etc); improvements in energy efficiency (e.g. fuel efficient vehicles, improved processes in the industry, etc); reductions in deforestation rates and reforestation; sustainable building (e.g. improved insulation and lighting); improvements in agricultural techniques (e.g. recovery of impoverished soils, management of livestock and manure to reduce methane emissions, etc); waste management; geoengineering (e.g. oceans fertilisation, carbon sequestration, etc); and changes in lifestyles. In addition to the development of specific techniques of mitigation, an important component of this area of research is cost-benefit modelling studies attempting to simulate how much each of these techniques would cost to implement on a large scale and to what extent they would effectively mitigate climate change.

Homogenisation of Climate-Change Science

Having described the main areas of research in climate-change science and represented it by using the fractal model, I will now examine the mechanisms of homogenisation and their effects on climate-change science. There are two main mechanisms of homogenisation: translation (Callon 1986; Latour 1987) and standardisation (Latour 1987; Jasanoff and Wynne 1998; Lampland and Star 2009; Edwards 2010).

Homogenisation Mechanisms: Translation

The concept of translation (Callon 1986; Latour 1987) was developed within actor-network theory (ANT). In the context of the problem of communication between different expert communities it is a useful notion for understanding how groups of scientists with heterogeneous interests begin to converge on their goals. In other words, it is related to interest heterogeneity, which might prevent experts communities from communicating and collaborating due to diverging goals (e.g. Sundberg 2006, 2007).

According to actor network theory (Callon 1986; Law 1986; Latour 1987, 1991, 2005), scientists are part of sociotechnical networks composed of their peers as well as of a range of other actants involved in the production and stabilisation of scientific facts, such as technicians, funding agencies, policy-makers, etc. Non-humans are also part of these networks, including other living things and objects. This is a central point in ANT, the idea of hyper-symmetry, i.e. that an equal ontological status should be attributed to humans and non-humans. Within this framework agency is attributed to all things including animals, objects, and phenomena.

According to ANT proponents, scientists, when developing theories, instruments, etc., seek to build up as many alliances as possible, or in other words, to mobilise actants in support of their research programmes. By doing so, they acquire a more dominant position in the developing network. A crucial part of this process is what ANT calls translation, which consists of a group of scientists (or any other group of actants) making the interests of other members of the network converge with theirs. A successful process of translation may result in a scientist or a group of scientists making their work, techniques, instruments, methods, etc., a *obligatory passage point* as other members of the network come to believe that to achieve their own goals they need to use these techniques, instruments, methods, etc.

In climate-change science the most important process of translation was carried out by climate modellers. Climate models, particularly GCMs, have acquired a central role in climate-change science (Jasanoff and Wynne 1998; Shackley et al. 1998; Demeritt 2001; Edwards 2010). They are the main tool used to bring together data collected across the several subsystems and regions that compose the climate system:

> For climate change research, common approaches are developed across disciplinary boundaries, taking as the object of study the atmosphere, troposphere, stratosphere, cryosphere (ice), biosphere, geosphere, lithosphere, and hydrosphere. Other approaches focus on various marine and terrestrial ecosystems and land use. Data from these systems are assembled into a variety of computer simulation models, with general circulation models (GCMs) at the top of the hierarchy of complexity (Jasanoff and Wynne 1998, p. 49).

GCMs do not only assemble data. They also produce global simulations which are used by other scientists, particularly those studying the impacts of global warming (Shackley and Wynne 1995b; Shackley et al. 1998). This puts them in a central position in climate-change science, in that they bring empirical data together and mediate the flow of information from observational scientists to impacts experts.

Although it has become commonsensical in climate-change science that no experiments can be made on planetary level and consequently computer models are essential for studying climate change, a STS researcher has to ask how GCMs became so central in this field for two reasons. Firstly, STS research has shown that nature itself cannot impose particular research approaches on researchers (Yearley 1990). There are always a range of approaches with which phenomena can be scientifically investigated. Furthermore, all research methods and techniques have strengths and weaknesses. If compared, for example, to other climate models, GCMs have advantages and disadvantages:

> It is simply our objective to point out that they [GCMs] score well with respect to certain criteria, but not to others. For example, if the goal is defined as providing long-term climate predictions which are based on current scientific perception of the physical mechanisms involved, then GCMs are certainly a strong candidate. But while GCMs are, therefore, capable of exploring some key features of climate change, including variable time horizons and, potentially, regional scales and variability, they are much less suitable for integrating with other physical and socioeconomic models, or for performing uncertainty analysis and associated stochastic prediction. Moreover, they are resource intensive and rather intractable. There is a widespread experience with them although, as we have discussed, confirmation of their reliability, especially for the purpose of making projections, remains a difficult issue. Finally, they are not very accessible or transparent and feedback from other scientific communities as to their validity is not readily achieved (Shackley et al. 1998, p. 183).

The centrality of GCMs in climate-change science was explained by some STS scholars by arguing that they have become obligatory points of passage (Edwards 2001;

Sundberg 2007). This means that climate modellers made them become indispensable within the climate-change network. Through a process of translation they have made the interests of other groups of scientists converge with theirs. Other climate-change scientists and other actors, such as policy-makers and funding agencies, began to believe that climate models are the most important tools to investigate climate change. As a consequence, their work became inevitably linked to modelling. Shackley et al. (1998), although not using the concept of obligatory point of passage, provided the best description available in the STS literature of GCMs mobilising several different actors involved with climate-change science:

> Another way of putting this is that GCMs (*contra* other models or methods) come to act as a sort of *common currency* between groups of scientists and policy-makers – each considers they have something to gain in intellectual, scientific, funding and social terms – from being involved in their development and use – and that this commonality serves as a way of linking-up such groups into loose coalitions (Shackley et al. 1998, p. 186).

According to Shackley et al. (1998, p. 186), the most important sets of relations between General Circulation Modellers (GCMers) and other actors in the climate-change network are: between GCMers and policy-makers; between GCMers and the climate impacts community; and between GCMers and other domains of science that are also committed to improving climate simulations. Let us examine how Shackley et al. described each of these set of relations separately to understand them better.

According to Shackley et al. (1998), GCMers are linked to scientists from other areas of the studies of causes and processes of climate change because they provide them with opportunities of collaboration related to modelling development and validation, which depend on very detailed data from several fields of science[33]. As result, GCMers tended to have much more extended networks than other climate modellers:

---

[33] Oreskes et al. (1994, p. 642) pointed out that from a philosophical point of view validation does not mean that a model can reliably represent natural phenomena: “Model results may or may not be valid, depending on the quality and quantity of the input parameters and the accuracy of the auxiliary hypothesis”. Agreement between a model and a data set therefore does not mean that the model reliably represents the reality, but that there is consistence between a particular model run and a given data set.

> A modeller using a simple model who wants to include the carbon fertilisation feedback or the effect of aerosols in the model, for example, does not require an extended collaboration with biologists or atmospheric chemists. What is needed are a few 'best estimates' from the world's experts on these subjects, probably accessible from the published literature.
>
> In GCM 'extension work', by contrast, ecologists and hydrologists are needed to model, for example, the intricacies of the movement of water from the soil through vegetation to the boundary layer, to extend micro-level models of catchments so they can be applied at the resolution of GCM grid-squares; and so on through a myriad of other possible examples (Shackley et al. 1998, p. 188).

The other group of scientists who are part of the network of GCMers use the outputs of these models, such impact modellers and economists studying the costs of climate change (Shackley et al. 1998, pp. 190-191). One example of this are crop modellers: "the 'climate impacts community' explores the effects of climate change on agriculture by deriving data from individual grid-points of GCMs and feeding it into crop productivity models" (Shackley et al. 1998, p. 190).

Finally, GCMs have great prestige among policy-makers, which is reflected in them receiving generous funding from funding bodies. Shackley et al. (1998, p. 192) provided a number of reasons for this. Firstly, GCMs are data providers for impact and economic studies whose research efforts are strongly linked to climate policy. As a result, GCMs are of great importance for any knowledge-based policy-making effort. Secondly, policy-makers tend to prefer GCMs over simpler models based on the assumption that their scientific credibility would help build consensus in policy-making:

> The argument that scientific credibility (and preferably certainty) is necessary for the accomplishment of political consensus appears to hold powerful sway in many political and policy cultures and, if GCMs are held out as the most reliable and robust of climate models, it follows that they will enjoy an elevated status in policy circles (Shackley et al. 1998, p. 192).

In addition, among climate models, only GCMs carry the promise of being able to provide future simulations with detailed regional data, which is central for policy-makers to develop local climate policies. For this reason, GCMs appear to be more suitable for policy purposes. Finally, Shackley et al. also speculated that the complexity of GCMs might have contributed to their status among policy-maker because it would protect them against criticism:

> A further possible and implicit advantage for policy makers of using GCMs may be their sheer complexity and impenetrability. As such, they cannot easily be challenged by critics, whether environmentalists or industrialists, most of whom cannot possibly comprehend the complexities of such a large computer model, let alone articulate its deficiencies.

Summing up, GCMs become obligatory points of passage in the climate-change network because GCM modellers mobilised several different groups of actors and made their interests converge with the development of GCM modelling. Empirically-oriented scientists adopted the role of providing GCMs with empirical parameters; the impacts community consumed GCM output; and policy-makers based their political negotiations in GCM data, or data from researchers working on impacts, adaptation, and mitigation of climate change, which depended on GCM output. Scientific and policy-making efforts related to climate change to a large extent revolved around these models.

This process of translation is useful to understand how interest heterogeneity can be dealt with within a field like climate-change science and how a potentially infinite myriad of interactions between different groups of scientist are channelled into a social pattern[34]. It homogenises the field and create patterns of collaboration and of information flow. However, translation is really helpful only if we abandon the hyper-symmetry principle proposed by the ANT. This principle prevents us from examining the different nature of the links established between GCMs and other social groups. The connections of GCMers with ‘data suppliers’, ‘GCMs’ output consumers’, and policy-makers are all equated. As Collins (2012) has pointed out, if from an ontological viewpoint things are treated as equivalent to human beings, there is no sense in talking about concepts such as expertise, trust, tacit knowledge, domain’s language, etc. There

[34] There is a caveat to this idea that needs to be stressed. Firstly, GCMs being obligatory points of passage does not mean that necessarily all scientific communities always work towards satisfying the interests of the GCM community. There are exceptions to this. Sundberg (2006; 2007), for example, provided examples of field-experimentalist meteorologists based in Stockholm pursuing their own interests rather than collecting data that could become climate models input. These meteorologists, however, acknowledged that climate models were obligatory passage points in climate-change science. For this reason, in their applications for funding they usually argued that their data would be useful for developing better model parameterisation. Their data collection, however, usually was not carried out in a way that would produce data suitable to be fed into GCMs. Sundberg (2006) argued that in this case parameterisatons played the role of boundary objects as they kept meteorologists, climate modellers, and research councils in the same network without exerting coercion on any of them. Translation, therefore, did not work and empirically oriented scientists did not become enrolled in the modellers’ network. This however does not invalidate the notion of translation as Callon (1986) argued that it might be successful or not.

can be only alliances between different actants. In this context, sociological analysis cannot go to deeper levels and shed light on issues such as how heterogeneous groups of scientists develop mechanisms to facilitate communication between them. Only homogenisation processes can be examined as the whole issue of heterogeneity of social groups, in any sense other than interest heterogeneity, does not make sense from the ANT viewpoint.

In order to understand how different experts communicate and collaborate to produce knowledge on climate change it is essential to identify what type of interaction takes place between the different groups of actors. In this regard, a number of questions emerge. For example, Shackley et al. (1998) pointed out that GCMers need to collaborate with hydrologists to develop their models. They indicate that this is a collaborative work in which both groups of scientists actively interact. This leaves several questions to be addressed, such as: how do these scientists bridge the gaps between their expertises? How intense are these collaborations? If we compare these links with those between GCMers and impact modellers there are also some differences. Impact modellers need GCMs output, however, Shackley et al. (1998) suggested that impact modellers do not have the expertise to assess different GCM runs so as to choose which is more suitable as a data source for their models. They therefore have to trust the judgements of GCMers on what simulations are more appropriate to serve as input to their models. This is a different type of link if compared to that between GCMers and hydrologists. Whereas in the latter case it is a two-way collaboration in which both groups of scientists are interested in interacting, in the former case only impact modellers seek to interact with GCMers. Finally, when it comes to the connections between policy-makers and GCMers, this is a completely different type of link if compared to the others described above in that it goes beyond those directly related to knowledge production.

This is not to say that agency is strictly a human characteristic. If agency is defined in a certain way non-humans can also be regarded as agents. Pickering (1995, p. 6), for instance, argued that extreme weather phenomena, such as storms, floods, and droughts, had agency, in the sense that they engage with our bodies and with our minds. They can bring about severe impacts on the environment and on human societies. One could also argue that carbon dioxide has agency, which fits well within the theory of anthropogenic climate change. Yet, as Pickering (1995) has also pointed out, there are differences between human agency and material agency. There is intentionality in human agency, whereas material agency lacks it. Scientists' behaviour is future-oriented in the sense that they establish clear goals and objectives for their actions. The same quality cannot be attributed to carbon dioxide or to scientists' machines, such as mass spectrometers or computer models. Furthermore, non-humans cannot acquire and transmit collective tacit knowledge, which means that, although they might be part of humans' social world and have an influence in people's social lives, i.e. a pet can make people happy, sad, angry, etc., as much as money can bring about several different emotions in people, they cannot engage in social interactions that depend on following social rules (Collins 2010, pp. 124-125). It is not the objective of the present work to examine in detail the interactions between humans and non-humans, therefore objects are not protagonists in my narrative. Yet, researching their interactions with humans is still a legitimate field of investigation within STS as long as the differences in terms of intentionality between them are acknowledged.

In this sense, the idea of translation should not be altogether abandoned. It can be very useful as long as we abandon the hyper-symmetry principle and distinguish the different types of interactions that take place between actors. In fact, Shackley et al.'s (1998) examination of the dominant position of GCMs in climate-change science is interesting precisely because they do not follow ANT all the way. Although Shackley et al. do not go into the details of the interactions between the different communities of climate-change scientists - this was not the main goal of their work - they acknowledge that there are different types of interactions between different groups of social actors. They pointed out, for example, that the interactions between impact modellers and GCMers, for example, were based on trust because these groups had different types of expertise. They could only make this point because they did not follow the hyper-symmetry principle.

In sum, the notion of translation is useful to understand how heterogeneous communities establish links between themselves through the convergence of goals. Translation homogenises interests and underpins collaborative efforts. This notion, however, is a sociologically interestingly only if the hyper-symmetry principle is abandoned and it is acknowledged that human agency is different from material agency.

Homogenisation Mechanisms: Standardisation

> To standardise an action, process, or thing means, at some level, to screen out unlimited diversity. At times, it may mean to screen out even limited diversity (Star and Lampland 2009, p. 8).

The second mechanism of homogenisation of different forms of life to be discussed here is standardisation. Although standardisation has also brought up ethical issues related to whether standardising excludes or make certain groups invisible (e.g. Star and Lampland 2009), I will not examine these issues here. Rather, I will stick to the focus of this work and concentrate on the extent to which standardisation facilitates communication between heterogeneous expert groups. In science and technology standardisation refers to processes of production of knowledge and technology that are standardised so that data, research techniques, methods, etc., can be transferred across national borders, research projects, and scientific domains. Standardisation is particularly helpful to reduce instrument heterogeneity, although it also has implications for other types of heterogeneity as it changes research practices and research cultures. Standardised data collection methods and techniques associated with data processing may reduce the incompatibility between data sets, which Edwards (2010) calls data friction.

In climate-change science, standardisation is an essential mechanism for generating data sets that climate modellers can use. It is essential for modellers that data are produced in the ‘right’ shape and that they can travel across national borders and disciplines until they reach modelling research centres. Latour has theorised this point in an influential way which is useful to understand the relevance of standardisation mechanisms.

According to Latour (1987), the existence of obligatory points of passage is not a sufficient condition for actants to keep control of a network. It is also necessary to ensure that things (people, events, phenomena, etc) travel through the network and reach its centres in the appropriate shape. In other words, they have to be transformed into what Latour calls immutable and combinable mobiles. This means that they have to be made mobile, so that they can be transported; stable, so that they are not distorted when being transported; and combinable, so that they can be combined with other immutable mobiles (Latour 1987, p. 223). In other words, they have to be standardised. Again, this idea is very useful to understand climate-change science as long as it is disconnected from the hyper-symmetry principle, which, as I have argued above prevents sociologists from examining different mechanisms of building bridges between different social groups.

In the case of climate modelling, heterogeneous data collected from all parts of the world on a range of climatic phenomena have to be transformed into figures, tables, graphs – inscriptions in the Latourian vocabulary (Latour and Woolgar 1979) –, which can travel and reach climate models in a suitable shape for becoming model input or to be used for model validation. This is a challenging task. Data are collected from sources as heterogeneous as surface stations, weather balloons, ships, satellites, rockets, paleoclimate proxies, and so on (Jasanoff and Wynne 1998, pp. 34-35; Edwards 2001, pp. 60-61). These data sets are assembled by using different instruments, techniques, and skill levels, which results in data with different levels of precision and accuracy; they have different spatial resolution and cover different time intervals; and data are collected in different regions and cannot always be extrapolated to other localities (Jasanoff and Wynne 1998, pp. 34-35; Edwards 2001, pp. 60-61). Jasanoff and Wynne (1998, pp. 34-35) provided some examples of the heterogeneity of climatic data sets:

> The sources of data used in measuring the Earths' past are wide-ranging: standard temperature records taken by governments agencies; centuries-old descriptions, ships' logs, paintings of Alpine glaciers and outdoor scenes (used as observations, although often not intended as such); historical records of climate change found by drilling into ice cores, and reading the record of radiocarbon locked up in tree rings (Broecker 1992); temperature measurements around cities that must now be adjusted to compensate for the slight upward skew around such heat islands. Patently social productions, such as parish records from previous centuries, may have to be related to observations of nature, such as the chemical analysis of fossilized pollen. Many

> problems of incompatibility, unevenness, and lack of standardization are associated with such aggregation of records. Means of interlinkage, of common identity, between such diverse entities are often attempted – sometimes successfully.

Edwards (2001, p. 60) provided further examples:

> Most thermometers are located on land and clustered in urban regions, where 'heat island' effects raise local temperatures above the regional average. Meteorological records at sea tend to be drawn from shipping lanes, ignoring the globe's less travelled areas. For the last several decades, records from the atmosphere above the surface have been drawn from increasingly extensive commercial aircraft, radiosonde (weather balloon), and rawinsonde (radar-tracked radiosonde) networks, but these too are concentrated in particular areas. Coverage in the tropics and in the Southern Hemisphere is particularly poor.

These heterogeneous data sets have therefore to be standardised and the data processed so that they can become models input. Edwards (2010) described two major processes related to the production of global data sets: *making global data*, which consists of internationally coordinated efforts to record global weather and climatic data; and *making data global*, which consists of efforts of adjusting, interpolating, and extrapolating data from heterogeneous data sets to produce global records that can be fed into climate models. The former consists of attempts to standardise data collection so as to reduce the diversity of methods and techniques: "Standards act as lubricants. They reduce friction by reducing variation, and hence complexity, in sociotechnical processes, and they 'black-box' decisions that would otherwise have to be made over and over again" (Edwards 2010, p. 251). The latter consists of attempts to process heterogeneous data sets. These processes also aim to reduce heterogeneity, but they come into play after data have been produced. In both cases the goal is to transform heterogeneous data into standardised data sets that can be readily fed into computer models or compared to their outputs. They are homogenisation mechanisms.

With regards to making global data, a number of international programmes were set up particularly after the middle of the 20th century to collect standardised weather and climatic data across the globe. A particularly important programme was the World Weather Watch, which coordinated the international sharing of weather data produced by satellites and radiosondes. Edwards (2010) pointed out that despite these efforts standards have been applied differently in different places so that fully-standardised global data sets have never been generated. As Star and Lampland have pointed out (2009, pp. 6-7) standards are distributed unevenly, which means that their impacts and the extent to which they become obligatory vary across different social groups. Furthermore, although they are implemented with a view to standardise practices across different localities, different communities approach, interpret, and use them in different ways: “We must not lose sight, however, of the simple fact that standards are intensely local, in the sense that, despite their global reach, they touch very specific communities in very specific contexts” (Star and Lamplad 2009, p. 16). In the case of global climatic data sets, Edwards pointed out seven elements that imposed some resistance on the effective implementation and adoption of standards in different research environments (Edwards 2010, pp. 251-252): institutional inertia; funding constraints; technical difficulties of application; problems of integration with other instruments, systems, and standards; operator training deficits leading to incorrect implementation; differences among local interpretation of the standard; and passive and/or active resistance from organisations and individual practitioners. The result of these standardisation efforts were not homogeneous data sets, but heterogeneous, incomplete, and inconsistent data[35].

Climate modellers, however, needed comprehensive global data sets for their models and had to make do with the data available. In order to make heterogeneous data sets useful for modelling, a number of procedures were also developed to ‘make data global’. Data had to be processed and transformed into data sets that would match the data points on the models’ three-dimensional grids. A number of data analysis models have been developed for this purpose: “What I call data analysis models (or data models, for short) are really a vast family of mathematical techniques, algorithms, and empirically derived adjustments to instrument readings” (Edwards 2010, p. xv). In addition, in the 1990s, reanalysis models have emerged. These models reanalyse weather data and produce comprehensive data sets of a variety of climatic variables:

[35] Further examples on difficulties in standardising climate research can be found on the paper on meta-data by Edwards et al. (2011). For a collection of papers on standardisation in other parts of our social lives see Lampland and Star (2009).

> In reanalysis, past weather records (not climate data) are run through complex data assimilation models – originally designed for weather forecasting – to produce a single, uniform global data set for 50 years or more. Traditional climate data consists mostly of averages for single variables (temperature, precipitation, etc.) over periods of a month or more. Reanalysis produces a much different kind of data: all-variable, physically consistent data sets containing information for millions of grid-points every six hours. Although biases in the models prevent them from displacing traditional climate data, climate statistics calculated from reanalysis data can reveal 'fingerprints' of climate change not detectable in traditional data (Edwards 2010, p. 16).

Although reanalysis provides researchers with consistent climatic data sets across all variables, several complications arise related both to the data used as input and to the models used to process these data (Edwards 2010, pp. 326-335). It uses a really wide range of data sources and a number of models are deployed to standardise them, which also have biases. For this reason, reanalysis data have been mostly used as complementary data rather than as primary data.

Despite the large number of issues revolving around these standardisation techniques, they are a crucial process in current climate-change science, as they are the key for the generation of global climatic data. These processes mediate the production of empirical data and the integration of data sets into climate models. They are, therefore, central mechanisms in holding climate-change science together. These mechanisms, however, along with the translation process that made GCMs obligatory points of passage, only tell part of the story. Climate-change science has not become a homogeneous transdisciplinary field of investigation such that mechanisms of building bridges between heterogeneous domains are not needed. Although standardisation facilitates data exchange between communities and translation creates a certain degree of convergence of interests among expert groups, there is still a great deal of diversity in climate-change science related to expertise, instruments, and epistemic cultures. This is the point at which mechanisms of building bridges between expert communities come into play. To understand fully how knowledge on climate change is produced it is necessary to understand on what basis different experts communities interact and exchange knowledge. The descriptions above set a general background against which other questions emerge, such as: What expert communities are working closely together? What type of expertise underpins the exchange of knowledge between different domains? Are these exchanges carried out on the basis of primary source knowledge or is there some mutual socialisation which could result in the acquisition of some type of specialist expertise? If there is no mutual socialisation, what processes could mediate the interactions between these different communities?

Chapter Summary

In this chapter I have provided a general description of climate-change science and, based on the STS literature, I have examined some of the mechanisms of homogenisation that are at work in this field. Firstly, I introduced the IPCC and argued that this institution to a large extent frames research into climate change. I then divided climate-change science into subareas of research and represented it diagrammatically by using the fractal model. I then worked on a very high-fractal model, the whole climate-change science and examined two major mechanisms of homogenisation that play an important role in this field: translation and standardisation. A few descriptive points can be made on the basis of this analysis. Several authors have pointed out that climate models, particularly GCMs, play a central role in climate-change science. They are the main tools used by climate-change scientists to assemble data from several fields and work out how the climate system works and changes in a global scale. The inflow of data into models, however, is not a straightforward process. Data have to be standardised and processed before being used by modellers. This is because data are generated by using several different methods, instruments, and skill levels. As a result records have been produced with different levels of precision, accuracy, spatial resolution, and covering different time periods. Furthermore, different data sets have different geographical coverage. This has given rise to international data collection programmes whose main purpose is to generate and share standardised data sets as well as to a number of models designed to process heterogeneous data sets and to produce homogeneous global climate records. These data are then fed into computer models, which simulate the past, present, and future climate. Their output is then used by impact experts.

From a more analytical point of view, I have argued that both mechanisms of homogenisation, i.e. translation and standardisation, are helpful for understanding how knowledge is produced in climate-change science. Translation funnels interests towards the production of data that can be fed into climate models and, in turn, towards the use of model output by the impacts community. In other words, it creates a certain degree of convergence of interests which make some communities communicate and collaborate[36]. Standardisations reduce the 'friction' (Edwards 2010) between different methods and techniques of data collection and between heterogeneous data sets. It helps data travel through different domains of practice and from one local area to others until becoming part of global data sets. These mechanisms however cannot make climate-change science homogeneous enough so that bridges between different domains are not necessary. They provide very interesting background information for further research, but they only tell the beginning of the story. I will now move to the second part of this thesis. I will examine paleoceanography as a case study and identify bridge-building mechanisms between expert communities to understand how heterogeneity is dealt with in this sub-area of climate-change science.

[36] I am not arguing that from a normative point of view this is the right or the best social structure that climate-change science could or should have. Some authors (Shackley and Wynne 1995b; Rayner and Malone 1998; Shackley et al. 1998; Demeritt 2001; Yearley 2009; Hulme and Mahony 2010) have argued that there are alternative and more pluralistic ways of producing knowledge on climate-change. Although I generally sympathise with their arguments, I am not adopting a normative approach in this work and will not examine their argument in detail. My point is essentially to shed light on how knowledge *is* produced but not on how it *should* be produced.

Chapter 4 - Paleoceanography

This chapter is an introduction to the next three chapters of this thesis where I examine bridge-building mechanisms between expert communities within paleoceanography and between paleoceanographers and other expert communities. I will introduce paleoceanography and describe how paleoceanographic research is carried out. I will describe paleoceanography as a subfield of paleoclimatology and the different stages of paleoceanographic research. This will prepare the ground for the sociological analysis that will be presented in the following chapters of how experts in paleoceanography communicate and collaborate among themselves and with other expert communities.

Paleoceanography and the Fractal Model

Paleoceanography can be represented as being four fractal levels below climate-change science (see figure 9). It is a subfield of paleoclimatology and it focuses on reconstructing past oceans and their interactions with other parts of the Earth system. It can also be depicted in the fractal model in a different way (see figure 10): it can be represented as a subfield of geology, which is a subfield of the Earth sciences, which, in turn, are a subfield of the Western Sciences. Both representations are correct. As pointed out by Collins (2011, p. 286), there are several ways of defining the boundaries between domains of practice and representing them with the fractal model that are not mutually exclusive. In the present work I use both representations as both are useful for understanding the links of paleoceanography with other fields of science.

Paleoclimatology seeks to reconstruct past climate before the emergence of consistent instrumental climate records. It is a subdiscipline of geology and focuses on the climatological aspects of the history of the Earth. Paleoclimatologic reconstruction is very valuable to climate-change science as the timescale of the instrumental record is short extending only approximately 150 years back into the past[37]. Paleoclimatic data,

[37] Satellite measurements have only been carried out since the 1960s. Instrumental measurements have been conducted for a longer period, although it is still short if we consider the geological time scale, which extends a few billions years into the past. Temperature measurements, for instance, have been taken for up to 300 years, but with scattered geographical representation (Burroughs 2001, pp. 140-151). Land temperature measurements were carried out only in parts of the northern hemisphere until the late

on the other hand, may reveal characteristics of the climate system extending as far as millions and sometimes even billions of years back into the past. The only way to study long-term climate processes is by using paleoclimatic data.

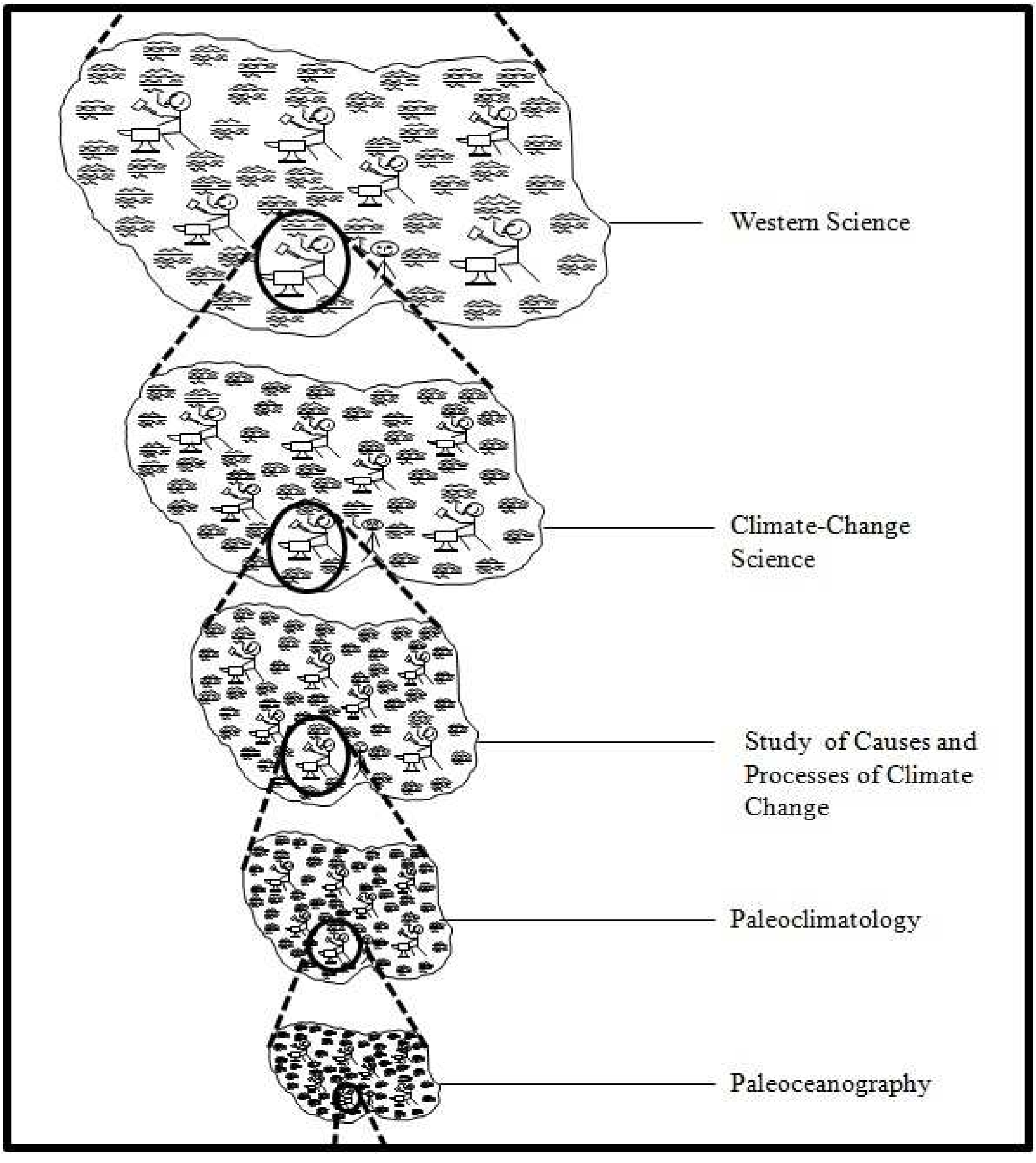

Figure 9: From Western sciences to paleoceanography through climate-change science.

---

nineteenth century, when an increasingly broader coverage gradually reached other areas of the planet (Burroughs 2001, pp. 140-151). It is only by interpolating records that it is possible to reconstruct land temperatures back into 1860s. Sea-surface temperatures have also been measured since approximately 1860 with buckets that collected water from the side of ships (Burroughs 2001, pp. 140-151). The coverage was also scattered as only the main ship routes have consistent records.

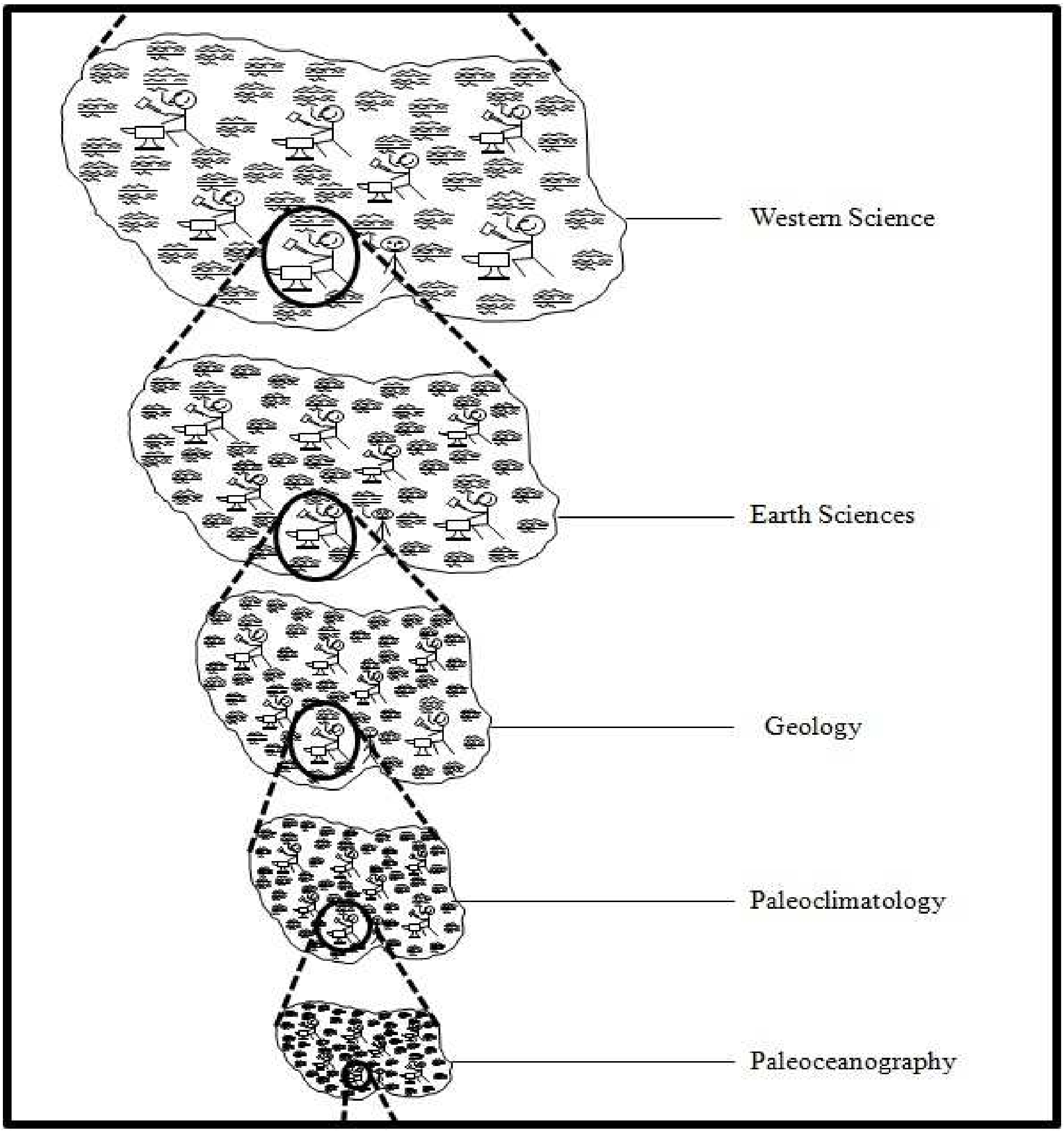

Figure 10: From Western sciences to paleoceanography through the Earth sciences.

Paleoceanographers produce data that reflect the state of the oceans in the past. The boundaries between paleoceanography and paleoclimatology, however, are not clearly defined as the different subsystems that compose the climate system cannot be completely disentangled. A great deal of data produced by paleoceanographers is also influenced by processes that are not purely oceanographic as past oceans interact with other parts of the climate system. For example, paleoceanographic records provide information on variables such as the concentration of carbon dioxide in the atmosphere, the volume of ice on the continents, etc. For this reason, several paleoceanographers also identify themselves as paleoclimatologists.

Paleoclimatology

Paleoclimatologists reconstruct the Earth's past climate by analysing a number of different archives, such as marine and lake sediments, outcrops, ice cores, tree rings, corals, rocks, leaves fossils, and historical records of climate-related phenomena. Different kinds of data can be extracted from each archive and different techniques are deployed on each of them. Tree-rings growth, for instance, depends on several climatic factors, such as temperature and precipitation. By measuring the width of them it is possible to collect data on those climatic variables. Ice cores drilled in Antarctica and Greenland contain samples of past atmosphere trapped in the ice that can be used to study changes in atmospheric composition. Their composition also contains data on other climate variables, such as past temperatures, precipitation, volcanic eruptions, etc.

Each paleoclimatic archive has their advantages and limitations. Marine sediments, for instance, are a source of data that goes back approximately 170 million years into the past. They do not provide however high-resolution data, containing no data on short-term climatic variation. They usually contain information describing intervals ranging from thousands to tens of thousands of years[38]. Ice cores, on the other hand, encapsulate data that goes as far as several hundred thousand years back in time. Although they cover a much shorter time scale, their resolution is significantly better and some cores can be used to reconstruct yearly intervals.

These archives also have other kinds of limitations, such as geographic representativeness[39]. A single type of archive cannot provide long-term high-resolution data on the whole planet. To produce a comprehensive reconstruction of the planet's climatic history it is necessary to combine data from several archives. Tree rings, for example, provide data spanning ten thousand years into the past. However, trees from

[38] In exceptional circumstances scientists have found cores from which they could extract data on intervals of hundreds of years and even tens of years (Bradley 1999, p. 4)

[39] Besides geographical representativeness, length of time, and resolution, there are other limitations such as dating accuracy, levels of inertia with regards to climatic variations, and so on (see Bradley 1999, pp. 4-8).

the tropics are not suitable for climate reconstruction, as there are no pronounced annual cycle of growth in these areas (Burroughs 2001, pp. 154-157). Moreover, they do not provide data that is representative of the oceans, which account for over 70% of the Earth's surface. Marine sediments, on the other hand, have a wider geographical coverage as they can be extracted from several areas in the oceans. They do not, however, carry much information that can be used to reconstruct the climate of continental areas.

Paleoceanography

Paleoceanographers use marine archives to reconstruct past oceans and their interactions with other parts of the climate system. There are two main types of marine archives: marine sediments (collected directly from the seafloor or from outcrops) and corals. Sediments accumulate slowly on the seafloor. Their physical and chemical compositions provide clues about the environment where they came from. Corals reefs grow in seawaters and can also be used to reconstruct a number of other climatic variables, such as temperature, nutrient availability, sea level, and water clarity.

These archives have very distinctive characteristics. They provide data with radically different resolutions as corals record seasonal variations whereas marine sediments usually record climatic variations on millennial or centennial time scales. Their geographical distribution is different as well. Marine sediments can be collected in all latitudes and in most parts of the oceans. Corals, in contrast, are mostly found in tropical areas[40]. Moreover, coral records extend from years to thousands of years back in time, whereas marine sediments record data from thousands of years to tens of millions of years back. Another difference is that marine sediments produce continuous records whereas coral records extend some hundreds of years, which is the lifetime of individual corals. To produce longer records it is necessary to band together data produced on different individuals.

[40] There are also deep sea corals, which contain data from other geographic areas, but this is still a new field of research that has not been consolidated yet.

The production of knowledge based on them involves similar stages, which have to be adapted to the types of material being analysed. I will use the production of knowledge using marine sediments to illustrate how knowledge is produced in paleoceanography.

Stages of Research in Marine-Sediments Paleoceanography: Writing Proposals

Research in marine-sediments paleoceanography can be divided into six stages: writing proposals for funding; collecting sediments; preparing samples; samples analysis; the interpretation of data; and writing papers. This is not a linear model and research projects need not necessarily go through all of them.

The first stage in any research project in paleoceanography is writing an application to obtain funding for it. Funding may be requested for various types of projects including those aiming to address questions such as why the climate system was so hot 56 million years ago; how Antarctica became permanently covered in ice approximately 34 million years ago; what ocean circulation changes bring about abrupt climate change during glacial-interglacial cycles, etc. Proposals are also written to obtain funding for fieldtrips in which paleoceanographers collect samples and for subsequent analysis. Furthermore, paleoceanographers also request money to develop new techniques of analysis and to refine those already in use.

Collecting Sediments

The second stage of research in paleoceanography is collecting material for analysis. This is usually done by going on research vessels equipped with tools to collect sediments from the seafloor[41]. The idea underpinning these cruises is that sediments

[41] The Integrated Ocean Drilling Programme (IODP) is the most important organisation carrying out research cruises. It is funded by several developed countries. It has two vessels and several platforms that are used for extracting material from the seafloor. One of its vessels, the *Joides Resolution*, is widely used by paleoceanographers. It is the only ship used for paleoceanographic research in the world that can drill the bottom of the sea. It is equipped with a rig that can collect material deposited as deep as two kilometres into the earth. There are several other vessels collecting sediments from the sea floor, but instead of having a rig on them they use other devices, such as piston corers or gravity corers, which cannot reach similar depths. See the IODP website for further information: http://www.iodp.org/.

slowly accumulate on the seafloor. Although the rates of accumulation vary across the globe and through time, it takes on average between 500 to 2000 years for one centimetre of sediments to be deposited on the bottom of deep sea. If no tectonic phenomenon rearranges the positions of the layers of sediments, the deeper one drills the older the sediments are. Seafloor sediments are continuously recycled in areas called subduction zones, where tectonic plates meet. In those areas, one plate goes underneath the other so that seafloor sediments are sunk into the mantle. Due to this phenomenon, the oldest sediments recovered from the seafloor are approximately 170 million years old.

A number of tasks are carried out during research cruises. Once cores are extracted from the seafloor they are taken to onboard laboratories. Micropaleontologists, experts in microfossils, take samples from the cores and by analysing the types of species found in them provide provisional dates for the material collected. To do so, they have to have an in-depth knowledge of all species of a particular subgroup of microfossils, their evolution, the time periods when these species lived, and the environment where they dwelled. These dates are important for the drilling operation itself as it gives people operating the coring devices a sense of how close they are to the sediments they are seeking.

The cores are also described in terms of their physical properties, such as colour, types of sediments, recovery rate, whether or not the sediments look disturbed, etc. This basic description is crucial for future research as it gives other researches clues as to the time period when sediments that compose a core were deposited and as to the types of environments they carry a chemical signature from. After this basic description is carried out the cores are stored and labelled so that when scientists return to shore they have accurate information about where they were collected, the depth at which they were found, and their basic characteristics[42].

[42]Paleoceanographers need not necessarily collect their samples themselves. Research cruises usually recover large amounts of material which take years to be processed and analysed. If an expedition is carried out by the IODP, scientists who take part in the cruise have preference in choosing cores. Those which are not selected by them are stored in cores repositories and after two years any scientist can apply

Paleoceanographers also collect samples from uplifted marine sediments found in continental areas. Those outcrops may contain sediments older than those collected from the bottom of the sea, extending hundreds of millions of years back in time. Field expeditions are organised to collect data from them. Scientists describe their physical characteristics - such as the types of sediments, colour, hardness, porosity, bedding, etc. and take samples either by using simple tools such as hammers or by drilling through the layers of sediments. However, only the minority of the paleoceanographic community work on those sediments. They tend to be much more disturbed than sediments extracted from the seafloor and their resolution tends to be lower. They are mostly used for studying the very ancient history of the Earth, which goes far beyond the sedimentary record found at the bottom of the sea[43].

Preparing Samples

The third stage in research in paleoceanography is preparing samples. Sedimentary cores are taken to laboratories and cut into parts that can measure from less than one centimetre to a few centimetres, depending on the resolution required by a specific research project. They are then washed over sieves where the fine fraction is separated from the coarse fraction. The fine fraction can be used for certain types of analysis, such as grain size analysis, which provides information about ocean currents. In the coarse fraction there are microorganisms' fossil shells. Those fossils can be used for geochemical analysis or for assemblage counts as will be described below.

Several microorganisms' fossils are used by paleoceanographers to generate data, such as foraminifera, diatoms, radiolarians, dinoflagellates, coccoliths, etc. The most used type of fossil in paleoceanography is foraminifera (forams), a phylum of microorganisms that belong to the kingdom Protista. They live either at sea surface

---

for them. If the cruise is run by another institution, scientists who were not involved in it sometimes ask the cruise's chief scientist for cores to use in their research projects.

[43] For a description of geologists studying outcrops see Frodeman (2003, pp. 96-116) and Almklov and Hepsø (2011).

(planktonic) or on the seafloor (benthic). These organisms secrete calcium-carbonate shells, which after they die sink and are deposited on the seafloor. The chemical composition of their shells is influenced by several climatic phenomena, such as sea water temperature, ocean productivity, water acidity, the amount of ice on continents, ocean circulation, the carbon cycle, etc.

When preparing samples for analysis scientists have to pick suitable fossil species by looking at them under a microscope and identifying them through their morphology. Certain types of geochemical analysis require that the microfossils are chemically cleaned from substances that might contaminate them. For instance, foraminifera shells have chambers that may contain clay. Clay contains magnesium, which in the case of the geochemical analysis of the ratio of magnesium to calcium, which is a proxy for temperature, may alter the temperature signal. To prevent this, foraminifera shells have to be crushed and cleaned by using chemical reagents.

Analysing Samples

After preparing samples there are two types of analysis that are most frequently carried out. One of them is assemblage counts. Micropaleontologists count the number of individuals of each species in a given sample. As different species live in different environmental conditions, the count provides clues on the state of the climate system where these microorganisms lived. Certain species, for example, live in cold waters whereas others prefer warm waters. Micropaleontologists usually find patterns when analysing samples. Some species are found in greater number in certain parts of a core than others, which indicates variations in the climate system through time.

The other type of analysis consists of geochemical techniques. Specific species of microfossils are picked and run in mass spectrometers, which are machines that analyse their chemical composition. In this case the picking of microfossils is not as complicated as in assemblage counts, as researchers are usually only interested in the few species they will analyse. After samples of those species are produced they are put in the mass spectrometer. This machine then produces read-outs with information on the chemical composition of the shells.

The geochemical analysis uses chemical elements that provide indirect data on past climates, the so-called proxies. Each proxy is used to reconstruct a particular property of the climate system, although usually there are many environmental variables controlling them. I will use the analysis of oxygen isotopes in foraminifera shells, which are proxies for seawater temperature, salinity, and continental ice volume, as an example[44].

Oxygen is the second most abundant gas in the atmosphere. It is also a component of water vapour and of ocean water. There are three isotopes of oxygen: ^{16}O, ^{17}O, and ^{18}O. ^{16}O is by far the most abundant, accounting for approximately 99.76% of all oxygen. It is followed by ^{18}O, which accounts for 0.2%. The ratio of ^{18}O to ^{16}O (hereafter $\delta^{18}O$) in seawater is influenced by water temperature and by the hydrological cycle (Cooke and Rohling 2001; Ruddiman 2008, pp. 359-361).

The hydrological cycle affects the isotopic composition of seawater. An important part of the hydrological cycle consists of seawater evaporating in the tropics area, where temperature is higher, and moving towards the poles. While air masses move towards higher latitudes they go through several cycles of precipitation and evaporation. As the atmosphere becomes colder in high latitudes, it holds less water vapour. For this reason, water masses that reach the poles have smaller amounts of water vapour than when they were created in the tropics. When those masses reach very high latitudes, water vapour becomes snowfall and precipitates on the ice sheets. The water vapour might be stored in the ice sheets for thousands of years, but eventually these water molecules return to the oceans through ice sheet runoff.

[44] Isotopes are atoms with different numbers of neutrons but the same number of protons. Proton number determines atomic element type (its chemical properties at large) while differences in neutron number determine physical properties (e.g. mass, radioactivity). Oxygen, for instance, is made up of 8 protons. It may have 8, 9, or 10 neutrons. There are therefore three isotopic variations of oxygen: ^{16}O, ^{17}O, and ^{18}O, all with very similar chemical properties but different physical ones.

As water vapour makes its way towards the poles, air masses become depleted in ^{18}O due to the cycles of evaporation and precipitation they go through. This works in the following way: When seawater evaporates, a larger amount of the lighter ^{16}O molecules becomes water vapour than the heavier ^{18}O enriching the oceans in ^{18}O. Similarly, because ^{18}O is heavier than ^{16}O, when water vapour condenses it tends to precipitate faster. As a result, snow precipitation on the ice sheets is rich in ^{16}O. These water molecules are then stored in the ice.

Ice sheets wax and wane through the history of the Earth. Whenever they grow, more ^{16}O-rich rainfall precipitates on them and are stored in the ice, reducing the amount of ^{16}O-rich waters in the oceans. Whenever they shrink, ^{16}O-rich waters melt into the oceans. A similar process happens in glaciers, even if they are in low latitudes. When snowfall reaches them, the molecules of water are ^{16}O-rich due to the cycles of evaporation and transpiration they have gone through before being deposited there. Consequently, the larger the amount of ice covering the continents, the higher the amount of ^{16}O-rich waters stored in ice sheets and glaciers.

This mechanism impacts the ratio of ^{18}O to ^{16}O of seawater[45]. As the chemical composition of foraminifera shells responds to the environment where they live, the oxygen molecules that make up the calcium carbonate of their shells ($CaCO_3$) is also impacted by the seawater isotopic composition.

This, however, is not the whole story. There are other variables that influence the isotopic composition of foraminifera shells. When seawater evaporates, it becomes saltier and, consequently, dense. This seawater rich in ^{18}O then sinks making deep seawaters have a high $\delta^{18}O$.

---

[45] This process is more complicated than this as water in different parts of the globe might have different $\delta^{18}O$ due to local phenomena such as exchanges between ice from ice sheets and water around their margins (Cooke & Rohling 2001, p. 12) and river waters delivery on the oceans (Ruddiman 2008, p. 360). Furthermore, seawater mixing through ocean circulation takes hundreds of years to take place, which means that $\delta^{18}O$ might not be uniform across the oceans. These phenomena have to be taken into consideration by researchers when interpreting $\delta^{18}O$ data.

Furthermore, the $\delta^{18}O$ of foraminifera shells is also dependent on the temperature of the seawaters in which they calcify. When the shells are precipitated there is an increase in the $\delta^{18}O$ of the calcium carbonate if compared to the $\delta^{18}O$ of the seawater. The lower the seawater temperature is the greater the difference between the $\delta^{18}O$ of the foraminifera shell and of the seawater.

By analysing the oxygen isotope composition of foraminifera shells paleoceanographers therefore obtain a signal that is controlled by seawater isotopic composition and by seawater temperature. These data, if compared to other proxies that are used to disentangle the different environmental variables that influence it, i.e. seawater temperature, salinity, and continental ice volume, can be used to reconstruct past climates. The isotopic analysis is carried out in mass spectrometers[46].

Another example of a proxy is carbon isotopes that make up the calcium-carbonate shells of foraminifera. They provide information on productivity in the oceans, exchange of carbon between reservoirs (e.g. vegetation, surface waters, deep waters, etc), and ocean circulation. The ratio of trace metals to calcium in foraminifera shells also provides paleoceanographers with a wide range of information. When the foraminifera secrete their shells they incorporate small amounts of trace metals such as magnesium, boron, strontium, etc. The incorporation of magnesium, for example, is temperature dependent, therefore its ratio to calcium reveals information about past temperature. Boron's incorporation into foraminifera shells is influenced by water pH. The elemental ratio of boron to calcium is therefore an ocean acidity proxy. pH data can also be transformed into data about CO_2 concentration in the atmosphere in the past.

[46] Samples of foraminifera shell are put in mass spectrometers, which analyse the mass of the atoms that make up the samples. By reacting the calcium carbonate molecules from foraminifera shells with phosphoric acid mass spectrometers obtain CO_2. CO_2 has one atom of carbon and two of oxygen. Oxygen, as pointed out above, has three isotopes: ^{16}O, ^{17}O, and ^{18}O, but ^{17}O exist in such lower concentrations that it is not very relevant in this analysis. Carbon has two stable isotopes ^{13}C and ^{12}C. If a CO_2 molecule is composed of one atom of ^{12}C and two ^{16}O, its mass is 44. If it is made up of one atom of ^{13}C and two ^{16}O, its mass is 45. Finally, if it has one atom of ^{12}C, one ^{18}O, and one ^{16}O, its mass is 46. The ratio of $^{45}CO_2$ to $^{44}CO_2$ therefore is the ratio of ^{13}C to ^{12}C ($\delta^{13}C$), whereas the ratio of $^{46}CO_2$ to $^{44}CO_2$ is the ratio of ^{18}O to ^{16}O ($\delta^{18}O$) (see Cooke and Rohling 2001 and references therein for further information).

Data Interpretation

Data interpretation[47] is a crucial step in paleoceanography. Paleocanographers usually do not produce data only for the sake of producing climatic records. They want to understand certain climatic mechanisms and produce explanations for certain phenomena. They have to connect the data they produce with other environmental phenomena that could affect the phenomena they are interested in. To do so, they have to interpret their data in the light of other data sets already published. This requires an in-depth understanding of the Earth system, of its main mechanisms of change, and of the scientific literature.

Law (1980, pp. 16-18) pointed out that in sedimentology, which is a geological field close to paleoceanography and that overlaps with it, there is no unique methodological and theoretical approach. A range of complementary techniques is used and their results integrated. The same applies to paleoceanography. Paleoceanographers usually produce data with a number of techniques and try to bring them together. This is because all data sets have limitations and none of them produce data comprehensive enough to be used without comparison with other records. Furthermore, when interpreting a given data set paleoceanographers do not usually seek to find general laws. Rather, their ultimate goal is to identify the mechanisms of climate change driving particular phenomena[48]:

> In geology, the goal is not primarily to identify general laws, but rather to chronicle the particular events that occurred at a given location (at the outcrop, for the region, or for the entire planet). This means that hypotheses are not testable in the way they are in the experimental sciences (Frodeman 1995, p. 965).

Paleoceanographers seek to fit the different pieces of a puzzle together. To do so they need to include in their interpretation as many relevant parts of the Earth system as possible. Unlike physics where the major goal might be finding very simple universal

[47] I am here reproducing the labels using by paleoceanographers to describe their work. Data interpretation, however, is not restricted to this stage of research. All steps of research involve interpretive steps, including making judgements on where to collect sediments, developing hypothesis on the age of the sediments just brought on board, choosing samples, and so on.
[48] Paleoceanography is a subfield of geology. This is why Froderman's ideas apply to paleoceanography.

laws, in paleoceanography, as in other geological fields of science, scientists seek to develop comprehensive explanations of particular phenomena:

> […] our overall comprehension of the Cenomanian–Turonian boundary event is determined through an intricate weighing of the various types of evidence (e.g. lithology, macro- and micropaleontology, and geochemistry). This overall interpretation is then used to evaluate the status of the individual pieces of evidence (Frodeman 1995, p. 963).

For instance, if paleoceanographers find in a sedimentary core evidence from oxygen isotopes that sea water $\delta^{18}O$ was much lower some time ago in a given locality they can ask several questions that might help them interpret this data set: Is this signal brought about by temperature or ice cover on continents? Was it a global or a local phenomenon? What were the causes of this phenomenon? Are there other proxies or archives showing the same trends confirming that this data set is accurate?

To address these questions they might start by measuring the ratio of magnesium to calcium, which is a proxy for paleotemperatures, in foraminifera shells found in the same core. By doing so, they can disentangle the temperature signal from the signal brought about by the amount of ice cover on continents. If they find that, for example, temperature rose a few degrees, they have to identify what could have caused this. To do so, they need to be very well informed about the scientific literature to know whether other researchers have already found similar trends in other locations. If so, this is evidence that the phenomena might be global. If not, it can be the case that they found a local climate change, which has to be explained by using local variables. It is also necessary to take into consideration the time scale of the phenomenon. If it took place in a million years time scales, the main forcing is likely to be related to tectonics. If it happened on a tens of thousands year time scale, the main forcing to be taken into consideration are orbital cycles. If it is a shorter phenomenon, having happened on a time scale of hundreds to few thousand years time scale, then the most likely forcing is changes in greenhouse gases. Taking into account all these considerations of time scale paleoceanographers have to identify what other phenomena happened at the same time that could be part of the climatic process that led to the increase in temperature. In other words, they need to discover which climate feedbacks were triggered by the initial forcing. Are there evidences of changes it the carbon cycle that could have brought about a release of carbon in the atmosphere? Are there evidences of volcanic activity? Are there evidences of changes in ocean circulation? To do so, they have to look at other data sets that can provide them with evidence to address these questions. These data sets might have been generated by using marine sediments or other archives. Synchronising data produced by using different techniques or on different archives, however, is challenging. Several techniques are used for producing age models and each of them has different resolution and uncertainties so that linking different records is not altogether straightforward[49]. The limitations and strengths of each age model have to be carefully taken into consideration when interpreting data. In sum, it is necessary to have a deep understanding of how the Earth system works, of how paleoceanographic data is produced, and of the scientific literature to be able to interpret paleoceanographic data.

There are some tools that can help researchers interpret data. Statistic manipulation may help them find cycles and patterns in their records. Computer models may also be very useful. They can be used to test hypothesis in that they can bring together several data sets and simulate the whole climate system.

It is worth noting that scientists interpreting a data set are not necessarily the same who produced it. Some research projects in paleoceanography consists of scientists reinterpreting data already published in the literature and putting forward innovative approaches to the understanding of certain phenomena.

Writing Papers

Once paleoceanographers have produced new interpretations of a given phenomena or added new angles to the already existing interpretations, they write up papers and submit them to journals. This is the final step in any research project. Publishing papers in high-profile journals is essential for a successful scientific career. It gives scientists prestige and enables them to obtain more senior positions.

[49] Some of the main techniques used in paleoceanography for developing age models are radiocarbon dating, magneto-biostratigraphy, stable isotopes stacked records, and Uranium-thorium series. Further information on them can be found on Bradley (1999) and Noller et al. (2000).

Chapter Summary

In this chapter I have outlined what paleoceanography is to prepare the ground for the subsequent discussion on expertise and communication between scientists working in paleoceanography. I have described paleoceanography as a subfield of paleoclimatology that research past oceans and their interactions with other elements of the climate system. Paleoceanographers mainly extract data from two types of materials: marine sediments and corals. I have then described the main stages of research in paleoceanographic research on marine sediments: writing proposals, collecting material, preparing samples, analysing samples, interpreting data, and writing papers. I will move now to the analysis of the mechanisms that mediate communication and collaboration within paleoceanography.

Chapter 5 - Low Fractal Levels: The Case of Paleoceanography

In this chapter I will examine a low fractal level in climate-change science: paleoceanography. This means that the level of diversity within this domain is considerably smaller if compared to higher fractal levels, such as climate-change science as a whole. Yet, there is a wide range of contributory experts that compose this field. I will argue that communication in paleoceanography is mediated by its domain language, which is rich enough in technical details to afford informed conversation between different contributory experts.

The first task to be done in this chapter is to identify the different types of experts that are part of paleoceanography and what the language they speak is about. If we return to the fractal model (see figure 9, page 107), we could ask who the different stick figures that compose this field are and what they are talking about. In other words, what the different types of contributory experts that make up paleoceanography are and what the language spoken by them is about. I will begin by introducing the different types of contributory experts.

Contributory Expertise in Paleoceanography

In terms of contributory expertise there is a range of subspecialisations in paleoceanography. The most important is the division between paleoceanographers, micropaleontologists, and geochemists. The basic distinction between ‘pure’ paleoceanographers and these other empirically-oriented experts who contribute to paleoceanography is that although paleoceanographers use a wide range of geochemical and micropaleontological techniques, their use of them is instrumental. They are much more focused on understanding paleoceanographic and paleoclimatic phenomena than in the development of new techniques. They occasionally become involved with the development of new types of geochemical or micropaleontological techniques, but they do this in collaboration with geochemists and micropaleontologists as they cannot do this by themselves.

Micropaleontologists are experts in microfossils. They specialise in groups of organisms, such as foraminifera, diatoms, dinoflagellates, coccoliths, radiolarian, etc. Their main skill is the ability to distinguish a wide range of species by looking at their morphology under the microscope and to relate them to the environment and to the time periods they lived in. Micropaleontologists work on several questions related to the groups of organisms they specialise in, such as their evolution, taxonomy, geographical distribution, etc. This is a field of research in itself and some of its members use their skills to contribute to paleoceanographic research. Their main contribution to paleoceanography consists of using their 'trained eyes' to distinguish the different species found on sediments and to count them. By doing so, they can make inferences about the state of the environment in which these fossils lived[50]. Unlike researchers who are strictly paleoceanographers, they do not only have an instrumental knowledge of a few species of microorganisms. They specialise in distinguishing the whole range of species of a micro-organism group throughout long time intervals.

Geochemists are experts in the analysis of the chemical composition of different elements of the Earth system. Their main tool is mass spectrometers or similar machines, which measure isotope ratios or elemental ratios. Their research is not limited to paleoceanography. They also contribute to a number of other fields of investigation in the Earth sciences, such as cosmochemistry, igneous geology, volcanology, petrology, mantle geochemistry, etc. When they are involved in paleocanographic research their main contribution is the development of new analytical techniques, such as new proxies, or the refining of existing ones so that more accurate and precise measurements can be made. A considerable part of their time is spent in laboratories preparing samples and running them in mass spectrometers or similar machines that make analytical measurements. As geochemists' expertise is in applying techniques, they usually apply geochemical techniques on several different types of archives.

[50] Even though the strongest skill of micropaleontologists is examining fossils and producing data on them, they usually also become involved in other stages of research in paleoceanography. They are very important, for example, in research cruises. They can provide other researchers with approximate estimates of how old different layers of sedimentary cores extracted from the sea floor are by examining the fossils found on them. This helps collect seafloor cores from specific time periods.

Micropaleontologists and geochemists usually do not only contribute to paleocanography by generating data with their specialised skills. They also interpret these data and publish papers in journals. They therefore have to be able to interpret the records they produce according to the shared understanding within the paleoceanographic community of how the Earth system works. Furthermore, some micropaleontologists whose research interests lie in paleoceanography can prepare samples for routine geochemical analysis and run the machines themselves. Similarly, some geochemists acquire an instrumental knowledge of picking fossils so that they can pick a few species they might use in the geochemical analysis.

There are other subspecialisations within paleoceanography. Experts tend to specialise in some time intervals, phenomena, research techniques, geographic areas, and archives. These different types of specialisation are intertwined. To research certain time periods, for example, it is necessary to use certain types of archives. Or to research a given phenomena it is necessary to deploy certain techniques. If a paleoceanographer is interested in ocean currents, for example, he or she might use carbon isotopes, neodymium elemental ratios, and so on. Or if he or she is interested in sea water temperature, he or she might use the elemental ratio of magnesium to calcium, oxygen isotopes, and so forth.

Based on this description we can now return to figure 3 and use it to make sense of the division of labour between different types of experts in paleoceanography. The stick figures represent groups of scientists who specialise in: the Last Glacial Maximum, glacial-interglacial cycles, deep time[51], ocean currents, ocean acidification, orbital cycles, anoxic events, abrupt climate change, the North Atlantic, Antarctica, etc. Furthermore, some of them would be scientists working on the interface between paleoceanography and other geological fields of research, such as micropaleontologists specialised in microorganisms that can be used to generate paleoceanographic records,

[51] Deep time usually refers to time periods older than approximately 2.5 million years ago when the glacial-interglacial cycles began. This concept is an interesting metaphor. As sediments tend to continuously be deposited on the seafloor, layer above layer, the deeper you drill through sedimentary layers, the older the sediments are. When scientists use the term deep time, they are referring to the fact that sediments from millions of years ago are usually buried deeper than ‘younger’ sediments.

and geochemists developing new paleoceanographic proxies. Each of these subspecialisations are also sub-domains of paleoceanography, being at a fractal level below paleoceanography as a whole.

At this point it is important to remember that these specialisations are at the collective level of analysis. I am describing groups of individuals who share expertise on specific aspects of paleoceanography. At the individual level, scientists usually have a combination of specialisations in particular events, time intervals, phenomena, archives, and techniques. I will provide some examples to illustrate this point. The first one is from an interview with a paleoceanographer who works on reconstructing abrupt climate change that took place over the past million years. He specialises in using marine sediments to reconstruct these events focusing particularly in ocean circulation changes and subsequent reorganisations of the climate system:

> Will: Ok, so, I'm a paleoceanographer who works on trying to understand I guess the coupling of ocean circulation change and climate change and particularly abrupt time scales, so sort of fast changes in climate. So these are reorganisations that occur in the climate system on maybe hundreds or tens to hundreds of years. And we know through understanding the way in which the climate system operates that those changes have to involve ocean circulation change. So, my research is really about reconstructing in the paleo-sense, so in time, in the past, different deep circulation patterns and then trying to couple them with surface records or other terrestrial records to try and put pieces together the way in which these changes have occurred and how that impacts on, yes, so ultimately the climate.
>
> Tiago: What's the time interval you work on?
>
> Will: I work on a range of time intervals in the past. If we break down climate into different time scales, as you're probably aware, there are orbital time scales, which are hundreds of thousands of years variability, so this is ice age to warm age cyclicity. So, I certainly work on those sort of time scales over the last million or so years. Then I work right down to this sort of time scales in the last glacial periods where we have these abrupt warmings called Dansgaard-Oeschger warming. So, I work on understanding those. Right through to climate of the last few hundred years, trying to reconstruct decadal scale, so tens of years sort of variability, marrying up the proxy paleoceanographic record with the instrumental observational climate record. So, a whole range. I don't do the same sort of deep time perspective. I don't typically. I have in the past a little bit worked on the middle Miocene and sort of early deep time perspective, but it's really below some say millions of years, the Pleistocene interval, really.

The following quotation is from an interview with a paleoceanographer with distinct research interests. She specialises in an event called the Messinian Salinity Crisis, which took place approximately 6 million years ago in the Mediterranean. She has a narrow focus in terms of events and geographical area. She also specialises in some particular isotopic systems, i.e. strontium and neodymium isotopes:

> Tiago: What kind of archives do you use to generate data?
>
> Lisa: I generate isotopic data. Mostly strontium isotopes but I'm also involved in neodymium isotope projects.
>
> Tiago: Do you work mainly on marine sediments or on a wider range of things?
>
> Lisa: Certainly the isotopes side has been only marine. But it's fairly odd marine conditions. So, it's Mediterranean when the Mediterranean was very nearly isolated from the global oceans. So, it's what we call marginal marine settings, so it's not open oceans, in other words.
>
> Tiago: Could you tell me broadly speaking about your research interests. So, I'm trying to situate the time intervals you're interested in, the data sources, the phenomena, etc?
>
> Lisa: So, most of my research has been focused on the Mediterranean during what's called the Messinian Salinity Crisis, which is an extreme climate event which occurred between 5 and 6 million years ago, during which the Mediterranean accumulated certainly 1500 possibly over 2 kilometres of salt, the extracted salt was about 6% of the world's salt. So, it's a big deal. And the salinity at that time in the Mediterranean clearly got extremely high. But it also varied a lot. So, in fact there was a period when it was considerably fresher than it is today as well. In other words it was a period when it was highly sensitive to connectivity between it and the Atlantic. And the reason it was so sensitive is because prior to the formation of the Gibraltar Strait there were two gateways not one and those gateways were shutting. Gibraltar opened about 5 million years ago, and when it did it restored pretty much the conditions that we have today. So, it's that period of tectonic restriction of the corridors which is recorded by this event. And a lot of what I've done is to generate data and to do with some numerical box modelling that tries to reconstruct the hydrological balance budget of the Mediterranean during that period. In other words to work out what's coming in from the Atlantic, what's going out into the Atlantic and whether we can make that work in terms of what goes on in the Mediterranean. So, that's my main research interest.

Shared Contributory Expertise?

The section above provides hints that the different types of experts that contribute to paleoceanography share, at least to a certain extent, their contributory expertise. Most scientists working in paleoceanography that I interviewed had at least a basic training in some geochemical and micropaleontological techniques. Paleoceanographers, for instance, usually know how to pick a few species of microorganisms for geochemical analysis and can run the sample through a mass spectrometer when it comes to routine techniques. Geochemists producing data on microfossils, for example, usually learn to pick the species they need from sedimentary cores. Some micropaleontologists also learn how to carry out routine geochemical techniques, such as carbon and oxygen isotopes analysis, at some point in their careers, particularly in the early stages when they are being trained. This means that at some point in their careers these experts had some immersion in practices from other subspecialties of paleoceanography. One could argue that this could help integrate the paleoceanographic community as different types of experts would be socialised in some of the techniques deployed by their peers which would led them to acquire certain degree of contributory expertise in them. This would result in them having an expert appreciation of each other's work and would facilitate collaboration and communication.

This is not the case, however. These scientists, when engaging in these practical activities have immersion only in standardised practices that could be taught to anyone, including people from outside the community. These practices provide scientists only with a glimpse of what other groups of experts do. They do not help a great deal in bridging the gaps between different expert communities. I will set out a distinction between standardised contributions and domain-language-based contributions that will help make this point clearer. It will also bring to light the importance of paleoceanography's domain language in linking different groups of contributory experts.

Standardised Contributions

Standardised contributions are those that can be competently performed without mastering the language of the domain in which they are being made. These tasks are frequently delegated to technicians. In paleoceanography, they typically refer to routine activities involved in the collection of material and in the preparation and analysis of samples, such as the washing of sedimentary cores and the running of mass spectrometers to carry out measurements with standardised techniques. They are not theory-free or 'language-free' as there can be a language about any activity, regardless of how simple they are. People can, for instance, talk about washing a sedimentary core in a more or less informed way, depending on how well they understand this practice. But it is possible to wash a core without mastering the language of paleoceanography.

This point can be better understood if we consider the criticism of the distinction between the material and the conceptual components of a laboratory put forward by Latour and Woolgar (1979). According to them, all material components of laboratories, such as mass spectrometers, microscopes, etc., which are now uncontroversial, have in the past been the subjects of debates in the literature of other fields:

> It would be wrong to contrast the material with conceptual components of laboratory activity. The inscription devices, skills, and machines which are now current have often featured in the past literature of *another field*. Thus, each sequence of actions and each routinised assay has at some stage featured as the object of debate in another field and has been the focus of several published papers. The apparatus and craft skills present in one field thus embody the end results of debate or controversy in some other field and make these results available within the wall of the laboratory (Latour and Woolgar 1979, p. 66).

Standardised contributions have, therefore, frequently been the subject of controversies in the past, but these controversies were settled and the laboratory procedures and methods have become standardised. When certain procedures and methods become standardised they are taught as if they were trivial and straightforward truths. At this point, they will be transferred to technicians, students, or to whoever is in charge of carrying them out without the need to mention all the controversies that preceded their stabilisation. Once this point is reached standardised contributions become autonomous from the fields of expertise where they originated. Their language might overlap with the language of the field from which they originated and with the field where they are applied, but it will be purified from the theoretical intricacies of these other domains.

Standardised contributions are performed on the basis of tacit knowledge (Barley and Bechky 1994; Barley 1996; Hong 2008, pp. 551-552)[52]. Their complexity and the complexity of the language about them vary depending on the task. Even when they are based on very simple procedures they still require tacit knowledge to learn how to follow them correctly. As it has been pointed out above, rules do not contain the rules for their own application, therefore it is necessary to acquire collective tacit knowledge to be able to apply laboratory procedures correctly (Collins 2010). This tacit knowledge,

[52] Researchers have provided illustrations of the skills that technicians have. Barley and Bechky (1994), for example, set out an in-depth description of these skills in medical science. Hong (2008) provided examples of the skills necessary to carry out geochemical analysis.

however, can be transferred without the need to understand theoretical aspects of the domain where the research is being carried out. In the case of paleoceanography this can be exemplified with tasks such as washing cores and preparing samples. Washing cores over sieves to separate the fine from the coarse fraction, for example, is a very simple task that can be performed by any individual:

> Tiago: Washing cores doesn’t involve any skill whatsoever? Any person could do it?
>
> Shaun: Yes, anyone, there’s no pre-requisites. In other places you can get school students doing it in the summer because it’s so simple. It’s just mud, spray, dip it into a jar. So it’s very simple, it’s kind of standard factory work.

Another example of this is sample cleaning. Cleaning samples with chemical reagents may require a lot of concentration and precision, but it is also a relatively simple technical task that can be quickly taught. One geochemist working at the interface between geochemistry and paleoceanography compared it to cooking in that it is just a matter of following recipes. These recipes have to be very carefully followed, but it is not necessary to have any understanding of the language of paleoceanography to be able to carry out these tasks:

> Gabriel: Actually what we do, well it's quite technical, and it's quite complex scientifically, but what you're actually doing in the lab with your hands is very simple. It's just like cooking. It's very careful cooking, very precise cooking, but it's cooking (laughter). It's just following a recipe in the very minimal, in the very least it's just following a recipe. I've got high school students who have been able to do this kind of chemistry in the past. It's just add this much to that, add that much to that. As long as you're safe and you kind of know what you're doing it is not a big deal.

Standardised contributions in paleoceanography also include more complicated tasks, such as running and maintaining mass spectrometers. These tasks are sometimes delegated to technicians once the geochemical techniques and methods have become established. In other words, once they become standardised they can be learned without becoming a full-blown geochemist. For an individual to be able to carry out these activities competently it is necessary to have the ability to make judgements related to data quality and to how to repair the machines when they break down. Similarly to other types of standardised contributions, these judgements depend on the acquisition of collective tacit knowledge as they have to be performed according to socially shared standards. But these judgements are also independent from the language of paleoceanography. In routine analysis, such as the measurement of stable isotopes, there are a number of checks that are used to make sure the mass spectrometer is operating normally. For instance, standards of known chemical composition are frequently run through the machine and if the numbers produced deviate too much from the expected values it means that there is something wrong with the mass spectrometer. In this case, the judgements that have to be made to work out whether the machine is working properly are based on a fully-developed and standardised methodology so that for one to be able to make them competently it is not necessary to speak paleoceanography's domain language. These judgements can potentially be taught to anyone who has finished high school[53].

In terms of maintaining a mass spectrometer where established techniques are used, the judgements that have to be made are also standardised. Maintaining one of these machines consists of making sure the machine is working properly and of being able to identify problems and troubleshoot them whenever necessary. For instance, sometimes there are leaks in the system and carbon dioxide from the atmosphere enters into the mass spectrometer. As a consequence, the results of the analysis are contaminated. In this case the problem has to be identified and repaired.

When it comes to cutting-edge techniques measurements are much more difficult to make and take much more time to be made. If the technique and the methodology are yet to be established and standardised, they cannot be delegated to technicians as geochemists will have to work out how to make the measurements. If, on the other hand, the technique and the methodology are already established, highly skilled technicians can take on the task. Measuring boron isotopes in foraminifera shells is an example of this. They are a proxy for past ocean pH which can be converted into data

[53] This does not mean that people running mass spectrometers only follow these standardised methods to make these judgements. During my fieldwork I met technicians who had already been granted their PhDs in paleoceanography. For this reason, they could also use their knowledge on the range of numbers that should be produced by the mass spectrometer to assess data quality. If the analysis of a given sample produced unexpected results they would check if there was any issue with the machine. However, this was not a necessary skill for them to do their job competently.

on past atmospheric concentration of carbon dioxide. Boron isotopes, however, are very difficult to measure for several reasons. Firstly, the amount of boron in foraminifera is really small so that researchers need many more shells to produce data than if they were analysing, for example, stable isotopes. Secondly, boron is ubiquitous in the environment so that samples can easily be contaminated. Consequently, it is necessary to setup 'boron-free' laboratories and to clean the samples very carefully. There are also difficulties in running the mass spectrometer as it is difficult to keep the level of boron contamination in the machine low. For this reason, these machines are not always in optimal conditions for running boron samples. In an interview a geochemist specialised in measuring boron isotopes in foraminifera pointed out that one of the most important judgements related to these measurements relate to data quality as the machine is constantly in sub-optimal conditions:

> Tiago: Are there some specific kinds of judgements that you have to teach your students how to make?
>
> James: Yes, the critical one is about data quality really. And that's kind of a hard thing to, you have to be pretty careful but then you don't want to be too careful. So, the machine has sort of three stages of operation. It's either crap, it's either ok, or it's really good. And when it's crap you don't want to do anything, when it's ok that's when you want to get your data, when it's good you definitely want to get your data. You might want to save your most precious samples until it's working really well. But then that might mean that you're down there using the machine for an entire week and you only get one day that you think it's good enough but actually you could have got data on every single one of those days and the data would have been adequate. It's teaching people that it needs to be this good, doesn't need to be that good, but definitely it doesn't want to be bad, but you got to make that judgement about where you're sort of drawing your cut-off. When you stop running the machine or when you work through the night. That's one of the hardest things to teach them.
>
> Tiago: If it's hard, how do you go about it? Just by doing it?
>
> James: It's experience. They will make mistakes and they will spend three days waiting for the machine to be perfect, and you'll be like well what are you doing? Or they'll show you some standard data and the standard deviation is .35 per mil and that's worse than, that's too bad. Why did you go and collect all your data when your blank was high? Or why did you go ahead and did this when the machine was sub-optimal. So, they learn by their mistakes really. You can kind of help them minimise the impact of those, but that's the only way they learn.

These judgements on data quality can be made solely on the basis of an understanding of how a mass spectrometer works. In the case of boron analysis, it requires considerable experience in running the machine and learning about its different stages of operation. But an assessment on whether it is working well or not is based on technical procedures. Standards here are crucial. Blanks are also useful as they show whether the level of contamination is high[54]. In this case, again, judgements can be made on the basis of standardised procedures. In the quotation above, however, there is a judgement that depends on a broader theoretical understanding of paleoceanography, or, in other words, on having contributory expertise in this field: the interviewee mentions that a given student should have kept his or her most precious samples for when the machine was working at its best. This involves judgements on which samples are more important than the others. These judgements are based on an understanding of the goals of the research project, on the availability of samples from certain regions, and on the age of the samples. This means that if there were a technician in charge of the machine the researcher for whom the samples were being analysed would have to tell the technician in advance about what samples should be kept for when the machine was in its optimal state.

Individuals who have the appropriate expertise to make standardised contributions may improve laboratory procedures or methodologies. However, this technical competence does not enable them to develop new techniques, such as new geochemical proxies. To do so, they would need to understand how the Earth system works, how different chemical elements behave in different environmental conditions, how to frame a research project to develop a new proxy, etc. To do this it is necessary to be socialised in the language of paleoceanography and become an expert in this field. When a geochemist is developing a new proxy, the first step is to create hypotheses on how a given chemical element will react to certain changes in the Earth system. The same applies to micropaleontological techniques as it is necessary to have hypotheses as to how certain species respond to environmental changes. A geochemist that I interviewed who works on improving techniques and on applying them to address paleoceanographic problems, when asked about how to develop a new geochemical proxy explained that the first step is to have a hypothesis that an isotope or elemental ratio will vary due to a climatic parameter:

---

[54] The standard procedure in geochemical analysis of foraminifera consists of the fossils being cleaned and dissolved in solutions and then run into mass spectrometers. Blanks consist of running these solutions without the foraminifera in them to check whether the machine produce unexpected values due to contamination.

> Tiago: In terms of the development of a new proxy, are there a number of steps that you have to go through?
>
> Matt: Yes, I guess you have to first of all you have to demonstrate the potential. That might be either in the laboratory, so you come up with an idea, so maybe this isotope or elemental ratio should vary with some climate parameter. And first of all you got to have the expectation that this is going to hold true. So, for instance, for magnesium calcium that would be some expectation that temperature will affect the latter's strain and the incorporation of things. So you've got this idea that it might work. Then you have to show that it does work.

Only scientists who deeply understand the mechanisms of change in the Earth system and how they affect different chemical elements can develop hypothesis on the potential of proxies. In contrast, once the techniques and methods for measuring a given proxy are standardised they can be readily delegated to people who do not have (interactional and/or contributory) expertise in paleoceanography. They will then be able to make standardised contribution to paleoceanographic research, but will not be able to make domain-language-based contributions.

Domain-Language-Based Contributions

Several tasks in paleoceanographic research rely on being able to speak the language of this community. It is necessary to have a theoretical understanding of the history of the Earth system, of its main mechanisms of change, and of the principles underpinning the different techniques used to reconstruct past climate to be able to make a range of informed judgements in a research project in paleoceanography. I call contributions of this type domain-language-based contributions.

The distinction between standardised contribution and domain-language-based contribution is relational. Processing a core is a standardised contribution to paleoceanography because it does not depend on speaking the language of paleoceanography to do it. However, as it has been pointed out above, there is a language, although a very simple language, about processing cores. If people processed cores just for the sake of doing it, in other words, if it were a goal in itself, then this would be a domain-language-based contribution. On the other hand, from the point of view of paleoceanography, processing cores is a standardised contribution. There are several other practices in this domain that depend on an in-depth understanding of the of the paleoceanography language to be competently carried out.

Domain-language-based contributions in science depend on an in-depth knowledge of the scientific literature. If scientific theories change, these judgements also change. For instance, paleoceanographers interpreting a climate record that they have generated have to be immersed in the relevant literature to be able to assess whether their data reveals anything new, controversial, or trivial. Following the literature, however, is not simply a matter of reading papers when they come out. As Collins and Evans (2007) pointed out a huge number of papers are published every year and many of them are never read by anyone other than the editors of the journals where they were published and by their reviewers. If one wants to make domain-language-based contributions one has to understand how the relevant community evaluates different papers. For this reason, it is essential to be immersed in the community of experts. If a layperson randomly finds scientific papers or theories on the internet and reads them, he or she might end up with a very misguided idea of what experts in a given field believe (e.g. Weinel 2007).

Weinel (2010) pointed out that scientists usually make two types of judgements when they assess the scientific literature: technical judgements and social judgements. The former relates to whether, from a technical point of view, a given paper is a relevant and sound piece of research. This includes judgements related to whether the scientific community believes the paper is the result of a well carried out research; whether the data presented are reliable; whether it is methodologically sound; whether it dialogues with the relevant literature; whether it has a significant contribution to the literature or not, and so on. Secondly, scientists also make judgements based on domain-specific discrimination (Weinel 2010, pp. 159-160), which means that they also evaluate papers on the basis of social criteria. This includes factors such as the status of the authors of the paper within the scientific community; whether the authors are regarded as good researchers or not; the prestige of the university where they work; how dense their networks within the community are, and so on (Collins 1975; Shapin 1994; Collins and Evans 2007, p. 50 footnote 10; Weinel 2010).

The stage of research in which domain-language-based contributions are most evident is during the interpretation of data. As pointed out in the previous chapter, data interpretation depends on a deep knowledge of the history of the Earth system, of its main mechanisms of change, of the principles underpinning the proxies used to generate data, and of the different data sets that could help interpret a given data set. For a scientist to have this knowledge it is essential to keep up with the cutting-edge scientific literature in paleoceanography. There are several other steps in research that depend on them, such as choosing a site to collect samples, choosing samples for analysis, choosing the appropriate techniques to address a research question, improving research techniques – as it has been exemplified above –, and so on.

In the case of choosing samples, for example, it is essential that researchers can identify the composition of sediments and link this composition to the state of the Earth system when the sediments were deposited. At this point expert judgements of similarity and difference have to be made. Researchers have to know the different types of sediments, such as white limestone, marlstones, black shales, etc., and be able to identify them. Distinguishing between types of sediments is an expertise that involves somatic and collective tacit knowledge. It involves somatic tacit knowledge in that the researcher's eyes have to be trained to identify the differences between the layers of sediments. It also involves collective tacit knowledge as it is necessary to apply a collectively shared system of classification of sediments which involves rule-following. The point here, however, goes beyond just classifying layers of sediments. Changes in the type of sediments indicate changes in the environment, which researchers have to take into account when sampling. These are crucial information and researchers can only link sediment types to the environment if they understand how the Earth system works.

For example, during an interview with a paleoceanographer with strong sedimentological skills she pointed out that sampling cannot be done by a person who does not understand how to link lithology to Earth system processes. This can lead to random data that do not capture signals that could be clearly found by a skilled researcher:

> Wendy: For instance this meeting I was at last week in Salamanca, one of the great criticisms that the Dutch group had, the Dutch group do a lot of the astrochronology, the time scale of the thing of the Mediterranean, very, very effectively. And there was an Italian group and one of the criticisms that the Dutch group has done is that they just take sample every 5 centimetres, whatever, they don't pay any attention to the lithology. And one of the things is that the Mediterranean is very sensitive to climate change because it's surrounded by land, actually it gives an amplified climate signal. And one of the results of that is that there's a lithological response to climate variation and it's absolutely amazing. You can correlate bed by bed right across the Mediterranean back to 10 million years ago. It's incredible. To find those cycles, they are obvious when you look at the cliff. But they do vary in thickness, because sedimentation rates changes, and because all sorts of things change. If you take samples every 5 centimetres with no bearing on those lithological cycles, then actually you may well not pick them up (laughter) because it's not that they are not there, but the periodicity within your data is kind of you haven't paid any attention to where the cycles are. And that's what the Italians were doing. And funnily enough they are in real trouble and doing all sorts of bizarre things. But they were not getting out cycles in a section where they were clearly there. So, I think in answer to your question if you do blind sampling you may end up with rubbish. You need to know about the samples you take. You need to understand the system. […]. You have to know what you're looking for or you don't see anything.

The point in this quotation is that it is essential to have an expert understanding of the phenomena that took place in the Mediterranean area to make informed judgements on how to select samples in this area. A consequence of this is that this task could not be delegated to a technician or to a first-year geology student who is not fluent in the language of paleoceanography and on how to link this linguistic understanding of how the Earth system works to changes in sedimentary rocks. In other words, this is a domain-language-based contribution (see appendix B for a more detailed example of the variables that affect sampling in the Mediterranean region).

Communication through the Domain Language

This distinction between standardised contributions and domain-language-based contribution is useful here because it reveals that it is not shared contributory expertise that mediates communication in paleoceanography. As pointed out above, frequently micropaleontologists and paleoceanographers are able, for example, to run routine analysis in mass spectrometers. This, however, does not give them very deep insights into the expertise of geochemists. This is because when it comes to geochemical techniques micropaleontologists can in general only make standardised contributions. What distinguish geochemists from other experts is not their skills in running routine analysis, but their expertise in developing new analytical techniques and new methods. Micropaleontologists and paleoceanographers cannot do this because this is not their area of expertise. They do not have a direct practical engagement with geochemists practical activities. What mediates interactions and data sharing between these communities is paleoceanography's domain language.

An alternative explanation could be that paleoceanography is an inter-language trading zone in which experts would communicate through an inter-language, be this a pidgin or a creole. This is not the case. Inter-languages are trade languages which are developed to mediate interactions between different social groups so that local trade is possible. As Galison (2010, p. 32) has pointed out trading zones are intersections between domains, where meaning is not fully shared:

> The key concept here is *incomplete* coordination. I hand you a salt shaker and in exchange you pass to me a statuette. We may agree to the trade — we do not in any sense have to agree to the ultimate use, signification, or even further exchange value of the objects given. The *only* thing we have to come to accord about is their exchangeability. While for me the statuette may be a religious object, for you it could be a purely aesthetic or functional one — on this we do *not* have to agree. We strip away meaning and memory when we pass the object to a trading zone.

Paleoceanography currently is a consolidated domain with its own language and a number of stable institutions that sustain it, such as conferences, journals, research groups, and so on[55]. It is not a domain where a trade language provides different experts with the possibility of communication without full mutual comprehension. The different types of contributory expertise that make up this domain have detailed understanding of

[55] Arguably paleoceanography might have emerged as a result of a trading zone between scientists with backgrounds in different Earth-science disciplines, such as geochemistry, micropaleontology, sedimentology, oceanography, and so on. Trading languages might evolve and become full-blown creoles, which are the language of new and autonomous domains (Collins et al. 2007). There is however no detailed history of this field written so that only further research will indicate whether this conjecture is plausible. However, this is not crucial information for the present work as I am focusing on the current state of paleoceanography and not on its history.

what other contributory experts do. This understanding is provided by paleoceanography's domain language.

I have already alluded to what the paleoceanography domain language is about in the previous section. It includes several elements:

- How the Earth system works, its mechanisms of change, and how these mechanisms could have brought about changes in past climates and in past oceans.
- The history of the Earth system and how its climate and its oceans have changed through time.
- The different techniques used to produce paleoceanographic data, including geochemical, micropaleontological, and others, such as sedimentological techniques. This includes a general understanding of what different research instruments do and of the principles behind the generation of paleoceanographic records. It also includes knowledge on the strengths and weaknesses of different techniques.

At the collective level these different elements of the paleoceanography language are intertwined. Conversations about processes that took place in the Earth system are dependent on the data sets and techniques that were used to produce knowledge on these processes. Similarly, paleoceanographic data only makes sense if they are related to wider Earth system processes. At the individual level one might find a great deal about the history of the Earth system in textbooks, for example, without any link being established between this information and the data production techniques used to reconstruct this history. Similarly, one might master techniques used in paleoceanography but deploy them to address questions in different research areas. For example, some geochemical techniques, such as radiocarbon dating, can be used by archaeologists to date prehistoric archaeological material in a context disconnected from paleoceanography. In paleoceanography, however, all scientists have to be fluent in all these aspects of this language to be able to make domain-language-based contributions.

As pointed out above, for geochemists to develop new analytical techniques it is necessary to have an in-depth knowledge of how changes in the Earth system might impact particular geochemical proxies. Similarly, micropaleontologists have to understand how changes in the Earth system are reflected in their samples. This does not mean that all scientists who contribute to paleoceanography have an in-depth understanding of the functioning of the whole Earth system, but that they have a general understanding of its main mechanisms of change in geological time scales and an in-depth understanding of those processes they are specialised in. It is during data interpretation, however, that the need to master all the different aspects of the paleoceanography language reaches its apex. Data interpretation relies on bringing together data produced with several different techniques. For scientists to interpret particular records they have to be able to make sense of other records that help them fit their own data into the big picture. To do so, they need to understand the limitations and strengths of each technique, the caveats involved in their application, where potential errors could be, the status attributed to particular techniques within the community, and so forth. This means that being able to speak the paleoceanography's domain language implies that each type of expert has to have interactional expertise in the techniques deployed by other experts. Some examples of this follow.

Micropaleontologists and paleoceanographers usually have no contributory expertise in improving geochemical techniques. However, they have interactional expertise in the production of geochemical data, which means that they have a linguistic understanding of the principles behind proxy measurements, the main weaknesses and strengths of particular techniques, and so on. This is essential for them to be able to integrate geochemical data into their own work[56]. The following quotation of an interview with a micropaleontologist specialised in foraminifera exemplifies this point:

[56] In paleoceanography scientists use 'raw' data produced by using techniques they do not have contributory expertise in to help them interpret their own data as well as data that has already been processed and calibrated. In both cases, it is necessary to have interactional expertise in how this data is produced. If they are using 'raw' data they will usually have to process and calibrate the data themselves, which is part of the contributory expertise in interpreting data. When they use data that is already processed and calibrated they have to understand the steps involved in doing so so that they understand the weaknesses and strengths of the data set.

Tiago: Are you also interested in geochemistry?

Sian: Yes, I have to understand geochemistry, interpretation level at least. I'm trained to understand and use routinely stable isotopes from foraminifera. I've got training for trace metals, but not very high level, I have to be honest. I know how to prepare samples and I can make sense of the numbers I'm getting, but I'm not an expert. I did some collaborative work with people working on organic geochemistry from sediments. But again this is not giving me such an incredible expertise. But most people I think in the field know about organic geochemistry or other proxies. I think it's quite common in paleoclimate now if you're specialising in one aspect you know quite a lot on all the other aspects of paleoclimate.

Sian points out that she does not have much contributory expertise in the generation of geochemical data. She only knows how to prepare samples for carrying out trace metals analysis (which is a standardised contribution to paleoceanography). She states however that she has to be able to interpret geochemical data. This is because, as pointed out above, to interpret any paleoceanographic data it is necessary to bring together data from several different proxies. This is a crucial point. In the case of geochemical data for scientists to be able to interpret them they do not have to know all the laboratory details behind the production of these data. They have to understand the principles that underpin the generation of geochemical data, such as what environmental phenomena affects the archive on which the data are produced, what the uncertainties and weaknesses of the data set are, and so on. The fact that Sian knows how to prepare samples for trace-metal analysis, however, is not really relevant for the purpose of interpreting trace-metal data as this is a standardised contribution to paleoceanography. To interpret these data it is necessary to have a linguistic understanding of what a geochemical proxy is and what it responds to. In other words, it is necessary to have interactional expertise in the proxies that will be integrated into the data interpretation.

Even scientists who have very little laboratory skills can make sense of geochemical data, which is further evidence that being involved with generating these data is not really essential for interpreting them. An example of this is a micropaleontologist specialised in nannofossils:

Tiago: How well informed are you about these other types of proxies, archives, etc., that you try to integrate with your work.

> Tina: I have to be really well informed. I have to be informed as to how you generate them. But I don't need to know necessarily the specific lab details. I have to know what their limitations are, what any potential errors there might be with them, and I need to know what is controlling their changes in order to be able to use them, if I want to use them, if they are reflecting temperature, or if they are reflecting salinity, or whatever. So, I have to know what the controls are. So, in that respect yes I have to be very well informed on those records that I'm using, yes. But I don't have the expertise to necessarily generate them myself. And I would also obviously talk to the people who have generated them to understand like if there's something slightly strange about them why that might be, if there's a kind of coupling why is that there, and kind of help get real sorts of specifics, so accuracy of the machine, errors, reproducibility and things that I need to get help with that and obviously ask for advice.
>
> Tiago: I'd guess that when you read a paper probably you've got uncertainty ranges, error bars, there some numbers there, but there's more to it than just these numbers.
>
> Tina: Exactly, that's part of why we are in a group, why we don't work remotely. A lot of it is that we do need to talk to each other. I go and talk to Richard about how he might generate an age model. But what I want to say is well actually how certain are you? Is that completely clear-cut? Or he might say well actually there's a little more uncertainty in this particular interval because of this. But I suppose that the point is while I understand what he gives me, I also understand probably the questions that I need to ask him in order for me to be able to use it. And obviously get information from him. That's why it's really necessary to kind of have these types of groups that are really diverse in their expertise. And talk.

The interviewee makes two interesting points in this quotation. Firstly, she does not understand all the laboratory details involved in generating data produced by other people that she uses. In other words, she does not even have the contributory expertise to make standardised contributions in paleoceanographic research such as producing geochemical data in the laboratory. However, she still has to be very well informed about the principles behind the generation of these data sets, the weaknesses and the strengths of the different techniques. She also commented on the importance of working in a group so that she could talk to colleagues who had contributory expertise in producing data using these other proxies. In this case, the transmission of knowledge would be linguistic as it would be through conversations and not through the practical acquisition of new skills. In other words, her interpretation of geochemical data is based on her interactional expertise in the principles behind the production of these data.

The same idea applies to the other contributory experts in paleoceanography. They also have contributory expertise in a narrow set of practices, but have an interactional expertise in other practices. An example of this is a geochemist specialised in Uranium-thorium series dating. He applies this technique to several archives, such as sedimentary cores, corals, speleothems, seawater, etc. to date them. He also keeps up with research related to other proxies and archives.

> Tiago: So, in terms of other proxies and archives that you don't work directly on them. Do you also try to keep up with the work that's being done on them?
>
> Matt: Yeah, you have to, really. And the way you do this is essentially by going to conferences. You pick up the idea that people are working on something at the conference and you then you look at the paper later on. It's really important to keep abreast of what's going on in other fields because when you then try to use other people's proxy records to help interpret yours then you really do need to understand all the caveats and problems with those other records because if you take them blindly at face value then your interpretation can potentially then just be wrong.

In other words, by going to conferences Matt has linguistic immersion in subcomunities of paleoceanography that generate data on proxies he has no contributory expertise in and by doing so he acquires interactional expertise in other proxy systems. In an email, I asked him for an example of a proxy that he was integrating with his own work that he did not work directly on:

> Matt: The most relevant example I can think of is the Mg/Ca paleothermometer. I do not work directly with this but we are putting together a paper which will compare our data to sea-surface temperatures inferred using this tool. We are producing a record of the intensity of past interglacials in Siberia, which is of great importance due to the potential of methane release from melting permafrost. We are comparing our record with data of tropical sea-surface temperatures. When doing this it is important for us to take account of the limitations and potential pitfalls of the Mg/Ca technique. Specifically the effect that carbonate ion and salinity have on the conversion of Mg/Ca measurements into paleotemperatures. I have been made aware of these caveats through conference presentations which have then directed me to read up on the relevant literature.

In sum, the different types of experts who contribute to paleoceanography do not have contributory expertise to produce records using all types of paleoceanographic archives and proxies. They specialise in certain archives and techniques. Micropaleontologists, for example, have contributory expertise in distinguishing between a large number of fossils. Geochemists and paleoceanographers, on the other hand, usually have a much more limited knowledge of microfossils’ species so that they cannot carry out micropaleontological work. In other words, they cannot make the whole range of judgements that a micropaleontologist can make when it comes to distinguishing between microfossils species and linking these species to particular types of environment. Even within micropaleontology there is a division of labour, so that micropaleontologists do not work on all groups of microorganisms. They usually specialise in particular groups. These different groups of experts, however, understand the weaknesses, the strengths, and the principles behind the several techniques that they do not master. Some techniques are so widely used, such as the measurement of stable isotopes in foraminifera shells that most, if not all members of the community understand linguistically the geochemical principles behind these techniques very well[57]. There is therefore a shared understanding of at least the most popular techniques within the paleoceanographic community and this understanding is part of paleoceanography's domain language.

Maintaining the Domain Language

For this domain language to be maintained it is necessary that the different groups of contributory experts that make up paleoceanography interact on a regular basis. This happens in a similar way to other expert communities. Collins (2011, p. 277), for instance, pointed out that members of the gravity waves physics community meet each other in several meetings, workshops, conferences where they talk about their most recent work. Furthermore, they also exchange emails, share data online, and participate in video conferences.

In paleoceanography, the situation is very similar. In most university departments I visited, the different types of contributory experts who contribute to this field had their offices in the same corridors or at least in the same building and would frequently talk to each other about their work. They also frequently write papers together. In paleoceanography scientists rarely publish single-authored papers. Labour is divided so that individuals with different types of contributory expertise can integrate their expertises to address research questions. This makes different types of contributory

[57] These proxies are so widely used because they are very well established and quickly applicable. They have a long history having been used in paleoceanography since about the middle of the 20th century (McCave and Elderfield 2011). Furthermore, oxygen isotopes can be used in association with other techniques to generate age models for sedimentary cores (Lisiecki and Raymo 2005), which makes them very useful for a wide range of experts.

experts work in close collaboration. They also meet in departmental research seminaries and in several conferences per year where they present their work to their peers. Furthermore, there is a great deal of informal activities in conferences, such as coffee breaks, dinners, where experts talk informally about their work. In all these situations they meet members of their research networks who are based in other universities and in other countries. It is therefore a routine activity for them to interact and to collaborate among themselves. All these interactions keep these scientists immersed in paleoceanography's domain language and help the new generations of experts to be socialised in the language of this community. They keep this domain language alive.

Chapter Summary

In this chapter I have argued that paleoceanography is a low-fractal-level domain, being composed of experts with different contributory expertise that interact to produce knowledge on past oceans and on past climates. I have pointed out that it has a rich technical language that mediates the communication between different types of contributory experts. To do so I drew a distinction between domain-language-based contributions and standardised contributions. This distinction is useful for two reasons. Firstly, the notion of domain-language-based contributions shows that although it might appear that in paleoceanography different contributory experts have a practical understanding of each other's research activities, this is not the case. Members of this field actually have interactional expertise in their peers' processes of production of data. Secondly, domain-language-based contributions depend on being fluent in the language of paleoceanography. Consequently, all scientists who are members of this community have to be fluent in this language. This gives more support to the idea that paleoceanography's domain language is the main mechanism of communication within this community.

This is not to say that there cannot be other mechanisms of communication at work, such as boundary objects, in paleoceanography. For instance, a mass spectrometer could be regarded as a boundary object in this field. For geochemists these are machines in which they can run sophisticated measurements and develop new analytical methods or techniques. They also use the data they produce to better understand paleoceanographic phenomena, but their main goal is to develop their techniques and to make better measurements. In the case of paleoceanographers, on the other hand, the meaning of these machines is different. They are used with a view to obtaining numbers useful for advancing paleoceanographic knowledge. Their use of them is much more instrumental. For this reason, paleoceanographers frequently hire technicians to run their samples. One of my interviewees drew this distinction very clearly:

> Ben: I think there's one thing that being an isotope geochemist is that we're very focused on data and data quality. And lots of the things we do for instance here is cutting edge, trying to do things differently. Measuring carbon isotopes and oxygen isotopes is not challenging. If I compare something that we do or something I do here [this geochemist works in a department with a strong geochemical orientation] compared to what [people in a department with a stronger paleoceanographic orientation do], there's a lot more focus on the analytical part. I think for some people doing that is just about sending the samples away or giving to a technician, getting the numbers. In my work there's a lot about being on the machine, trying to improve my measurements because it all that matters in the end for the work I do, that I can measure well, and it's not easy to measure, which means that you have to do a lot of development to get your measurement right. You can easily spend, in my PhD the first two and a half years I just used to develop and improve techniques of measuring to make it better than it was before because then you can answer your questions

Mass spectrometers can therefore be regarded as boundary objects in paleoceanography as different groups of experts attribute different meanings to them. Their having this characteristic certainly facilitates interactions between geochemists and paleoceanographers. However, paleoceanography's domain language is far more important for understanding communication in this context than boundary objects, which have much more relevance in situations in which heterogeneous expert communities know little about each other.

Chapter 6 – Collaboration within a Medium-Fractal-Level Domain: Fractionated Trading Zones and Interactional Expertise

In the previous chapter I have examined paleoceanography as an example of a low fractal level and argued that its domain language mediates communication in this field. In paleoceanography communication between different contributory experts usually runs smoothly. In this chapter I will examine a different configuration: a fractionated trading zone formed at a medium fractal level. At this level, the shared language spoken by all members of this domain is not as dense as in lower fractal levels, such as the domain language that mediates communication in paleoceanography. It does not enable different types of experts to have informed conversations without building bridges between their domains.

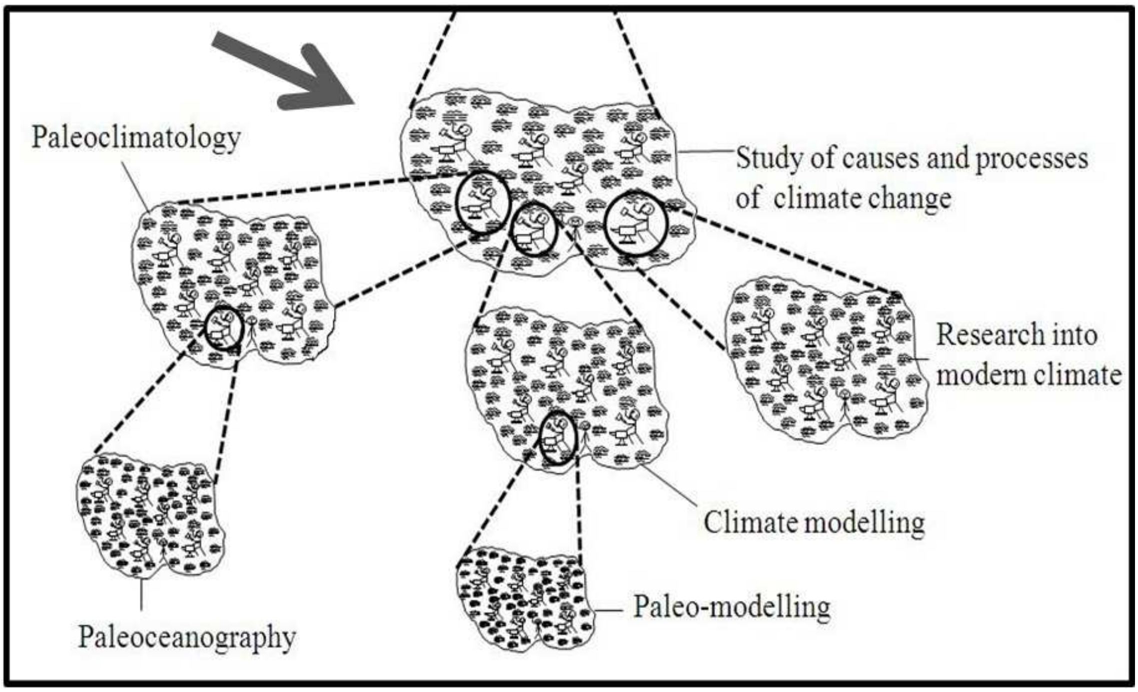

Figure 11: Paleo-modelling, paleoclimatology and paleoceanography in the fractal level. The arrow points to the fractal level that is examined in this chapter.

I will examine the interactions between paleo-modellers, i.e. computer modellers who apply their models to paleoclimatic research, and paleoclimatologists, with a particular focus on paleoceanographers (see figure 11) as during my fieldwork I concentrated my data collection on paleoceanography. Paleo-modellers, however, collaborate with members of different subspecialties of paleoclimatology, therefore much of the data collected on modellers are not only about their interactions with paleoceanographers but also about paleoclimatologists at large. The fact that most of my data on paleoclimatologists come from a study on paleoceanography does not represent a problem here. A significant number of the interviewees who contribute to paleoceanography also contribute to other subfields of paleoclimatology. Furthermore, most of the scientific meetings that I have attended during my fieldwork were not strictly in paleoceanography, but in paleoclimatology as whole. The social patterns found in paleoceanography when it comes to the interactions of the members of this field with modellers are very similar to those found in other sub-communities of paleoclimatology.

Paleo-modelling and paleoclimatology are at the same fractal level at a medium fractal level: the study of causes and processes of climate change. As I have pointed out in chapter 3, this is a very diverse field of investigation, being composed of several expert communities (e.g. atmospheric physicists, oceanographers, biologists, glaciologists, paleoclimatologists, computer modellers, etc). Although all experts at this level have a general understanding of how the Earth system works, the different specialisations related to focusing on particular subsystems of the Earth system, to particular time periods, and to particular research techniques, make the shared language spoken by all these communities too generic too afford informed communication between all of them.

Most members of the current generation of paleo-modellers have never produced data and most members of the current generation of paleoclimatologists have never done computer modelling. These different groups, therefore, have little or no contributory expertise in each other’s practices. There are exceptions to this as I met a small number of paleoceanographers who had run simple models to interpret the data they generated and in rare cases even Earth system models. But for the majority of the members of these communities there is no shared contributory expertise between them[58]. This

[58] Sundberg (2006, p. 56) pointed out that a similar social division of labour exists in the meteorology department in Sweden where she carried out her ethnography. In this department modellers and experimentalists rarely engage with each other’s research practices. PhD students have been striving to include modelling and data collection in their projects, but have faced difficulties to do so because of the

heterogeneity has given rise, as I will argue below, to some problems in communication. In this chapter I will argue that these communities have engaged in an effort to socialise each other mostly through the acquisition of interactional expertise so as to communicate more effectively.

Before I move on to the next section, I will make a terminological clarification. During my fieldwork it was common to hear from my respondents that there is a division in the paleoclimate community between modellers and 'data people'. According to them, data people are empirically-oriented scientists that produce data, i.e. paleoceanographers, geochemists, and micropaleontologists as well as members of other subfields of paleoclimatology, such as dedrochronologists, ice corers, and so on. Modellers, on the other hand, are scientists developing simulations of the climate system in computers, which bring together different data sets. In the remainder of this work, the term data people will be occasionally used to refer to the empirically-oriented scientists who contribute to paleoclimatology.

Paleo-Modelling

Paleo-modelling is a subfield of computer modelling composed of modellers who simulate paleoclimates to better understand processes of climate change in the past. This is a very small group of scientists compared to paleoceanographers and to modern and future climate-change modellers.

Paleo-modelling can be subdivided into subspecialties. Most paleo-modellers specialise in a specific type of model. There are different types of paleo-models including, for example, statistical models, box models, and GCMs. Statistical models correlate climatic variables to find statistical patterns in the way they interact. For instance, a statistical model could be set up to correlate temperature and sea level rise. Box models are also relatively simple models. They represent parts of the climate system as

large amount of time necessary to master both practices. As we will see below, the same holds true in paleoceanography.

rectangular 'boxes' and simulate simple interactions between those boxes. An example would be two boxes where one would represent the Atlantic Ocean and the other the Pacific Ocean. The two boxes would be interconnected and their exchanges of chemical elements would be simulated. There are also more complex types of box models that include several more boxes representing more parts of the Earth system. GCMs, as described in chapter 3, are tridimensional representations of the Earth. The planet is divided into tridimensional grids and physical interactions between energy, flows of gases, and liquids are simulated between them. Fully-coupled GCMs, which include representations of all parts of the climate system, are the most complex climate models available.

There are other types of specialisation which are relevant to understand the paleo-modelling community. There are modellers specialised in geochemical modelling and others specialised in climate modelling. In the context of paleo-modelling, geochemical modelling consists of simulating the flows of chemical elements between different reservoirs by using computer models. This can be done by using models with different levels of complexity, such as box models or intermediate-complexity Earth system models, which, similarly to GCMs, divide the Earth into grids, but have a lower resolution. Climate models, on the other hand, are models developed for simulating present and future climate change, such as GCMs, which are applied to past climates. Although some of them also simulate geochemical proxies, such as oxygen isotopes, they essentially simulate physical processes within the climate system.

Paleo-modellers tend to be very eclectic when it comes to time periods as well as to the events they model. Most of the paleo-modellers that I have interviewed were interested in a wide range of geological periods and had been involved in modelling a large number of events. Yet, individual scientists tend to specialise in certain phenomena. One of the scientists I interviewed, for instance, had done a great deal of work on biogeochemical cycles, particularly on the carbon cycle. Another one was very interested in the history of the cryosphere, i.e. how the amount and distribution of ice in the planet has varied through the Earth history.

Trade at Work: Collaboration between Data People and Paleo-Modellers

Paleo-modellers and data people currently have strong collaborative ties. They setup different types of collaborations depending on the goals of specific research projects. This is a collaborative trading zone.

Data people usually become interested in collaborating with modellers to test different hypotheses they have developed to interpret their data. They sometimes are not sure about which variables have triggered a climatic process. They might have several hypotheses and the data alone cannot provide answers as to which one is the most plausible. There are several feedbacks in the climate system and sometimes it is difficult to identify which variable was the forcing and which variables were the feedbacks of a given event. In these situations data people sometimes collaborate with modellers who simulate how the Earth system reacts to alterations in different climatic variables. The models then provide insights on the plausibility of particular hypothesis. Data people also collaborate with modellers to select where to collect data. In order to work out which areas are particularly sensitive to certain types of environmental changes they sometimes use models output to refine strategies for data collection.

Paleo-modellers are also interested in collaborating with data people. They need, for instance, to feed data into their models. In these cases, they collaborate with data people who review the literature and compile data for them. Paleo-modellers use these data in three ways. Firstly, they use data produced by paleoclimatologists as parameters in their models (see chapter 3 for an explanation on what model parameters are). Secondly, they also need data to set up the boundary conditions of their models. Boundary conditions are parts of the Earth system that a model cannot change during a simulation. Different models and different models runs require different types of boundary conditions. Some of this information comes from other subfields of geology, such as data on topography. Other types of information are provided by paleoceanographers or by other scientists working on other areas of paleoclimatology. This could be, for example, the concentration of CO_2 in the atmosphere or, in models that do not have a fully represented ocean, sea-surface temperature. Furthermore, modellers need data to validate their models[59]. Once they have finished setting the models up they run them and compare their output with data sets to check whether the model is producing reasonable results. If not, the models have to be adjusted.

Contributory-Expertise Heterogeneity and Communication between Data People and Paleo-Modellers

> T: Do you also do modelling, computer modelling?
>
> Dan: No, no, that's another skill. It's its own world. It's a very different career path. It's a different world.

As exemplified in the quotation above, during my fieldwork it was common to hear from my respondents that there is a division in the paleoclimatology community between modellers and 'data people'. Data people would be working closer to the empirical world producing and interpreting data whereas modellers would produce simulations of the Earth system.

The first point to be made is that it is not the use or non-use of models that differentiates these communities. Although it is true that data people have a stronger empirical orientation, their production of data also involves modelling steps. Geochemical proxies, for instance, are based on conceptual models of how chemical elements are exchanged between different reservoirs responding to climatic or other environmental changes. In this sense, scientists working on paleoclimatologic problems cannot be strictly divided into those who do modelling and those who do not do modelling. Most computer modellers and at least part of the scientists involved with data generation acknowledge this.

[59] Oreskes et al. (1994) pointed out that from a philosophical point of view models cannot be validated. See footnote 33 on page 94 for further information on this point.

There are other reasons that explain the heterogeneity between these expert cultures. Firstly, they use different research instruments and as a result the data or output they produce are not always compatible having different resolutions and coverage. This brings about the need for data to be processed before travelling between these communities (see chapter 3 for more on data processing). The most relevant point here however is the different contributory expertise of data people and modellers. Paleoclimatologists tend to be very involved with all steps of generation of data. Even if in different parts of their careers, most of them take part in fieldtrips, prepare samples, analyse them, interpret the data produced, and publish them. Paleo-modellers, on the other hand, rarely become involved with any step of data generation. Their expertise is in writing computer codes that represent climatic phenomena, running their models, assessing the results of models' runs, interpreting these results in the light of the literature, and debugging the models. In this sense, although there are different subspecialisations among 'data people', their contributory expertise is much more similar to each other than to paleo-modellers' contributory expertise.

Because of the heterogeneity between these communities there are some communication difficulties between these groups, which are currently being minimised through a process of mutual socialisation[60]. Paleo-modellers sometimes do not understand the uncertainties and the assumptions involved in the generation of paleoclimatologic data. Conversely, data people also acknowledge that they do not always understand all the assumptions and simplifications made in different models. The following quotation from an interview with a paleoceanographer is an example of this:

> Tiago: How about the modellers, do you think they are learning something from you or from the group here or just getting the data and putting into their models?
>
> Tom: Well, similar. They are dealing I guess with the same problems that we have that it is sometimes difficult to understand the amount of uncertainty in the other domain. So, basically because I don't understand really all the details of the model I get an output and it's hard for me to tell what the pitfalls of these results are. And for the modellers it's basically the same with our data. We give them sea-surface temperature, for example, and they could take that for granted

[60] Sundberg (2006) also found in her research in a department of meteorology that there are difficulties in the interactions between modellers and empirically-oriented meteorologists. She pointed out that there was a lack of communication between these groups (2006, p. 59). In paleoceanography, although there is a divide in the community between modellers and empirically-oriented scientists, there is a great deal of interaction between these groups and they appear to increasingly be able to communicate effectively.

> and just accept, ok, let's say twenty thousand years ago at that position temperature according to the data was 20 degrees. But we know of course that there's also a lot of uncertainties related to the gathering of that kind of data, the geochemical analysis, the whole set assumptions behind that. So, that I think is one of the difficulties.

As I will argue below, by and large, these communities have been trying to address these difficulties by socialising each other. Some issues still remain in their interactions, however. In some extreme cases some data people have altogether dismissed modelling as a legitimate research tool in cases where models have generated output that did not match their data. In the following quotation a paleoceanographer who collaborates closely with modellers provided an example of this:

> Wendy: There's a group in Leeds who also does paleoclimate stuff. And there's a quite large data, there are some modellers there too, but there's the data, pollen and vegetation mainly. And that was a very good example of this where the student had simulated the Miocene climate, she got hold of their data and done a comparison. And as far as they were concerned because the model didn't agree with their data, therefore the model was wrong. And actually what she was interested in was actually trying to establish how realistic the difference was. Was there really a difference between the data that they've got from the late Miocene sediments and the modern, or the pre-industrial or whatever. And my collaborator demonstrated really quite clearly that actually no there were these big errors on it and then therefore, and as far as they were concerned the model was wrong. So, that was a very clear example. And in fact what we've done and what we're doing as a response to that is, it became very heated, and people said thing that perhaps they shouldn't, is to actually split the paper. So, the paper will now be a paper mainly on quantifying the uncertainties associated with data with very little modelling in it at all. Ok? And then another paper that will be about the modelling which will use that data set. Because she perceived that it's easier to get agreement with data people on what the uncertainties are without demonstrating that the models can't reproduce it or whatever. Do you see what I mean? It's almost without the model it becomes less contentious. So, yes, there is your example. It really does exist and it's quite difficult. And it is simply, a lot of the comments that we got in the emails were things like why do you bother using this model when it clearly says crap (laughter). Kind of missing the point. But there's this thing, this simplistic view of a large part of the data community is that the models are there to generate the same pictures the data tell you. And actually that's categorically not what models are meant to do. Data is just part of the story, it's not the aim, it's not the focus. They are useful tools, but I think if you spend your life reconstructing ancient climate then you think, that is the end point.

Although there are still some difficulties in the interactions between some members of these communities, there are far more examples of them collaborating and valuing each other’s work. This is largely because paleo-modellers and data people have been working hard towards bridging the gaps between them through a process of mutual socialisation.

Paleo-Modellers and Interactional Expertise in Paleoceanography

To understand the bridges that have been built between these communities it is important to understand when and how their interactions began to take place. I have interviewed some paleo-modellers who pioneered this field in the UK. According to them, it was only in the 1980s that climate modellers became interested in modelling paleoclimates. They were then outsiders stepping on a different field of science and willing to contribute to it. I will not reconstruct the history of paleo-modelling here as this falls beyond the scope of this thesis and this could be the topic of a whole doctorate research. It is important however to point out that paleo-modelling emerged when computer modellers became interested in past climates. Most of them had a background in mathematical physics. To do their job they had to learn a great deal about the history of the Earth system, about the main mechanisms of change in the Earth system in geological time scales, and about the different data sets available on past climates. These were basic conditions for them to setup their models to simulate past climates. As pointed out above, these are the three main elements that constitute the language of paleoceanography. They had therefore to acquire interactional expertise in this field. As a result, there is an overlap between the languages of these communities. This mediates communication between them when they are talking about climate processes.

Although there is this overlap, there is heterogeneity between these communities as well. Most modellers have not acquired any contributory expertise in producing data nor have they become immersed in the literature on data generation. When they read the data literature they focus on new interpretations of paleoclimatic phenomena rather than on the details of data generation. The following quotation by a GCM paleo-modeller that I interviewed illustrates this point. He states very straightforwardly that he does not keep up with the literature on data generation. He tries to keep himself up to date with the literature on paleoclimatic processes and by going to conferences he keeps his knowledge on the major trends in data generation up to date:

> Tiago: Thinking about coming from physics and modelling the system, especially if you're working across different time periods and different time scales as well I would guess you would

> have to do a lot of work to catch up with the literature on the Earth system and what is known about the Earth system during the Cretaceous or whenever?
>
> Louis: Yes, that's right. As I said by focusing on the processes they are across periods, that's why I can sort of cope with that. The things I don't keep up with particularly on the literature is actually things like the actual data collection because obviously there are loads of data coming in from around the world in terms of all different aspects and keeping an eye on that. That's one of the things I do, I think it's quite natural for paleoclimate modellers. […]. One of the things I use conferences for and yesterday, the past two days was a good example, because by going to conferences you get a good synthesis of what's going on. And that's the only way I can cope because I can't follow every individual, I don't read every single data collection for the early Eocene. But what I do do is I go to conferences where I hear Jim Zachos summarise the data for the early Eocene and that's actually what I need because that's the only way I can work. It's this big-picture level. He showed some data points on a map yesterday. I know what proxies mean. I can tell you the strengths and weaknesses of those proxies. I could not tell you any special circumstances for each data point because I haven't in detail read the precise paper. I basically looked at the paper and said oh that's it, it's a TEX measurement of 26 degrees, and that's what I need. I didn't read all the details of saying whether there's anything special. I can't tell you the precise dating methods they use and things like that. Because you just have to work, you have to assume, it's true for all science in a way, you're working on the shoulders of giants in some senses. You have to assume that the people who collected the data knew what they were doing in some sense. And people who then incorporated into the bigger picture knew what they were doing.

This quotation exemplifies the point that most modellers have no contributory expertise in generating data and that usually they do not keep up with the literature on data production. For this reason, they are not able to make judgements on the quality of specific papers reporting on data produced on a given time period. They rely on data people who have the expertise to make these judgements to summarise the main trends in the data literature for them. This usually happens at conferences. By doing so, they keep their interactional expertise in paleoclimatology up to date, particularly their interactional expertise in processes of past climatic change. This means that they know what relevance is attributed by paleoclimatologists to particular arguments and theories put forward in the literature as well as they learn about the main trends revealed by the data sets available.

With regards to having interactional expertise in proxy systems used to generate data, i.e. knowing the principles behind them, there is a wider range of possibilities. I met modellers who were very confident about their understanding of paleo-data. Louis, in the quotation above, for example, states that he has a general understanding of the principles behind the different techniques used in data production, their weaknesses, and strengths. In an email he provided examples of his understanding of different proxy systems:

> Louis: I would say that I have knowledge that some proxies have a variety of different calibration curves and the reason for the differences (e.g. for TEX, for del18O for Mg/Ca, for CLAMP). In some cases I have my own ideas of the relative merits etc (derived from doing my own stats, etc). I also have an understanding of some of the processes underpinning, but even more importantly, I know when those processes are poorly understood, e.g. I know the detailed physics (I suspect better than at least a few geologists) of del^{18}O fractionation processes. I know the basic chemistry of why Mg/Ca is a temperature proxy. I know that there is only a hand-waving argument of why CLAMP works, and that the hand-waving argument also points to other issues (e.g. that CO2 concentrations may also influence CLAMP but this is not taken into account). I also know that the exact processes that control the emissions of the compounds used in TEX are not well know, partly because you cannot culture the bugs

.

Furthermore, his linguistic understanding of these paleo-data associated with his contributory expertise in modelling enable him to develop his own criticisms of particular proxy systems:

> Louis: I've touched upon this, but I will give you one detailed example. Leaf Margin Analysis (LMA) and CLAMP (Climate-Leaf Analysis Multivariate Program, which I had to look up because I'd forgotten what the acronym stood for!) are related techniques to get a temperature signal from analysing the structure of fossil leaves, specifically their shape. The former is simpler but only gives mean annual temperature, the latter gives more climate variables. In the literature, there is a lot of debate about the relative benefits of each approach. The strength of LMA is its very simplicity. The weakness of LAMP is that although it gives more variables (e.g. seasonal temperatures) it is harder to 'score' the fossil leaves and is more subjective. Moreover there is a lot of correlation between the climate variables and it is not clear if we are really producing accurate estimates.
>
> However, beyond the published debate, there are also some other issues which I have debated with the leaders in this area and we have not reached a satisfactory conclusion. The first issue is that there is only a hand-waving argument about the background biophysics and it is related to the structure of cells on the edge of a leaf. If this is correct, I would argue that the logical conclusion is that both methods should also include CO2 since it is known that the number of certain structures on a leaf (stomata) depends on CO2. This would mean that you need a different calibration curve for each co2. Nobody has yet given me evidence that this hypothesis is not correct. More interestingly, I can suggest a test of this by applying the method to more recent time periods (Last Glacial Maximum, 21000 years ago) BUT the experts have not taken me up on this challenge!!! I cannot explain why.
>
> A further problem is equally profound but unfortunately I have not had time to publish it. I got interested in the LMA issues and so worked with a social science statistician here at Bristol. What we found was that the data suggest that LMA (and CLAMP) are fundamentally flawed as applied presently because they don't include light levels (mainly dependent on latitude). What we found was that if you did a regression at different latitudes, even the slope of the line changed! This has potentially major consequences for the inferences.

The amount of interactional expertise different paleo-modellers have in paleoclimatic data is variable. Louis is very well informed about different proxy systems because he has been immersed in the data community for around twenty five years, talking with its members, and co-authoring papers with them. I also met a paleo-modeller who was even more deeply immersed in the data literature than the one quoted above. This paleo-modeller does intermediate-complexity biogeochemical modelling. This is significantly different from GCM modelling. His model has a smaller resolution than GCMs and poorer representations of certain parts of the Earth system, such as the atmosphere. He models geochemical exchanges between different parts of the Earth system. Whereas most GCMs include a few, if any, proxy systems, such as oxygen isotopes, his model includes a wide number of proxy systems. It can generate as output simulated sedimentary cores that would be found in certain areas of the ocean. As his modelling depends on understanding these geochemical processes, this modeller has a much deeper immersion in the data literature than most paleo-modellers:

> Jack: And so to do the paleo you have to build the model, you have to build the whole representational cycle from scratch. Often no one's understood properly the cycle. So, it's a lot of reading and then, so you end up understand or having to understand how the proxies formed because you've got to put basically all those processes into the model. So aside from the vital effects everything before the organism incorporates something you've got to often understand from scratch or by piecing together bits of the literature. So you do get a really in-depth, or I end up with a really in-depth view, hopefully an in-depth view just because I have to build the model and there's not an existing model around, existing models of ocean lithium cycling that builds for the future. So, it has to be bespoke and new and then you have to try and. No, you get really close to the data because often many of the things that, bits of knowledge in the literature about how the cycles for instance lithium in the oceans works is coming from the data community as they are trying to understand the proxies and doing experiments associated with that. So, you end up following very closely the proxy or the proxy relevant literature.

This modeller, however, is an exception rather than the rule in the paleo-modelling community. Several of my data-people interviewees pointed out to me that I should interview him because he was quite an exceptional scientist. His interactional expertise in proxy systems was far above the rest of the paleo-modelling community. In contrast, there are paleo-modellers who have not reached an in-depth understanding of data production in paleoclimatology. The following GCM modeller is an example of this. Like the other modellers above, he has no contributory expertise in data generation. However, his knowledge of proxy systems is not very deep as the knowledge of the modellers mentioned above:

Tiago: So, when did you really start learning about the paleo-data?

Roger: Not really until I started a postdoc here, which was, I knew a little bit through a postdoc that I had here on a project where the idea was to build an Earth system model. And so from that I learned a bit more about other components of the Earth system, like the ice sheets and the biogeochemistry, but still my focus was very much on the atmosphere and ocean, on the physical climate. But I picked up a little bit on the paleoclimate at that stage. But it wasn't really until I started a postdoc here about 6 or 7 years ago. I really had to learn about the paleo-data side. And that was because my project was very much then, that postdoc was very much on the paleoclimate side. It was basically it was a very broad project it was joint with the British Antarctica Survey and it was trying to understand the evolution of ice on the planet over the last 50 million years. So sort of icehouse to greenhouse transition. And then I started at that stage working very closely with the people at the British Antarctic Survey and people at the USGS in America and also people here in the Earth sciences department. And just from various conferences and meetings and things that is when I started really learning a lot about the paleo-data. Although I would say that if you've been to Urbino you probably know a lot more about the paleo-data than I do (laughter). I'd never been there and still my knowledge of paleo-data is somewhat lacking. It was basically this postdoc that started about 7 years ago jointly funded by the British Antarctica Survey when I really started getting my teeth into the data.

The point I am trying to make is that paleo-modellers have variable levels of interactional expertise in paleoclimatic proxies. This level depends on their particular research interests, i.e. what type of modelling they do and what type of phenomena they simulate, and on the depth of their immersion in the data community. There are, however, several efforts made by these communities to mutually socialise each other, as it will be argued below.

Data People and Interactional Expertise in Paleo-Modelling

Similarly to what has happened in the whole of climate-change science, computer modelling has grown in importance in paleoclimatology over the past two decades (see chapter 3 for more on this). Data people have also had to learn about modelling to be able to collaborate with paleo-modellers. Their work was facilitated by the fact that modellers were already acquiring interactional expertise in their domain so that both communities were equally well informed about mechanisms of climate change in geological time scales and about the history of the Earth. They had therefore to acquire interactional expertise in computer modelling, particularly in the models used to simulate paleoclimates. Different members of the community acquired different levels of interactional expertise as different researchers have been more or less involved in collaboration with modellers. But it is a general trend that data people have increasingly been learning about paleo-modelling.

The following quotation is an example of this. A paleoceanographer points out that he has no contributory expertise in modelling, but he still can bridge the gaps between their expertises through language:

> Mark: I suppose a good example of that would be when I work with climate modellers. I couldn't run a climate model simulation and I couldn't interpret the code. I'm not a very good computer programmer because I haven't done much computer programming. So, I couldn't start to reprogram a climate model or understand exactly why a climate model is producing some particular result. But I can ask the modellers why is it that your climate model has such and such feature and they can usually explain in plain English why that would be the case. So, I was saying it's not about limited understanding but it's about limited expertise.

The following paleoceanographer make a similar point:

> Tiago: So, when you collaborate with modellers to which extent do you try to be well informed about the codes?
>
> Robert: Not the codes because I don't have time to be able to go into the code and identify sub-routines that relate to coupling of ice sheets to Antarctica temperature or something. There's no time to do that. But what I seek to understand is, I ask lots of questions to modellers because I want to know what they've parameterised, what are the weaknesses, what are the strengths, what are the things that we may need to carry out a sensitivity test on them. What is the physical-evidence base to support the way in which the model's been built.

The following quotation is from an interview with a paleoceanographer who worked in a multidisciplinary project with paleo-modellers, physical oceanographers, and climate modellers, to better understand ocean circulation in an area in the southeast of the African continent and its coupling with the rest of the climate system. When I asked him about his collaboration with modellers he pointed out that he also did not know much about the codes and equations underpinning climate models. However, he was well informed about the strengths and weakness of different models. He also pointed out that communication between him and modellers was not problematic because they all knew about the processes they were researching:

> Tiago: I'm particularly interested in this collaboration with modellers because I know that nowadays modelling is a big thing. In a way in climate science everybody either depend on modellers or wants to work with them. In this specific collaborations how well informed are you about the setup of the models and how well informed do you think the modellers are about your own work?
>
> Nick: […] How well do I know the model? Ok, that's an ongoing process because we're trying something new. It's a learning curve for both sides. So I'd say at the moment not particularly well. I understand the basics and understand the different modules within the computation schemes to a broad extent. And understand the capabilities of the model and the weaknesses and the strengths of the model. And I can understand the approach that we're taking and we're only getting involved in those sorts of discussions. But if you ask me to go in and say look there's some dissipation factor that's wrong and you need to change the coding, then that's not what I do. And equally from the modelling side, the modellers who we are working with would certainly understand the proxies in a conceptual way. So, as I've just described carbon isotopes they would be able to tell you the same thing. If they could pick forams and understand how to run a stable isotope mass spectrometer? No. But equally they would understand the uncertainties around the proxies, which are important things for the modellers. You don't need to know the full details of each other disciplines to be able to effectively collaborate.
>
> Tiago: And this learning process, how are you going about it? Just by talking and reading?
>
> Nick: In essence, yeah, to a large extent certainly talking and reading. *Don't forget that because it's process based we all understand the processes that we're trying to tackle.* That's a common ground for us to talk about. The problems and then we come at it from our different disciplines, which makes it easier. So, yes we're reading and talking, but equally we have workshops, we have summer schools within this project for example. The last summer school we had, we had all of the PIs, myself, the modellers. So, we gave an overview of the, in my case the proxy world, and people were talking about the modern physical oceanography. Yes, basically all of these different kinds of, the modelling world, as an entry level kind of, yes, this is what the model configuration is and this is what it can do, and so forth. And I presented, for example, all of the proxies in ocean circulation, what the theoretical basis is for those proxies, the weaknesses, the strengths, etc., etc. So, we do things like that (emphasis added).

Data people, therefore, usually have no contributory expertise in writing the codes and the equations necessary for the setup of paleo-models. Most of them, however, understand the main strengths and weaknesses of different types of models or seek to learn about them when collaborating with modellers through talking to them. This understanding most of the time is linguistic as data people usually do not run models themselves. Interactional expertise, therefore, mediates their collaborations. As I pointed out above, the level of interactional expertise is variable as data people might be more or less involved in collaborations with paleo-modellers.

There is another element that mediates these interactions. Data people can make judgements related to how effectively the models simulate the climate system because they understand the processes modellers simulate and can therefore assess how accurate the simulations are[61]. This is because originally this was part of the language of their community, i.e. the mechanisms of change in the Earth system and how they have affected paleoclimates over the Earth history. In this sense, the fact that paleo-modellers had to acquire interactional expertise in paleoclimatology also made it easier for data people to interact with them.

.

Developing a Trading Zone: Mutual Socialisation at the Community Level

There are different efforts made by paleo-modellers and data people towards developing interactional expertise in each other's domain, i.e. to improve mutual understanding within this trading zone. At the community level, there is an effort to educate the new generation of paleo-modellers and paleoclimatologists so that they become well informed about each other's fields. In the quotations in the previous sections there are some indications of this. Nick mentioned that he organises summer schools for the participants of the interdisciplinary project he works on and in these meetings different experts provide each other with an overview of their own fields so as to improve their understanding of each other's work. This socialisation is linguistic as it does not involve any practical activities. It can only provide them with interactional expertise in each other's field.

---

[61] Lahsen (2005) carried out research on the interactions between climate modellers and atmospheric scientists and discussed a similar point. She argued that atmospheric scientists might be better equipped to criticise GCMs than modellers themselves. This would be for two reasons. Firstly, because GCMs have become so complex that modellers can barely spend any time learning about empirical research on the atmosphere. They concentrate most of their reading time on modelling papers. Secondly, because of the emotional investment of modellers on their own creations, they would be reluctant to openly criticise the models they develop. The latter point, although interesting from a STS viewpoint is not relevant for a discussion on expertise, so I will not examine it here. The former point, on the other hand, brings to light an interesting point: empirically-oriented scientists might also be able to make judgements on models outputs. In atmospheric physics, Lahsen argues that this is because they know more about the empirical atmosphere than modellers. The paleo-modellers that I interviewed, however, were very well informed about the empirical phenomena they simulated. Even if they did not have time to follow the entire data-production literature, as most data people also do not have, modellers still know about the literature on the phenomena they simulate. There is however one type of judgement that data people might be in an advantageous position to make because of their contributory expertise: those related to the quality of the data that is used as input in models. Whenever paleo-modellers use data from a particular subspecialty of paleoceanography, members of this subspecialty will be in a better position to assess the quality of the data used as input. This is one of the reasons why paleo-modellers collaborate with data people so that the data compilation for a particular project is done by those who are more deeply immersed in the data generation literature and therefore more aware of the shortcomings of specific data sets.

The Urbino Summer School in Paleoclimatology, which I attended, was also designed with the goal of socialising the new generation of paleoclimatologists and paleo-modellers in both areas of investigation. In this event around thirty experts with different contributory expertise, including paleoceanographers, micropaleontologists, geochemists, paleo-modellers, working on the whole range of geological time intervals and using a variety of techniques, lectured for three weeks graduate students with backgrounds in data generation and in paleo-modelling. As a result, these young researchers receive basic training in areas of expertise which are not theirs. Furthermore, there is great deal of informal socialisation in this event, as students and faculty frequently go out together for dinner and for drinks. The lectures and the informal socialisation result in the students acquiring some level of interactional expertise in the most important topics of research in paleoclimatology and paleo-modelling. It is not possible to provide a precise measure of how much interactional expertise is acquired there as this depends on the background of each student, on how seriously they take the lectures, and on how frequently they engage in informal conversations about science with faculty members and with other students. As the summer school lasts three weeks, however, it is not expected that they will in this period become full-blow interactional experts in all subfields of paleoclimatology as this would need a much longer immersion in all these communities. But it provides them with at least low levels of interactional expertise in most subareas of paleoclimatology[62].

Some universities have also developed courses where students have training in paleo-modelling and paleoclimatology. These initiatives were deliberate and reflected a collective sense that interactions between the modelling community and paleoclimatologists could be improved. A geochemist who applies his expertise to address paleoclimatologic problems told me about a joint Masters programme where students are trained in modelling and in collecting data. The initial motivation for setting up this course was a frustration caused by difficulties in communication between modellers and data people:

[62] During the Summer School there are also a few practical activities, such as a field trip, in which all participants make measurements and write the log of an outcrop, and some exercises such as filling out spreadsheets to develop age models for sedimentary cores and solving geochemical equations. These activities, however, are very short and take much less time in the Summer School if compared to lectures so that they are not enough for anyone to become fully-fledged contributory or interactional expertise in any of these activities.

Tiago: And how are these new collaborations going on?

Tim: They're good. I find that there's sometimes a slight communication problem so I decided to do something about this. We have a Masters programme here in Earth system science, which I've just taken over and we just started really. And the philosophy of this Masters course is to produce people who will hopefully go on to a PhD who have a basic training, it can only be a basic training in both modelling and the observational side of science. And the main reason I'm interested in doing this is that I find that there's sometimes gaps in understanding on both sides that lead to problems. So, in order for a modeller like Bruce to model, he models neodymium isotopes in the ocean, he needs to understand how the basic chemistry works, how it all works. Anyone who does that needs to understand. Many modellers do and some don't actually. The relationships are relatively easy to start and build but also require some effort in educating each other. Because I'm also ignorant about what exactly a model can do very often I find. I call them up and say let's do this and usually you can't do that, that would cost years of computing time.

Tiago: How do you think this mutual education would work in practical terms?

Tim: It just works by conversations in the corridor, going to seminars, I go to modelling seminars, Bruce goes to data seminars. As I say, teaching this course together, so there's some people, modellers in [the Department of] Geography, me, Bruce, some other observational people, we all teach it together. I think that has helped us learn a bit about exactly what we all do from day to day. Hopefully the products if they go into academia will be better equipped to, will have a better basic training in both sides of the field that will help them to have a better mutual understanding.

In this quotation, there are two important elements. Firstly, the effort of the community to train a new generation of experts that will be better informed about modelling and data production. The students receiving training in both areas is a deliberate attempt of the community to intensify the links between these fields so that this trading zone works more effectively. Again, this is not going to make any of the students a full-blown contributory expert in all these practices. As Tim points out in the quotation, they receive only a *basic* training in modelling and data production. Secondly, individual efforts made by modellers and 'data people' to improve their understanding of each other's fields. I will first elaborate on the former and then move on to the latter.

Besides Masters courses where young scientists are trained in both fields, there are also students carrying out doctoral research that includes modelling and data generation. However, most people do not become experts in both fields. Their research usually is in one of these fields and has also a component of the other so that the student acquires a general understanding of the other area. Modellers might, for instance, compile data to put in their models. They might also go to the field with a supervisor who has expertise in collecting data. They will then collect samples and generate data on them. However, as there is too much specialisation involved in becoming full-blown data generator, they usually do it under the supervision of their supervisors and do not become fully accomplished in the 'data side' at the end of their PhDs. A paleoceanographer who is very skilled in sedimentology described to me how she co-supervised PhD students with climate modellers:

> Tiago: So, you've got some students that you're supervising along with modellers here. So, how is this process of transmitting these skills and probably combining these two skill sets?
>
> Karin: What I have that my modelling colleagues don't have in fact is field skills. I actually worked as a professional field geologist for five years. So, I know an awful lot about interpreting rocks and sediments in the field. I have taken three of my current set of students out into the field and taught them what it is that you need to look for, how you log, how you map, how you take samples, trying to give them exposure to the rocks that actually they do their analysis on, or the sediments from which their data are drawn whether they are published or whatever. So, I do quite a lot of that. I try and teach that as much as I possibly can in the field. So, if they got to go and collect samples in China I go too. If they got to go to collect samples in Morocco I go too. […]. I have a commitment to make sure my students, and simply that they couldn't do it without me there because they simply don't know how. They wouldn't know, just wouldn't be able to start because most of them, have I got any student who has an Earth sciences background? I've got one who has an Earth sciences background. The one who I went to Morocco is a geographer, the one who I went to China is a chemist. The one I'm doing with George actually has a degree in GIS I think. These are students who come from very very varied backgrounds but the thing they tend not to have is the geology and so I do that.
>
> Tiago: And is it a bit like an apprenticeship?
>
> Karin: Yes, none of them do, because if they actually had a field-based PhD I would have to take on someone with an Earth sciences background. So, none of them have a huge component of fieldwork in their projects. So, actually what you're doing mostly is you're training them, but in the time available they will never be competent to do the job, which sounds a bit snide, but that's just the way it is. They are never going to be field geologists. But they need to understand how the field data side is done. For instance the one I went to Morocco with has actually given a talk to the group. What she wanted to do was to write a talk that was specifically about how the fieldwork was done because she had never done it and she was quite right in thinking that actually there's nobody else in the department who knows how it's done either. So, she's done things like, she's taken a picture, a photograph, of a section, and she then merges my sketch of it to show what it is that I'm picking out of that. And then she takes a picture of a logged section and then shows my log alongside and correlates it across. So, this is actually an opportunity to show people just how precise you have to be at looking in the field. So, is it an apprenticeship? No, because they don't actually get to be proper field geologists. I may well have some more field-based geology students in the future. So, those would be. I've had one in the past who was a geologist and who did a geological, sort of a sedimentology PhD.

There are two central points in this quotation. Firstly, she points out that modellers usually do not have the skills to do fieldwork. Furthermore, she argues that PhD students whose research focuses on modelling usually do not become full-blown experts in field geology. They acquire a general understanding of what field geologists do, but not enough to go to the field by themselves and collect samples for future projects. The type of expertise they acquire by doing this is certainly not any of the low levels described in the Periodic Table of Expertises (Collins and Evans 2007), those that involve only ubiquitous tacit knowledge (i.e. beer-mat knowledge, popular understanding of science, or primary source knowledge). This is because they are actually having linguistic and practical immersion in the practice of generating data. However, this immersion is not enough for them to become fully-fledged experts. They acquire, therefore, very low levels of contributory expertise in generating data by using certain techniques. This might help bridging the gaps between these two domains, but because this is only a limited contributory expertise, it is also necessary a great deal of interactional expertise on top of it, to effectively iron out potential issues in communication and in secondary-data use.

Developing a Trading Zone: Socialisation at the Individual Level

At the level of specific collaborations between scientists, especially when it comes to more senior researchers who have not been trained in the other domain, the gaps between paleo-modellers and data people are bridged through reading the literature and through talking. Reading the literature is an important part of this process as by doing so scientists can learn about the main techniques and the main trends in both fields. However, reading the literature alone is not enough for being socialised in a domain as it only provides people with primary source knowledge (Collins and Evans 2007). It is necessary to have a sense of what papers are regarded as the most relevant, what papers are outdated, what techniques have been improved, etc. A large part of this mutual education is done by talking, which provides scientists with interactional expertise. As Tim exemplifies in the quotation on page 163 this happens through informal 'conversations in the corridor' and through going to seminars where they are linguistically socialised in the field they do not have contributory expertise in. In the quotation on page 155 Louis makes the same point. He is a senior modeller and he does not have enough time to follow the entire literature on data generation. He tries to keep himself up to date with the literature on the climatic processes he is interested in by going to conferences where paleoceanographers and other data people present data compilations on specific phenomena and time periods. I have attended two conferences and a research seminar in which paleo-modellers and data people were presenting papers in the same room, asking each other questions after the presentations, and chatting in coffee breaks. These events are important occasions for scientists from these fields to acquire or to keep their interactional expertise in each other's domain up to date.

The same happens in summer schools. For example, during an informal conversation with a professor of micropaleontology about the Urbino Summer School in Paleoclimatology he said that this event was very productive for faculty because there they could spend a great deal of time with other members of the community talking informally. He also pointed out that many papers emerged out of these informal chats. I checked the publication list of some of some of the academics that teach in Urbino and found that there are many collaborations between scientists who teach there, including several cross-domain collaborations. These included a large number of papers co-authored by modellers and data people. This is also evidence that there is a great deal of mutual socialisation and 'trade' between members of these communities taking place in this event.

In specific projects, scientists also need to acquire interactional expertise in practices they are not involved with. Modellers might need a deeper understanding of the data they are working on in terms of their uncertainties, caveats, etc. Paleoclimatologists, on the other hand, might need to better understand the models that their collaborators use in terms of their setup so that they know what their resolution is; what the main assumptions underlying them are; what climatic processes are effectively being modelled and what processes are not; and so on. This is necessary for them to be able to interpret the output of models and link them to their understanding of the Earth system. The following quotation from an interview with a paleoceanographer exemplifies this point:

> Kate: Yes, I know that models have got lots of uncertainties themselves and they make a lot of assumptions. And unless you are in that field you just don't know what they are. And I'm learning at the moment, because I just started this new collaboration with modellers now, I'm kind of learning where some of this assumptions are. You could easily have a career in

> paleoclimatology and not understand all the assumptions that go into climate models, because it's quite a distinct field.
>
> Tiago: You said that you are learning about climate models now. How are you going about this process of learning?
>
> Kate: Just by talking to the modellers themselves and learning what they need as a kind of input parameters into the model, so how they estimate what vegetation was like 40 million ago, how they estimate what latitudinal temperature gradients were and.. There are so many different types of climate models and some of them you prescribe a lot of variables right there and then others the model is so complicated that you just fix a few variables at the beginning and then the model itself predicts things like vegetation and what have you. It's different kinds of plug-in components you can put in. But when you read the modelling literature unless you are in the field it's difficult at first sight to know how much exactly of the output of the climate model are totally free and how much has been driven partly by what they assume or what they prescribe at the beginning. So, those are the kind of things that I'm learning which of course is essentially an understanding of how good those predictions are.

Kate points out that she is learning about climate models and what the assumptions underlying them are. This learning process is linguistic as she is not being trained in running models. She will not directly take part in climate modelling, but she is still seeking to understand the model that her collaborator uses so that she can provide him with data and also be able to make sense of the outputs of the model's run. She is therefore acquiring interactional expertise in the modelling techniques used by her collaborators so that she can effectively collaborate with them.

Developing a Trading Zone: Ambassadors

Another way of bridging the gaps in trading zones, such as the one between paleo-modellers and data people, is through ambassadors (Ribeiro 2007c; Collins 2011; Reyes-Galindo 2011). According to Collins (2011), in physics, when one group of scientists depends on knowledge produced by another group that they do not speak the language of, sometimes one of its members is sent to spend some time immersed in the other group's language. As a result, he or she becomes a special interactional expert in the other domain. This person becomes an ambassador who can help his or her original group by answering technical questions and queries related to the other domain in which he or she acquired special interactional expertise:

> Sometimes interactional expertise can also be used to bridge middle-level practices. For example, GW [Gravitational Waves] detection involves a search for correlations with

> electromagnetic signals such as might be seen by astronomers watching the explosions of stars, so a bridge between GW physics and astronomy is needed. This is not a trivial matter, as Imitation Game experiments have shown – such groups of physicists do not speak each other's practice languages. The solution is to delegate particular individuals belonging to the GW physics-practice to learn some astronomy practice language, to gain interactional expertise, and to form bridges with different kinds of astronomer: one bridge for those investigating x-ray emissions, one for visible light emissions, one for neutrino bursts, and so on. Each delegate has to become a *special interactional expert* with respect to the community to which he or she is to build a bridge. The delegated individuals, in so far as they succeed, can then answer technical questions and queries from GW physicists on behalf of, say, x-ray astronomers, without always referring back to those astronomers – this is how one detail of the technical cooperation between these middle-level practices is made possible (Collins 2011, pp. 287-288).

In my fieldwork in paleoceanography I did not meet any ambassador who had only interactional expertise in another subspecialty to help his or her group. I met however ambassadors who were bridging the gaps between paleo-modellers and data people with their contributory expertise. For instance, a department composed of several paleo-modellers hired two data people to help them carry out model-data comparison. One of them was specialised in deep time, whereas the other worked on the Quaternary, which is the period spanning approximately the last 2.5 million years. The following quotation is from one of the paleoceanographers who was an ambassador at this paleo-modelling department:

> Tiago: I had a look at your profile on the university website and I wasn't quite sure if you're a data person, if you like, or a modeller, or a person who's doing both.
>
> Karin: I don't do any modelling. I do generate data. But quite a lot of the work since I've joined this is about data modelling comparison and is trying to bridge that gap.
>
> […].
>
> Karin: As I said when I moved here I was brought in to be the kind of deep time data person for this modelling group. And I have therefore done some work with them on things like, yes, model-data comparison, and compiling data sets so that they can test their models in a meaningful way, trying to incorporate elements of data generation into models in order to make that comparison more robust.

In this case, a scientist with contributory expertise in generating paleoceanographic data on deep time was brought to work in a paleo-modelling group to help the modellers bridge the gaps between data production and modelling. She had spent a great deal of time immersed in this group of modellers. She did not acquire contributory expertise in modelling, but could communicate really well with her colleagues:

> Karin: I don't really have problems communicating with modellers because actually I understand what it is they're after. I sometimes I don't think they scrutinise the data hardly enough. But that's not the same thing as not understanding what they are after.
>
> […]
>
> Karin: The thing is that I actually do, I'm at that interface, and I'm pretty unusual in that respect because my data gets put into models and is used by modellers. I don't think that many people are on that interface, but I am.

In other words, instead of the paleo-modellers sending one of them to acquire special interactional expertise in some subspecialty of paleoclimatology, they hired a data person who ended up acquiring some degree of special interactional expertise in modelling. Lisa, in turn, could help the modellers with her contributory expertise and help deepen the process of mutual linguistic socialisation between these communities.

Another example of an ambassador that I found in paleoceanography was more similar to the example provided by Collins, although it involves the acquisition of some practical skills. This example consists of a paleoceanographer who was part of a paleoceanography group working in an interdisciplinary collaborative project with experts from different fields, including paleo-modellers. One of the goals of the project was to analyse the output of computer models and compare it with the paleoceanographic data available. Analysing the output of models, however, is not a trivial task. Models produce huge amounts of data and it is necessary to learn how to find the 'right' data. To do so, a paleoceanographer who was part of this collaborative project was sent to acquire contributory expertise in this particular task. He spent a few days among modellers learning how to do this and then returned to his own group, where he used the newly acquired skills:

> Tiago: So, it's more of a cooperative kind of interaction. You don't have to learn loads about modelling to be involved in this.
>
> Tom: Not really the practical science. In terms of the practical science it's really only this post-processing. So, you have basically rows of numbers and numbers and you need to learn how to extract the ones that you're really interested in, which is sometimes not very straightforward. The amount of data is so large that you can't just do it simply in excel or something. It's just no boundaries.

> Tiago: How did you go about learning what to do with these data?
>
> Tom: That was one purpose of those visits I did recently to Germany. Certain software that you need to learn is basically command based. It is not very user friendly. I had to learn that kind of language that extract the data that you want to look at.
>
> Tiago: So, then you went there, you learned how to use the software and now you're more comfortable.
>
> Tom: Yeah, yeah, so now via the internet basically I have access to certain data platforms there. And I can from here now play with that, play with their output.

This paleoceanographer, however, was not becoming a full-blown contributory expert in modelling, but only in how to process model's output:

> Tiago: So, you're basic learning how to interpret the data, but you're not trying to become well informed about how they set up the models?
>
> Tom: Well you try to get as deep as possible but there's a certain limit, I'd say. Especially in the technical side, they have their own programming languages which I don't know. I'm not going to the code of the model and really read it and try to understand the details.

This is a different type of ambassador. It consists of an individual who spend some time with another groups and acquire contributory expertise in a very narrow skill-set, but which is very helpful for his own research group. This also helps bridge the gaps between these communities. It does not integrate them further on the community level, but it is a type of link that may enable individual research groups to interact more effectively.

Chapter Summary

In this chapter I have examined how two heterogeneous expert communities – paleoclimatologists and paleo-modellers – that are at the same fractal level at a medium fractal level build bridges between themselves. At medium fractal levels, language is not as specialised and technical as in low fractal levels. There is no domain language rich in technical details spoken by all experts that might facilitate communication. As a result, communication issues between expert communities might emerge. For collaborative research projects to emerge it is therefore necessary to creation trading zones. In the case of data people and paleo-modellers, they have developed a fractionated trading zone as these domains are heterogeneous among themselves and there is no new inter-language emerging out of their interactions. As I have argued above (see the introduction of this thesis), there are two types of fractionated trading zones: those based on boundary objects and those based on interactional expertise. In the case of paleoclimatology and paleo-modelling, interactional expertise is the main facilitator in the interaction between these communities. Data people and paleo-modellers usually do not have contributory expertise in each other's techniques. Paleo-modellers, on the one hand, in general do not master the processes involved with data production and do not follow the data-generation literature. For this reason, they do not know all the shortcomings and uncertainties of specific data sets. Paleoclimatologists, on the other hand, usually do not have an in-depth knowledge of paleo-models setup. They rarely have contributory expertise in the codes and algorithms used by modellers. Their interactions are mediated by, on the one hand, paleo-modellers having acquired interactional expertise in the history of the Earth system and in its mechanisms of change; and, on the other hand, by members of both groups having acquired some degree of interactional expertise in each other's techniques. Data people usually have a general understanding of what the different types of models are and of their strengths and weaknesses. Similarly, paleo-modellers have a general understanding of the main paleoclimatological proxies and of the chemical principles underpinning them. As I have argued towards the end of this chapter, this interactional expertise is acquired and kept up to date through the attendance of scientific meetings (e.g. conferences, seminars, summer schools), and through conversations with collaborators.

I have also pointed out that these communities are working towards becoming more integrated, i.e. developing mechanisms to make communication more effective within this trading zone. They are training the new generation of scientists to have a good understanding of modelling and of data generation. To do so, they have been deliberately making an effort to offer courses, such as Masters courses, or summer schools, in which the new generation of scientists that contribute to paleoclimatological research receives a basic training in data generation and in modelling. In these courses, students acquire or update their interactional expertise in the domain which is not their own.

Members of the paleoclimatology and of the modelling community also jointly supervise PhD students so that young researchers receiving training in data generation and in modelling. These students, however, are not becoming experts in both fields. Rather, they acquire some very low levels of contributory expertise in the field which is not their own. This contributory expertise is not enough to mediate communication, therefore it necessary that these students also acquire a great deal of interactional expertise to be able to communicate effectively with members of the other domain.

Finally, I have also described the role of ambassadors, i.e. individual scientists who build up links between different expert communities. I provided two examples of ambassadors in this chapter. I described the case of a data person who had been hired by a modelling group to help them with her contributory expertise in data generation and with her knowledge of the data literature. Her being in the same department of these modellers resulted in her acquiring a great deal of interactional expertise in modelling, which puts her in a privileged position to collaborate and communicate with this other group of experts. I also described the case of a paleoceanographer who had visited a modelling group and acquired some skills that his group did not have and that would strengthen their collaboration with the modellers. There are also other types of ambassadors already described in the STS literature such as those who bridge the gaps between two domains through special interactional expertise (e.g. Ribeiro 2007c; Collins 2011; Reyes-Galindo 2011). Although I have not met any of them, it is possible that this type of ambassadors also exists in paleoceanography and in other areas of climate-change science.

Chapter 7 - Expertise, Trust, and the Fractal Model

> Large segments of the contemporary social world have become opaque for their members. […]. More often than not we have to act in the dark, as if facing a huge black box, on the proper functioning of which our needs and interests increasingly depend. Trust becomes an indispensable strategy to deal with the opaqueness of our social environment. Without trust we would be unable to act (Sztompka 1999, p. 13).

In chapters 5, I have examined how experts communicate and collaborate in paleoceanography. I described it as a low fractal level and argued that knowledge is communicated and exchanged between its members because they all speak paleoceanography's domain language. This language is about the history of the Earth system, its main mechanisms of change, and the different techniques of data production. This language is rich in technical details and affords informed conversation between groups with different contributory expertise. I then examined the trading zone between paleo-modellers and paleoclimatologists in which communication is mediated by interactional expertise. Although low fractal levels' domain language and interactional expertise are very important mechanisms of social integration, there are other mechanisms that play a role in facilitating the communication and the exchange of knowledge between heterogeneous expert communities. One that is especially relevant is *trust*. Trust mediates communication between expert communities when there is little expertise shared between them and one group of experts can make few or no judgements about the knowledge generated by the other community.

Trust has recently been examined within Collins and Evans' theoretical framework by Reyes-Galindo (2011) who argued that in domains of physics that interact minimally, such as theoretical physics and experimental physics, knowledge exchange strongly depends on trust. This is because scientists from these fields have little immersion in each other's domains and consequently a low-level understanding of each other's practices. There is therefore no mutual acquisition of interactional expertise. In this sense, theoreticians cannot make meaningful judgements on the quality of the experimental data they occasionally use in their work. Scientists working on the same domain of practices would on the other hand be able to make informed judgements about other scientists' work on the basis of their shared expertise.

This point has also been made by STS scholars who research climate-change science. Shackley and Wynne (1995b) have pointed out that trust is an essential mechanism for facilitating interactions between two groups of modellers: GCMers and impact modellers. Future climate change projections produced by GCMs are used by impact modellers as input in their models. Crop modellers, for example, run simulations of future crops and seek to work out the relation between yields and meteorological variables. To do so, they use GCM output, such as temperature and precipitation, as input in their models. Interpreting and assessing GCM data, however, is not a straightforward task. It is necessary to acquire a great deal of tacit knowledge to do this. "[…] much of the knowledge and judgement needed to assess and interpret GCMs is often tacit and based on long experience of modelling; hence it is difficult to formalise or communicate in the literature" (Shackley and Wynne 1995b, p. 118). Crop modellers, however, belong to a different scientific community and usually do not have the expertise to assess and interpret GCM data. As a result GCM output reaches them black-boxed and is 'taken on trust'.

This example is not an isolated result. Shackley et al. (1998, pp. 190-191) pointed out that there are several other fields of expertise involved with the study of climate change that use GCM output, such as economists and modellers specialised in simpler models, but cannot make informed judgements related to assessing and interpreting their output. For this reason, they also take GMC output 'on trust'. Demeritt (2001, p. 309) extended this point further and pointed out that most climate-change scientists do not fully understand all the technical details underlying GCMs and "are forced to put their faith in technical expertise that they do not fully understand".

In this chapter I will examine the role played by trust in facilitating communication between expert communities producing knowledge on climate change. I will relate trust to expertise and to the fractal model. As climate-change science is a very wide field of research, knowledge exchange between communities which share small amounts of tacit knowledge cannot be mediated solely through a shared language or through interactional expertise. I will argue that trust plays an important role in these interactions. I will examine different fractal levels and different sociological configurations, i.e. single domains and fractionated trading zones, and seek to identify the importance of trust in mediating communication between different scientific communities.

Trust as a Sociological Phenomenon

Trust plays an important role in human societies and particularly in modern societies (Sztompka 1999, pp. 11-14). In STS, relevant research has been carried out, for example, on how trust is built on experimental results (Shapin 1994; Collins 2001); on public trust in science and in scientists (Wynne 1989); on how trust relates to conflict and performance in large-scale scientific projects (Shrum et al. 2001); on how patients come to trust or distrust healthcare treatment (Brown 2009); and on the role played by trust in the interactions between scientists and research regulatory bodies (Hedgecoe 2012).

I am particularly interested in trust as a social mechanism to mediate communication and collaborations when there is limited or no knowledge of the technical intricacies of a given domain. I will use the definition of trust set out by Giddens, which has more power to explain trust between experts than alternative definitions[63]. Giddens defined trust as

> confidence in the reliability of a person or system, regarding a given set of outcomes or events, where that confidence express a faith in the probity or love of another, or in the correctness of abstract principles (technical knowledge) (Giddens 1990, p. 34).

[63] Trust has been defined in several ways in the sociological literature and I will not attempt to carry out a comprehensive review of the topic here. Luhmann (2000), for instance, defines trust as a solution to the problem of risk. Individuals trust each other in situations in which they deliberately acknowledge that there is a risk involved in the interaction. Seligman (2000), on the other hand, has a very distinct definition. He argues that trust is something that comes into play when social roles are negotiable, i.e., there are no clear expectations towards certain social actors.

It is particularly relevant in situations where activities or systems are not completely visible to other actors:

> Trust is related to absence in time and in space. There would be no need to trust anyone whose activities were continually visible and whose thought processes were transparent, or to trust any system whose workings were wholly known and understood. It has been said that trust is 'a device for coping with the freedom of others', but the prime condition of requirements for trust is not lack of power but lack of full information (Giddens 1990, p. 33).

Giddens pointed out that one of the instances where trust comes into play in modern societies is when lay people deal with expert systems, which are "systems of technical accomplishment or professional expertise that organise large areas of the material and social environments in which we live today" (Giddens 1990, p. 27). For example, most people have very limited or no knowledge of architecture, but they trust that the buildings they live and work in are not going to collapse. People also trust that most of the time cars will work properly, that at crossroads all traffic lights will not go green at the same time making cars coming from different directions crash, and so on. Most individuals, however, do not have full technical knowledge to assess the quality of theses expert systems. For this reason they can only trust or distrust them. As Reyes-Galindo (2011, p. 123) pointed out, trust in this context means *suspension of doubt* in a given body of knowledge/technology produced by a group of experts.

This insight will inform the rest of the discussion on trust carried out in this chapter. The examples given by Giddens are on lay people dealing with experts systems. Because they cannot make judgements on these expert systems *they can either trust them or not*. In science trust comes into play when experts cannot make judgements about knowledge produced in domains of practice that they have no expertise in[64]. In these situations they usually trust the judgements made by the relevant experts. Trust in this sense is not in the individuals making particular judgements, but in the community

[64] It is worth noting that trust is not necessarily opposed to expertise in that experts have to trust the community in which they immerse themselves to acquire their expertise. Students, for example, have to trust the knowledge of their teachers and supervisors to become fully socialised in a scientific domain of practice. Therefore, the idea that trust comes into play in science when scientists have no expertise to make informed judgements on a given body of knowledge is true in the case of full-blown experts, but not in the case of individuals being trained to become scientists.

that sustains these judgements. In other words, they do not trust particular experts and their individual knowledge, but the expertise shared within the relevant community.

Trust in individuals is also part of scientific life. For instance, scientific results are rarely checked by other research groups. Similarly, when scientists read papers they trust that the authors are reporting accurately the methods underpinning the research and the findings of the study. How trust is built in particular research groups and in particular researchers have been the focus of relevant STS research (e.g. Collins 1975; Shapin 1994; Collins 2001). I will not examine this aspect of trust here as this falls beyond the scope of this work.

Trust and the Fractal Model

The lower a fractal level is, the richer the understanding the different contributory experts have of the technical details of each others' work. In paleoceanography, for instance, there is a domain language that is rich enough to sustain an informed conversation between the different contributory experts that contribute to it. If we look at levels below paleoceanography, such as paleoceanographers specialised in glacial-interglacial cycles, or micropaleontologists specialised in foraminifera, the language becomes even richer and more specialised. If we move to levels above it, the language becomes more generic and so does the conversation between experts.

As I have argued above, in science trust comes into play when experts can no longer make informed judgements about knowledge produced by other experts. In these situations they have to rely on the judgements made by other scientists. In low fractal levels, in which there are rich technical languages, such as in paleoceanography, scientists are in most occasions able to make judgements about the work of other experts. Trust, therefore, plays a relatively small role in mediating communication and collaborative efforts. In medium and high fractal levels, such as climate-change science as a whole, the situation changes. If communication does not involve highly detailed technical issues, the domain language can sustain a conversation between different types of experts. In these cases the conversation will be based on popular understanding of science, which consists of black-boxed and simplified versions of scientific theories. Trust, therefore, plays an important role in mediating interactions between different experts. If communication involves highly technical details, then, scientists have to bridge the gaps between their domains through some of the mechanisms mentioned above. Bridge-building mechanisms that do not lead to immersion in the tacit knowledge of the other community, such as boundary objects and inter-languages (Ribeiro 2007b), imply that communication will take place on the basis of limited mutual understanding. In this case trust also plays an important role in facilitating communication. Among the bridge-building mechanisms mentioned above, only interactional expertise can mediate interactions that depend on informed technical conversations involving information from low fractal levels. In this case, trust plays a smaller role, as scientists' linguistic understanding of each other's practices gives them access to the tacit knowledge underpinning highly technical judgements.

I will in the remainder of this chapter provide examples to illustrate the interplay between trust and expertise through the fractal model. I will begin with low fractal levels then examine medium and high fractal levels.

Low Fractal Levels: Paleoclimatology and Paleoceanography

To understand the role played by trust in low fractal levels, it is essential to consider the degree of specialisation within different domains. Collins (2011, pp. 278-279) has pointed out that in domains with little specialisation, for example, individual sports, such as tennis, snooker, or board games, such as chess, all full-blown experts have similar skills and can make informed judgements about each other's work. In fields with higher specialisation and therefore a higher level of social division of labour experts have different contributory expertise and a common language that mediates their interaction. In the case of domains with low levels of specialisation, there is a great deal of shared contributory expertise. In these cases trust, does not play a very important role in mediating interactions. In chess, for instance, all players have the same contributory expertise, although some of them are better players than other. In any case, there are no practices in chess which only some specialised players would have access to, whereas other players would only reach black-boxed versions of them. All accomplished chess players can make informed judgements about each other practices.

In the case of domains with high levels of specialisation, there are several practices which particular experts do not have a physical engagement with. An example of this is paleoceanography. As mentioned in chapter 5 there are several types of subspecialties in this field: paleoceanographers, micropaleontologists, geochemists, and subspecialisations within these areas of expertise related to time intervals, events, phenomena, techniques, and types of archives and proxies used. Experts who contribute to paleoceanography have contributory expertise in a limited number of these subspecialties. The communication between experts with different contributory expertise is facilitated by the paleoceanography's domain language, which is rich enough in technical details so that different contributory experts can have informed conversations among themselves about their work. As a result, much of what experts in paleoceanography know about their field is learned linguistically and not through physical engagement with practices. In this case, even though experts do not share contributory expertise, they still can make several judgements on each other's work. They can assess whether the data interpretation by a given expert makes sense, whether the methods chosen for a particular research were appropriate, whether the data produced by particular groups fit within the general understanding of how the Earth system works, and so on. There are however, certain judgements that particular groups of contributory experts cannot make on each other's work. A large number of judgements involved in the process of producing data are black-boxed. These includes standardised judgements, i.e. those necessary for making standardised contributions, and, more importantly, domain-language-based judgements, i.e. those necessary for making domain-language-based contributions. For instance, all the judgements made by micropaleontologists related to distinguishing different types of microfossils so as to produce assemblage counts, which can be converted into, for example, temperature proxy data, are black-boxed and not visible to other experts who are not able to make these judgements themselves. Other experts working in paleoceanography, for example, geochemists, can make judgements related to the plausibility of their data, i.e. whether they fit within the general understanding within the community of Earth system processes in a particular time period. However, they cannot assess the very judgements related to the production of data. These judgements can only be trusted or distrusted by them.

I will now provide an example of this. This example is on the use of ice-core data by paleoceanographers and relates to interactions taking place in a low-fractal-level domain: paleoclimatology. There is significant integration between different subfields of paleoclimatology as data produced on particular archives are usually useful for scientists working on other archives as these data help them interpret their own records. The paleoclimatology language is dense enough to allow experts to have informed conversations between them. This is because all paleoclimatologists have at a least a general understanding of the history of the Earth and of its main mechanisms of change. The different types of specialisation define how in-depth their understanding of particular time periods, events, phenomena, and so on, are, but all members of this community have at least a general understanding of what each other do.

Paleoceanographers, for example, frequently use data produced on non-marine archives to compare with their own data sets. This is because all proxies record multiple signals and it is not possible to disentangle them without comparing the proxy data with other records. Furthermore, by examining data from other archives paleoceanographers can assess how other parts of the system behaved during certain events, which help them interpret climatic changes found by using the data sets that they generated. Ice cores are one of the most useful paleoclimatic archives for paleoceanographers.

Ice is continuously deposited on the ice sheets covering Antarctica and Greenland. By drilling the ice sheets and extracting ice cores, paleoclimatologists recover a valuable climatic archive. By examining ice-cores data they can reconstruct annual climatic variations extending as far as 800 thousand years in the past. Ice cores became well known for their temperature and atmospheric CO_2 concentration records. Paleoclimatologists use bubbles of atmospheric air trapped in the ice to reconstruct past atmospheric composition. By doing so, they have produced an accurate record of atmospheric CO_2 over the past hundreds of thousands of years. This record was compared with temperature records and a co-variation of atmospheric CO_2 and temperature was found (e.g. Petit et al. 1999). Ice cores also record a number or other climatic variables including volcanic activity, snowfall patterns, wind speed, solar activity, etc (Bradley 1999, p. 126).

Paleoceanographers working on the last million years frequently compare ice-core data to their own data for a number of reasons, which includes the development of age models for their own data sets; to have a broader picture of events they are interested in; and to verify whether the records they have produced match other data sets.

A somewhat similar expertise to the one needed to generate data on marine sediments is necessary to produce data on ice cores. The process of producing data on ice cores also consists of drilling and recovering cores, preparing samples, geochemical analysis, and interpretation of results. The interpretation also relies on understanding how the Earth system works and its mechanisms of change. There are however two differences between working on ice cores and on marine sediments: the specific techniques deployed in the generation of data; and whereas marine sediments reflect the behaviour of the oceans and its interactions with other parts of the Earth system ice cores reflect the behaviour of the cryosphere and its interactions with other parts of the Earth system. Although the focus is slightly different, there is still a large overlap, as the Earth system is complex and changes in any of its subsystems tend to affect all the others.

Paleoceanographers can sometimes make judgements on the interpretation of ice-core data. Their expertise can be applied to make these judgements particularly those that are made on the basis of their understanding of the history of the Earth and of its main mechanisms of change. Due to the importance of ice cores in Quaternary[65] paleoclimatology, most Quaternary paleoceanographers also have to understand the principles underpinning production of data on this archive. In most cases, however, they

[65] The Quaternary consists of the geological period spanning approximately the last 2.5 million years.

cannot make judgements related to the production of data on ice cores as this is not where their contributory expertise lies:

> Tiago: When you use other kinds of archives to compare them with your records, do you try to understand the ins and outs of the other kinds of archives by talking to people, reading specific literature on that, or do you just trust the data that you find?
>
> Nick: No, absolutely you need to understand again the basis of the proxies, what they are telling you, what the caveats are in terms of their use. You can't just use them blindly and just accept that they are reliable. But there are also, it's not just what the proxies tell you, the age models, how the age models are synchronized, all those things are massively important when you're trying to put data together and come up with a convincing set of arguments. No, you absolutely have to understand the record. Occasionally you can contribute to their understanding of those records.
>
> Tiago: Obviously all these archives have different caveats, different error bars and uncertainties and stuff. A person who is not directly involved producing them, you as a person who is not directly involved with that. Do you think that there are different levels of understanding so that you can't reach the deepest level because you haven't worked with them?
>
> Nick: Probably, almost certainly yes. I've never been and drilled ice cores. It's a bit like saying I have a good understanding where the errors are in marine sediments because I have taken a whole heap of marine cores. So, I've been on ships, and you pull them up, and you understand ultimately where the errors can potentially be. But ice cores, no, I've never taken an ice core, so at some level my understanding stops and I trust what I'm told. And that's the same when they look and try and compare their records to what we're producing. But in terms of the depth of the understanding, you understand the basis of the proxies, you understand the age models, you understand exactly what they've done, because it's in the literature, so you can make up your own judgement whether you believe the interpretation. You can have your own interpretation of the data or place more weight on a particular interpretation, some other interpretation. So, you don't just accept necessarily what they're saying and then have to fit your data into their understanding. You can challenge that and do what you want, as long as you can justify.

In this quotation Nick points out that when he uses data sets produced on ice cores, he uses them on the basis of a combination of trust in the expertise of the ice-core community with his own expert judgements. Trust comes into play because he has never been directly involved with the production of the data so that he has no knowledge of where the errors can potentially be in this stage of research. This means that he does not have contributory expertise in generating ice-cores data, i.e. in sampling ice cores, choosing analytical techniques, etc. When he uses ice-cores data he trusts the community of experts in ice cores and their judgements related to data production. Yet, he feels confident to make expert judgements related to interpreting ice-cores data, because he understands the principles behind generating them, the caveats related to their use, and so on, i.e. this judgements are based on his interactional expertise in the ice-cores proxy systems that he integrates with this work

Based on the quotation above, one could argue that these judgements are made on the basis of primary source knowledge, as Nick argues that information about the basis of the proxies, the age models, and what the authors of a given paper have done are in the literature. Indeed, this information can be found in scientific papers. But this is not enough for making informed expert judgements. Nick himself pointed out in another part of the interview that there is some degree of integration between scientists working on ice cores and those working on marine sediments. However, this integration is weaker than in the case of those working on the same archives, which is something expected as paleoclimatology is a fractal level above the domains where data are produced. This point supports the idea that his judgements on ice-cores data are not based on primary source knowledge:

> Tiago: How integrated is this community, the different archives people getting together and talking?
>
> Nick: How integrated are we? Not overly. Yeah, there are again organisations, for example Pages, which are multidisciplinary, so people from ice cores, marine cores, etc. And it's certainly something that we all promote. So, we have cross-disciplinary research meetings and I've organised those in the past myself. So, we're integrated in that sense. On a day to day basis I guess you've got to integrate for a reason ..
>
> (Telephone rings and he answers it).
>
> Tiago: I think there's the AGU conference.
>
> Will: Ok, so, yeah, that's a good example of where all geoscientists come together, so all styles of paleoclimatology. But again whether you would call that even integration I don't know because we all have our own sessions. So, there will be ice-core sessions, there will be speleothems sessions, and then there will be integrated sessions where different, which are more processes, if you think of processes again, everyone will try and come at it from a different angle. So, yeah, there's integration there, but there's also your discipline-specific interest.

In other words, Nick also has some immersion in the communities of paleoclimatologists who work with different archives, although this immersion is not as deep as in the case of marine-sediments paleoceanography, which the fractal model should lead us to expect.

In sum, paleoclimatology is a low fractal level, therefore, different types of contributory experts are able to make certain judgements on the work of other contributory experts, particularly those related to the interpretation of data produced by using the most popular techniques. These judgements are based on a combination of their understanding of the processes that take place in the Earth system with how these processes are reflected in particular archives. Will is an example of that. He can interpret ice-core data as long as he acquires interactional expertise in the relevant proxies. With regards to the production of these data, however, he has no contributory expertise in it, therefore he trusts the judgements made by the experts who produce them.

Interactional Expertise, Fractionated Trading Zones and Trust

At medium or high fractal levels communication between experts belonging to different groups is possible as long as it is not about technical details that 'belong' to lower fractal levels (Collins 2011). Scientists from different areas who study the causes and processes of climate change, for example, atmospheric physicists and paleoceanographers can have conversations about the climate system, but the conversation will not be as rich in technical details as a conversation between two paleoceanographers. In the case of collaborations that require higher levels of technical detail experts might acquire interactional expertise to facilitate their interactions, as we have seen in chapter 6. Examples of this are the interactions between paleo-modellers and paleoceanographers. Trust also plays a role in mediating interactions between these communities. The quotation by Louis on pages 155 exemplifies this point. Louis is a GCM modeller who states that he does not have contributory expertise to generate data nor does he keep up with all the literature on data production. For this reason, he is not aware of particular circumstances related to data points that he uses in his work. He essentially trusts the judgements made by the scientists who generate and compile the data. However, this is not a case of him just taking at face value any data set he finds in the literature, i.e. of primary source knowledge. He frequently attends conferences where he updates his interactional expertise in the production of paleoceanographic data by watching leading paleoceanographers' talks and chatting informally with them. He does not however check the accuracy of the particular data sets he uses and would not be able to do so because he has no contributory expertise in paleoceanography. He trusts the judgements made by his colleagues who produce these data sets.

It is worth noting that in this case, as in the example on paleoceanographers being able to make judgements on whether data produced by ice-core paleoclimatologists on the basis of their own expertise, paleo-modellers can also assess paleoceanographic data sets. They can make judgements related to whether these data match other data sets already produced and whether they bring any new insights into the understanding of paleoclimatic phenomena. If they have acquired enough interactional expertise in paleoceanography, they will also have their own criticisms of particular proxy systems. Yet they also cannot make judgements related to the production of particular data sets. Paleo-modellers, however, in general have to put more trust into the work of paleoceanographers, than paleoceanographers into each other's work. This is because modellers usually do not follow in details the data production literature nor are they used to compiling data sets. They tend to focus on the big trends in the data literature. They are therefore not familiar with all issues surrounding different data sets.

I will provide another example of this, in which a modeller was not able to make judgements on data production. This example was provided to me by an interviewee. This example is linked to a controversy on bipolar glaciation during the Eocene. I will summarise this controversy before discussing how the issue of trust relates to it.

In 2005 a paper was published in *Nature* arguing that during the Eocene, which is the period of time spanning from approximately 56 million years to 34 million years in the past, there were some transient bipolar glaciations (Tripati et al. 2005). This means that during certain intervals of the Eocene ice sheets grew on both poles. The Eocene, however, is known for having been a greenhouse period, i.e. a warm period with little or no ice cover on the poles, in opposition to the icehouse period that begun at the end of the Eocene, when a persistent ice sheet grew in Antarctica. Northern Hemisphere glaciation is believed to have started tens of millions of years later, between 10 and 6 million years ago.

The authors pointed out that there were three events when they believed that there had been major glaciations on the Northern Hemisphere. They focused on a single one that began approximately 42 million years ago. They produced data on sedimentary cores collected in the Equatorial Pacific area. They used two lines of evidence to support their arguments. Firstly the authors measured the $\delta^{18}O$ of foraminifera shells, which showed a sharp rise during the event. As I described in chapter 4, $\delta^{18}O$ is controlled by seawater temperature, continental ice volume, and salinity. To disentangle the different signals, they measured the ratio of magnesium to calcium on foraminifera shells, which is a proxy for seawater temperature. Their results indicated that the increase in $\delta^{18}O$ responded predominantly to the continental ice cover. The numbers indicated an amount of ice cover too large to be deposited solely on Antarctica. The authors inferred that there had been substantial ice cover on the Northern Hemisphere as well during this event.

The second line of evidence was related to the calcite compensation depth (CCD). The CCD refers to the depth at which calcium carbonate dissolves because seawater becomes corrosive to it. The reason for that is that below the CCD the ocean is under-saturated in calcium carbonate. Microfossils that have a calcium-carbonate shell sinking from the ocean surface are dissolved once they cross the CCD. There are a number of variables that influence the depth of the CCD, including the amount of dissolved carbon in the oceans and sea level rise. The authors, measured changes in the CCD before and after the $\delta^{18}O$ excursion that took place approximately 42 million years ago and concluded that it became significantly deeper after $\delta^{18}O$ began to rise. They interpreted this as caused by the reduction in the sea level brought about by the glaciation.

This paper generated debate and further research within the community. In the same issue of *Nature*, for example, a review was published pointing out that

> […] a general acceptance that glaciations occurred in the middle to late Eocene will probably require further evidence. The suggested existence of large Northern Hemisphere ice sheets in the

> Eocene is highly controversial. Moreover, the fidelity of the magnesium content of $CaCO_3$ as a measure of temperature demands further scrutiny (Kump 2005, p. 333).

In the following years a number a papers were published contesting the ideas put forward by Tripati et al.[66]. Edgar et al. (2007, p. 908), for instance, pointed out that sediments deposited in areas where CCD changes took place were not the most appropriate for collecting stable isotopes data because the occurrence and preservation of microfossils was sensitive to these changes. For this reason, they produced new oxygen isotopes records on cores from a different area in the Equatorial Atlantic Ocean to test the hypothesis of the occurrence of bipolar glaciations in the early Eocene. Their data provided evidence for a much smaller variation in $\delta^{18}O$, which does not support the idea that there would have been a large ice sheet on the Northern Hemisphere. According to them, there might have been small glaciers in Greenland during this period, but not a major glaciation.

The authors argued that the difference in the records was caused by Tripati et al.'s use of outlying data points and a sparse data set. Furthermore, they pointed out that in a previous paper (Lear et al. 2004) it had already being argued that the core recovered in the Equatorial Pacific should not be used for reconstructing Mg/Ca ratios before 35 million years old as it had issues related to contamination and poor preservation. In other words, they should not have used Mg/Ca ratios to disentangle the temperature from the seawater isotopic composition signal in their records.

A year later a paper led by a modeller (DeConto et al. 2008) argued that bipolar glaciation could have happened only after 25 million years ago. This conclusion was based on a climate model that was run to identify the threshold for bipolar glaciation in terms of CO_2 concentration in the atmosphere. The result was that the maximum concentration was approximately 280 ppm, which was only reached 25 million years ago. This threshold made the theory of bipolar glaciation in the Eocene, when CO_2 concentration in the atmosphere was much higher than 280 ppm, very implausible.

---

[66] Examples other than those presented in the main text are Eldrett et al. (2009) and Stickley et al. (2009).

Even though the theory of bipolar glaciation in the Eocene has been directly criticised as in Edgar et al. (2007) and shown not to be very plausible (DeConto et al. 2008), Tripati still works on developing new proxies to test the strength of her records. In her website she acknowledges that this is a very controversial theory, but she believes that there is evidence that supports her ideas[67]:

> Although controversial, the results of this work has started to challenge the traditional and widely-held views that 1) the Eocene epoch was characterized by ice-free conditions, as we have found evidence for ephemeral glaciations beginning in the late middle Eocene (~42 million years ago - so a few million years younger than the vegetation-rich deposits from the Arctic and Antarctic) and 2) that no ice was present in the Northern Hemisphere until 10 Ma. Our work on Ocean Drilling Program sediment cores has shown there may be sedimentological and geochemical evidence for some ice storage at both poles during ephemeral glaciations at 42 and 34 Ma, a pattern consistent with CO2 as a primary driver of glaciations.

This example illustrates controversial aspects of paleoceanography. The interviewee who pointed this controversy out to me stated in an email that most paleoceanographers were convinced by the paper by DeConto et al. (2008) that there was no significant bipolar glaciation during the Eocene:

> Kate: I think the DeConto paper is pretty compelling, and most scientists in the community do not think there was significant bipolar glaciation in the Eocene. There was probably small, upland glaciers on Greenland etc., but nothing like the huge US continental scale ice sheet that Tripati originally argued for. I wouldn't bet my mother's life on that, but I would wager a significant chunk of money on it.

The same interviewee, who works in the interface between geochemist and paleoceanography, pointed out in another email that when Tripati et al.'s paper was published she was able to spot the error in the data. She afterwards met a modeller who intended to run a project to investigate the plausibility of the paper by Tripati et al., but when she explained the issues with the data, he changed his mind:

> Kate: Another example I can think of, is when a proxy record managed to get through peer review which caused a big stir as it argued for larger ice volume than today at a time of higher CO_2 levels in the past. As a geochemist I was able to look at the data in detail and find a serious flaw with it. Immediately after the publication, I was talking to a modeller who was about to invest a serious amount of time and money into trying to model extreme glaciation under high CO_2 conditions. When I explained to him the specific issue with the proxy at the particular

[67] http://atripati.bol.ucla.edu/morepaleoclimateresearch.html (last accessed on 15/06/2012).

> sampling site he decided against it. I should mention that subsequent published papers have also demonstrated that the original work was flawed. It is unusual for something like that to slip through the peer review net, but if it does it gets put right by subsequent papers. I guess one of the issues relevant to your work is how well modellers can keep up with all the literature in cases such as this.....

Kate, in this example, spotted the error because she was in the cruise where the cores were collected. However, as there was a paper in the literature reporting the problems with producing Mg/Ca on sediments older than 35 million years old collected in this particular site, anyone immersed in this literature should be able to find this error:

> Kate: There were a few issues with the data - some specific to the sampling site used. Because I was on the ship that collected the samples I knew about some of these issues. Hence if you look at figure 3 of the Lear paper, you'll see it’s been shaded out some of the data as it was suspicious - and this was published BEFORE the Tripati paper! This really should have been picked up by reviewers. It basically means that the Mg/Ca ratios cannot be used to calculate past temperatures. The bipolar glaciation idea was also based on some outlying points in the oxygen isotope record (it's bad practice to use outliers in this way anyway - and that should have been picked up by any geochemist regardless of proxy). The oxygen isotope record reflects both temperature and ice volume, so without a Mg/Ca temperature record, it cannot be interpreted in terms of ice volume (see Kump News and views article on the Tripati paper).

As I have argued above, people with different specialties usually do not keep up with the literature on data generation that does not relate to their immediate research interests. The modeller cited by Kate, for example, was not aware of the issues with the data used by Tripati et al. (2005). When talking to her he learned about them and gave up a project in which he would assess the plausibility of glaciations in situations with high concentrations of carbon dioxide in the atmosphere. He did so based on Kate’s advice. In this case, he essentially *trusted her expertise* as he could not make judgements about the data used by Tripati et al. (2005). Paleoceanographers working on more recent time scales would probably also not be aware of the issues in Tripati et al.’s data as well[68]. However, by talking to members of the community involved with data production on the Eocene they could probably become aware of them. By doing so, they would also trust the judgements made by the relevant experts.

[68] The other issue with Tripati’s data was the use of outlying data points. This is a more ubiquitous type of judgement in science that a wider range of experts should be able to make.

Medium and High Fractal Levels

I have argued above that in interactions in which a low-fractal-level domain language or interactional expertise mediates communication between members of different expert communities there is also an element of trust mediating communication. For this reason, even in low fractal levels trust plays a role in facilitating communication between different contributory experts. In cases of medium and higher fractal levels where there are no mechanisms for building bridges between different contributory experts, the shared language between different specialties is too generic and there are no or very few judgements that an expert can make about other fields of expertise that are at the same fractal level. I will examine two examples of this. Firstly, the understanding of paleoceanographers of studies of impacts, adaptation, and mitigation of global warming. Secondly, a field of scientific investigation that is part of the Earth sciences and of the study of causes and process of climate change, but is not strongly related to paleoceanography: cloud physics (see figure 12 for a representation of these domains in the fractal model).

The study of the impacts of climate change, adaptation practices, and mitigation are only at the same fractal level of paleoceanography at a very high fractal level. As described in chapter 3, these fields of research are a heterogeneous mixture of several disciplines from natural sciences and human sciences. They are subfields of climate-change science. In going from paleoceanography to this level, one must traverse paleoclimatology, the study of causes and process of climate change, and finally reach climate-change science as a whole. The distance in terms of fractal levels is so large that usually paleoceanographers know little about these other fields. Their knowledge of them is not much deeper than *popular understanding of science*. They cannot make expert judgements related to them unless there is a significant overlap between their own research interests and these areas. There are only a few of these overlaps. An example of this is geoengineering techniques that involve fertilising the oceans. In this case, micropaleontologists and paleoceanographers specialised in productivity might be able to contribute to debates related to this technique. But in most cases paleoceanographers do not have an in-depth knowledge about studies of impacts, adaptation, and mitigation of climate change. For this reason, they cannot make the judgements that scientists working in these fields can make.

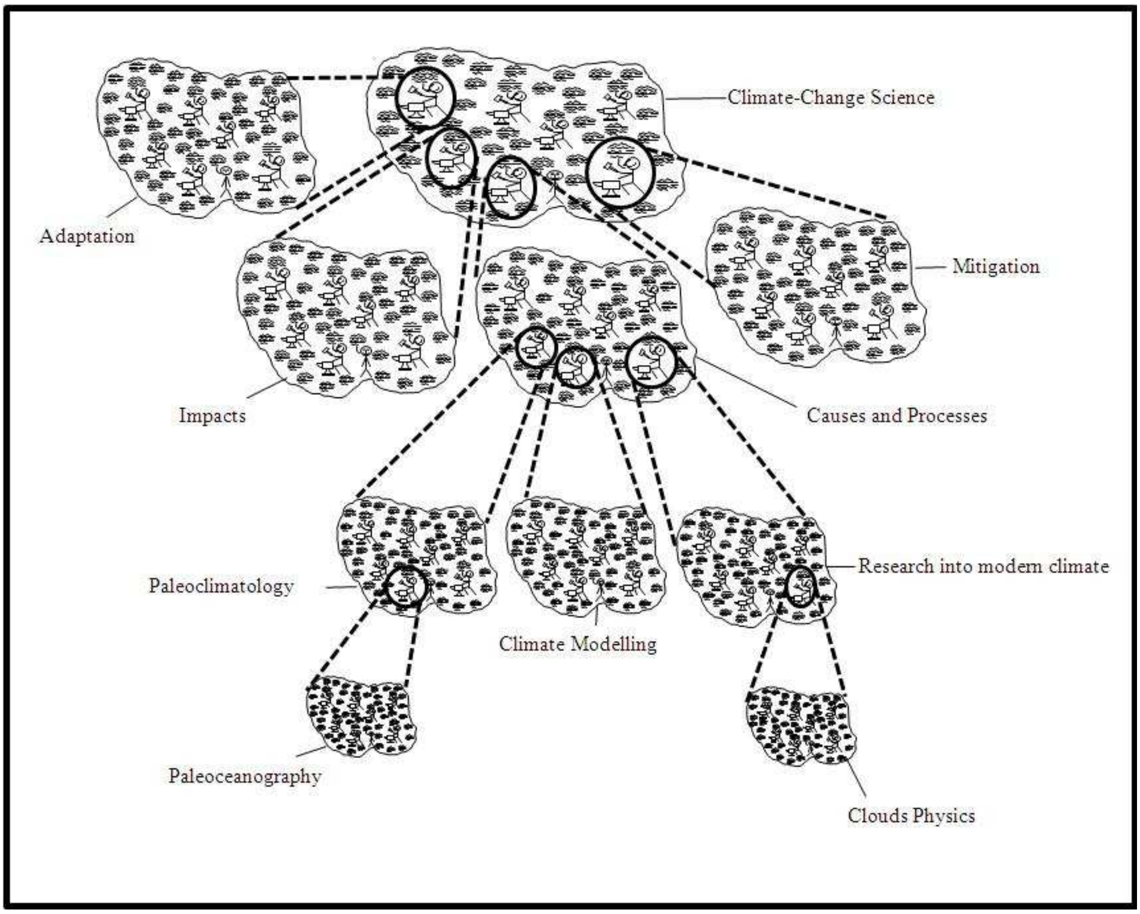

Figure 12: Paleoceanography and clouds physics in the fractal model.

The following quotation, in which a paleoceanographer points out that she is not very well informed about the study of impacts, adaptation, and mitigation of climate change, illustrates this point:

> Tiago: How about these other areas that might be a bit more distant from your specific field of expertise, such as impacts, adaptation, mitigation. How familiar are you with this kind of research?
>
> Gemma: I'm not familiar with the research. I keep abreast of what's in the news, but I just don't have time to go into the research in that theme.

Another paleoceanographer made the same point:

> Tiago: How about these other bits of the debate in terms of impacts, adaptation.
>
> Mark: I'm much less confident talking about that. My expertise would be much more in the scientific basis rather than adaptation and technology, building sustainable businesses and sustainable cities and societies, that's not my area at all. Although actually I'm interested in that. So, for instance the IPCC has three reports and the scientific basis is the one that I would contribute to.

The second example is on cloud physics and paleoceanography. These domains are at the same fractal level only at a medium fractal level: the studies of causes and processes of climate change. Although there is shared language about how the Earth system works that mediates conversation between these communities, at this point language is not specialised enough to afford highly technical conversations.

Cloud physics is a very specialised field of atmospheric physics. Clouds are an important part of the atmospheric system and have a dual role in the climate system. They reflect part of the incoming solar radiation back to the outer space having therefore a cooling effect. They also prevent the solar radiation that is reflected by land back to the atmosphere from returning to outer space, which also creates a warming effect. Paleoceanographers usually have a limited understanding of this field. One of the reasons for this is that they cannot reconstruct clouds in the past. This point is exemplified by the following quotation, in which a paleoceanographer points out that her understanding of cloud formation is very limited:

> Tiago: But even in terms of the scientific basis, there are so many phenomena. I was wondering whether a paleoclimatologist understands, I reckon not everything, but at least a bit about clouds, aerosols, all these hundreds of processes that are going on in the climate system?
>
> Gemma: So, your question is if I understand about cloud formation. No, I don't. But what I understand is that, I mean what is important to me is to know that we don't really have any good proxies for this in the past, and I need to know what the modellers are assuming. I know that the modellers have a big problem with clouds, it's probably one of the biggest uncertainties in the models. And that's about all I know. I think until we have a good way of figuring out how clouds have changed in the past, because I'm looking at the past there's not much point in spending the next year learning all the physics that I'd need to understand modern day cloud formation. There

> are just levels of understanding out there. I know enough to know where the uncertainties are in my field. To really understand cloud formation is a research field itself.

In the examples above, where different fields of science are at the same fractal level only at medium or high fractal levels, there is not much shared expertise between them. For this reason, paleoceanographers, for example, cannot make informed judgements about cloud physics or about studies of impacts, adaptation, and mitigation of climate change. In this case, they can either trust or distrust the judgements made by other scientists, as they cannot assess them based on their own tacit knowledge. In medium or higher fractal levels it is necessary that a large amount of trust is involved in communication between different types of scientists. Otherwise knowledge cannot travel from one community to others.

Chapter Summary

In this chapter I have argued that trust and expertise are related. Trust plays an important role in mediating knowledge exchange and communication between scientists. Trust comes into play when scientists have no shared expertise that enables them to understand and/or to make judgements about each other's work. Trust in this case consists of suspension of doubt.

I have related trust to the fractal model. At different fractal levels trust has different levels of importance as a mediator between different communities. At low fractal levels, i.e. where there is a domain language rich in technical details that enables different experts to hold informed conversations, trust is a less important social mechanism than in higher fractal levels, where the shared language is too generic. However it still plays an important role in low-fractal-level domains with high levels of specialisation. This is because different contributory experts, although sharing a common language, do not have contributory expertise in each other's practices so that they cannot make expert judgements about practices which are not their own. This is not to say that there are no judgements that they can make on the work of their peers. They can make several judgements related to how the data produced by their colleagues are interpreted, to whether these data sets match the rest of the literature, and they usually even have their own evaluations of different proxy systems. They only cannot make judgements related to the very process of data production. In this case they have to trust the relevant contributory experts.

Similarly, in cases in which members of a community acquire interactional expertise in another domain that is at the same medium or high fractal level, therefore forming a fractionated trading zone, trust plays a similar role. These communities have their own literature, which are only partly known by members of the other domain. In the case of paleoceanography and paleo-modelling, for example, paleo-modellers cannot make judgements related to the quality of particular data sets. They trust the judgements of paleoceanographers who can make these judgements. Similarly, paleoceanographers are not really well informed about particular model runs. Therefore, when experts who are members of a fractionated trading zone working on the basis of interactional expertise need data or model output from the field which is not their own, they usually trust the judgements of the relevant experts.

I have also argued that the further up one goes in the fractal level, if there are no mechanisms mediating communication other than the domain language, the less is one's ability to make judgements about the knowledge produced by other experts that are at the same fractal level. In this case, trust and expertise are therefore 'inversely proportional'. This is a general model that explains some aspects of communication and of the interactions between scientists.

Conclusion

This thesis is a study of mechanisms that mediate communication and collaboration between heterogeneous expert communities in climate-change science. As this field is too broad to be the topic of a single PhD research I have narrowed down my research topic and focused on paleoceanography and on the interactions of members of this community with other fields of climate-change science. I have used the fractal model developed by Collins (2011) to guide me through different levels of analysis. I have begun examining a narrow domain, paleoceanography, and travelled through different fractal levels to reach wider domains, such as the study of causes and processes of climate change and even climate-change science as whole. By doing so, it was possible to examine different mechanisms of communication at work at different levels of analysis. The main contribution of this thesis to the STS literature was to identify how different bridge-building mechanisms between heterogeneous expert communities work at different levels of analysis.

The methods underpinning this research were chosen based on the notion of participant comprehension (Collins 1984; 2009). I sought to immerse myself in climate-change science as a whole and in paleoceanography, in particular. The main goal was to acquire interactional expertise in the latter and a basic linguistic understanding of the former. To do so, I have interviewed experts, attended scientific meeting and summer schools, and visited laboratories. At the end of the fieldwork, I had not become a full-blown interactional expert in paleoceanography, but I had acquired sufficient levels of interactional expertise to analyse this field sociologically. Further immersion in this field might bring to light a more detailed picture of how scientists interact among themselves.

A number of conclusions can be drawn based on the study of paleoceanography and of the interactions between paleoceanographers and other climate-change scientists. The main conclusions relate to the fractal model and to how scientists communicate at different fractal levels. Different sociological configurations, such as low fractal levels with domain languages rich in technical details, fractionated trading zones based on interactional expertise, and trust were found at different levels of analysis. I will present them in this concluding chapter and I will make some tentative generalisations on how these findings might be also be extrapolated to climate-change science as a whole and perhaps even to Western science in its entirety. These generalisations will certainly benefit from further research that might support or contradict them. Some unexplored topics and some limitations of the present work have also come to light, which call for further research.

The Fractal Model and Bridge-Building Mechanisms

The idea behind the fractal model is that whenever one zooms in on a given domain of practice one finds that it is composed by narrower domains, which in turn are also composed of even narrower domains. One can zoom in and out and will find the same pattern: different types of contributory experts who communicate through a shared language. If we look at low fractal levels, such as paleoceanography, the domain language is rich in technical details and affords informed conversation between the different types of contributory experts that make up the domain. In middle or high fractal levels the shared language becomes more generic and as a result conversations become more superficial. The level of technical detail that can be conveyed with the domain language is reduced the further up we go in the fractal model. If we look at climate-change science, for instance, there is not much overlap between the languages of paleoceanographers and experts in adaptation techniques. Communication between them, in situations in which there are no bridge-building mechanism at work, does not involve much more than popular understanding of science.

Two bridge-building mechanisms were examined in this work and linked to the fractal model: interactional expertise and trust. Interactional expertise was examined in two different sociological configurations: as the mechanism underpinning domain languages and in interactional expertise trading zones.

The first mechanism that was examined was domain languages. Domain languages might be rich in technical details or not depending on the fractal level. In low fractal levels it is rich enough to afford informed technical conversations between different contributory experts. In this work I have examined paleoceanography as an example of this. This is an autonomous field of expertise with its own institutions, such as journals, conferences, and so on. In paleoceanography, there are several different types of contributory experts with distinct specialisations. There is a rich technical language in this domain, which includes information on the history of the Earth system, on its main mechanisms of changes, and on the techniques deployed to generate paleoceanographic data.

I have also examined fractionated trading zones in which communication is mediated by interactional expertise. This is the case of the current generation of paleo-modellers and paleoclimatologists. The contributory expertise of these groups is considerably different and very few members of these communities have any contributory expertise in each other's practices. This trading zone is at a medium fractal level, i.e. the study of causes and processes of climate change. The domain language at this level is not rich enough to afford conversation on detailed technical matters. In order to be able to communicate effectively these groups have developed bridge-building mechanisms between them. They have been undergoing a mutual process of linguistic socialisation. In the case of paleo-modellers, they had to learn about mechanisms of change of the Earth system in the geological time scale to be able to set up their models. This first step already facilitated communication between both communities. There are however still some occasional difficulties in communication between these communities when it comes to the techniques deployed by them. This is because different paleo-modellers have different levels of interactional expertise in the generation of paleo-data and, similarly, different members of the data community have different levels of interactional expertise in paleo-modelling.

These communities, however, have been working towards improving their communication and increase their levels of interactional expertise. Some of these efforts take place at the individual level. In collaboration between paleo-modellers and data people scientists frequently linguistically socialise each other through talking. At the community level, there is an effort to train the next generation of experts so that they have a good understanding of data generation and modelling. This happens in different ways. Firstly, through a number of courses, including summer schools and Masters courses, in which students are lectured on both fields. Secondly, at certain universities PhD students are carrying out projects that include elements of data generation and of modelling. In this case, they are being socialised in the language as well as in the practices of both communities. They are not however becoming full-blown contributory experts in both domains. They are becoming fully-fledged contributory experts in only one of these fields and acquiring low levels of contributory expertise in the other. The greatest benefit from this training, therefore, is the linguistic immersion in the domain which is not their own. This generation is likely to have much fewer problems in communication that the current one.

With regards to generalising these finding to other fields of science, there are some patterns that could be expected to be found elsewhere. The development of fractionated trading zones only makes sense at medium fractal levels. At low fractal levels there are rich domain languages that mediate communication between different contributory experts. At medium and high fractal levels, in contrast, the domain language is not rich enough in technical details so that groups of experts might acquire interactional expertise in another domain to be able to communicate better with its members. The same applies to other types of trading zones, such as boundary objects and inter-language trading zones. It is plausible to expect that they are only developed at medium and high fractal levels where the domain language does not afford conversation rich in technical details and shared meaning.

I have also presented the role played by ambassadors in mediating communication between different expert communities in trading zones that work on the basis of interactional expertise. Ambassadors were originally defined as individuals who are sent by their research groups to spend some time with members of another expert community to acquire special interactional expertise in their practices and afterwards return to their groups (e.g. Collins 2011; Reyes-Galindo 2011). By doing so they are able to help their original groups by make clarifications on the science carried out by the other community. I have extended this definition by providing examples of other two types of ambassadors. Firstly, those who are sent to another research group to acquire some specific skill that lacks in their own research group. Secondly, scientists who are hired to integrate a group with a different expertise to theirs. In this case, they use their contributory expertise in collaborative projects and/or by answering queries of their host group.

In the trading zone between paleoclimatologists and paleo-modellers ambassadors work in association with a general effort of these communities to bridge the gaps between them. In this case, they are part of the mutual socialisation process that these communities are currently undergoing. Ambassadors, however, can also exist in contexts where there are no wider efforts of entire communities to socialise each other (Collins 2011). In this case it has some advantages and limitations if compared to the mutual socialisation strategy. In this situation, ambassadors are a kind of shortcut in terms of communication between experts' communities. They provide groups of experts with the possibility of learning about a different domain of expertise or of benefiting from the expertise of another domain without the creation of a large-scale trading zone. This can be a very practical solution for groups of scientists that need advice or technical help from experts from other domains, but who do not want to become fully immersed in the language of other fields. The main limitation of ambassadors in these contexts is that they can only bridge the gaps locally. They may be very useful for a particular laboratory or research centre, but they do not benefit the community as a whole as the efforts of mutual socialisation examined above.

Finally, the last mechanism of communication examined in this work was trust. I have defined trust as suspension of doubt. It comes into play in situations in which people do not have the expertise to make expert judgements on bodies of knowledge or on technologies. As currently the social world is extremely segmented into a wide range of domains of practice, individuals increasingly depend on trust to make their social lives viable. All communication between individuals with different contributory expertise involves some degree of trust. However, the contribution of trust to communication varies depending on the social configuration.

In the case of low fractal levels where there is a rich domain language mediating communication, scientists can make some types of judgements on the work of their peers, even when they have a different contributory expertise. Geochemists, for example, can assess whether an assemblage count carried out by a micropaleontologist agrees with the rest of the literature, whether the interpretation of the micropaleontological data fits within the shared understanding of the paleoceanographic community of how the Earth system works, and so on. They cannot however make judgements related to the very process of picking and counting microfossils, because this is not their field of expertise. They therefore may either trust or distrust the expertise of the micropaleontological community. Trust, in this case, comes into play when knowledge production is black-boxed and different contributory experts only have access to the final products of research projects.

The situation is similar in medium and high fractal levels in which interactional expertise mediates communication between different expert communities. Paleoceanographers, for instance, can make judgements as to whether model outputs match the paleoceanographic literature or whether a given model has represented all relevant variables in a given run. Conversely, computer models can also make judgements as to whether data generated by a group of paleoceanographers meet the standard interpretation of climatic processes and as to whether their interpretation of data is sound. There is, however, an element of trust in the interactions of paleo-modellers and data people. Data people, in general are not knowledgeable about models' codes and setup. They can only collaborate and exchange knowledge with paleo-modellers if they trust the expertise of the modelling community. The same is also true in cases in which paleo-modellers use paleoceanographic data. Paleo-modellers who have acquired interactional expertise in certain data-generation techniques understand the principles behind the data production done with these techniques. However, they usually do not follow the entire data literature, only the main trends related to the climatic processes they are interested in. For this reason, they have to trust the judgements of data people when it comes to data production.

In medium or high fractal levels in which there are no bridge-building mechanisms between different expert communities at work, trust plays a dominant role when knowledge travels from one community to other. In climate-change science as a whole, for example, there are research areas whose expertises are significantly different from each other. Experts as diverse as anthropologists studying the impacts of climate change on vulnerable communities, atmospheric physics researching the role of clouds in the climate system, and architects working on low-carbon building, are part of the same domain. If members of these communities are to collaborate in a research project or to use knowledge produced in one of the fields which is not their own, but they have not acquired any interactional expertise in the other domains, they will not be able to assess the knowledge produced in the other communities on the basis of their own expertise. They will have to trust the judgements and the expertise of the members of the other communities.

Only in domains with no social division of labour is trust not part of social interactions. All accomplished snooker players, for instance, can make informed judgements about each other's activities. There are no black-boxed activities that different members of these communities cannot make judgements on. In this sense, trust is an essential mediator of interactions between different social groups in domains where there is specialisation.

Other Models of Communication and Collaboration

In the present work I have mainly used Collins and Evans's theoretical framework (Collins and Evans 2007; Collins 2010, 2011) to examine communication within paleoceanography and bridge-building mechanisms between paleoceanographers and other climate-change scientists. I have combined the notions of expertise and trading zones (Collins et al. 2007) to work out how experts in paleoceanography collaborate, communicate, and exchange knowledge. This does not mean that these concepts can explain the whole complexity of climate-change science as a whole and of the general problem of communication and collaboration between different expert communities.

In the introduction and in chapter 3 of the present work I have mentioned some other important STS concepts that may also shed light on how different experts interact to produce knowledge on climate change. One of them is boundary objects. Boundary objects are objects, concepts, places, and so on, that are at the intersection of different social words and are flexible enough to be interpreted in different ways by members of different social groups (Star and Griesemer 1989). There are studies in the STS literature in which computer-model parameterisations and remote-sensing technology were identified as boundary objects that facilitated the interactions between different expert communities in climate-change science (Sundberg 2007; Kwa 2005). During my fieldwork I found some preliminary evidence that there are boundary objects operating in paleoceanography. For instance, mass spectrometers and proxy data have different meanings in different sub-communities of paleoceanography. Geochemists, for example, have as an ultimate research goal the development of very precise and accurate measurements. They usually also apply these measurements to advance the understanding of paleoceanographic issues, but their ultimate goal is to develop better measurement techniques. Most paleoceanographers, on the other hand, make a much more instrumental use of mass spectrometers and proxy data. They also want to make precise and accurate measurements, but their ultimate goal is to produce data that can help them address questions related to past climates. It is also likely that computer models and some concepts are also boundary objects in paleoceanography, such as the ocean conveyor belt, climate sensitivity, etc.

It is important to bear in mind, however, that these boundary objects have much less importance in paleoceanography and in the interactions between paleoclimatologists and paleo-modellers than, respectively, paleoceanography's domain language, and the interactional expertise in each other's field acquired by paleo-modellers and paleoclimatologists. Boundary objects certainly help minimise 'friction' between and within these communities, but as there are linguistic bridges mediating the communication of their members, boundary objects play a much smaller role in their interactions than in settings in which there is not as much mutual understanding between different social groups (e.g. Sundberg 2006)

Besides the bridge-building mechanisms mentioned above I have also examined mechanisms of homogenisation of expert communities, such as translation (e.g. Callon 1986; Latour 1987) and standardisation (e.g. Jasanoff and Wynne 1998; Lampland and Star 2009; Edwards 2010). These mechanisms, instead of bridging the gaps between different expert communities, reduce the heterogeneity between them. The notion of translation as it is defined by proponents of actor-network theory consists of the process through which particular groups of actants create a certain convergence of interests in a given network so that other actants align their interests with theirs. I have argued that this mechanism is relevant for understanding how communication and collaboration come about in heterogeneous fields of science. If different groups of experts do not have similar interests it is very unlikely that they will ever collaborate (see Sundberg 2006; 2007 for an example of this). I have, however, pointed out that the hyper-symmetry principle put forward by proponents of actor-network theory (e.g. Callon 1986; Law 1986; Latour 1991, 2005) should be abandoned if one wants to understand how heterogeneous groups of experts communicate and collaborate. This is because by equating humans and non-humans this principle prevents us from examining crucial sociological phenomena, such as tacit knowledge (Collins 2012), and the very idea of heterogeneity is lost.

The notion of standardisation is also very useful for understanding how heterogeneous groups of experts communicate. It is particularly useful for reducing instrument heterogeneity so that it becomes easier for data, methods, procedures, measurements, and so forth, to travel through global inter and multidisciplinary networks. In climate-change science, a number of data collection procedures and techniques have been standardised and a range of data processing techniques were developed so that standardised data sets that can be readily fed into models are generated. In chapter 3 I have described how meteorological data have became standardised through the development of a number of international data collection and data sharing programmes as well as of a number of models to process the data collected (Edwards 2001; 2010). I have also found examples of standardisation in paleoceanography. An example of this is the development of international standards that are used in mass spectrometry. These standards are samples of known chemical composition that are frequently run through mass spectrometers to check whether these machines are working correctly and providing accurate results. There probably are several other standards being currently used in paleoceanography and in climate-change science which help reduce communication issues and knowledge exchange between different groups of experts. Standardisations, however, have not homogenised climate-change science so that it has become a homogeneous transdisciplinary field of investigation (Jasanoff and Wynne 1998; Edwards 2001). The international standardisation programmes have experienced several difficulties when the standards were locally applied (Edwards 2010). As Star and Lampland (2009) have pointed out, standards are never uniformly applied across different locations. For this reason, although they might reduce 'data friction' (Edwards 2010), they do not decrease the relevance of bridge-building mechanisms. For communication to occur it is essential that these mechanisms work on top of these homogenisation mechanisms.

Final Remarks

The overall question set out in the introduction of this work was how different types of experts communicate and collaborate to produce knowledge on climate change. As pointed out above, there are several ways in which experts bridge the gaps between different fields of expertise, such as inter-languages, interactional expertise, boundary objects, and trust. These mechanisms are also sometimes combined with homogenisation mechanisms, such as translation and standardisation. The findings of this thesis are useful for understanding how and where domain languages, interactional expertise trading zones, and trust come into play to mediate communication and how these mechanisms can be linked to the fractal level.

In this conclusion I have also attempted to make some tentative generalisations about these bridge-building mechanisms, which included pointing out that in low-fractal-levels domain languages are the main bridge-building mechanisms; linking trading zones – be they fractionated or inter-language - to medium and high fractal levels; and specifying the different roles played by trust in different fractal levels. These patterns might also be found in other fields of climate-change science and in other fields of science. In fields where the links between different types of experts are looser than in paleoceanography, it is likely that boundary objects will play a much more important role than linguistic bridge-building mechanisms. Further research, however, needs to be undertaken so as to clarify what patterns of interaction are predominant in other areas of climate-change science.

Appendices

Appendix A: Examples of Interview Schedules

Example 1: Topics covered during interviews with paleoceanographers at the University of Cambridge and at the University of Oxford.

. Interviewees’ academic background.

. Interviewees’ research interests (in paleoclimate/paleoceanography).

. What archives, proxies, organisms, do the interviewees work on?

. Stages of a research in a project carried out in the interviewees’ area.

. Skills needed in the interviewees’ research area.

. Judgements made by interviewees in different stages of their research.

. How did the interviewees acquire the different skills they have.

. What the community do the interviewees belong to? Paleoceanography?.

. What kinds of journals do the interviewees read and publish in?

. What kinds of conference do the interviewees attend?

. Social division of labour in the interviewees’ research groups and research projects (other experts, students, technicians, etc.).

. Social division of labour in papers published by the interviewees’ (concrete examples).

. Particular characteristics of the interviewees’ universities and of other research centres in the UK and abroad.

. Major countries in terms of production of paleoclimatic knowledge.

. Interactions between the interviewees and other experts (e.g. modellers, other paleoclimatologists).

. How well informed the interviewees are about other proxies and archives and about climate models?

Example 2: Topics covered during interviews with technicians at Cardiff University

. Interviewees’ background:

. What are the interviewees’ main tasks?

. What stages of paleoceanographic/paleoclimatic research are the interviewees involved with?

. How well informed are the interviewees about the literature and about other stages of research?

. How well informed are the interviewees about other fields of science (other proxies, computer modelling)?

.What kinds of skills are needed in the interviewees work?

. Have the interviewees ever published scientific papers? Why?

. What are the interviewees’ motivations for doing this job?

. Do the interviewees often read scientific journals?

. Do the interviewees attend scientific conferences?

. Who do the interviewees work with? What is the social division of labour within the interviewees’ research group.

. What are the interviewees’ career plans?

Appendix B: Domain-Language-Based Judgements: Choosing Samples for Paleoceanographic Research in the Mediterranean Region

In this appendix I provide a more detailed example of domain-language-based contributions. I further discuss the example provided in chapter 5 on the need to understand how the Earth system works for one to be able to choose samples for paleoceanographic research. In chapter 5 I have presented the following quotation of a paleoceanographer explaining why people have to be able to link lithology to how the Earth system works to be able to select samples correctly:

> Wendy: For instance this meeting I was at last week in Salamanca, one of the great criticisms that the Dutch group had, the Dutch group do a lot of the astrochronology, the time scale of the thing of the Mediterranean, very, very effectively. And there was an Italian group and one of the criticisms that the Dutch group has done is that they just take sample every 5 centimetres, whatever, they don't pay any attention to the lithology. And one of the things is that the Mediterranean is very sensitive to climate change because it's surrounded by land, actually it gives an amplified climate signal. And one of the results of that is that there's a lithological response to climate variation and it's absolutely amazing. You can correlate bed by bed right across the Mediterranean back to 10 million years ago. It's incredible. To find those cycles, they are obvious when you look at the cliff. But they do vary in thickness, because sedimentation rates changes, and because all sorts of things change. If you take samples every 5 centimetres with no bearing on those lithological cycles, then actually you may well not pick them up (laughter) because it's not that they are not there, but the periodicity with your data is kind of you haven't paid any attention to where the cycles are. And that's what the Italians were doing. And funnily enough they are in real trouble and doing all sorts of bizarre things. But they were not getting out cycles in a section where they were clearly there. So I think in answer to your question if you do blind sampling you may end up with rubbish. You need to know about the samples you take. You need to understand the system. […]. You have to know what you're looking for or you don't see anything.

In this quotation, Wendy points out an important characteristic of the Mediterranean: it is surrounded by land, therefore, climatic signals are amplified there. She then links this to the lithology of sediments collected in this sea. She argues that you find the same sequences of sedimentary layers from the last 10 million years in outcrops and sedimentary cores collected across the Mediterranean area. These lithological sequences respond to climatic variations. However, the width of the rock beds varies because sedimentation rates vary in different areas. This means that, for instance, sediments from 5 million years ago might have formed a thicker bed in a site than in others. For this reason, although the same climatic signals are recorded across the Mediterranean, they are expressed in sedimentary formations that have beds with different thickness. I will elaborate on this point to explain how changes in the Earth system can influence the formation of sediments.

An example of sedimentary formation in the Mediterranean seafloor is sapropel, which are dark-coloured sedimentary layers rich in organic matter[69]. They are found in the Mediterranean seafloor in between layers of sediments with lower amounts of organic matter, such as marlstones and limestones. The deposition of these layers of sapropel on the Mediterranean seafloor is associated with a number of mechanisms of change in the Earth system.

Organic matter is either transported by rivers to the oceans or is produced by organisms that photosynthesise in the sea-surface area. When this organic matter sinks and reaches the seafloor it is usually consumed by living organisms that live in deep waters. If it is being buried in the seafloor this is considered by the paleoceanographic community as an indicator that the deep waters were anoxic. For this reason, living organisms cannot live there and consume organic matter, which, in turn, ends up being buried.

Oxygen is usually brought to deep waters through the sinking of surface waters, which are oxygenated through exchanges with the atmosphere. These ventilated surface waters sink because of changes in density gradients between them and deeper waters. Water density is basically influenced by two variables: temperature and salinity. Cold waters are denser than warm waters. Waters with large amounts of salt diluted are denser than waters with smaller amounts of salt.

The mechanism that influences the sinking of surface waters can be explained as follows. Surface waters in the Mediterranean originate from the Atlantic Ocean and get through the Gibraltar Strait. Once they flow eastwards in the summer, water evaporates because of the high temperatures causing a rise in salinity. High temperatures offset the

[69] This entire explanation on the formation of sapropel in the Mediterranean seafloor is based on Rohling (2001), who in a very digestible text summarises a great body of literature on this topic.

increase in salinity so that these waters do not become denser than deeper waters. When the summer ends and temperature drops, however, water density rises and it sinks. However, it does not immediately reach the deeper layers of the water column. Firstly, these waters sink to an intermediate layer between sea-surface waters and deep waters.

The mechanism that makes intermediate waters sink relates to North Mediterranean waters. These waters do not have a great increase in salinity in the summer because of higher precipitation in this area. However, when the summer ends, cold air masses coming from the continent cool this water down to much colder temperatures than in the southern areas of the Mediterranean. Because of this cooling, water becomes dense, sinks, and reaches the intermediate layer. These northern waters have different salinity and temperature from the Eastern Mediterranean waters that sank because of high salinity. When water masses with different properties but similar density mixes the result are waters with higher density than the original water masses. These intermediate waters then sink and bring oxygenated waters to deep waters.

The formation of sapropel is attributed to an intensification of African Monsoons. Approximately every 21,000 years there is a monsoonal maxima, which is a response to orbital cycles that influence the incidence of solar radiation on the planet. During a monsoonal maximum, there is an intensification of precipitation in Northeastern Africa, which results in an increase in the Nile's water volume. Consequently, there is a growth in the fresh water discharge in the Mediterranean. This input of freshwater prevents seawater in the Eastern Mediterranean from increasing its salinity and consequently sinking. As we have seen, the mechanism is made up of two components: the increase in salinity in the Eastern Mediterranean and the cooling of northern Mediterranean waters. Without the salinity mechanism, the temperature mechanism is not strong enough to make surface waters sink beyond the intermediate water layers. As a consequence no oxygen reaches the deep sea and deep waters become depleted in oxygen. As there is no oxygen, most living organisms that would consume the organic matter sinking from surface waters die. Organic matter is then buried in the seafloor during these periods and form sapropel layers.

When paleoceanographers look at marine sediments from the Mediterranean area, they do not only see different types of sediments, such as sapropels and limestones. Through the lithology they see complicated Earth system processes involving organic matter, ocean circulation, fresh water discharge, monsoons, orbital cycles, etc. This information underlies their judgements on sampling, as it gives them a sense of where specific climatic cycles are found in sedimentary formations. Judgements on sampling are therefore based on mastering the language of paleoceanography.

List of References

Almklov, P. G. and Hepsø, V. 2011. Between and beyond data: How analogue field experience informs the interpretation of remote data sources in petroleum reservoir geology. *Social Studies of Science* 41(4), pp. 539-561.

Anderson, W. and Adams, V. 2007. Pramoedya's chickens: Postcolonial studies of technoscience. In: Hackett, E.J. et al. eds. *The Handbook of Science and Technology Studies* Cambridge, Massachusetts and London, England: MIT Press, pp. 181-204.

Ashmore, M. 1989. *The Reflexive Thesis: Wrighting Sociology of Scientific Knowledge*. Chicago: University of Chicago Press.

Atkinson, R. and Flint, J. 2001. Accessing Hidden and Hard-to-Reach Populations: Snowball Research Strategies. *Social Research Update* (33).

Barley, S. R. 1996. Technicians in the workplace: Ethnographic evidence for bringing work into organizational studies. *Administrative Science Quarterly* 41(3), pp. 404-441.

Barley, S. R. and Bechky, B. A. 1994. In the backrooms of science. *Work and occupations* 21(1), pp. 85-126.

Barnes, B. 1982. *T.S. Kuhn and social science*. New York: Columbia University Press

Barnes, B. et al. 1996. *Scientific Knowledge: A Sociological Analysis*. Chicago: University of Chicago Press.

Barnes, B. and Edge, D. 1982. Introduction. In: Barnes, B. and Edge, D. eds. *Science in Context: Readings in the Sociology of Science*. Milton Keynes: The Open University Press, pp. 233-249.

Ben-David, J. 1960. Scientific Productivity and Academic Organization in Nineteenth Century Medicine. *American Sociological Review* 25(6), pp. 828-843.

Berger, P. 1963. *Invitation to Sociology: A Humanistic Perspective*. New York: Doubleday.

Biernacki, P. and Waldorf, D. 1981. Snowball Sampling: Problems and Techniques of Chain Referral Sampling. *Sociological Methods and Research* 10(2), pp. 141-163.

Bijker, W. E. et al. 1987. *The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology*. Cambridge, Massachusetts: The MIT Press.

Bloor, D. 1976. *Knowledge and social imagery*. 1991 ed. Chicago: University of Chicago Press.

Bloor, D. 1983. *Wittgenstein: A Social Theory of Knowledge*. London and Basingstoke: The Macmillan Press Ltd.

Bloor, D. 1992. Left and Right Wittgensteinians In: Pickering, A. ed. *Science as Practice and Culture*. Chicago and London: The University of Chicago Press.

Bloor, D. 1997. *Wittgenstein, Rules and Institutions*. London and New York: Routledge Chapman & Hall.

Bloor, D. and Edge, D. 2000. For the Record: Knowing Reality Through Society. *Social Studies of Science* 30(1), pp. 158-160.

Boyce, T. 2006. Journalism and Expertise. *Journalism Studies* 7(6), pp. 889-906.

Bradley, R. S. 1999. *Paleoclimatology: reconstructing climates of the Quaternary*. San Diego: Academic Press.

Broecker, W. 1992. Global Warming on Trial. *Natural History* 101(April), pp. 6-14.

Brown, P. R. 2009. The phenomenology of trust: A Schutzian analysis of the social construction of knowledge by gynae-oncology patients. *Health, Risk & Society* 11(5), pp. 391-407.

Burroughs, W. J. 2001. *Climate change: A multidisciplinary approach*. New York: Cambridge University Press.

Callon, M. 1986. Some elements of a sociology of translation: domestication of the scallops and the fishermen of St Brieuc Bay. In: Law, J. ed. *Power, action and belief: A new sociology of knowledge*. London: Routledge, pp. 196-233.

Carr, E. S. 2010. Enactments of expertise. *Annual Review of Anthropology* 39, pp. 17-32.

Caseldine, C. J. et al. 2010. IPCC and palaeoclimate – an evolving story? *Journal of Quaternary Science* 25(1), pp. 1-4.

Charlesworth, M. et al. 1989. *Life Among the Scientists: An Anthropological Study of an Australian Scientific Community* Melbourne: Oxford University Press.

Clarke, A. and Star, S. L. 2008. The social worlds framework: A theory/methods package. In: Hackett, E.J. et al. eds. *The Handbook of Science & Technology Studies*. Cambridge, Massachusetts and London: The MIT Press.

Coffey, A. and Atkinson, P. 1996. *Making Sense of Qualitative Data: Complementary Research Strategies*. Thousand Oaks, London, and New Delhi: Sage Publications.

Collins, H. M. 1974. The TEA set: Tacit knowledge and scientific networks. *Science Studies* 4(2), pp. 165-185.

Collins, H. M. 1975. The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics. *Sociology* 9(2), pp. 205-224.

Collins, H. M. 1981. Introduction: Stages in the Empirical Programme of Relativism. *Social Studies of Science* 11(1), pp. 3-10.

Collins, H. M. 1984. Researching Spoonbending: Concepts and Practise of Participatory Fieldwork. In: Bell, C. and Roberts, H. eds. *Social researching: politics, problems, practise*. London: Routledge and Kegan Paul, pp. 54-69.

Collins, H. M. 1985. *Changing order: Replication and Induction in Scientific Practice*. 1992 ed. Chicago: University of Chicago Press.

Collins, H. M. 2001. Tacit knowledge, trust and the Q of sapphire. *Social Studies of Science* 31(1), pp. 71-85.

Collins, H. M. 2004a. *Gravity's Shadow: the Search for Gravitational Waves*. Chicago: University of Chicago Press.

Collins, H. M. 2004b. How Do You Know You've Alternated? *Social Studies of Science* 34(1), pp. 103-106.

Collins, H. M. 2009. Walking the Talk: Doing Gravity's Shadow. In: Puddephatt, A.J. et al. eds. *Ethnographies Revisited* London and New York: Routledge pp. 289-304.

Collins, H. M. 2010. *Tacit and explicit knowledge*. Chicago and London: University of Chicago Press.

Collins, H. M. 2011. Language and Practice. *Social Studies of Science* 41(2), pp. 271-300.

Collins, H. M. 2012. Performances and Arguments. *Metascience* 21(2), pp. 409-418.

Collins, H. M. and Evans, R. 2002. The third wave of science studies: studies of expertise and experience. *Social Studies of Science* 32(2), pp. 235-296.

Collins, H. M. and Evans, R. 2007. *Rethinking expertise*. Chicago and London: University of Chicago Press.

Collins, H. M. and Evans, R. forthcoming. Quantifying the Tacit: The Imitation Game and Social Fluency. *Sociology*.

Collins, H. M. et al. 2007. Trading Zones and Interactional Expertise. *Studies in History and Philosophy of Science Part A* 38(4), pp. 657-666.

Collins, H. M. et al. 2006. Experiments with interactional expertise. *Studies In History and Philosophy of Science Part A* 37(4), pp. 656-674.

Collins, H. M. and Kusch, M. 1998. *The Shape of Actions: What Humans and Machines Can Do*. Cambridge, Massachusetts and London: The MIT Press.

Collins, H. M. and Pinch, T. J. 2005. *Dr. Golem: How to Think about Medicine*. Chicago and London: The University of Chicago Press.

Collins, H. M. and Sanders, G. 2007. They give you the keys and say `drive it!' Managers, referred expertise, and other expertises. *Studies In History and Philosophy of Science Part A* 38(4), pp. 621-641.

Collins, H. M. and Yearley, S. 1992. Epistemological Chicken. In: Pickering, A. ed. *Science as Practice and Culture*. Chicago and London: The University of Chicago Press.

Cooke, S. and Rohling, E. J. 2001. Stable isotopes in foraminiferal carbonate. *Southampton Oceanography Centre Internal Document* (72).

De Vries, G. 2007. What is Political in Sub-politics? *Social Studies of Science* 37(5), pp. 781-809.

DeConto, R. M. et al. 2008. Thresholds for Cenozoic bipolar glaciation. *Nature* 455(7213), pp. 652-656.

Delamont, S. 1992. *Fieldwork in Educational Settings: Methods, Pitfalls and Perspectives*. London: Falmer.

Delamont, S. 2004. Ethnography and Participant Observation. In: Seale, C. et al. eds. *Qualitative Research Practice*. London, Thousand Oaks, and New Delhi: Sage Publications.

Delamont, S. 2012. 'Traditional' Ethnography: Peopled Ethnography for Luminous Description. In: Delamont, S. ed. *Handbook of Qualitative Research in Education*. Cheltenham and Northampton, Massachusetts: Edward Elgar.

Demeritt, D. 2001. The Construction of Global Warming and the Politics of Science. *Annals of the Association of American Geographers* 91(2), pp. 307-337.

Denzin, N. K. 1995. The Experiential Text and the Limits of Visual Understanding. *Educational Theory* 45(1), pp. 7-18.

Doing, P. 2004. `Lab Hands' and the `Scarlet O': Epistemic Politics and (Scientific) Labor. *Social studies of science* 34(3), pp. 299-323.

Doing, P. 2008. Give Me a Laboratory and I Will Raise a Discipline: The Past, Present, and Future Politics of Laboratory Studies in STS. In: Hackett, E.J. et al. eds. *The Handbook of Science and Technology Studies*. Third ed. Cambridge, Massachusetts and London: The MIT Press.

Edgar, K. M. et al. 2007. No extreme bipolar glaciation during the main Eocene calcite compensation shift. *Nature* 448(7156), pp. 908-911.

Edwards, P. N. 2001. Representing the global atmosphere: computer models, data, and knowledge about climate change. In: Miller, C.A. and Edwards, P.N. eds. *Changing the atmosphere: Expert knowledge and environmental governance*. Cambridge, Massachusetts: The MIT Press, pp. 31–65.

Edwards, P. N. 2010. *A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming*. Cambridge, Massachusetts: The MIT Press.

Edwards, P. N. et al. 2011. Science friction: Data, metadata, and collaboration. *Social Studies of Science* 41(5), pp. 667-690.

Eldrett, J. S. et al. 2009. Increased seasonality through the Eocene to Oligocene transition in northern high latitudes. *Nature* 459(7249), pp. 969-973.

Epstein, S. 1995. The Construction of Lay Expertise: AIDS Activism and the Forging of Credibility in the Reform of Clinical Trials. *Science, Technology & Human Values* 20(4), pp. 408-437.

Faulkner, W. 2007. `Nuts and Bolts and People': Gender-Troubled Engineering Identities. *Social Studies of Science* 37(3), pp. 331-356.

Feyerabend, P. 1993. *Against Method*. London and New York: Verso Books.

Fischer, F. 2009. *Democracy and expertise: Reorienting policy inquiry*. Oxford: Oxford University Press.

Fleck, L. 1935. *Genesis and development of a scientific fact*. 1981 ed. Chicago: The University of Chicago Press.

Forsey, M. 2010. Ethnography as Participant Listening. *Ethnography* 11(4), pp. 558-572.

Forsey, M. 2012. Interviewing Individuals. In: Delamont, S. ed. *Handbook of Qualitative Research in Education*. Cheltenham and Northampton, Massachusetts: Edward Elgar.

Frodeman, R. 1995. Geological reasoning: Geology as an interpretive and historical science. *Geological Society of America Bulletin* 107(8), pp. 960-968.

Frodeman, R. 2003. *Geo-logic: Breaking Ground Between Philosophy and the Earth Sciences*. Albany: State University of New York Press.

Fu, D. 2007. How Far Can East Asian STS Go? *East Asian Science, Technology and Society: an International Journal* 1(1), pp. 1-14.

Fujimura, J. H. 1992. Crafting Science: Standardized Packages, Boundary Objects, and “Translation”. In: Pickering, A. ed. *Science as Practice and Culture*. Chicago and London: The University of Chicago Press, pp. 168-211.

Galison, P. 1989. The Trading Zone: The Coordination of Action and Belief. Preprint for TECH-KNOW Workshops on Places of Knowledge, their Technologies and Economies. In. UCLA Center for Cultural History of Science and Technology, Los Angeles.

Galison, P. 1996. Computer Simulations and the Trading Zone. In: Galison, P. and Stump, D.J. eds. *The Disunity of Science: Boundaries, Contexts, and Power*. Stanford: Stanford University Press.

Galison, P. 1997. *Image and Logic: a Material Culture of Microphysics*. Chicago: University of Chicago Press.

Galison, P. 2010. Trading with the Enemy. In: Gorman, M. ed. *Trading Zones and Interactional Expertise*. Cambridge, Massachusetts and London: The MIT Press.

Galison, P. and Stump, D. J. 1996. *The Disunity of Science: Boundaries, Contexts, and Power*. Standford: Stanford University Press.

Giddens, A. 1990. *The Consequences of Modernity*. Cambridge: Polity Press.

Gieryn, T. F. 1983. Boundary-work and the demarcation of science from non-science: Strains and interests in professional ideologies of scientists. *American sociological review*, pp. 781-795.

Gilbert, G. N. and Mulkay, M. 1984. *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse*. Cambridge: Cambridge University Press.

Giles, J. 2006. Sociologist fools physics judges. *Nature* 442(7098), p. 8.

Glesne, C. 1997. That Rare Feeling: Re-presenting Research Through Poetic Transcription. *Qualitative Inquiry* 3(2), pp. 202-221.

Goodwin, C. 1995. Seeing in Depth. *Social Studies of Science* 25(2), pp. 237-274.

Gorman, M. E. 2002. Levels of Expertise and Trading Zones: A Framework for Multidisciplinary Collaboration. *Social Studies of Science* 32(5-6), pp. 933-938.

Grundmann, R. 2013. “Climategate” and The Scientific Ethos. *Science, Technology & Human Values* 38(1), pp. 67-93.

Hacking, I. 1992. The self-vindication of the laboratory sciences. In: Pickering, A. ed. *Science as practice and culture*. Chicago and London: The University of Chicago Press, pp. 29-64.

Hammersley, M. 2010. Reproducing or Constructing? Some Questions about Transcription in Social Research. *Qualitative Research* 10(5), pp. 553-569.

Hedgecoe, A. M. 2012. Trust and regulatory organisations: The role of local knowledge and facework in research ethics review. *Social Studies of Science* 42(5), pp. 662-683.

Hess, D. 2001. Ethnography and the Development of Science and Technology Studies. In: Atkinson, P. et al. eds. *Handbook of Ethnography*. London, Thousand Oaks and New Delhi: Sage Publications.

Hess, D. J. 1994. *Samba in the night: spiritism in Brazil*. New York: Columbia University Press.

Hildebrandt, M. and Gutwirth, S. 2008. Public Proof in Courts and Jury Trials. *Science, Technology & Human Values* 33(5), pp. 582-604.

Hong, W. 2008. Domination in a Scientific Field: Capital Struggle in a Chinese Isotope Lab. *Social Studies of Science* 38(4), pp. 543-570.

Hulme, M. and Mahony, M. 2010. Climate change: What Do We Know about the IPCC? *Progress in Physical Geography* 34, pp. 705-718.

Hulme, M. et al. 2010. IPCC: cherish it, tweak it or scrap it. *Nature* 463(7282), pp. 730-732.

Idso, C. D. and Singer, S. F. 2009. *Climate Change Reconsidered: 2009 Report of the Nongovernmental International Panel on Climate Change (NIPCC)*. Chicago, IL: Heartland Institute.

IPCC. 2007a. Climate change 2007: the physical science basis: contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change. In: Solomon, S. et al. eds. *IPCC Fourth Assessment Report: Climate Change 2007*. Cambridge, United Kingdon and New York: Cambridge University Press.

IPCC. 2007b. Summary for Policymakers. In: Metz, B. et al. eds. *Climate Change 2007: Mitigation. Contribution of Working Group III to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change*. Cambridge, United Kingdom, and New York: Cambridge University Press.

Jacques, P. J. et al. 2008. The organisation of denial: Conservative think tanks and environmental scepticism. *Environmental Politics* 17(3), pp. 349-385.

Jasanoff, S. 1997. *Science at the Bar: Law, Science, and Technology in America*. Cambridge, Massachusetts: Harvard University Press.

Jasanoff, S. 2003. Breaking the Waves in Science Studies: Comment on H.M. Collins and Robert Evans, `The Third Wave of Science Studies'. *Social Studies of Science* 33(3), pp. 389-400.

Jasanoff, S. and Wynne, B. 1998. Science and decisionmaking. In: Rayner, S. and Malone, E.L. eds. *Human choice & climate change*. Vol. I: The societal framework. Columbus: Battelle Press, pp. 1-87.

Johnson, P. C. 2002. *Secrets, Gossip, and Gods: The Transformation of Brazilian Candomblé*. Oxford and New York: Oxford University Press.

Knorr-Cetina, K. 1981. *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science*. New York: Pergamon Press.

Knorr-Cetina, K. 1995. Laboratory Studies: The Cultural Approach to the Study of Science. In: Jasanoff, S. et al. eds. *Handbook of Science and Technology Studies*. First ed. Thousand Oaks, London and New Delhi: Sage Publications.

Knorr-Cetina, K. 1999. *Epistemic cultures: How the Sciences Make Knowledge*. Cambridge, Massachusetts Harvard University Press.

Kreimer, P. 2007. Social Studies of Science and Technology in Latin America: A Field in the Process of Consolidation. *Science Technology & Society* 12(1), pp. 1-9.

Kuhn, T. S. 1962. *The structure of scientific revolutions*. Chicago: University of Chicago press.

Kuhn, T. S. 1974. Second thoughts on paradigms. *The structure of scientific theories*, pp. 459-482.

Kuhn, T. S. 1977. *The Essential Tension. Selected Studies in Scientific Tradition and Change*. Chicago: University of Chicago Press.

Kump, L. R. 2005. Foreshadowing the glacial era. *Nature* 436(7049), pp. 333-334.

Kvale, S. 1996. *InterViews: An Introduction to Qualitative Reseach Interviewing*. Thousand Oaks: Sage.

Kwa, C. 2005. Local Ecologies and Global Science: Discourses and Strategies of the International Geosphere-Biosphere Programme. *Social Studies of Science* 35(6), pp. 923-950.

Lahsen, M. 2005. Seductive simulations? Uncertainty distribution around climate models. *Social Studies of Science* 35(6), pp. 895-922.

Lampland, M. and Star, S. L. 2009. *Standards and their stories: How quantifying, classifying, and formalizing practices shape everyday life*. Ithaca and London: Cornell University Press.

Latour, B. 1987. *Science in action: How to follow scientists and engineers through society*. Milton Keynes: Open University Press.

Latour, B. 1991. *We've Never Been Modern*. Cambridge, Massachusetts: Harvard University Press.

Latour, B. 2005. *Reassembling the Social: An Introduction to Actor-Network-Theory*. Oxford: Oxford University Press.

Latour, B. and Woolgar, S. 1979. *Laboratory life: The construction of scientific facts*. 1986 ed. Princeton: Princeton University Press.

Law, J. 1980. Fragmentation and Investment in Sedimentology. *Social Studies of Science* 10(1), pp. 1-22.

Law, J. 1986. On Power and its Tactics: a View from the Sociology of Science. *The Sociological Review* 34(1), pp. 1-38.

Lear, C. H. et al. 2004. Late Eocene to early Miocene ice sheet dynamics and the global carbon cycle. *Paleoceanography* 19(4), pp. 1-11.

Lisiecki, L. E. and Raymo, M. E. 2005. A Pliocene-Pleistocene stack of 57 globally distributed benthic δ18O records. *Paleoceanography* 20(1), p. PA1003.

Luhmann, N. 2000. Familiarity, confidence, trust: Problems and perspectives. In: Gambetta, D. ed. *Trust: making and breaking cooperative relations*. Electronic edition. Oxford: Department of Sociology, University of Oxford. pp. 94-107. Available at:

http://www.sociology.ox.ac.uk/papers/luhmann194-107.pdf. [Accessed: June 122, 2012].

Lynch, M. 1992. Extending Wittgenstein: The Pivotal Move from Epistemology to the Sociology of Science. In: Pickering, A. ed. *Science as Practice and Culture*. Chicago and London: The Chicago University Press.

Lynch, M. 2003. Editorial. *Social Studies of Science* 33(3), p. 325.

Lynch, M. and Cole, S. 2005. Science and Technology Studies on Trial: Dilemmas of Expertise. *Social Studies of Science* 35(2), pp. 269-311.

MacKenzie, D. 1978. Statistical theory and social interests. *Social Studies of Science* 8(1), pp. 35-83.

Malinowski, B. 1964. *Argonauts of the Western Pacific*. London: Routledge/Kegan Paul.

Marie, J. 2008. For Science, Love and Money: The Social Worlds of Poultry and Rabbit Breeding in Britain, 1900—1940. *Social Studies of Science* 38(6), pp. 919-936.

Mason, J. 2002. *Qualitative Researching*. London, Thousand Oaks and New Delhi: Sage Publications.

Matoesian, G. M. 1999. The Grammaticalization of Participant Roles in the Constitution of Expert Identity. *Language in Society* 28(4), pp. 491–521.

McCave, I. N. and Elderfield, H. 2011. Sir Nicholas John Shackleton. 23 June 1937-24 January 2006. *Biographical Memoirs of Fellows of the Royal Society* 57, pp. 435-462.

McCright, A. M. and Dunlap, R. E. 2000. Challenging global warming as a social problem: An analysis of the conservative movement's counter-claims. *Social Problems* 47(4), pp. 499-522.

McCright, A. M. and Dunlap, R. E. 2003. Defeating Kyoto: The conservative movement's impact on US climate change policy. *Social Problems* 50(3), pp. 348-373.

Merton, R. K. 1938. Science, Technology and Society in Seventeenth Century England. *Osiris* 4, pp. 360-632.

Miller, C. A. 2004. Climate Science and the Making of a Global Political Order. In: Jasanoff, S. ed. *States of knowledge: the Co-Production of Science and the Social Order*. London: Routledge, pp. 46-66.

Moore, A. 2010. Review: Beyond participation: Opening up political theory in STS: Mark Brown, Science in Democracy: Expertise, Institutions and Representation. *Social Studies of Science* 40(5), pp. 793-799.

Moore, F. C. 2011. Costing Adaptation: Revealing Tensions in the Normative Basis of Adaptation Policy in Adaptation Cost Estimates. *Science, Technology & Human Values*.

Neurath, O. et al. 1973. The Scientific Conception of the World: The Vienna Circle. In: Neurath, M. and Cohen, R.S. eds. *Empiricism and sociology*. Boston: D. Reidel Publishing Company, pp. 299-318.

Noller, J. S. et al. eds. 2000. *Quaternary Geochronology: Methods and Applications.* Washington, DC: AGU.

O'Riordan, T. and Jäger, J. 1996. The History of Climate Change Science and Politics. In: Jäger, J. and O'Riordan, T. eds. *The Politics of Climate Change. A European Perspective*. London and New York: Routledge, pp. 1-31.

O’Reilly, J. et al. 2012. The rapid disintegration of projections: The West Antarctic Ice Sheet and the Intergovernmental Panel on Climate Change. *Social Studies of Science* 42(5), pp. 709-731.

Oreskes, N. 2004. The scientific consensus on climate change. *Science* 306(5702), p. 1686.

Oreskes, N. and Conway, E. M. 2010. *Merchants of doubt: How a handful of scientists obscured the truth on issues from tobacco smoke to global warming*. New York: Bloomsbury Press, p. 355.

Oreskes, N. et al. 1994. Verification, validation, and confirmation of numerical models in the earth sciences. *Science* 263(5147), pp. 641-646.

Oteri, J. S. et al. 1982. Cross-Examination in Drug Cases. In: Barnes, B. and Edge, D. eds. *Science in Context: Readings in the Sociology of Science*. Milton Keynes: Open University Press, pp. 250–259.

Pamplin, B. R. and Collins, H. M. 1975. Spoon Bending: An Experimental Approach. *Nature* 257(5521), p. 8.

Papadopoulos, D. 2011. Alter-ontologies: Towards a constituent politics in technoscience. *Social Studies of Science* 41(2), pp. 177-201.

Pearce, F. 2010. *The Climate Files: The Battle for the Truth about Global Warming*. London: Guardian Books.

Petit, J. R. et al. 1999. Climate and atmospheric history of the past 420,000 years from the Vostok ice core, Antarctica. *Nature* 399(6735), pp. 429-436.

Pickering, A. 1995. *The Mangle of Practice: Time, Agency, and Science*. Chicago and London: University of Chicago Press.

Pinch, T. J. 1986. *Confronting Nature: The Sociology of Solar-Neutrino Detection*. Dordrecht: D. Reidel Publishing Company.

Polanyi, M. 1966. *The Tacit Dimension*. New York: Doubleday & Company.

Popper, K. 1934. *The logic of scientific discovery*. 2002 ed. London: Routledge and Kegan Paul.

Rapley, T. 2004. Interviews. In: Seale, C. et al. eds. *Qualitative Research Practice*. London, Thousand Oaks, and New Delhi: Sage Publications.

Rayner, S. and Malone, E. L. eds. 1998. *Human Choice and Climate Change.* Columbus: Battelle Press.

Reyes-Galindo, L. 2011. *The Sociology of Theoretical Physics.* Cardiff University. Unpublised PhD Thesis.

Ribeiro, R. 2007a. *Knowledge Transfer*. Cardiff University. Unpublished PhD Thesis.

Ribeiro, R. 2007b. The language barrier as an aid to communication. *Social studies of science* 37(4), pp. 561-584.

Ribeiro, R. 2007c. The role of interactional expertise in interpreting: the case of technology transfer in the steel industry. *Studies In History and Philosophy of Science Part A* 38(4), pp. 713-721.

Ribeiro, R. forthcoming-a. Levels of Immersion, Tacit Knowledge and Expertise. *Phenomenology and the Cognitive Sciences.*

Ribeiro, R. forthcoming-b. Tacit knowledge management. *Phenomenology and the Cognitive Sciences*.

Rip, A. 2003. Constructing Expertise: In a Third Wave of Science Studies? *Social Studies of Science* 33(3), pp. 419-434.

Robinson, A. B. et al. 1998. Environmental Effects of Increased Atmospheric Carbon Dioxide. *Journal of American Physicians and Surgeons* 3, pp. 171-178.

Robinson, A. B. et al. 2007. Environmental effects of increased atmospheric carbon dioxide. *Journal of American Physicians and Surgeons* 12, pp. 79-90.

Rohling, E. J. 2001. *The dark secret of the Mediterranean - a case history in past environmental reconstruction* [Online]. Available at: http://www.soes.soton.ac.uk/staff/ejr/DarkMed/ref-cond.html [Accessed: 07/06/2012].

Rosenberger, R. 2011. A Case Study in the Applied Philosophy of Imaging: The Synaptic Vesicle Debate. *Science, Technology & Human Values* 36(1), pp. 6-32.

Roth, W.-M. 2005. Making Classifications (at) Work: Ordering Practices in Science. *Social Studies of Science* 35(4), pp. 581-621.

Rubin, H. J. and Rubin, I. S. 1995. *Qualitative Interviewing: The Art of Hearing Data*. Thousand Oaks: Sage.

Ruddiman, W. 2008. *Earth's Climate, Past and Future*. New York: W. H. Freeman and Company.

Ryen, A. 2004. Ethical Issues. In: Seale, C. et al. eds. *Qualitative Research Practice*. London, Thousand Oaks, and New Delhi: Sage Publications.

Seligman, A. B. 2000. *The problem of trust*. Princeton: Princeton University Press.

Shackley, S. 1997. The Intergovernmental panel on climate change: Consensual Knowledge and Global Politics. *Global Environmental Change* 7(1), pp. 77-79.

Shackley, S. 2001. Epistemic lifestyles in climate change modeling. In: Miller, C. and Edwards, P.N. eds. *Changing the atmosphere: Expert knowledge and environmental governance*. Cambridge, Massachusetts: The MIT Press, pp. 107-133.

Shackley, S. et al. 1999. Adjusting to Policy Expectations in Climate Change Modeling. *Climatic Change* 43(2), pp. 413-454.

Shackley, S. and Wynne, B. 1995a. Global climate change: the mutual construction of an emergent science-policy domain. *Science and Public Policy* 22(4), pp. 218-230.

Shackley, S. and Wynne, B. 1995b. Integrating knowledges for climate change: Pyramids, nets and uncertainties. *Global Environmental Change* 5(2), pp. 113-126.

Shackley, S. and Wynne, B. 1996. Representing Uncertainty in Global Climate Change Science and Policy: Boundary-Ordering Devices and Authority. *Science, Technology & Human Values* 21(3), pp. 275-302.

Shackley, S. et al. 1998. Uncertainty, Complexity and Concepts of Good Science in Climate Change Modelling: Are GCMs the Best Tools? *Climatic Change* 38(2), pp. 159-205.

Shapin, S. 1979. The politics of observation: cerebral anatomy and social interests in the Edinburgh phrenology disputes. *On the margins of science: The social construction of rejected knowledge* 27, pp. 139-178.

Shapin, S. 1994. *A social history of truth: civility and science in seventeenth-century England*. Chicago: University of Chicago Press.

Shrum, W. et al. 2001. Trust, conflict and performance in scientific collaborations. *Social Studies of Science* 31(5), pp. 681-730.

Sismondo, S. 2009. Ghosts in the Machine: Publication Planning in the Medical Sciences. *Social Studies of Science* 39(2), pp. 171-198.

Skrydstrup, M. 2012. Modelling Ice: A Field Diary of Anticipation on the Greenland Ice Sheet. In: Hastrup, K. and Skrydstrup, M. eds. *The Social Life of Climate Change Models: Anticipating Nature*. New York and Abingdon: Routledge, pp. 163-182.

Star, S. L. and Griesemer, J. R. 1989. Institutional Ecology, Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907-39. *Social Studies of Science* 19(3), pp. 387-420.

Star, S. L. and Lampland, M. 2009. Reckoning with Standards. In: Lampland, M. and Star, S.L. eds. *Standards and their Stories: How Quantifying, Classifying, and Formalizing Practices Shape Everyday Life*. Ithaca and London: Cornell University Press, pp. 3-34.

Stephens, N. 2005. *Why Macroeconomic Orthodoxy Changes so Quickly: The Sociology of Scientific Knowledge and the Phillips Curve.* Cardiff University. Unpublished PhD Thesis.

Stephens, N. 2007. Collecting Data from Elites and Ultra Elites: Telephone and Face-to-Face Interviews with Macroeconomists. *Qualitative Research* 7(2), pp. 203-216.

Stickley, C. E. et al. 2009. Evidence for middle Eocene Arctic sea ice from diatoms and ice-rafted debris. *Nature* 460(7253), pp. 376-379.

Strauss, A. L. 1987. *Qualitative analysis for Social Scientists*. Cambridge, UK: Cambridge University Press.

Suchman, L. A. 2007. Feminist STS and the Sciences of the Artificial. In: Hackett, E.J. et al. eds. *The Handbook of Science and Technology Studies*. Cambridge, Massachusetts and London, England: The MIT Press, pp. 139-164.

Sundberg, M. 2006. Credulous Modellers and Suspicious Experimentalists: Comparison of Model Output and Data in Meteorological Simulation Modelling. *Science Studies* 19(1), pp. 52-68.

Sundberg, M. 2007. Parameterizations as Boundary Objects on the Climate Arena. *Social Studies of Science* 37(3), pp. 473-488.

Sztompka, P. 1999. *Trust: A sociological theory*. Cambridge: Cambridge University Press.

Traweek, S. 1988. *Beamtimes and Lifetimes*. Cambridge, Massachusetts and London: Harvard University Press.

Tripati, A. et al. 2005. Eocene bipolar glaciation associated with global carbon cycle changes. *Nature* 436(7049), pp. 341-346.

Tutton, R. et al. 2005. Myriad stories: Constructing Expertise and Citizenship in Discussions of New Genetics. In: Leach, M. et al. eds. *Science and Citizens: Globalization and the Challenge of Engagement*. London, New York: Zed Books, pp. 101-112.

Ungar, S. 1992. The rise and (relative) decline of global warming as a social problem. *The Sociological Quarterly* 33(4), pp. 483-501.

Urama, K. C. et al. 2010. *The African manifesto for science, technology and innovation*. Nairobi: African Technology Policy Studies Network.

van der Sluijs, J. et al. 1998. Anchoring Devices in Science for Policy: The Case of Consensus around Climate Sensitivity. *Social Studies of Science* 28(2), pp. 291-323.

Warren, C. A. B. 2002. Qualitative interviewing. In: Gubrium, J.F. and Holstein, J.A. eds. *Handbook of interview research: Context and method*. Thousand Oaks, London, New Delhi: Sage Publications, pp. 83-101.

Weart, S. R. 2003. *The Discovery of Global Warming*. Cambridge, Massachusetts and London: Harvard University Press.

Weinel, M. 2007. Primary source knowledge and technical decision-making: Mbeki and the AZT debate. *Studies In History and Philosophy of Science Part A* 38(4), pp. 748-760.

Weinel, M. 2010. *Technological Decision-Making Under Scientific Uncertainty: Preventing Mother-to-Child Transmission of HIV in South Africa.* Cardiff University. Unpublished PhD Thesis.

Winch, P. 1958. *The Idea of a Social Science and its Relation to Philosophy*. London: Routledge and Kegan Paul.

Wittgenstein, L. 1953. *Philosophical investigations*. Oxford: Blackwell.

Wolfinger, N. H. 2002. On Writing Fieldnotes: Collection Strategies and Background Expectancies. *Qualitative Research* 2(1), pp. 85-93.

Wynne, B. 1989. Sheepfarming after Chernobyl: A case study in communicating scientific information. *Environment: Science and Policy for Sustainable Development* 31(2), pp. 10-39.

Wynne, B. 2003. Seasick on the Third Wave? Subverting the Hegemony of Propositionalism: Response to Collins & Evans (2002). *Social Studies of Science* 33(3), pp. 401-417.

Yearley, S. 1990. Researching the Precambrian Biosphere: Constructing Knowledge and Shaping the Organization of Scientific Work. *Social Studies of Science* 20(2), pp. 313-332.

Yearley, S. 1999. Computer Models and the Public's Understanding of Science A Case-Study Analysis. *Social Studies of Science* 29(6), pp. 845-866.

Yearley, S. 2009. Sociology and Climate Change after Kyoto. *Current Sociology* 57(3), pp. 389-405.