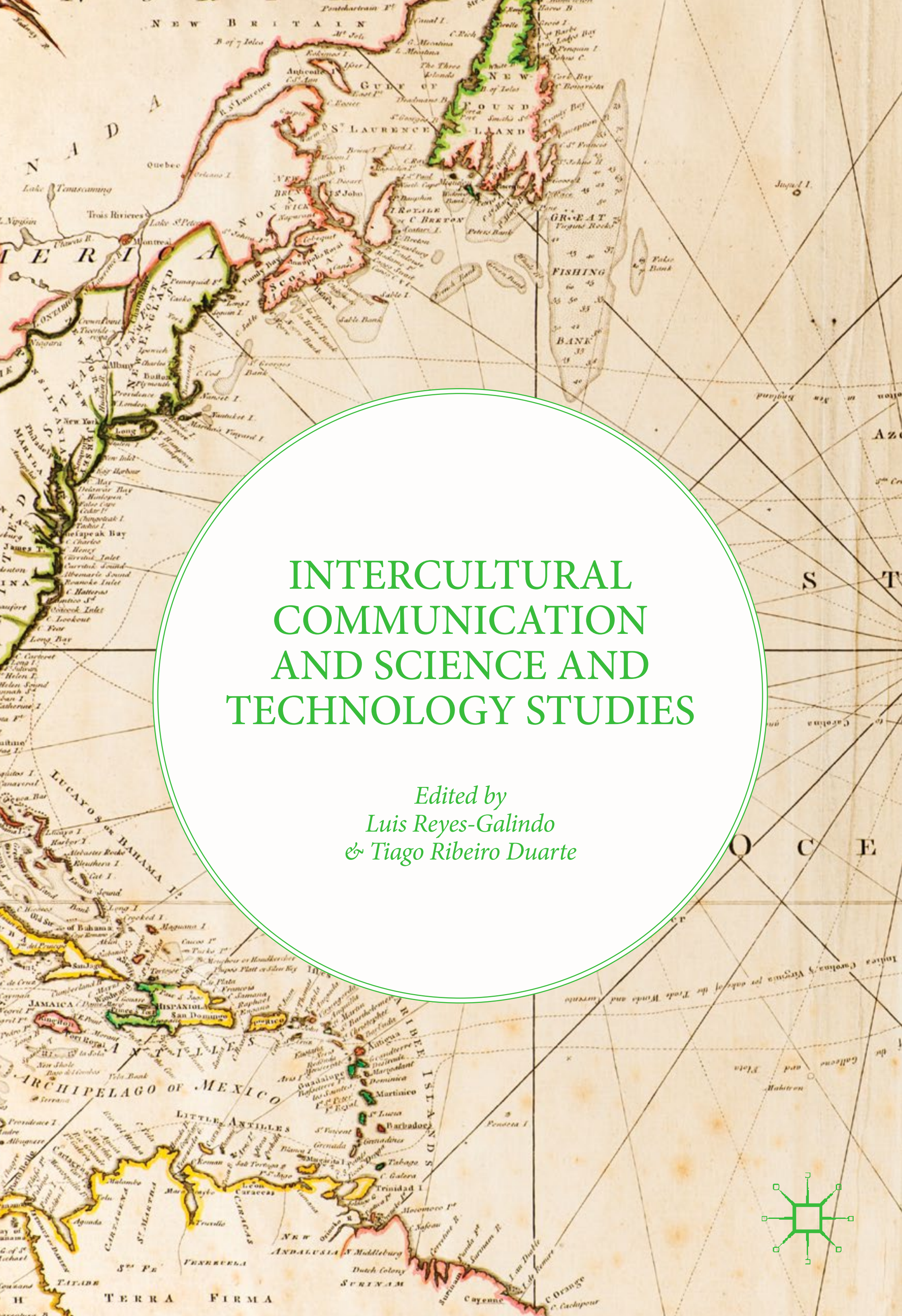

INTERCULTURAL COMMUNICATION AND SCIENCE AND TECHNOLOGY STUDIES

Edited by
Luis Reyes-Galindo
& Tiago Ribeiro Duarte

Intercultural Communication and Science and Technology Studies

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CHAPTER 1

Introduction: Intercultural Communication and Science and Technology Studies

Luis Reyes-Galindo and Tiago Ribeiro Duarte

The study of intercultural communication nowadays entangles such a wide variety of subjects and possibilities for empirical work that the topic must often be approached from a wide, multidisciplinary perspective. In this heterogeneity, linguistic approaches have converged with the interests and methods of anthropology, sociology, philosophy, communication sciences and other social sciences (Di Luzio et al. 2001; Bührig and Jan 2006; Jackson 2012; Paulston et al. 2012). Yet arguably, the major theoretical problem in researching 'intercultural communication' may be—as in most academic matters dealing with culture—the very plasticity and variability of meaning of the term (Leeds-Hurwitz 2011). Curiously, 'culture' as a theoretical a priori sometimes makes itself more of a theoretical burden than a resource. For example, to pair culture with notions of socio-cultural homogeneity can deter the recognition of pluralism and

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heterogeneity as intrinsic constitutive characteristics of contemporary societies (Moon 2010).

Indeed, intercultural communication scholars sometimes equivocate distinctions between the borders traced by language, society, statehood, nation, ethnicity, knowledge and 'culture'. Pallotti (2001: 295), for example, divides intercultural communication studies into two broad families: studies of situations where interactions take place between speakers of the same language focused on 'encounters in which speakers had a good command of the language, which was then ruled out as a possible problematic source' but in which cultural backgrounds are not shared; and work in 'applied linguistics [that] has stressed the consequences for interaction of speakers' limited command of a second language'. For Pallotti, framing interculturalism depends on being able to place linguistic 'outsiders' in a scale of adequate performance as defined by a dominant, homogenous, first-language/culture, which limits the research range of action to these circumstances. Working from a different model, Günthner and Luckmann (2001) differentiate between intercultural and intracultural communication defined as interactions between, for the first, members from the same society, and for the second, members from different social groups. Thus, for Günther and Luckman, 'true' intercultural situations can only exist when two radically different 'societies' clash. In both cases, it is hypothesised that intercultural exchanges somehow rely on the existence of dominant, homogeneous 'cultures', with little place for internal variation as a constitutive element of societies.

In contrast, the field of Science and Technology Studies (STS)—largely absent within the intercultural communications community—can problematize 'culture' as an implicitly homogenising concept in important ways. Although science and technology are constitutive elements of contemporary societies (despite differences in the way they manifest themselves locally), science in particular is characterised as being a highly esoteric field of knowledge whose inner sanctums are both linguistically, epistemically and socioculturally inaccessible to most members of the wider societies they are immersed in. Moreover, although scientific 'core sets' (Collins 1992)— that is, the group of vanguard knowledge-producing experts in a specialty field—often include scientists from different national, cultural and geographical regions that nevertheless share a common esoteric language, it is often the case that scientists from different core sets, even within a same scientific field, cannot dialogue informedly between themselves

(Reyes-Galindo 2014). Therefore, for most of its history, STS has had to reflect upon the deep cultural fragmentation observed within the sciences themselves, while at the same time recognising that sciences on other levels operate as homogenised fields (Fleck 1935/1981; Kuhn 1962/2012; Galison 1997; Knorr-Cetina 1999; Collins 2011; Monteiro and Keating 2009; Duarte 2013). This is what Galison and Stump (1996) have named the 'disunity thesis'. This questions how it is that, despite being constantly struggling to delimit themselves from other cultural spheres and achieving relatively high cultural, epistemological and institutional autonomy, the sciences *do* manage to cooperate and transmit knowledge between very local contexts.

For intercultural communication studies in STS, Knorr-Cetina's concept of 'epistemic culture' is particularly notable for its conceptualisation of cultural fragmentation without specific reference to the linguistic or national boundaries. Knorr-Cetina defines epistemic cultures as 'those amalgams of arrangements and mechanisms-bonded through affinity, necessity, and historical coincidence—which, in a given field, make up how we know what we know' (1999: 1). Influenced by the empirical 'practice turn' in STS that ran contrary to the idea of *a* scientific method and *a* unique empirical or theoretical zeitgeist pervading all science, Knorr-Cetina illustrated how a defining characteristic of modern science is the heterogeneity of 'epistemic styles' in which each science creates knowledge. Thus, the way in which molecular biologists on the one hand and high energy physicists on the other go about creating and sustaining knowledge claims about the world is different enough to warrant speaking about actual cultural differences and not just differences in technique or object of study. Knorr-Cetina's work is deeply influenced by Geertz's notion of culture as a 'pattern of meanings embodied in symbols, a system of inherited conceptions expressed in symbolic form by means of which men communicate, perpetuate, and develop their knowledge about and attitudes towards life' (Geertz 1973: 89, cited in Knorr-Cetina 1999: 10).

Although Knorr Cetina focused mostly on 'macroscopic' scientific fields such as 'molecular biology' and 'physics', further work particularly on the latter has found even greater internal fragmentation—Merz (2006) has recently focused on theoretical physics in high energy physics (HEP), while Merz and Knorr-Cetina (1997) also focus on theoretical physics but note a variety of subcultures that conform it. While not specifically using the concept of epistemic culture, Kennefick (2007) has carried out a sociological analysis of how in a niche research field of gravitational astrophysics, differences in epistemic styles lead to irreconcilable controversies between members of different epistemic cultures. Reyes-Galindo (2011) has further argued that physics can be divided into an even more pluralistic world than the areas of 'experiment', 'theory' and 'phenomenology' posited by both Knorr Cetina, Merz and Galison—within these 'mesoscopic' cultures there is further internal fragmentation into more specific microcultures. For example, pure theoretical physics can be carried out in a 'data fitting' style which is a radically different approach from that of the 'first principles' theorist. Likewise, phenomenological physics (physics that tries to create models of real-world scenarios) can be based on a 'simulation' or an 'analytical' style.

Based on the great cultural heterogeneity of science, STS scholars have sought to provide answers to the practical problem of how, *despite* being cultural loci with extreme degrees of internal fragmentation, scientific fields still manage communication and knowledge-transmission across cultural borders (Star and Griesemer 1989; Galison 1997; Jeffrey 2003; Ribeiro 2007a, b; Monteiro and Keating 2009; Collins 2011; Duarte 2013; Centellas et al. 2014; Reyes-Galindo 2014)—answers of possible interest to intercultural communication beyond science and technology settings.

This is not to say that 'intercultural' work in the sense *of inter-national*, *inter-linguistic,* or *inter-ethnic,* are not also part of STS. Although an exhaustive review of this literature lies outside the scope of this chapter, examples of these include: comparative studies of science and technology across international borders (Harding 1994; Kent et al. 2015; Jasanoff 2005; Wade et al. 2015), science and technology as received within 'global south' and alternative geo-political and national contexts (Bonneuil et al. 2014; Bribois 2014; Geissler and Kelly 2016; De Laet and Mol 2000; Drori 1993; Duque and Rajão 2014; Greenhalgh 2016; Hecht 2002; Lin and Law 2014; Rosemann and Chaisinthop 2016; Rusike 2005), postmodern and decolonial STS (Anderson 2002, 2009; Adams 2002; Harding 2008; Lachenal 2016; Seth 2009; Veran 2002), and STS in 'traditional' cultural settings (Wynne 1992; Agrawal 1995; Blaser 2009; Leach and Fairhead 2002; Colwell-Chanthaphonh and Ferguson 2008; Brandt 2014).

Intercultural Models in STS (1): Trading Zones

Perhaps the best-known model proposed for understanding intercultural communication within STS is Galison's (1996, 1997) *trading zone*, a concept borrowed from the linguistic study of pidgins, creoles and hybrid languages where two cultures speaking different languages encounter each other (Holm 2004).

Galison's historical studies of HEP concluded that, within physics, one could trace three different 'cultures' that can be historically shown to develop independently: 'experiment', 'theory' and 'instrumentation', drawing them as analogous to language cultures outside science. Galison (1997) followed these three traditions across time at various HEP experimental sites, noticing for example that major changes in cultures of instrumentation, experiment and theory generally did not coincide with changes in the other areas. Using Kuhn's (1962/2012) terminology, Galison realised that lack of synchronicity for 'scientific revolutions' across the three HEP subcultures meant they must be regarded as autonomous cultural domains. The autonomy was not simply one of 'technical' matters, as other elements that marked differences included: ontological repertoires; epistemic and doxastic attitudes; the materiality of practices; foundational myths and historical developments; group structures and division of labour; interaction-network topology, amongst others (Galison 1987, 1997).

Despite this internal heterogeneity, Galison registered how, in practice, all three disciplines *must* work together in HEP to produce reified knowledge to stabilise the results of an experiment. According to Galison, HEP subfields do this by establishing 'trading zones' where coordinated activity occurs to reach the final goal of, ultimately, publishing a final experimental 'result': a measurement, a model, a piece of new machinery, or a theory. The trading zone is ultimately a generalised 'space' where each culture brings in simplified elements of their own linguistic tradition.

In the trading zone, action is coordinated by the creation through interaction of a 'pidgin', or a 'reduced language that results from extended contact between groups of people with no language in common' (Holm 2004: 5). According to Holm, pidgins are characterised by grammatical and lexical simplifications of the contributing languages (which must differ significantly to begin with); by their limited temporal stability; by a social distance constantly maintained between the interacting groups; and by equality in power between the groups. 'Creoles' develop from pidgins as these become the first languages of an entire community group that is independent from the originating languages. Creoles are also not reduced languages, as they must be usable in all situations and not only the reduced domain (e.g. trade) that gave rise to the originating pidgins.

Galison (2010: 36) further explains that '*trade* focuses on coordinated, local actions, enabled by the *thinness* of interpretation rather than the thickness of consensus. Thin description is precisely what makes it possible for the experimentalist and the theorist to communicate, albeit in a register that by no means captures the full world of either, let alone both'. In this sense, trading zones are not places where frames of meaning become fully shared by different social groups. Partial understandings enable communication and exchange to take place. While trading zones are often posited as 'finished' spaces where there is ample shared meaning, Galison here stresses the temporality and the constantly changing nature of trading zones, as well as the possibility of interaction to occur even in instances where there are only 'inter languages' (simplified vocabularies considered less than pidgins proper) or other linguistic strategies being used.

Intercultural Models in STS (2): Trust

Trust has been a fundamental theoretical concept across sociology, and there is a strong STS tradition based around trust to make sense of scientific practice (Collins 2001; Hedgecoe 2012; Lewis and Atkinson 2011; MacKenzie 2001; Shapin 1994; Stephens et al. 2011; Reyes-Galindo 2014). Nevertheless, STS scholars constantly confuse related but differentiable types of trust and in their analysis often bundle them together uncritically. In particular, there is a tendency in STS to posit inter-personal trust as the major theoretical concept. Addressing this issue, Reyes-Galindo (2014) put forward a trust-based framework to understand communication across fragmented scientific domains. This framework locates instances of scientific intercultural communication within three different levels of 'social distance', and then links these to the type of trust that allows communication to be carried out, thus providing some order to STS perspectives, while linking them to mainstream sociological scholarship. This trust framework also sets bounds on the 'depth' of knowledge that can be exchanged based on Collins and Evans' (2007) typology of expertise, which places emphasis on processes of enculturation into a scientific form-of-life as a means to understand the communication of expertise across cultural divides.

A trust-based approach complements STS coordinated-action models such as Galison-type trading zones, in that the latter model has already been noted by Collins et al. (2007) to be applicable only in instances where power relations between the interacting groups is horizontal, and when there is a large social distance between the interacting cultures. To give a diametrically opposite example of a Galison-type 'coordinated trading zone', Collins et al. provide the example of a galley slave/slaver interaction. Clearly in such a case, even though there might be interaction and some mutual language might develop, the imposed hierarchy between the groups makes it impossible for truly coordinated action to arise. The class of trust-based interactions described in Reyes-Galindo (2014) in turn fall into what Collins and Evans classify as an 'inter-language trading zone', characterised by horizontal power relations that are facilitated by a linguistically homogenizing substratum. Trust in these cases acts as a bond that allows action to be coordinated, despite persistent socio-cultural and conceptual incommensurability.

As was argued in Reyes-Galindo (2014), trust in one of its three forms ('interpersonal', 'referred or institutional', or 'suspension of doubt') is a prerequisite for meaningful intercultural communication in all settings. In fact, even in settings of *conflict*, if there is in fact communication happening, one of these forms of trust is a sociological prerequisite for meaningful interactions. It is also important that trust always implies a degree of epistemic deference to other social actors. This is in line with recent developments in the epistemology of testimony (Gelfert 2011) and a sociology of scientific consensus (Collins et al. Forthcoming) which call for a vindication of the role of trust in permitting modern science to flourish in an increasingly complex world of heterogeneous expertises.

Intercultural Models in STS (3): Expertise and Enculturation

Following the interpretation of the 'second' Wittgenstein first set out by the Edinburgh School (Bloor 1983, 1997) as much as by the writings of Fleck (1935/1981), Winch (1958), Polanyi (1966) and Collins and Evans (2007) have put forward a 'realist' language-based theory of expertise that has tackled intercultural communication in a markedly different manner. Collins and Evans' novelty is the introduction of *tacit knowledge* as the marker of what defines a particular person as a full member of a knowledge collective (what the authors refer to as a contributory expert). An individual is a full-blown 'expert' in a particular knowledge culture if and only if they possess a degree of tacit knowledge sufficient enough to communicate fluently with other members of the culture. While including epistemic dimensions like Knorr-Cetina's model, Collins and Evans' range of applicability spreads beyond the epistemic realm and brings back deeper cultural dimensions into STS that could link it to studies on the 'unspoken' dimensions of intercultural communication processes (e.g. Günthner and Luckmann 2001; Auer and Kern 2001).

Departing from Galison's horizontal-heterogeneous trading situation, the tacit knowledge-based approach has typically focused on situations where a marked epistemic hierarchy between the interacting cultures (there is, however, 'generally cooperation' in the sense that the interaction is not enforced or imposed by pure force), as well as there being linguistic homogeneity in the sense of both cultures speaking the same mutually comprehensible base language (e.g. English). This is where the tacit dimension enters, for as noted by Polanyi (1958, 1966), the tacit dimension remains untouched by mutual comprehension of the base language. Collins (2010) has laid particular stress on the non-somatic dimensions of communication by coining the term 'collective tacit knowledge' to cover those cultural elements that cannot be made explicit except by socialisation into a cultural group. Although the notion of the collective tacit has been challenged by scholars such as Turner (1994), influential empirical studies in STS have made and continue to make fruitful use of tacit knowledge to explore scientific and technological cultures, including processes of communication and of misunderstanding (Pinch 1980; MacKenzie and Spinardi 1995; Doing 2004; Schmidt Horning 2004).

By considering the tacit in non-horizontal terms, interesting applications of the expertise model have arisen, particularly in situations where individuals from one culture are deeply socialised into a different culture through deep linguistic immersion, to the degree that they appropriate large amounts of tacit knowledge through the process. Collins and Evans (2007) have argued for the existence of these so-called 'interactional experts', where holding 'interactional expertise' implies the ability to proficiently apply tacit knowledge in communicating with members of another culture *in their own terms,* but without (necessarily) becoming a full-blown member of that culture (Reyes-Galindo and Duarte 2015).

Interactional expertise has been used to analyse a wide range of intercultural situations where individuals connect with a different culture: acquiring the ability to read scientific literature (Collins 2014); initiating and fostering cooperation in a fragmented scientific field (Duarte 2013); the somewhat ironic use of misunderstanding as an aid to fruitful communication and management strategies (Ribeiro 2007a); the training of linguistic 'ambassadors' in scientific cooperation within Big Science collaborations (Collins 2011; Reyes-Galindo 2014); and the important role of 'cultural translators' in technology transfer in industry settings (Ribeiro 2007b).

Intercultural Models in STS (4): Boundary Objects

A model that in terms of popularity may rival trading zones is Star and Griesemer's (1989: 393) *boundary object*, defined as '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' and that 'these objects may be abstract or concrete.' Boundary objects arise from reflections on Actor Network Theory (ANT) by Latour (1987), Callon (1984) and Law (1986). To very briefly summarize Star and Griesemer's take on ANT,[1] Latour and Callon's core proposal is that processes of translation be ultimately thought of as processes of standardisation between social worlds (Clarke and Star 2008). Translation comes about when two 'actors' come into contact. Upon contact, each actor will then mobilise resources, build alliances and reify networks established with other 'actors' to attempt to impose its particular worldview, thus also increase its capacity for acting in the world. The reconfiguration of power relations will ultimately lead to the establishment of a dominant understanding of the world—probably a modification of the original ones, given the dialectical nature of the interactions. The stabilisation of a particular interpretation concerning the world is reached, that is, there comes about a 'standardisation' of language through power.

Boundary objects are the results of this standardisation, but unlike Latour and Callon, Star and Griesemer advocate for a more 'ecological' understanding of standardisation in which it merely serves as the root for multiple interpretations of a piece of the world. Unlike the coercive process of translation through which frames of meaning are standardised, boundary objects enable the formation of networks around objects that are interpreted differently by members of different social worlds. In the case study presented in the original paper, Star and Griesemer focus on a zoology museum, in which processes of standardisation allow the site to work despite socio-cultural groups with enormously different interests and conceptualisations of the museum's ultimate purpose. For example, although the museum's collection was largely increased through the work of non-scientific collectors, in order to form part of the museum the collection must be processed according to agreed standardised scientific techniques that might nevertheless be meaningless from an amateur's perspectives. Likewise, the museum 'meant' different things to the directors, to conservation societies, to collectors and other parties, yet the museum-as-object remained an ongoing institution that served as a focal point for all groups.

As Star (2010: 613) herself noted, there is a tendency to overuse the concept to refer to anything that has *interpretative flexibility*, which can range from 'The Beatles (or other very well-known people … the national flag, the Bible, a particular film, or other famous things)'. Evidently, such a wide usage makes reference to a boundary object trivial. So, apart from interpretative flexibility, Star mentions a crucial factor that made the original concept analytically useful: the original focus on institutions and infrastructures as the primary loci of interest. Indeed, this is the central idea in the parallel development, within this tradition, that concentrated on infrastructure and standardisation as sites of coordinated communication (Star and Ruhleder 1996; Bowker and Star 2000). The topic of information infrastructures has also become particularly relevant in the study of communications in the Internet age, online communities and computational cultures (Star 1995).

Book Structure

The book is divided into two thematic sections. Chapters 2–5 focus on issues of interdisciplinary communication. Chapters 6–9 examine intercultural communication beyond the boundaries of science, exploring communication between a range of heterogeneous actors, including scientists, publics, artists, journalists and so on.

In Chap. 2, Luis Reyes-Galindo considers the most direct and arguably the most ubiquitous type of 'scientific communication': communication between scientists themselves. The chapter focuses on how theoretical physicists establish dialogue with physicists who are 'near' and 'far' from theoretical knowledge cultures, considering how one can actually probe the concept of cultural proximity empirically. Reyes-Galindo shows that three levels of social distance may be associated with actual communication between theoretical physicists, and other theoreticians and scientists outside of theoretical physics proper—for example, computer modellers, data-analysts, experimenters, technicians and non-physicists, and non-scientists. At minimum social distance, there is relatively unproblematic person-to-person communication (*pace* the existence of tacit knowledge barriers); at medium distances, the communication is mediated by institutionalised means; while at the largest distances 'communication' happens through the exercise of what a theoretician during an interview refers to as 'informed blind trust'. Starting from an empirical perspective and a pool of interviews with a variety of physicists from across the entire range of theoretical subcultures identified by previous sociology of physics, the chapter presents two theoretical results: a richer typology of trust-based interactions that correlates trust to an empirically explicable parameters; a perspective of theoretical physics that is culturally more heterogeneous and rich than traditional portrayals, yet where physics is not broken up into isolated islands of practice but rather is bound together by trust-mediated knowledge exchanges. The final proposal is that this comprehensive model of a trust-based sociology of knowledge is a solution (though not by any means the only one) to Galison's 'problem of disunity'.

In Chap. 3, Tiago Ribeiro Duarte examines interdisciplinary efforts between paleoclimatologists and paleo-modellers, which are distinctive fields of expertise. Although both seek to reconstruct past climates, whereas paleoclimatologists are observational scientists who generate data based on a range of archives, such as ice-cores, marine sediments, tree rings, corals and so on, paleo-modellers produce no empirical data, but run simulations of past climates in computer models. These communities have been building up collaborative ties over the past few decades and have faced several communicative challenges for working together. Duarte argues that members of these fields are involved in a deliberate process of mutual linguistic socialisation through which they are seeking to improve communication. This process involves a number of initiatives, such as the creation of summer schools and graduate programmes where students learn from experts from both fields, the joint supervision of PhD students by paleoclimatologists and paleo-modellers, informal conversations at conferences and in collaborative projects, and the formation of university departments with academics from both research areas. Even though this process does not lead members of these communities to acquire a high level of linguistic competence in the field which is not their own, it nevertheless facilitates communication between them.

Chapter 4 also examines challenges in interdisciplinary communication. Marko Monteiro carried out an ethnography of a multidisciplinary project, *Amazalert*, that sought to improve computer modelling of deforestation in the Amazon. One of the main goals of this project was to add social variables to computer models. To do so, members of this project, which included modellers, social and environmental scientists, took part in two workshops. Monteiro participated in one of these workshops playing a dual role—that of an anthropologist who attempted to contribute to the project goals as well as that of an ethnographer who studied the interactions that took place in the event. During the workshop, experts from different fields disagreed on whether social variables could be quantified and transformed into model input. Monteiro argues that ethnography can be an important tool in such projects as it can bring to light the communicative challenges that emerge from interdisciplinary collaboration. Making these challenges visible is the first step towards transforming them into productive misunderstandings—that is, in opportunities for improving communication. Monteiro also argues that ethnographies of multidisciplinary projects can help make science 'humbler' (Jasanoff 2003), recognise its limitations and have weaker expectations towards the role it can play in informing policy-making.

In Chap. 5, Meritxell Ramírez-i-Ollé examines her own fieldwork, in which she studied the production of knowledge in dendroclimatology, and the relations she built with her informants to reflect upon the challenges of communicating STS knowledge to publics. She points out that STS can communicate about 'science in action', but not about 'science as a product'; in other words, about how scientific knowledge is generated, rather than about the theories, methods, data and so on produced by scientific experts. However, STS is frequently regarded as a criticism of science, particularly by natural scientists. Ramírez-i-Ollé argues that the challenge of communicating science in action is to build up a community of knowledge that trusts STS and is willing to learn from STS scholars about science in action. Based on her interactions with dendroclimatologists during three years of fieldwork, she presents three strategies she deployed to build up contingent trust relations with her informants. Although admitting the strategies did not entirely convince them of the importance of STS, she argues that they brought about greater mutuality between them, along with more tolerance towards STS work. These strategies included *performing a scientific status*, presenting herself as a scientist who carried out neutral and impartial research; *eye-witnessing completeness*, which means that she strived to write an account of dendroclimatological work that would be regarded as credible by her informants; and *encouraging civil scepticism,* that is, she tried to generate opportunities for her and her informants to comment on each other's work in a way that the comments would be regarded as constructive, instead of offensive critiques.

In Chap. 6, Carina G. Cortassa turns a philosophical eye towards the topics of expertise and models of science communication in the field of Public Understanding of Science (PUS). Cortassa first focuses on the so-called 'cognitive deficit' models that have dominated PUS debates for decades—sometimes, she argues, rather implicitly. While recent social science-oriented PUS scholarship has sharply rejected all notions of an intrinsic epistemic imbalance between experts and non-experts, that is, the so-called 'democratisation of science' move, Cortassa's perspective is both bold and counterfactual. Rather than focusing on the elimination of the expert/non-expert hierarchy, she argues that the PUS starting assumption ought to be that *there is* indeed an epistemic asymmetry, and that only after recognising this fact can PUS *then* work upon the elaboration of realistic descriptive projects, or progressive normative science communication models. Like Reyes-Galindo in Chap. 2, Cortassa draws attention to the fact that within science itself the notion of an established imbalance between experts in a particular subject and non-experts is a non-contested epistemic situation. Cortassa also uses the notion of trust to illustrate this point, but in her case the argument hinges on recent philosophical scholarship on scientific and expert testimony-as-trustable-knowledge.

In Chap. 7, Carr and Reyes-Galindo turn to the skies and carry out an analysis of how the British mainstream press vilified and demonised seagulls along a series of articles published across 2015. The authors show that two strategies were used to negatively portray seagulls: anthropomorphising gulls to then turn them into 'antisocial' characters; and using a discourse of 'pestilence', dirtiness and displacedness to argue that seagulls did not belong within 'healthy', urban environments. While most of these arguments could and would have been refuted with ease with a minimal approach to zoological experts, Carr and Reyes Galindo show how the mainstream press simply ignored technical dimensions, or approached putative, fit-for-purpose 'experts' to support fantastical, sensationalist and extravagant claims. The authors end by considering how traditional journalistic values can be poised to either resonate or conflict with scientific values and how this requires a reframing of *effective* scientific journalism that understands *both* journalistic and scientific cultures simultaneously.

Chapter 8 by Jamie Lewis and Julia Thomas concentrates on an entirely different dimension of science communication: public engagement of psychiatric genetics through the arts. The authors use their rich, practical experience of participating and organising art-science events to examine how art can be used to foster common spaces to promote dialogues between artists, scientists, patients and other non-scientific publics. Borrowing from Galison's idea of trading zones, Lewis and Thomas describe engagement events as 'buffer zones', spaces where the introduction of the arts can mitigate the anxieties and conflicts that have historically made difficult debates around the science of psychiatric genetics and its publics. Lewis and Thomas importantly point out that these engagement events, unlike other types of science communication, such as popularisation, do not aim to convey knowledge from or about the science (and in many ways, 'protect' the science), but are rather spaces where conversations around psychiatric genetics can become more fluid, particularly through the use of metaphorical language.

The last chapter, Chap. 9 by Letícia Cesarino, approaches intercultural communication in a very different way from the preceding contributions, describing how agrotechnology information 'travels' through different systems and scales. Adopting a theoretical perspective that mixes Luhmann's systems' theory with a post-representational perspective, Cesarino examines a case study of agricultural technology transfer from Brazil to four West Africa countries. Inspired by the 'ontological turn' in STS, she understands communication as a process that is not restricted to humans, but as one that encompasses human and non-human actors. In her case study, she examines a particular boundary object, the *paysan*—West African cotton-producing farmers—as it moved through the different and discontinuous scales of a development project. Its movement not only connected different systems, but also responded to their own self-referentiality. Therefore, although the *paysan* changed when travelling through systems and scales, they also preserved some characteristics, mainly that of being regarded as having a deficit in agricultural technique.

Note

1. It is worth noting that Star and Griesemer's interpretation of ANT is very distinct from what Latour, Callon and Law originally posited. In ANT the very notions of culture, society, social groups and so forth assume whole new meanings (Law 1992; Latour 2005). Indeed, there is no a priori society or culture in this theory, but only associations of humans and non-humans that are constituted, changed and might be stabilised for some periods. The 'social' only emerges at the end of processes of heterogeneous interactions between human and non-human actors. Language, worldviews and frames of meaning are all secondary in this theory to processes through which actors seek to enrol other actors in their networks to increase their power and control over particular associations. The symbolic interactionism version of ANT by Star and Griesemer violates some of the fundamental principles of this theory as it foregrounds interpretative processes and does not question the ontological divide between nature and culture, focusing primarily on humans and on their interactions mediated by meaning. Materiality, however, is not absent from symbolic interactionism, as there is a growing interest among adepts of this theoretical approach in infrastructures and data. This, however, has not led to a conjunction with the 'ontological turn' pioneered in STS by ANT.

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PART I

Interdisciplinary Communication

CHAPTER 2

Linking the Subcultures of Physics: Virtual Empiricism and the Bonding Role of Trust

Luis Reyes-Galindo

Galison (2010) describes Science and Technology Studies' (STS) emphasis on 'the locality of practice' as the result of a rich empirical tradition that has given rise to the so-called problem of disunity (Galison 1996b)—resolving how knowledge is transmitted between scientific fields, despite deep linguistic and cultural fragmentation.[1] Two well-known solutions to the problem of disunity are Galison's *trading zones* and Star and Griesemer's *boundary objects* (Galison 1996a, 1997; Star and Griesemer 1989), both characterized by the establishment of common and neutral linguistic spaces that coordinate action and distribute epistemic legitimacy between different knowledge cultures within highly heterogeneous interdisciplinary contexts (Galison 1996a; Monteiro and Keating 2009; Wilson and Herndl 2007). Here I draw attention to a different understanding of the problem of disunity that does not rely on the need for 'multiple worlds organized ecologically around issues of mutual concern and commitment to action' (Clarke and Star 2008, pg. 113) as a prerequisite for the communication of scientific knowledge. This will be done by analysing

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the role of trust in allowing communication to occur in different physics communication settings.

Different kinds of trust corresponding to different levels of interaction will be identified in order to define a scale of 'social distance' in terms of knowledge practices between distinct cultural groups. Additionally, Collins and Evans' (2002, 2007) enculturation model will be introduced to make sense of interactions that are not typically thought to involve trust relationships directly. This 'enculturation model' hinges on cases of the passive ingress of one cultural group into another's domain where the linguistic contact space lies inside the boundaries of one of the groups, with the other then being 'parasitic' on it: this is the stepping stone to evaluating communication settings that depart from the 'common ground' cooperative settings by Galison and Star and Griesmer.

I first present interview material to illustrate the mechanisms underlying the communication of experimental knowledge to theoretical physicists across varying degrees of 'social distance', focusing on the social mechanisms that make these exchanges possible. Theoretical physics lends itself to this analysis because it is a highly heterogeneous field made up of highly autonomous subcultures, some of which interact more directly than others with experimental cultures. 'Social distance' will then be related to the types of trust established between theoretical physicists and experimental cultures.

The empirical material is taken from interviews with over 20 theoretical physics researchers at seven European universities and one in Latin America; these were part of a doctoral research project concerned with analysing various elements of the practice of theoretical physics. The interviewees were chosen with a view to covering the widest possible spectrum of theoretical subcultures of the theoretical physics community, and except for one were all tenured, full-time researchers at prestigious physics institutes or schools. The choice of interviewees was also based on the author's close acquaintance with the culture of theoretical physics and convenience samplings stemming from his work as a research assistant and postdoctoral researcher in theoretical solid-state models of quantum vacuum forces for over ten years. The empirical study relied on a qualitative 'participant comprehension' approach which has been previously used in canonical

Table 2.1 Interviewees' main research areas

High-theory	*Phenomenology and modelling*
(H1) stochastic quantum mechanics, (H2) dark matter and energy, cosmology, condensed matter (H3) theoretical optics and applied mathematics, (H4) particle physics, quark models (H5) thermodynamics and statistical mechanics, (H5) gravitational wave theory, (H6) general relativity, (H7) quantum computing and information theory, (H8) astrophysics, (H9) biophysics, (H10) particle astrophysics and cosmology, (H11) theoretical optics	(P1) applied quantum field theory and acoustics, (P2) computational nanophysics, (P3) computational nanophysics, (P4) computational quantum chromodynamics (QCD), (P5) gravitational wave experiment analysis, (P5) optic experiment analysis, (P6) quantum electrodynamics modelling, (P7) Bose–Einstein condensate modelling, (P8) computational particle physics

sociology of physics research (see Collins 1984). The interviewees' main research topics are outlined in Table 2.1, divided between high (or pure) theory and theory of a more 'applied' type.[2]

The Social Gap Between High-Theory and Experiment

Cultural and linguistic fragmentation through specialization occurs across all scientific activity, but physics offers a particularly interesting case because of the very distinct division of labour that exists between theory and experiment—markedly different from, for example, some of the biological sciences where the division between theory and experiment is not seen to be a critical structuring factor of the field as a whole.[3]

The mathematically oriented, theoretical subculture that I focus on in the first sections of this chapter will be referred to as *high-theory*. High-theory is the kind of activity most often thought to comprise 'theoretical physics', concentrating on posing, manipulating and finding solutions to the equations that govern the most basic interactions between physical bodies using highly mathematical language (and increasingly, computers). It is sometimes also referred to as 'pure' theoretical physics in contrast to 'applied' theory that deals with concrete physical systems. High-theory is often abstract, dealing with hypothetical physical universes, physically unrealized scenarios and so on. Some high-theoreticians are only a step away from doing applied mathematics, concentrating on problems that are only loosely tied to a physical interpretation of the mathematical techniques being used.

How far from an experimental physics culture is high-theory in its daily practice? Consider the following reply when a senior high-theoretician was asked about the relevance of experiment for his own work:

> It is quite clear that nowadays you can do excellent theoretical physics without knowing how to even tighten a screw. A good theoretician can acquire profound and solid physical intuition while being completely detached from experiment.

Yet this same theoretician remarked only moments later:

> For theory, experiment is a fundamental guide. Physics is still a science with an experimental foundation. One must always be aware of how the experimental results were arrived at. One must have a clear idea of the limitations of the experiments so that one can appreciate to what degree what one is doing is really well founded. Theoretical physicists aren't always aware of these details, but it is one of the main barriers you face in this field.

A simultaneous portrayal of theoretical physics as both fully detached but at the same time 'grounded' in experiment is often encountered when interviewing high-theoreticians about the role of experiment in their work.[4] This rhetorical stance, which considers physical theory as an empirically based science, is common to theoretical discourse, even though members of high-theoretical cultures only rarely maintain direct contact with experiment. Some high-theoreticians may even devote entire portions of their professional lives to making novel and 'empirically testable' predictions of observable phenomena, yet never work directly with a lab. Thus, a senior high-theoretician who has worked on optical phenomena throughout his career explained,

Reyes-Galindo (R.G.): You've made quite a few theoretical predictions of phenomena. Do you have any direct contact with a laboratory where you can say, 'do this' or 'don't do this'?

Theoretician: No, no, no. In this conical refraction episode, astonishingly—and actually for the first time—I had some contact with an experimental group in Dundee. I've just recently encountered them. I'm predicting all kinds of things, like if you put

> crystals in series what would you get, and so on. That's something where I'm directly involved, but it's unusual for me. I don't normally do that. There are different types of theorists, and some people work very close with the experimentalists. It's fun to do that, but I tend not to.

Full appropriation of laboratory knowledge can only happen through direct and prolonged engagement with laboratory culture; without direct contact with an experimental culture, high-theoreticians cannot 'have a clear idea of the limitations of the experiments' or 'appreciate to what degree what one is doing is really well founded'.[5] So, while few theoreticians would deny the impact of experiment on high-theoretical work,[6] high-theory's minimal contact with experimental cultures indicates that if experiment is a guide for theoretical work, it cannot be so through a simplistic sort of direct empiricism.

You Need a Busload of Faith to Get By

Elaborating on how the appropriation of experimental knowledge comes about in his own work, a young theoretician specializing in high-energy physics, superstring theory, gravity and other (as he described it) 'sexy' high-theory subjects explained,

> At some point I have to take on *faith* what experimenters tell me. I know that there are important questions that need to be answered like the cosmological constant, dark matter, the spectrum of Cosmic Microwave Background radiation or fluctuations you can see, problems in fractional quantum Hall effect or high temperature superconductivity. I've never done any of those experiments, and I don't understand most of the experiments, but you know, I have faith in these problems that need answering. (Author's emphasis)

Of course, the idea of faith as a foundational element of physical theory contradicts the typical views of physics as a directly empirical science. Yet high-theoreticians constantly refer to trust in explaining how experimental knowledge trickles down from the lab to the theoretician's blackboard or simulation—though not without some degree of resistance, even for a candid interviewee such as the one above. When pressed to explain what this 'faith' implied, the same theoretician remarked that it was not so much a matter of 'blind faith' but rather of selecting who to believe, a kind of 'trust', since, given enough time and resources, he was sure that the experimental results on which he based his work would end up being verified:

> I'd call it trust, but trust based on lots of evidence and trust that *I can test it at any time*. Certainly I don't understand the way LHC [Large Hadron Collider] works. But I could. I could sit down and spend three years of my life figuring this out. (Author's emphasis)

How realistic is the possibility of *personally* carrying out this fact-checking? When asked about the possibility that another theoretician might fully understand the large hadron collider (LHC) results he uses in his own work, the theoretician replied that, although he could not name such a person on the spot, there certainly *had* to be theoreticians at the European Organization for Nuclear Research (CERN) who would be able to understand these experiments:

> I know theoreticians who understand it. I'm sure they didn't put a fuse together, but yeah, I know … uh … for example the ones who work at LHC full time, they could tell you what all the quirks were, what could be going wrong, what to worry about.

But for a high-theoretician to spend three years of his or her life understanding the intricacies of an LHC-like experiment would require spending three years away from practice *as* a high-theoretician. That a theoretician would have the intellectual capacity to fully understand an experiment, given enough time, is not in doubt, but the practical requirements of fact-checking the scientific output of an alien knowledge culture conflicts with the fact that this would lead him away from the professional high-theoretical world. As a theoretician who has turned to more applied work explained,

> In my work I must make an effort, an enormous effort, to see if what I am saying has anything to do with what is out there, with an experiment. I think there are two kinds of theoreticians. I am of the pragmatic kind, although if you look at the everyday stuff I do it's just as abstract as the people who do mathematical physics. [laughs] … The more I talk to them the deeper my

> knowledge about the experiment and the experimental result is. Nowadays I can actually see their naked experimental results without any analysis and know what it is they did, but that's as far as I can go. The part that I participate in the most is when I tell them, 'Look, why don't you try this new thing' and they'll reply something like, 'No, that's way too hard'. 'Why?' I'll ask. 'You can't do that to the coils'. 'Ah, ok'. Well, then you keep on talking. In general, the more you speak to them the better you're at it. You start saying, 'Ah yes, this is where the laser comes out. Of course, it has to be tuned into the transition I want to make, and here's the cell and here's the detector' which are all *black boxes.* (Author's emphasis)

Impeded from carrying out the fact-checking in person, theoreticians must resort to 'faith' or 'trust' in either the 'obviousness' of experimental trustworthiness or trust in the job carried out by unknown colleagues who have bothered to go and check the facts (e.g. the ones who work full time at LHC). Experimental results may then be trusted as second- or even third-hand accounts because of the certainty provided by the larger world of institutionalized physics. A senior theoretician working on quantum chromodynamics (QCD) simulations described the work of a colleague who works within a multi-site collaboration:

> We had a meeting this weekend, and M next door has gone to Durham, and he's taking some of our preliminary results up there, and he will discuss them with the experimenters. They will go away, and then of course they can e-mail back. You can then set up a dialogue where they don't quite understand exactly what we did, you see. We don't understand what they want to do. So you have to understand their physics. They're sceptical sometimes, and we're sometimes sceptical because you think, 'You didn't do this. You didn't do that. How does this work? I don't understand that.'

Theoreticians often used metaphors such as 'networks' or (more often) 'chains' of physicists to refer to the channels for the dispersion of knowledge from experiment to high-theory. Theoreticians are aware that experimental facts 'travel' from the lab to a theoretical setting not via single scientists, but rather via large numbers of individuals and knowledge webs that form intermediary links within the physics ecosystem, a large subset of which are not part of the high-theoreticians' own local network. One theoretician, also working on high-profile, high-theoretical topics, described physics as a continuum of experts in which high-theoretical physics represented one end and experiment the other, with intermediate actors overlapping each other's physics between the endpoints:

> The whole thing is this continuum where there are hard-core theoreticians at one end who only work on string theory, all the way through to the guy with the spanner, tightening up the nuts and bolts. There are thankfully big overlaps between each section, but yeah, I've never chatted to the guy with the spanner.

The need for overlaps and complex chains of knowledge between all elements of the physics 'continuum' was stressed by quoting a well-known piece of physics lore, which explains how in contemporary physics it is now impossible for a single person to cover the entire spectrum 'from theory to spanner':

> It's usually said that Fermi was the last guy to do [experimental work and theory simultaneously].[7] I don't know if that's a true statement. Certainly I don't know people that do it. I know of very impressive people that work on string theory—the geometry of extra dimensions—and at the same time do nuts and bolts work of top quark analysis from the data from LHC so they get their hands on the raw data and try to sift through and understand what's top quarks and what's not. That guy is a fairly extreme example because it seems that—to me at least—his two bits of work don't overlap. But to go all the way through, I don't think so.

Yet even data-analyst cultures—made up of physicists who statistically analyse 'raw' data from experimental runs—are often also characterized by minimal contact with experimental cultures. A telling example is found in the Laser Interferometer Gravitational Wave Observatory (LIGO) collaboration; LIGO is subdivided into four major groups of researchers: the on-site experimenters, the data analysis team that works with the raw data, the 'theoretical' data analysts who work with the mathematics behind the actual data processing and the 'future technology' team.[8] A senior LIGO member, leader of one of the data-analysis teams, described during an interview the last two groups as being the most far removed from experiment itself, with the raw data analysts having the closest contact with experimenters and working with the data-readings directly produced by the on-site team. Nevertheless, he also described how his data analysis group has little direct contact with the experimental on-site team itself, so that even when problems come up with the data to be analysed, he does not feel enough rapport to call the on-site experimental team directly:

R.G.: How does the actual communication go about? Do you ever call up the experimenters at the site? I was curious as to whether you just picked up the phone and…

Data analyst: Some people do. I don't know the guys at the site well enough to just pick up the phone and say, 'this is killing us', but I know the person I would talk to who is my expert and there's a chain … I don't know how it would get to them!

The 'expert' in this case turned out to be a graduate student who was sent to the experimental site for an extended period specifically to interact with the experimental team and be enculturated into it. The prolonged on-site exposure time was calculated so that the student could eventually have 'a foot in each camp' and would thus be able to 'translate' for the data analysis team what the experimental group said:

> You need a few people with a foot in each camp who can almost translate, but there are a lot of people who just don't care to make that effort. I've done a bit, but you know one of my PhD students went to the site for four months and this was great for us because he came back and we'd hear something about what the detector did. And he's been there and helped work on it and he'd say, 'yeah, *that* means *this*'.

In contrast to the trading zone or the boundary object models of two-directional information flow, the existence of 'ambassadors' and 'translators' of data requires only unidirectional flow, such that the ambassador becomes a linguistic 'apprentice' to the target culture. It is therefore not necessary to set up an intermediary linguistic or conceptual common zone of interaction, so long as there is at least one trusted individual who can be a direct *linguistic* link to the experimental production site (in none of the above cases was it necessary for the on-site link to be able to be proficient in the experimental tasks).[9] Once these 'ambassadors' come to grips with the language of the on-site data producers, the information is then amenable to transmission to the rest of the team in their own, translated, practice language. The data analyst team leader thus remarked,

> We have our weekly meeting about what we analysed the last week by telecom. We'd get our story together and try to poke in on this data. Sometimes

> we will come to them. If my student was at the site he'd just come and pass it on. […] To do something like that you need a point-to-point contact between the two groups, and that can be someone who's visiting somewhere, or it could just be a relationship that's grown up.

Other Conceptual and Technical Barriers to Communication

The barriers between high-theory and experiment are not only due to differences in technical or linguistic aspects but can also reach to the even more fundamental levels of how a problem is conceptualized. Although a full analysis of this is beyond the scope of this chapter, a few observations are in order. One of the interviewees, who described himself as being halfway between the world of high-theory and applied mathematics, interestingly explained how experimenters also tend to find that what is relevant about a physical system is very different from what a high-theoretician would consider important:

R.G.: Do you find it easy to communicate with experimental physicists?

Theoretician: No, no I don't. The reason is that they use different notations and different wave languages. It's irritating because sometimes you know from theory that a certain combination of variables is a parameter, one parameter, which is very useful. They use all the different constituent parameters. They keep 11, or three of them, together and they often miss the point. […]

He then added,

> They don't think geometrically. Interesting, you think they might but they don't. Often it's very helpful to do so. They tend to think arithmetically, which is irritating. […] There's a tendency in that direction which is very frustrating if you want to follow an argument.

Another young theoretician, who works closely with experimenters within a quantum optics lab setting, pointed out how the differences in time and material constraints also require one to adapt to different work cultures:

> On one hand there's teamwork, and having to rely on other people. On the other hand, adapting yourself … it can happen when you're working in [a] theoretical problem that you have a clear idea of where you want to go, that you begin down a road and it wasn't the correct one and you have to take another one. You also need a plan B there. As far as methodology goes you probably find fewer surprises.… In general, to understand things [theoreticians] try to simplify things as much as possible. We try to cleanse the problem of all the collateral situations, and leave it as clean as possible. In experiment sometimes it is impossible to perform such isolation.

Pickering (1999) has discussed how, apart from disparities in techniques and jargon, dissimilar ontologies also populate subcultures' perspectives on a physical system. Galison has described how in early QCD physics dissimilar ontological perspectives differentiated the theoretical from the experimental communities; according to Galison (1997), the language of experimenters was that of 'bubble chamber physics: lambdas, pions, kaons, protons, and sigmas embedded in the dynamics that describe their production and transformation', while the language of 'basic theory' (high-theory) was that of 'quarks, gluons and their interactions' (p. 652).

Varieties of Trust

As shown in the previous sections, the gaps in conceptual, methodological, technical, interpretative and linguistic elements 'force' theoreticians to trust or have faith in personally unverified experimental knowledge; but that this trust is bolstered by colleagues who form indirect links to experimental cultures. Trust can therefore take on a variety of forms: the direct appreciation of a colleague's skills or a well-earned reputation for good work; a theoretician–ambassador's passive acceptance of an experimental form of life which is initially like an alien culture; or, in more extreme cases, where the expertise links become obscured by social distance, simply 'blind faith' in a standard experimental result.

Although in everyday usage, these different types of trust are seldom differentiated, once examined in detail one can see very different mechanisms at work. Yet this differentiation is not often made in the scholarly literature, or it is mentioned only in passing. In their general review of the trust literature, Mayer et al. (1995, pg. 709) found that it suffers from

> problems with the definition of trust itself; lack of clarity in the relationship between risk and trust; confusion between trust and its antecedents and

> outcomes; lack of specificity of trust referents leading to confusion in levels of analysis; and a failure to consider both the trusting party and the party to be trusted in this same spirit.

Hardin (2002) also has a poignant analysis on the uses and abuses of 'trust' as an explanatory catchphrase, particularly in the social sciences, concluding that the incomplete analyses of trust that are common in the literature 'are often, though not always, conceptually confused' and that 'casual accounts might not even distinguish trust in another person, trust in a fact of nature, and trust in an institution' (pp. 55–56).

To better understand communication and knowledge transfer in science, I propose to look at the specific types of 'trust' that come into play in specific communication settings in order to understand how relationships between the groups of individuals in question shape their knowledge practices. My analysis is therefore also an attempt to overcome conceptual confusions on the different vernacular usages of 'trust' in STS; the intermixing of concepts as loose and diverse as *trust, trustworthiness, confidence, credibility, risk* and *certainty* and so on.

This chapter is not meant as a review of trust in STS as a whole, but rather as a starting point for refocusing future discussions on trust in relation to knowledge transfer. I will specifically concentrate the rest of my discussion on how the empirical evidence collected above points to a relationship between social distance and different trust-based mechanisms.

Trust and Social Distance

Issues on trust have a long pedigree within STS. As Shapin (1995) notes, issues of how validity, credibility, trustworthiness and trust arise in scientists and their accounts are tied to the very roots of STS, and the differentiation between validity and credibility is the birthmark of the field as an autonomous discipline. Shapin also noted the need to elucidate not only 'classes of credibility predicaments', but also 'the tactics of credibility-management that seem pervasively pertinent to those classes' (p. 258). This is the task to be carried out here in relation to the following predicament: at ever increasing social distances from experiment, how is it that theoretical physicists can claim their practice to be 'empirically sound'? The answer, following Shapin, will lie in examining the types of 'trust'-based tactics on which I have shown theoreticians to rely.

The most immediate ‘trust’ that has been illustrated here, trust in a familiar and reputable scientific colleague, is known as either ‘interpersonal trust’ or ‘foundational trust’ in the sociological literature. Interpersonal trust is not be discussed in detail here, having been analysed in depth in many other STS contexts (Collins 2001; Hedgecoe 2012; Lewis and Atkinson 2011; MacKenzie 1990, 2001; Shapin 1994; Stephens et al. 2011) and in the general sociology literature (Hardin 2002, 2006; Mayer et al. 1995; Sztompka 1999; Uslaner 2002) in how it relates to wider ‘systemic trust’ (e.g. Giddens 1990, 1991; Luhmann 1979; McDonell 1997; Sztompka 1999). Interpersonal and foundational trust, given that it is characterized by close inter-subjective bonds, constant contact between social actors and direct interactions, is the domain of *least social distance*; individuals that partake in developing this kind of trust maintain a good degree of interaction in common social settings.

As the distance between a high-theoretician and an experimental culture increases, another form of trust arises, a ‘trust-by-proxy’ that someone else has carried out the verification of knowledge or understood how an experimental result can be used in theorizing. In the ‘ambassadorial’ LIGO account, the proxy is himself linked to the group and the experimental culture directly by interpersonal trust bonds: the graduate student’s account and interactional skills have to be taken at face value by the theoretical research group, just as the graduate student must take at face value the experimental culture. But in other cases, such as the QCD collaboration, the trust bonds may be partially established by the institutionally sanctioned position of a person, and not necessarily on personal acquaintance.[10] Likewise, the young theoretician who sees the intermediary links in the spanner-to-theory chain disappear nevertheless ‘trusts’ that ‘someone’ has done the verification of experimental claims directly since he ‘knows’ that he could do it himself.

This transition from ‘visible’ to ‘partially visible’ to ‘invisible’ proxies is quicker as the social distance is increased. For example, in explaining the dynamics behind disseminating his own work in quark computer simulations, the senior QCD theoretician explained,

> You publish the paper. The paper goes out. The title obviously attracts other people. They might be the experimenters directly who have their own pet theorists, but it might be other people who have a parameterization for the decay rate which takes into account things we can’t calculate but in the middle sits this number we can calculate. I mean, *I can’t tell you their names.*

> I can think of people that might do that. It's important to just seed the world with this knowledge in chunks, and it gets picked out.[11] (Author's emphasis)

The collaborations he refers to here were set up directly by some of his colleagues as in the LIGO ambassador's case, but despite experimenters' work being fundamental to the project as a whole, he himself had little personal contact with the experimenters. Morgan (2001) and Haycock (2011) have referred to actors who accompany 'facts' in their journey from their place of origin to the place where they will be used once these are prepared for 'travel' as *chaperones*. High-theoreticians have nearby colleagues who begin the 'chain of chaperones' that connect high-theory with experiment, but that becomes rapidly obscure as social distance increases. Despite no interpersonal trust coming into play, the distant chaperones that lie beyond the theoretician's local social event horizon *must* be hypothesized to be just as trustworthy as the closer ones *if* the knowledge they produce is to be taken as a trustworthy 'fact' and *if* the knowledge chain is to remain unbroken. There is, however, a source of confidence in this trust: acquaintance with the *institutional* world of physics and the structure of the profession as a whole. This socialization is what justifiably separates it from absolute 'blind faith'.

Intrinsically tied to but different from this type of direct socialization is what Collins and Evans (2007) have referred to as 'meta-expertises': skills, technical knowledge and particularly social knowledge that allow individuals to make sense of other cultures through acquaintance with one's own, so that 'those with little scientific knowledge can sometimes make what amounts to a *technical* judgment on the basis of their *social* understanding' (p. 45). There is a limit, however, to the effectiveness of meta-expertises that is tied to the proximity of these cultures. It would likely be possible for, say, a chemist or a mathematician to make sense of the social world of physics and to make sense of its most standardized technical parts. In fact, academics, university managers and research project directors often rely on similar 'referred social knowledge' to coordinate activities with individuals from dissimilar knowledge cultures based on knowledge of their own traditions or of society at large (see, e.g., Collins and Sanders 2007). Nevertheless, the power of meta-expertises decreases as the knowledge cultures become dissimilar—that is, as social distance increases. An STS scholar could probably make sense of a 'hard' scientific culture quite well even while being a *social* scientist, but we also know that many social scientists unacquainted with the natural sciences fail to do so—and vice versa. Giddens (1991) has discussed such cases of 'referred trust', noting that one can rely on the actions of sanctioned professionals (e.g. masons and architects) not because of a personal acquaintance with them or with their social milieus, but because one is confident that their social worlds are close enough to our own that we can understand the trustworthiness of the institutions that sanction their roles (e.g. professional affiliations and accreditations).

Finally, we must consider the delegation of epistemic authority and depersonalization that distancing implies. The further a chaperone is from a high-theoretician, the less likely it is that the high-theoretician will have the necessary credentials to challenge the chaperone's authority, particularly between the furthest ends of the theory-to-spanner spectrum.[12] Given enough social distance, the chaperones themselves can become completely obscure to the endpoints of the chain. The change from partial to full obscurity implies a final change in the social mechanisms that give rise to 'trust': a transition into the *suspension of doubt*, which is as close as one can come in science to actual 'blind faith'.[13] In both social and epistemic terms, the suspension of doubt lies within the domain of the largest social distance, as all social interactions, direct or by proxy, disappear between the endpoints of emitter and receiver of knowledge.

The suspension of doubt is the sociological mechanism that allows knowledge to flow across the largest social distances. This kind of knowledge transmission is particularly important in classic STS work such as Latour's (1986, 1987) and Latour and Woolgar's (1979 [1986]) ethnographic studies of 'black boxed' laboratories: empirical knowledge factories whose end products are 'inscriptions'. As Robson (1992) points out when reconstructing the concept, 'the utilization of inscriptions [...] assists in enabling action at a distance'.

A Bundle of Trust: Virtual Empiricism

I have argued that different degrees of social distance lead to different kinds of 'trust' and to different types of trust that fundamentally underlie communication in physics. Although some authors choose to identify 'trust' solely with interpersonal dimensions (e.g. Hardin 2002; Shrum et al. 2001), many others do not make the distinction. The multiple vernacular uses of trust to some degree justify this practice, and there is no reason why we should deny this 'family resemblance' usage. Nevertheless, as has been argued, it is undeniable that what counts as 'trust' comes in

Table 2.2 Relation between trust, social distance and type of knowledge that can be exchanged

Social distance	*Dominant type of trust*	*Characteristic type of knowledge exchanged*
Minimal	Foundational/interpersonal trust	Collective, relational and somatic tacit/high-level explicit technical skills
Medium	Trust by proxy/institutional trust	Relational tacit knowledge/explicit technical references/meta-expertises and referred social judgement
Maximal	Suspension of doubt	Only explicit, inscription type

The labels marking the amount of social distance are heuristic names. The classification of tacit knowledge is taken from Collins (2010) and can be linked to work by Collins and Evans' (2007) in their 'Periodic Table of Expertise': at minimal distance, one can become fully socialized into a linguistic expert culture ('interactional expertise'), while at medium distances any type of 'ubiquitous tacit knowledge' can be gained—possibly along with bits and pieces of the collective tacit knowledge of a knowledge culture. At maximal distances, only inscriptions ('beer-mat knowledge' for Collins and Evans) can be acquired

distinct varieties. The three types of trust discussed above and their correlation to social distance are summarized in Table 2.2.

The important point here is that there is a qualitative jump between the observed mechanisms at work in the kinds of trust that supports knowledge flow as social distance increases. Additionally, the increase in social distance also implies limitations on the type of knowledge that can be meaningfully exchanged (Collins 2010; Collins and Evans 2007).

I will call the rhetorical intermixing of this set of trust-based strategies to deal with communication across social gaps *virtual empiricism*. 'Virtual empiricism' is *ab initio* an actor's category necessary for the analyst to make sense of how physics 'really is' an empirical science in spite of the deep cultural fragmentation between theory and experiment. Nevertheless, physics' virtual empiricism is *not* empty rhetoric—it is built up from tacit knowledge exchanges, socially embedded practices and acquaintance with the institutions of science, which allow theoreticians to justifiably (in a sociological sense) claim their work to be 'empirically sound'.

Virtual empiricism resonates with, but also has significant differences from, Shapin et al. (1985) similarly termed concept of *virtual witnessing*. Virtual witnessing, the establishment of trust in experimental procedures outside the circle of those directly involved in material witnessing, is illustrated by Shapin et al. (1985) in Robert Boyle's usage of literary and rhetorical devices to multiply the number of 'witnesses' to particular experiments (p. 65). In their account, Boyle tried to convince non-witnesses that his experiments were the trustworthy and accurate descriptions of careful experimental work by 'gentlemen scientists', by using naturalistic graphic portrayals and prolix descriptions of experiments.[14] Later on, this was translated into the institutional framework of the Royal Society.

The salient difference between virtual empiricism and virtual witnessing is methodological—virtual witnessing focuses on historical *processes* of institutionalization of scientific knowledge, while virtual empiricism takes trust as an *established* prerequisite for the communication of knowledge once it has been standardized. In this respect, virtual empiricism notes that institutionalization, while it may be seen as the end result of knowledge, cannot be sustained solely through institutionalization. The work of ambassadors, proxies and chaperones, the constant and sustained flow of knowledge, is just as important—no more, no less—as that of institutions in everyday science.

Reassessing Trust in STS Using Virtual Empiricism: Two Cases

The theoretical complexity added by virtual empiricism relative to the straightforward 'trust'-based accounts criticized by Mayer and Hardin can only be justified if it adds further explanatory power. In this last section, I will briefly reassess two important STS studies that problematize issues of trust and note how reassessing their findings using virtual empiricism can add a richer explanation of them.

As a first example, MacKenzie (1990, 2001) has shown that among technology users and producers one can see the existence of a 'certainty trough', a phenomenon in which different levels of certainty or trust arise depending on users' social distance to the site of production. Apparently contradicting the thesis that 'trust' is fundamental to technology and knowledge transfer, MacKenzie finds that uncertainty/trust levels fluctuate in a typical manner—rising/dipping at the extremes of the distance scale, but being low/high in the middle areas. As is done here, MacKenzie also identifies three major social distance scales of knowledge exchange:

1. Individuals close to the locus of technology production have higher levels of uncertainty regarding the technology because they know the limitations that only *insiders* (those socialized into the technology production culture) can grasp in detail; this implies minimal levels of 'trust'.

2. Users *institutionally* committed to a technology display the largest amount of certainty and trust in the technology.
3. Non-users (e.g. users of a competing technology) display the largest amounts of uncertainty, and least amounts of trust.

The apparent paradox that creators, who are much better informed about a technology, display significantly less trust than users who are further away is dissolved if, rather than looking at how an unspecified, monolithic type of 'trust' changes across social distance, one focuses on the *type* of trust that is *predominant* in each situation; the trough then would seem either to disappear or smooth out since

1. Experts within or socialized into the locus of production must display high levels of foundational and interpersonal trust if they are to be part of a community at all, but then may be highly sceptical of accounts that try to institutionally legitimize a technology or that try to hide away the intrinsic uncertainties of technology (Collins 1992). *At this distance, minimal-distance trust strategies are necessarily high, even though institutional or proxy-based trust is minimal.*
2. As distance from the locus of production increases, MacKenzie finds that institutionalization standardizes the usage of technology alongside an increase in the delegation of epistemic authority to 'experts' in order to minimize uncertainty, a phenomenon well known to the STS literature (Lahsen 2005; Star 1985). Thus, in mid-range settings, *institutional trust is high, even if there is no deep acquaintance through socialization of the core culture of a field of expertise.*
3. At the extreme ends, non-users may of course have no acquaintance or trust in any form, or they may be absolutely passive recipients or users of the technology or knowledge. In the latter case, we are still within the realm of virtual empiricism, specifically where the suspension of doubt operates. *Although interpersonal and even institutional trust is absent, 'trust' in abstract systems (i.e., meta-expertises and referred social knowledge) or suspension of doubt allows passive users to operate with the given technology, even if they hold nothing beyond operational knowledge of the technology.*

We should keep in mind that social distancing is a twofold process that at the far extreme involves the suspension of doubt as well as the relegation of epistemic authority to 'experts' and their 'standardized' opinion.

The case may then arise in which individuals do not use a piece of technology at all or in which they reject knowledge claims not because of acquaintance with them, but because of complete separation from the social world of their source. In that case, experts/knowledge producers and the group of 'non-users' will live in completely dissimilar or antagonistic social worlds, even though the experts may still be recognized as such. In the case of 'crank science', for example, many theoretical physicists have recorded how the former often attack the experts themselves as 'defenders of the orthodoxy' (Collins 2014). The scenario then lies *outside* the realm of virtual empiricism and within the realm of complete *mistrust*, for, as Luhmann (1979) argues, *some* type of familiarity between social actors is necessary if one is to talk about the possibility of any sort of trust relationship existing between them. Beyond virtual empiricism's border lies, for example, the abyss between 'experts' and 'laypeople' responsible for the lack of trust which has been identified as one of the fundamental problems of contemporary STS scholarship (Collins and Evans 2007; Irwin 2006).

A final application of virtual empiricism can be made by examining the work on Big Science collaborations by Shrum et al. (2001, 2007). The study they develop strongly questions the importance of 'trust' in STS analyses, finding 'trust' irrelevant to perceived success in strongly collaborative scientific contexts. Their empirical analysis of a number of large-scale collaborations leads to the claim that that 'the role of trust in Big Science has been greatly exaggerated', so that in collaborative contexts trust 'is not of any fundamental significance' (Shrum et al. 2001: 682). This contrasts with typical STS outlooks such as Knorr Cetina's (1999), which claim trust as fundamental to collaboration.

Working from an informed outlook on trust, Shrum et al. (2001) identify two forms of trust: 'encapsulated interest' trust, that is, trust relationships based on mutual, shared interests; and 'confidence' or 'an orientation towards institutions such as government or the media' (p. 687). In fact, upon closer scrutiny, what Shrum et al. argue is that encapsulated interest trust (of the minimal social distance type) is irrelevant, but that institutional trust is necessary for conflict to be minimized. But Shrum et al. earlier point out that what they call 'foundational trust' 'is necessary for collaboration in general, within science and without', yet that in being ubiquitous to all social interactions 'it warrants no special attention' (p. 686). Strangely enough, given that it is earlier a 'taken for granted' element of social interaction, Shrum et al.'s (2007) empirical analysis actually probes the relevance of *interpersonal trust*, while later slipping into a non-specific usage of 'trust' that forgets the initial differentiation made between the foundational trust, institutional trust and confidence (pp. 151–194). This leads them to the seemingly anti-climactic conclusion (when compared with their strongly dismissive statement about 'trust') that their empirical evidence 'pertains to *variations* in trust' (Shrum et al. 2007: 215) and that 'trust both is and is not important' (Shrum et al. 2001: 718).

Despite the confusion in terminology, the conclusion that minimal-distance trust is irrelevant in collaborative contexts is certainly a striking one, and Shrum et al.'s results are interesting to contrast with the LIGO account presented here. LIGO has also faced problems on how to deal institutionally with the distances between its various constituent cultures and has worked to create platforms of cooperation. Although, like Shrum et al., I have argued that institutional trust is *predominant* at certain distances, LIGO collaborators strongly pointed out that interpersonal trust is necessary to bolster overall successes. A top-level LIGO manager commented, for example, on how an ongoing effort to create a new comprehensive collaboration programme involved considerable help from the data-analyst team leader who during the interviews had placed a lot of emphasis on the necessity of having point-to-point interpersonal contact:

> Even within the theoretical area there might be difficulties amongst different levels to talk to, for example, numerical relativists about numerical simulations of black holes. That's one area where we had a lot of difficulty understanding their language and effort was put in. [F—the same data-analyst who described the ambassador student] was one of the leaders in starting a group called NINJA which helped create a platform in exchanging ideas. Not just ideas! Also to set up a language, a common language between these two. It requires a lot of effort.

The Numerical INJection Analysis (NINJA) group has decided to focus on very specific topics that are of common interest to all participating groups.[15] Unsurprisingly, these topics have the characteristics of boundary objects with which all the collaboration's members can interact with directly ('the merger phase of binary black hole (BBH) coalescence'). According to the top-level manager's description, NINJA is an attempt to create 'a common language' for collaboration—that is, the platform has all the characteristics of an emerging, coordinated trading zone. LIGO is similar to the institutional settings analysed by Shrum et al. in which trust 'is and is not important'. It is important for the data analyst to understand the glitches of experiment through the student socialized into the experimental site culture, and in turn, these sparse but vital interpersonal links ease the way and allow other communication infrastructures to operate efficiently. Likewise, the creation of boundary objects and trading zones is necessary within the larger collaborative structure, but these also rely on the interactional bridges that socialization practices create.

Rather than arguing that some sort of generic 'trust' *or* any specific form of it is *the* critical component of scientific communication, virtual empiricism starts out from the observed relevance of *all* forms of trust in scientific communication in all real settings and for all theoretical models of interaction (e.g. enculturation, boundary objects and trading zones). However, it then does specify the expected trust channel that ought to dominate depending on the social distance between interacting cultures, an issue that should be empirically accessible once a scale of social distance is defined. Then, at least one 'trust' element of virtual empiricism will be present for communication to occur, and it will mould the type interaction that is established between interacting cultures.

Finally, returning to the problem of disunity, I have illustrated that while common-ground strategies may dominate scientific communication in particular settings, enculturation practices have a similarly important place in sustaining communication at closer social distances. Enculturation relies on interpersonal trust, while common ground strategies depend on trust at intermediate and large distances—the virtual empiricist's expanded version of trust bridges social distance gaps that knowledge must flow across, whichever ones of these mechanisms are finally put into place.

Many issues on trust remain to be explored. The reverse communication flow of that analysed here—the communication of theoretical knowledge to experiment—has not been dealt with and certainly can make for an interesting follow-up to this study. It is expected that the flow of theoretical knowledge to other subcultures would be of a similar nature and initial studies of the connection between high-theory and other sub-cultures—for example, the relationship between physical theory and pure and applied mathematics—support this thesis (Reyes-Galindo 2011).

One would also expect that the approach, given that it is based on general sociological arguments, should be relevant not just to physics but to scientific communication in general, which would require further empirical investigation. Additionally, issues to do with the types of trust that may appear in science or technology are by no means exhausted by my analysis. There is ample room for discussions of trust in STS beyond the virtual empiricism domain: discussions of trust not only between individuals or even groups, but also extending 'trust' to such diverse areas as computerized systems high-theory in its daily practice?

Consider the following reply when a senior high-theoretician was asked about the (MacKenzie 2001), public institutionalized standards (Porter 1996) and even aesthetic practices in science (Carusi 2008).

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Notes

1. See Collins (1992), Galison (1997), Knorr Cetina (1999) and Feyerabend (1975) for the locality of practice. See Dupré (1995) and the works in Galison and Stump (1996) for specific discussions on the relationship between the locality of practice, language and metaphysics and the 'disunity thesis'.
2. Merz and Knorr Cetina (1997) use a typology of theoretical physics micro-cultures that divide its practice into 'mathematical', 'high-theoretical' and 'phenomenological'. Galison (1997) also spells out the difference between a 'theoretical' and a 'phenomenological' culture in non-experimental physics. Other classifications are of course possible: see Reyes-Galindo (2011), where a classification of theoretical physics into a more diverse range of 'epistemic cultures' is carried out in the spirit of Knorr Cetina's work on the diversity of experimental 'styles'. For brevity's sake, I have ignored this more accurate classification, as well as Merz and Knorr Cetina's well-known theses on the diversity of micro-cultures existing within experiment as a whole.
3. In general, biological 'theory' tends to be connected with experiment. Biologists seem comfortable with the supposition that even 'theoreticians' ought to have ample lab experience, an attitude rarely if ever seen in theoretical physics circles. Immunologist Medawar (1969) wrote that 'most scientists cannot be classified as either experimentalists or theorists, because most of us are both', a statement that, while plausible in biology,

is definitely not the case in physics (p. 57). However, Shrager (2010) points out that in molecular biology, there has been a shift towards a situation similar to that in physics, where data producers and data users are separated from each other, with computer scientists and statisticians acting as middlemen. One of the theoreticians interviewed for this project—a former theoretical biologist who had worked and co-authored papers with one of the most eminent molecular biologists of the twentieth century—commented,

> I arrived at theoretical biology just when the great era of theoretical biology had ended… the structure of DNA, how proteins are structured … the kind of informational picture of biology had just been completed. There followed an epoch where everyone was so impressed by the success of theoreticians in biology that they backed it long after it had produced anything worthwhile. I was brought into the field with a background in maths because they thought I'd have the skills to do some theoretical biology. I did experimental work, which quite rightfully you had to do too, but I was meant to be a sort of semi-theoretical biologist. And I consider it to be a bogus profession; nothing ever came out of it. When I say it's a bogus subject in biology I mean there is no theory [by itself]. I sort of left molecular biology, fair enough, when it was becoming sensible to be a theoretician of a different kind, mainly the kind who does computer science and tries to organize data.

4. As Pickering (1999) summarizes, in their popular writing physicists describe their field as one where 'experiment is seen as the supreme arbiter of theory' (p. 4). But Pickering (1981, 1984) has also shown how, in contradiction to this public façade, 'no scientific claim at either the instrumental or phenomenal level is absolutely compelling'. See also Duhem (1996).
5. See Collins (1984, 1992). Discussing the importance of tacit knowledge in particle detector technology transfer, Galison (1997) points out that in the dissemination of detector technology '[t]here is no doubt that there were instruments and effects the replication of which required no movement of personnel and objects' (p. 54). Nevertheless, these were exactly the instances in which 'scientist-to-scientist "craft exchanges" [did] not figure at all'.
6. There is, however, an important twentieth-century theoretical tradition that tends to downplay the role of experiment, which I refer to as the 'first principles' approach to physical theory. This tradition played a significant role in the discourse surrounding the search for Grand Unified Theory of

physics late in the century, particularly in popular expositions of theoretical physics. In physics lore, Einstein is often portrayed as the father of the unification ideal, and his search for 'symmetry' and 'beauty' fills first-principle theory mythology. Zee (1986), for example, writes,

> my colleagues and I in fundamental physics are the descendants of Albert Einstein; we like to think that we too search for beauty. Some physics equations are so ugly that we cannot bear to look at them, let alone write them down…

He added, 'when presented with two alternative equations purporting to describe Nature, we always choose the one that appeals to our aesthetic sense. […] Such is the rallying cry of fundamental physics'. Zee juxtaposes the attitude of 'fundamental physics' with those of

> phenomenological theories, constructed simply to 'explain' a given phenomenon. Theorists craft such theories to fit the data, and get out as much as they put in. They lead their phenomenological theories, rather than the other way around. Such theories may be of great practical importance, but typically they tell us little, if anything, about other phenomena, and I find them to be of no fundamental interest.

See Galison (1997: 643) for a discussion of this same situation within quantum chromodynamics.

7. Fermi is often portrayed as the last of physics' 'Renaissance men', a polymath who could build a crucial experiment just as well as he could construct the theory to explain it, see Dyson (2004) and Galison (1997: 798).
8. Laser Interferometer Gravitational Wave Observatory (LIGO) is a collaboration of over 800 scientists housed at numerous institutions around the world, with one central aim: to find theoretically predicted but as-yet-undetected gravitational waves. It is currently the largest project ever funded by the National Science Foundation. For a full history of LIGO, see Collins (2004, 2011a).
9. Collins (2011b) has described ambassador-like scientists in LIGO as possessing 'specialist interactional expertise', to stress that they have only linguistic and no practical immersion in the domain that they translate from, illustrating the existence of these 'interactional ambassadors' or 'special interactional experts' in the gravitational wave community (p. 287). Collins and Ribeiro (2007) have used the 'interactional expertise model' to analyse similar cases that involve the training of 'linguistic ambassadors' and

'cultural translators' to mediate intergroup communications in collaborative techno-scientific contexts.

10. See Giddens (1990, 1991), Luhmann (1979) and McDonell (1997) for theoretical analyses of institutional trust and how it is arises from interpersonal situations. See Shapin (1994) for a historical analysis of the emergence of interpersonal trust and its transition to institutional trust in early physical science (the depersonalization of trust) as well and Hedgecoe's (2012) empirical analysis of UK Research Ethics Committees and the intermixing of the interpersonal and institutional forms of trust in practical contexts.
11. As it turns out, when carrying out a citation analysis of this theoretician's publications after the interview, the author found that—unknown to either the interviewer or the interviewee—one of the articles the interviewer had co-authored in his period as a physics researcher had once been cited by the interviewee.
12. In one of the few in-depth case studies on theoretical physics work, Kennefick (2000, 2007) analysed a debate within a general relativity theoretical community in which computer simulationists lying outside the relativist 'core set' presented results that were at odds with the theoretical standard views. Kennefick has argued that the simulationists faced several disadvantages in trying to overcome the relativists' rejection of their result, including the lack of 'social capital' at their disposal and their ability to understand the 'evidential context' of their results as outsiders. Thus, social distance was associated with both their ability to meaningfully intervene in the controversy, as well as in finding a position of epistemic legitimacy within the community where they sought for the results to be relevant. See also Pinch (1986).
13. This take on the 'suspension of doubt' follows Schütz's (1932 [1967]) description of the 'natural attitude' (p. 98). See McDonell (1997), Sztompka (1999: 13) and Bourdieu (1975: 23) for sociological analyses of the suspension of doubt.
14. In relation to experimental knowledge, Gooding (1986) has used cases in the history of physics to explain how once a novel phenomenon has reached a point of stabilization, experimenters construct rationalized reconstructions of the experimental phenomenon in order to 'articulate a concept implicit in exploratory practices [...] or enable a phenomenon to be realized and made accessible to many observers' (p. 219). Gooding notes that rational reconstructions of experiments such as Faraday's or Davy's are the end results of processes of reification of meaning, as they go through stages of 'construal', interpretation', 'definitive interpretation' and 'exemplars', so that as the 'phenomenon' stabilizes its recipients become increasingly passive to the received knowledge.

15. The Numerical INJection Analysis (NINJA) project is described in the group's wiki as follows:

    The goal of the NINJA project is to bring the numerical relativity and data analysis communities together to pursue projects of common interest in the areas of gravitational-wave detection, astrophysics and astronomy.

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CHAPTER 3

Mutual Linguistic Socialisation in Interdisciplinary Collaboration

Tiago Ribeiro Duarte

Introduction

Language is a central topic in studies of interdisciplinary scientific communication and collaboration. Importantly, Science and Technology Studies (STS) have revealed that different scientific communities frequently seek to develop some degree of mutual understanding in order to improve their collaborative efforts (Galison 1996, 1997; Jeffrey 2003; Shrager 2007; Collins 2011; Reyes-Galindo 2014b).[1] STS case studies that have focused on the development of mutual understanding have concentrated on explaining how scientists working in particular projects develop linguistic skills to mediate communication while their communities remain largely uninformed about the language that is not their own (e.g. Galison 1996, 1997; Monteiro and Keating 2009; Collins 2011; Reyes-Galindo 2011, 2014b; Shrager 2007).

Here I explore a case in which two communities—paleoclimatologists and paleo-modellers—have engaged in a deliberate effort to mutually linguistic socialise each other to improve their collaborative efforts. In other words, paleoclimatologists and paleo-modellers make continual

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efforts to increase community members' competence in the language of the field that is not their own and to train the new generation of scientists to have a better linguistic understanding of both areas. Even though in some cases members of these fields learn a few skills from the other domain, most efforts focus on learning solely the language. They are thus not seeking to become 'contributory experts' (CE) in each other's fields (Collins and Evans 2007)— that is, to learn the skill-sets to be able to produce data, model output, and so on. Rather, their efforts are directed at acquiring linguistic competence to mediate communication between them.

Existing STS literature describes how in similar cases scientists use different levels of linguistic competence to communicate in collaborative projects. Studies have shown how scientists can acquire a high level of competence in the language of another domain to facilitate communication in collaborative projects (Shrager 2007; Reyes-Galindo 2011). In other words, they acquire 'interactional expertise' (Collins and Evans 2002, 2007, 2015; Reyes-Galindo and Duarte 2015), which consists of the mastery of a language.[2,3] Working from a different angle, Galison (1996, 1997, 2010) has pointed out that scientists frequently develop interlanguages—a combination of speaking in simplified registries with the merging of concepts of the languages of the scientists involved in the collaboration—to facilitate communication. Interlanguages are typically used in situations in which members of a collaborative project do not have high linguistic understanding of each other's field.

In this chapter, by examining the interactions between paleoclimatologists and paleo-modellers, I seek to elucidate what level of linguistic understanding is the result of these communities' efforts at mutual socialisation. I argue that, in most cases, members of these communities do *not* have enough immersion in each other's domain to acquire interactional expertise (IE). This process thus results in individuals developing lower levels of competence than IE in the language of their collaborators.

This point leads to the issue of how one can distinguish between IE from lower levels of linguistic understanding in real research settings. As the acquisition of expertise depends on a process of socialisation in the community of relevant experts (Collins and Evans 2007), this question directly relates to how much linguistic immersion one has had in the community's discourse. As IE consists of the mastery of the language of a field of expertise, a long process of immersion in the form of life of the expert field is a *sine qua non* for its acquisition. Scientists only master their own domain languages after years of training and supervision by senior experts. It is usually only after they finish their PhDs that they will be able to speak it at a high level of competence. Therefore, in the case of scientists acquiring IE in a domain which is not the one in which they have CE, several years of immersion are needed for them to completely master it. This process tends to happen in the following way. A paleoclimatologist who has never talked, and/or listened to paleo-modellers speak about their work, has no IE in their language. If she starts collaborating with a group of them in a project in which she has only a few informal conversations to clarify very basic points, she will start to acquire linguistic competence in their language. If the initial project leads to a longer one involving, for instance, a week-long workshop and regular conversations at university offices/corridors or through Skype, the paleoclimatologist will significantly raise her competence in paleo-modelling language, but will still fall short of being an interactional expert. If the collaboration goes on for several years, including several workshops, joint panels at conferences, extensive informal conversations, and so on, she might become so fluent that she will become indistinguishable from paleo-modellers when speaking about their domain. At this point, she will have mastered their language and will have reached the level of interactional expert.[4]

This chapter is based on the methodology of participant comprehension (Collins 1983, 1984, 2009; Reyes-Galindo 2014a), which has been used in a number of STS studies (e.g. Collins 1992; Pinch 1986; Reyes-Galindo 2011, 2014b; Ribeiro 2007). The methods deployed were semi-structured interviews and participant observation. Forty scientists based at British universities from different nationalities were interviewed, including paleoclimatologists, paleo-modellers, and experts from adjacent domains, such as geochemistry and micropaleontology.[5] Some interviews were followed up by email exchanges with the interviewees to clarify particular points that emerged as interesting when examining the interviews' transcripts. Participant observation included attendance at research seminars, international conferences, and an international summer school with experts from all over the world, and visits to laboratories in two British institutions.

Paleoclimatology and Paleo-Modelling

Paleoclimatology is an amalgam of disciplines whose main goal is to reconstruct the past climate, particularly from periods before the emergence of consistent climate records.[6] It is an empirical science that deploys a number of geochemical, micropaleontological, and sedimentological techniques, which are known as climate proxies, to work out how the climate system worked in the past. *Paleo-modellers*, on the other hand, use computer models to simulate past climates. This is an area of science that is not involved with the generation of empirical data, although empirical data is fed into climate models. The expertise, epistemic culture (Knorr-Cetina 1999), and the research instruments used by members of these domains are considerably different.

As I have described elsewhere (Duarte 2013), these fields are subdivided into several sub-domains. Paleoclimatology is made up of scientists who specialise in particular archives (marine sediments, ice-cores, tree-rings, corals, and so on), techniques, time intervals, climatic phenomena, amongst others. These experts do not master each others' languages as there is too much diversity within the field. For this reason, they tend to have a lower understanding of other subspecialties of paleoclimatology than those immersed in each of them. For example, paleoclimatologists specialised in time intervals of millions of years ago are not likely to have a high level of understanding of archives such as tree-rings, which can hardly produce data from much beyond a few thousand years back in the past. Yet, their understanding of tree-rings is still much higher than of those who are not paleoclimatologists due to their formal training. In this sense, conversation between experts specialised in different time intervals may be mediated by changes in registry (Galison 1997, 2010) or trust (Shapin 1994; Shackley and Wynne 1995; Reyes-Galindo 2014b). In specific projects in which they deem necessary a high level of linguistic understanding, they may look to acquire IE in each other's practices.

The same happens with paleo-modellers. They also tend to specialise in particular types of model (e.g. statistical modelling, box-modelling, Earth system models, etc.), climatic phenomena, time intervals, and so on. Each of these specialisations entails a high level of understanding of particular subspecialties, which modellers with other contributory expertise will not have. As argued above about paleoclimatologists, their communication might be mediated by changes in registry or trust. IE is acquired only in particular collaborative projects in which participants feel the need for a higher level of understanding of their collaborators' subspecialties.

Trade at Work: Collaboration Between Paleoclimatologists and Paleo-Modellers

Paleo-modellers and paleoclimatologists currently have strong collaborative ties. They set up different types of collaboration according to the goals of specific research projects. As pointed out above, paleo-modellers feed paleoclimatological data into their models.[7] In these cases, they collaborate with paleoclimatologists who review the literature and compile data for them.

Paleoclimatologists usually become interested in collaborating with modellers to test hypotheses they have developed to interpret their data. They are sometimes unsure about which variables have triggered a climatic process. Where several hypotheses are held the data alone cannot specify which is the most plausible. There are several feedbacks in the climate system and it can be difficult to identify which variable was the cause and which variables were the feedbacks of a given event. In these situations, paleoclimatologists sometimes collaborate with computer modellers who simulate how the Earth system reacts to alterations in different climatic variables. The models then provide insights into the plausibility of particular hypothesis. Paleoclimatologists 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 change they sometimes use models' output to refine strategies for data collection.

Paleo-Modellers and Interactional Expertise in Paleoclimatology

According to some paleo-modellers interviewed for the project, it was only in the 1980s that climate modellers became interested in modelling paleoclimates. At this point, they were outsiders stepping into 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 chapter. It is important however to point out that paleo-modelling emerged when computer modellers became interested in past climates. Most had a background in mathematical physics. To model paleoclimates 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, which are the main elements that constitute the language of paleoclimatology (Duarte 2013). As pointed out above, paleoclimatology is a heterogeneous domain that has many subspecialisations. Modellers would therefore not be able to acquire a high level of understanding of the whole history of the Earth, of all mechanisms of change of the Earth system and of all different proxy systems used to generate paleodata. They therefore had to acquire a high level of understanding of the particular climatic phenomena they were intending to simulate, the time intervals they were interested in, and the archives and proxies that could be used to study them so as to be able to do their modelling effectively. As a result, there is an overlap in the language spoken by members of these communities interested in similar phenomena and time intervals that mediates communication between them:[8]

Emma: When you're working with climate modellers that are interested in similar problems that you are, it's pretty straightforward because they might actually know what you're doing. They might know some of the literature that you know. So, they know the problems. In this case of James [a paleo-modeller Emma worked with], he knew perfectly the context and we were working on the same time interval.

Although there is this overlap, there is difference between these communities as well. Modellers began to learn about paleoclimates, but differences remained in their technical languages. Most modellers, for instance, have not become immersed in the literature on data generation. When they read the paleoclimatology literature, they tend to focus on new interpretations of paleoclimatic phenomena rather than on the details of data production. The following quotation from a paleo-modeller who had been immersed in the paleoclimatology community for around 25 years talking with its members and co-authoring papers with them 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 production up to date:

Louis: 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. 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.

However, he still has a high level of linguistic understanding of the proxy systems used to generate data that came from his immersion in the paleoclimatology community associated with collaborating with some of its members, as this extract from an email conversation I had with him reveals:

Louis: I believe I have a good critical understanding of these [well established] proxies, such as oxygen isotopes or palaeobotanical indicators (which both can produce quantitative estimates of climate), and sediments (which produce more qualitative estimates of climate, such as wet summers) for two reasons. Firstly, they have been around for a long time and therefore I have been exposed to a lot of the discussions on their strengths and weaknesses. In addition, because I have been working in this subject or almost 25 years now I have often been around when they were being developed/refined and when there is debate in the community, that is the best time for learning their strengths and weaknesses. Another reason I feel comfortable with these proxies is that I have worked and published comparisons of the proxies with my model output, normally working with leading scientists working with these proxies and the discussions associated with such work has left me well understanding the data.

The fact that Louis has a high-level of linguistic understanding of proxy systems, but does not follow the paleoclimate data production literature closely, should not be taken as meaning that he does not have IE in paleoclimatology. A study of the role of mathematics in physics helps clarify this point. Collins (2007) found through a survey he carried out in four different physics departments that the majority of physicists, when reading scientific papers from their field, do not typically perform a step-by-step check of the mathematical equations which are used to prove the main points. They either skip the maths part and assess the paper in terms of whether or not it makes sense according to the rest of the literature or have a quick look at the equations only to have a sense of what direction the mathematical reasoning follows. In this sense, when reading papers they tend to privilege a conceptual reading over following in detail the generally complex mathematical proofs.

Returning to the collaboration of paleo-modellers and paleoclimatologists, if we transpose the lessons from Collins to this case study, there is no reason why one should expect interactional experts in paleoclimatology to follow the entire paleoclimatological literature on data generation. What an interactional expert needs is a conceptual understanding of how data is generated—that is, the principles behind each proxy. If there are specific issues with particular data sets, it is not up to the interactional experts to detect them but to the contributory experts who master the relevant techniques. Then, these issues will spread to the rest of the community through linguistic exchange, be it written or spoken.

Having made this point about some paleo-modellers having acquired IE in paleoclimatology, it is worth noting that, unlike Louis, who has been collaborating with paleoclimatologists for over two decades, several of them have not reached a high-level of understanding of data production in paleoclimatology. The following modeller is an example:

Roger: […] 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 [the] Urbino [Summer School in Paleoclimatology] 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.

Paleo-modellers' variable levels of understanding of paleoclimate data are the main reason why members of these communities believe that a process of mutual linguistic socialisation is necessary to improve their communication. But, before examining this process, let us look at the paleoclimatological community and their linguistic understanding of paleo-modelling.

Paleoclimatologists and Interactional Expertise in Paleo-Modelling

The emergence of the collaborations between members of these communities is also related to a wider phenomenon taking place in climate science. Since the mid-1990s, computer modelling has grown in importance in paleoclimatology, as in climate science as a whole (Demeritt 2001; Edwards 2010; Jasanoff and Wynne 1998; Shackley et al. 1998). In climate science, it is argued that, as it is impossible to run experiments on global climate change, models are the best tools available. They enable scientists to bring together data from all over the world and run tests on them. As pointed out above, similar reasons have influenced paleoclimatologists to collaborate with modellers—namely, the possibility of testing hypotheses on how to interpret particular data sets. Consequently, a significant number of paleoclimatologists became interested in collaborating with modellers. Their work was facilitated by the fact that modellers were already trying to acquire linguistic competence in their domain. Paleo-modellers, in turn, had already acquired a high level of linguistic understanding of the mechanisms of climate change in geological time scales that were related to their research interest and in the parts of the history of the Earth relevant to their projects. Furthermore, paleo-modellers were also acquiring variable levels of linguistic competence in the principles underpinning the generation of paleoclimatic data that was particularly relevant to them. This created some overlaps in the languages of these fields, which helped communication.

However, to collaborate effectively with modellers, paleoclimatologists also had to learn about paleo-models. The following quotation exemplifies what paleoclimatologists sought to acquire a linguistic understanding about:

Tiago: So, when you collaborate with modellers to which extent do you try to be well informed about the [model] 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 the 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.

As I have argued above about IE in paleoclimatology—that it does not involve following in detail the data production literature—for IE in paleo-modelling paleoclimatologists are not expected to acquire any understanding of the model codes. Such understanding is part of the contributory expertise in modelling. If there are issues with these codes, contributory experts have to deal with them and the issue spreads in the community through language.

The level of understanding of paleo-modelling acquired by individual paleoclimatologists depends on how close and lengthy their collaborations were. As I will point out below, some paleoclimatologists were hired by paleo-modelling departments to help bridge the gaps between these fields. In these cases, paleoclimatologists can potentially become IE in paleo-modelling if they work in these departments long enough. In other cases, paleoclimatologists have a very low linguistic understanding of paleo-models or no understanding at all as the following quotation shows:

Isabel: Yes, I know that models have lots of uncertainties and they make a lot of assumptions. 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 learning what some of these assumptions are. You could easily have a career in paleoclimatology and not understand all the assumptions that go into climate models, because it's such a distinct field.

Thus, as with paleo-modellers when it comes to understanding the different proxy systems used in paleoclimatology, paleoclimatologists have variable levels of understanding of modelling. The widespread perception that several paleoclimatologists still need to have a much better linguistic understanding of paleo-modelling has also contributed to the deliberate effort of these communities to enhance each other's linguistic understanding through the process of mutual linguistic socialisation I describe in the next sections.

The Mutual Linguistic Socialisation Process: Formal Courses

As part of the mutual linguistic socialisation process that is being carried out by these communities, there is an attempt to educate the new generation of paleo-modellers and paleoclimatologists in both fields. These efforts, however, are not the same as those made to socialise students to become contributory experts in their own domain, which involve a great deal of hands-on activities—such as laboratory practice and fieldwork in the case of paleoclimatologists and computer programming and debugging in the case of modellers—and the continual supervision of senior academics. In this case, socialisation is mainly linguistic and involves a much lesser degree of practical activities. The ultimate goal is for graduate students to acquire a higher linguistic competence in both data production and modelling than that of the current generation of scholars—that is, something much closer to IE, although, as I argue below, the evidence is that this level of understanding usually cannot be reached through the means used by these communities. Attempts at mutual socialisation are being currently deployed in two main ways: the creation of formal courses where students learn about paleoclimatology and paleo-modelling and the shared supervision of PhD students by members of both communities.

The Urbino Summer School in Paleoclimatology was designed with the objective of providing a basic training for the new generation of paleoclimatologists and paleo-modellers in both areas of investigation.[9] In this yearly event, which started in 2003, around 30 experts with different expertise, including paleoceanographers, micropaleontologists, geochemists, paleo-modellers, working on the whole range of geological time intervals and using a variety of techniques, lecture for a three-week period graduate students with background in data generation and in paleo-modelling. As a result, young researchers receive basic linguistic exposure to areas of expertise which were not their own. Furthermore, there is great deal of informal socialisation in this event, as students and faculty frequently go out together for dinner and for drinks.

It is not possible to provide a precise measure of how much linguistic competence is acquired by students at the event as this depends on the background of each of them, 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, it is not expected that they will in this period become IE in any subfield of paleoclimatology or paleo-modelling that is not their own, as this would need a much longer immersion in the relevant communities. But it does provide them with at least a general overview of most subareas of these fields.

During the summer school, there are also 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, solving geochemical equations, and writing the commands to run a paleo-model. In this sense, the students also experience physical immersion (Ribeiro 2013) in some of these domains' practices. These activities, however, are very short and much less time is spent on them in the summer school than in the formal lectures, so that they are not enough for anyone to become contributory or interactional experts in any of the fields. Yet they do provide an opportunity for the students to have further linguistic socialisation in both domains as they have the opportunity to ask questions that would not be brought up if they were not doing these activities under the supervision of experts (Ribeiro 2013).

Universities such as the Massachusetts Institute of Technology (MIT) and the University of Bristol have also developed courses where students have training in both 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 paleoclimatological problems described a joint Master's programme where students are trained in modelling and in producing data. The initial motivation for setting up this course was a frustration caused by difficulties in communication between paleo-modellers and paleoclimatologists:

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 Master's programme here in Earth system science, which I've just taken over and we just started really. And the philosophy of this Master's 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 (emphasis added).

Student training in both areas is a deliberate attempt by these communities to intensify links between their fields. Again, this is not going to make any of the students contributory or interactional experts in all these practices. As Tim points out in the quotation, they receive only a *basic* training in modelling and data production. As at the Urbino Summer School, what is at stake here is learning these domain languages at a simplified registry, not at an expert level.

Mutual Linguistic Socialisation: Joint Supervision

Another mechanism that is being deployed by these communities to enhance mutual socialisation is the joint supervision of PhD students by paleoclimatologists and paleo-modellers so that their doctoral research includes modelling and data generation. In this case, as has been argued in the previous section, these students do not become experts in both fields. Becoming a contributory expert in a scientific domain involves experiences that fall beyond formal training (i.e. lectures and general coursework) and requires a process similar to apprenticeship in medieval craft guilds (Charlesworth et al. 1989: 92–93). It is necessary to spend years acquiring tacit skills under the supervision of senior researchers to become fully accomplished in the practices of a scientific domain. As Mody and Kaiser (2008: 385) have pointed out,

> only after extensive practice, drawing on a combination of text-based and tacit routines, do research skills become second nature for new technical trainees. Only after intense pedagogical inculcation do new recruits develop the 'disciplined seeing' or 'hands' of accomplished practitioners. (Goodwin 1994, 1997; Doing 2004; Mody 2005)

In addition, acquiring CE in two domains within a PhD timeframe is a highly challenging enterprise that may lead students to face serious difficulties:

Karin: I've got one student who is doing her thesis on both geochemistry and modelling. It's a big deal. I'm not sure we should do that again actually. I think it should be one. It's frustrating from all perspectives because there are two PhDs there. But she wanted to do it like that, so she has only herself to blame. But that's unusual. Typically, if they are data people there's usually a component of modelling, if they are modellers there's usually at least a component of data compilation.

The research project of the students receiving training both in paleomodelling and paleoclimatology thus is usually in one of these fields and has a smaller component of the other. They acquire a general linguistic understanding and very basic practical skills in the area that is not their main one.

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 a full-blown data generator, they usually undertake this activity under the supervision of their supervisors and do not become fully accomplished in the 'data side' at the end of their PhDs. A paleoclimatologist who is very skilled in sedimentology described to me how she co-supervised PhD students with climate modellers:

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. […]. 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. [...]. 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: No, 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 field-work 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. [...]. *So, is it an apprenticeship? No, because they don't actually get to be proper field geologists.* (Emphasis added).

These PhD students at times play a role similar to that of 'interactional ambassadors' in large physics collaborations (Collins 2011; Reyes-Galindo 2011, 2014b). These individuals (usually PhD students) are purposefully trained to be points of contact with other research groups. In these projects, when a group of scientists depends on knowledge or data produced by another group whose language and techniques they do not master, researchers are sent to spend some time working with the other group. The idea is that through immersion these ambassadors acquire some level of linguistic competence in the other domain—how high this level will be will depend on how long the immersion is. When they come back, they can then help their original group by answering technical questions and queries related to the work carried out by the group they visited.

While paleo-modelling PhD students who are jointly supervised by paleoclimatologists and paleo-modellers end up with only a basic understanding of what paleoclimatologists do, they can still contribute, even if modestly, to the process of building bridges between paleoclimatologists and paleo-modellers by explaining to their groups the basics of what scientists from the other domain do:

Karin: For instance the one [modelling PhD student] I went to Morocco with has actually given a talk to the group. What she wanted to

> do was to give 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.

Interestingly, in terms of the mutual socialisation process, what really matters in this example is the linguistic understanding these students develop of how paleoclimatologists do fieldwork, as it is only this understanding that is shared with their research groups. They do not acquire sufficient hands-on skills to deploy in the field, which means they cannot train other modellers to do fieldwork proficiently.

Mutual Linguistic Socialisation in Scientific Events and in Research Projects

Despite the mutual socialisation efforts described above, there remain difficulties in communication between the two communities. There are various reasons for this. First, the more senior researchers, unlike the younger generations, have not received training in the domain which is not their own. Secondly, the mutual socialisation process cannot provide the new generation of scientists with a high linguistic understanding of all subareas of paleoclimatology and paleo-modelling. In fact, none of the examples presented above provided students with opportunities to acquire IE in both fields. The remaining gaps between paleo-modellers and paleoclimatologists are bridged through reading the literature, talking, and attending conferences. Reading the literature is an important part of this process. However, reading the literature alone is not enough for being socialised in a domain as scientific papers cannot convey tacit knowledge (Collins and Evans 2007; Weinel 2010). For an individual to really improve his or her knowledge through reading the literature it is necessary for him or her to have a sense of what papers are regarded as the most relevant, what papers are outdated, what techniques have been improved, which can be learned through linguistic socialisation. A large part of this mutual education is done by attending conferences and by talking to relevant experts. This point is illustrated by the following geochemist:

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 [a modeller] goes to data seminars.

In the quotation on page **[pg. 9 in this document],** Louis makes a similar point. Louis is a senior modeller who does not have enough time to follow the entire literature on data generation and points out that he tries to keep himself up to date with the paleoclimatic literature by going to conferences.

I have attended two conferences and a research seminar in which paleo-modellers and paleoclimatologists were presenting papers in the same room, asking each other questions after the presentations, and chatting during coffee breaks. In these chats, scientists presented their views on particular papers, either supporting or criticising them. In some cases, experts would ask other scientists to clarify points they had not fully understood. These events are thus important occasions for scientists from these fields to acquire linguistic understanding in each other's domain or to keep it up to date or even to increase it. The same happens in summer schools. For example, during an informal conversation about the Urbino Summer School in Paleoclimatology, a professor of micropaleontology said that the event was very productive for faculty because 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. A paleo-modeller said the same during a lecture.

In specific projects, scientists sometimes need to acquire higher levels of linguistic understanding of a domain which is not their own. Modellers might need a deeper understanding of the data they are working on in terms of their uncertainties, caveats, and so on. Paleoclimatologists, on the other hand, might need to better understand the models their collaborators use. This may be necessary for them to be able to interpret their output and link them to their understanding of the Earth system. The following quotation from an interview with a paleoclimatologist exemplifies this point:

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. 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.

The following paleo-modeller made a similar point:

Mary: I think that one of the safest things to do is to involve the person that's made the data in your work, closely. For example, I have a piece of work with Jackeline at the moment, because we're using her data very closely. And we've created a relationship because of these science questions. That's a very good way of doing it. Otherwise I think that you have to read very broadly, and if you can't work with the person, phone them, email them, find someone working on very similar things and discuss it as far as you can, and really work hard to really understand the shortcomings rather than simply using it.

In sum, besides the mutual socialisation that is taking place between paleoclimatologists and paleo-modellers, there are also individual efforts made by members of these communities to increase their levels of linguistic understanding of the domain that is not their own. Most of them seek a simplified understanding of the instruments, practices, and data of their collaborators that is enough for them to make collaborative projects work. But, if they spend a long time collaborating with particular groups and therefore being linguistically socialised by them, they can potentially become IE in their domain.

Mutual Linguistic Socialisation: Ambassadors

The notion of ambassadors, which has already been mentioned above, can further help us understand the process of mutual linguistic socialisation between paleoclimatologists and paleo-modellers. For instance, a department composed of several paleo-modellers hired two paleoclimatologists to help them carry out model-data comparison. In this case, the paleoclimatologists bridged the gaps between the communities by combining their contributory expertise, which was valuable in collaborative projects, with the linguistic understanding of modelling that they had acquired by consistently interacting with paleo-modellers. The following quotation is from an interview with a paleoclimatologist who plays this ambassadorial role:

> Karin: I don't do any modelling. I do generate data. But quite a lot of the work since I've joined this department is about data modelling comparison and is trying to bridge that gap.
>
> […].
>
> 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.

Karin 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 don't think they scrutinise the data hardly enough. But that's not the same thing as not understanding what they are after.
>
> […].
>
> 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.

Conclusion

In this chapter, I have examined the mutual linguistic socialisation process that is currently being conducted by paleoclimatologists and paleo-modellers. Although members of these communities have been collaborating for over 20 years, few of them have become full-blown interactional experts in the specialty of their collaborators. There is a feeling among them that it is necessary to improve their general level of linguistic competence in the domain which is not their own. I have argued that most efforts involved in this process of mutual linguistic socialisation lead to a limited understanding of the language of their collaborators. There are only a few situations that may provide individuals with the opportunity to develop IE, such as the case of ambassadors analysed in the previous section.

Yet one should not conclude that this process of mutual socialisation is worthless. First, if particular members of these communities feel the need to acquire IE in the domain which is not their own, the mutual socialisation process will already have put them in an advantageous position compared with those who have not experienced immersion. Secondly, even if they do not reach the level of interactional experts, their increased understanding of the other domain may help mitigate misunderstandings in collaborative projects. Monteiro and Keating (2009: 19–20) have argued that, in interdisciplinary projects, misunderstandings should not be seen solely as unproductive, but as occasions that can give rise to opportunities in which erroneous interpretations and unshared assumptions might be identified. As a result, shared understandings might emerge. In spite of agreeing with Monteiro and Keating, I contend that it is much more advantageous if scientists already have from the outset sufficient understanding of each other's domains to avoid going through 'productive misunderstandings' in order to start communicating effectively. In this sense, although the process of mutual socialisation presented in this chapter will not avoid these occasions altogether, it can at least prevent some of them, making interdisciplinary collaboration less challenging for the two communities.

Notes

1. Nevertheless, these studies also conclude that highly developed shared frames of meaning are not a precondition for effective collaboration. Star and Griesemer (1989) have made interesting contributions to how cross-disciplinary research may benefit from a lack of shared frames of meaning.

2. Although interactional expertise has gone through a development process since it first appeared in a paper in 2002, it is currently understood as the mastery of the language of a field of expertise (Collins and Evans 2015; Reyes-Galindo and Duarte 2015). In this sense, despite the fact that IE has been used in some contexts to refer to low or medium levels of linguistic understanding (e.g. Collins 2011; Duarte 2013), it can be clearly defined as consisting of a high level of competence in a specific language. To use it to refer to situations in which individuals only have a low or medium linguistic understanding of a field of expertise is therefore out of step with the correct definition.
3. According to Collins and Evans (2002, 2007), one can acquire interactional expertise in any set of practices that require a minimal amount of skills to be carried out, including activities as varied as speaking a natural language, gardening, guitar playing, football, and science.
4. Collins and Evans (2014) claim to have developed a new methodology, the Imitation Game, which can distinguish interactional experts from people with lower levels of linguistic understanding of a particular field of expertise. I have not had the opportunity to use this method with paleoclimatologists and paleo-modellers to assess if they had acquired IE in the domain of their collaborators.
5. This included British, German, Uruguayan, American, French, Dutch, Danish, and Spanish researchers. The sample included people with research experience in all major countries where paleoclimatological and paleo-modelling research is carried out.
6. The timescale of the instrumental record is short, extending only approximately 150 years back into the past (Burroughs 2001: 140–151). 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 billion years into the past. Temperature measurements, for instance, have been taken for up to 300 years, but with scattered geographical representation. Until the late nineteenth century, land temperature measurements were carried out only in parts of the northern hemisphere; after that time an increasingly broader coverage gradually reached other areas of the planet. It is only by interpolating records that it is possible to reconstruct land temperatures back into 1860s. Since approximately 1860 sea-surface temperatures have also been measured, using buckets that collected water from the side of ships. The coverage was also scattered in that only the main ship routes have consistent records.
7. Paleo-modellers use these data in three ways. First, they use data produced by paleoclimatologists as parameters in their models. Secondly, they 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 paleoclimatologists. 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. 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.
8. The names of my informants were changed to preserve their identities.
9. I attended this summer school in 2011 and carried out participant observation.

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CHAPTER 4

Science and Policies of Deforestation in the Amazon: Reflecting Ethnographically on Multidisciplinary Collaboration

Marko Monteiro

Introduction

This chapter discusses results from an ethnography of a multidisciplinary and collaborative scientific project, with a focus on the Amazon. It argues that ethnographic engagement with scientific projects can help to illuminate some of the challenges of undertaking complex, large-scale environmental research. In the case analysed here, this collaboration involves multidisciplinary work between modelling, social science and environmental sciences. Such a broad scope of cooperation brings unexpected difficulties as well as unexpected gains to the science produced. As models become increasingly relevant to environmental policies in Brazil and other countries (Edwards 2010; Miguel and Monteiro 2014a, b; Shackley and Wynne 1996), and as large-scale scientific projects focused on climate and environment multiply, it becomes increasingly urgent to better understand

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how such science is produced in practice and the challenges such multidisciplinary efforts face.

I also argue that ethnography can be a powerful tool in both understanding how such projects operate (providing knowledge on how models are constructed and incorporated into policy) and in helping to make explicit some of the challenges of doing such cooperative work. Making these challenges visible can become a useful way to reorient how scientists deal with the difficulties of cooperating between disciplines. Instead of dismissing them as "problems with communication" which can be easily fixed (Monteiro and Keating 2009: 9), scientists could benefit from such ethnographic insight and start intentionally mobilizing such issues in the very process of knowledge production. This chapter does not propose to solve these problems or provide the relevant tools in any detail; my aim here is to begin the conversation by showing, through an ethnographic example, how cooperation faces challenges which are more than just lack of understanding, but relate to the specific ways in which interdisciplinary work happens in practice.

As current research has shown, interdisciplinary work in practice can be less marked by open-minded cooperation than by ambivalence, critique and even dishonesty (Fitzgerald et al. 2014), which permeate a politics of interdisciplinary work that is still not fully understood or described. Cooperation between social and natural sciences can be challenging: research priorities are not always shared, and ethnographies of such settings have shown how social scientific insights have been neglected or even met with hostility by natural scientists (Rabinow and Bennett 2012). As will be discussed in the ethnography of the Amazalert project, social scientists will often resist the kind of reduction necessary for data to become amenable to become model input (Jeffrey 2003), which creates difficulties in effectively materializing cooperative work, even as such cooperation is prioritized in current science. Suspicion and mistrust between disciplines is one feature of such cooperation that marks how many of these efforts are enacted in practice.

Yet it is in those places of noise and friction between different epistemic cultures and different social worlds attempting to produce answers together (i.e. scientists from different disciplines trying to come to grips with the drivers of deforestation in the Amazon) that a possible place for ethnography in such projects can be imagined as a productive part of enacting more open, participatory and humble science and technology. As an inherent part of interdisciplinary work, problems in communication cannot be simply "mitigated" or overcome by "more effective" ways of communicating: such misunderstandings should, on the contrary, be reflected on as part of the results of such endeavours, and recognized as part of the collaboration itself.

Ethnographers of science become deeply entwined in the practices they observe (Latour 1995; Latour and Woolgar 1986), sometimes participating in them directly (Forsythe 1998), as ethnographers of other social phenomena also do (Mosse 2006). However, by sharing relevant worldviews and becoming fluent in sciences other than their own, ethnographers become more than just participant observers: they are potentially also active knowledge producers, especially in projects related to climate change, in which there is interest in the "human and social" aspects of natural phenomena. These claims emerge from my own experience of doing ethnography of science, but these experiences are arguably generalizable to other instances where the ethnographer is embedded in collaborative scientific efforts in which both "natural" and "social" variables are being analysed (Forsythe 1998), and where expertise (scientific and otherwise) is an object of ethnographic interest in itself (Mosse 2007).

Ethnographies of science have since the mid-1980s consolidated a rich body of work (Franklin 1995; Hess 2001; Knorr-Cetina 1983), helping us delve into the intricacies of knowledge-making, offering new ways to perceive how science is constructed in practice (Lynch 1982) and enabling a richer understanding of how natural phenomena circulate as knowledge beyond their localized settings (Latour 1995). Latour and Woolgar's now classic study (1986), alongside others, has established a powerful way of undertaking the study of science in the making which has become very influential in Science and Technology Studies (STS) and beyond. However, in the study by Latour and Woolgar the force of the argument is partly an effect of the rhetoric of detachment present in the narrative: the anthropologist analysing seemingly bizarre and exotic practices as an outsider.

This narrative of othering the scientific practices observed, albeit a powerful way of making what would be familiar to the reader very exotic, also built into the argument a rather positivistic way of looking at science through ethnography and does not represent the view presented in this chapter about the place of ethnography in collaborative scientific projects.[1] Our point is to discuss entanglements between ethnography and the processes of knowledge production under ethnographic analysis, and not detachment, and to think how ethnography can participate in the processes of interdisciplinary knowledge production. Ethnography can help us to understand problems of communication as an integral part of how knowledge production happens, and not as something to be removed from the process.

I additionally argue that the project's final results are more "humble" (Jasanoff 2003) when compared with the claims made at its outset about the potentials of modelling for public policy. Jasanoff argues that the traditional post-war social contract between science and society, whereby scientists, usually producing knowledge isolated from society, were expected to be able to provide reliable expert advice, is increasingly in crisis. This social contract is based on a great amount of freedom for science to govern itself, being thus protected from outside interference or participation. She states that we need "technologies of humility", which are methods or institutionalized habits of thought to try to come to grips with those aspects of knowledge at the fringes of certainty and understanding, "the unknown, the uncertain, the ambiguous, and the uncontrollable" (Jasanoff 2003: 227).

> There is a growing need, I shall argue, for what we may call the 'technologies of humility'. These are methods, or better yet institutionalized habits of thought, that try to come to grips with the ragged fringes of human understanding—the unknown, the uncertain, the ambiguous, and the uncontrollable. Acknowledging the limits of prediction and control, technologies of humility confront 'head-on' the normative implications of our lack of perfect foresight. They call for different expert capabilities and different forms of engagement between experts, decision-makers, and the public than were considered needful in the governance structures of high modernity. (Jasanoff 2003: 227)

As argued below, in incorporating ethnographies more explicitly in the production of knowledge in highly interdisciplinary scientific efforts, one can begin to build such technologies or methods for humility by addressing incommensurability in social vs natural science data and understandings of science; and in terms of deflating expectations on the role modelling can have on policy.

After discussing in section "Environmental Science and the Amazon" how large-scale collaborative projects have become increasingly common ways of producing specific images of the Amazon and its relation to local and global climate, in section "Doing Ethnography of Science-Policy

Interfaces" I will delve into the potentials of doing ethnography at the interface of science and policy, where specific types of expertise are seen as having a privileged capacity to aid decision-making. Much like other scientific endeavours in/about the Amazon, Amazalert aimed to produce policy-relevant science, helping policy actors to make better decisions about environmental problems and sustainability. In section "Following the Amazalert Project" where the ethnographic data from Amazalert is discussed in some detail, I point out how difficulties related to incorporating social variables into modelling practices are revealing of broader challenges of cooperating among disparate fields of expertise. In the conclusion, I argue that the ethnographic engagement with science can help to illuminate some of the challenges of working between disciplines, making it a valuable tool to produce knowledge about science in the making and the challenges of cooperation.

Environmental Science and the Amazon

Recent science has constructed the idea of the Amazon as a complex system of interactions and feedback mechanisms, which demands multidisciplinary research (Monteiro et al. 2014). Such work has mobilized an array of research groups, major scientific endeavours and growing funds from several countries, especially from the USA and Europe. These projects are usually implemented through large multidisciplinary teams and build large material infrastructures in order to conduct increasingly ambitious work. Noteworthy among these is the LBA project—*Large Scale Biosphere Atmosphere Experiment in Amazonia*,[2] which is one of the largest collaborative research efforts in the region. It has had a significant impact in forming scientific capacity and in producing knowledge on the Amazon as a regional entity with a very specific role in the global climate system (Avissar et al. 2002; de Gonçalves et al. 2013).

In August 2015 the LBA inaugurated ATTO—the *Amazon Tall Tower Observatory*.[3] This 325-metre tower will collect data on the biosphere-atmosphere interactions to help science to understand the impact of climate change, the role of different chemical compounds in the atmosphere and the role of urban pollution (Andreae et al. 2015). As in other projects of this size, ATTO seeks to produce knowledge that is policy-relevant, thought to be central to mitigate the destruction of the forest and the possible climate changes brought by deforestation. Another recent project, AmazonFACE (Lapola and Norby 2014), seeks to understand if the

Amazon forest, understood as an “engine of photosynthesis” will consume enough atmospheric carbon dioxide (CO_2) to slow global warming down (Grossman 2016). This project will spray pure CO_2 into experimental lots to see how the forest reacts to CO_2 levels expected be reached by 2050.

Several of these projects work with diverse technologies besides monitoring towers. Among these, remote sensing and computer modelling are becoming increasingly more present in environmental and climate sciences (Edwards 2010; Monteiro 2012; Rajão 2012). Remote sensing provides rich data on deforestation, on the dynamics of land use and has participated for quite some time in the formulation of environmental policies in Brazil, especially through the work of INPE, Brazil’s National Institute for Space Research (Miguel and Monteiro 2014a, b; Pereira 2008). Modelling has thus gained importance over the last decades, especially in climate science, and has had enormous impact on our perception of environmental processes and climate changes.

Furthermore, models are a central element of global climate politics and policies (Lahsen 2005, 2009; Miguel and Monteiro 2014a, b). Thus one can argue that the emergence of a scientific view of the Amazon as a complex system of interactions is co-produced with policies relating to the Amazon and its role in global climate dynamics (Miller 2004). There is a clear relationship between the growing scientific interest in the Amazon and the complex array of policies that have been created in Brazil regarding deforestation and preservation of the Amazon rainforest (Hochstetler and Keck 2007). Recently, models have taken centre stage in climate governance worldwide (Miller and Edwards 2001) as well as locally in Brazil (Miguel and Monteiro 2014a, b). Since the growing interest in the Amazon is also related to the perception of its relevance to understanding climate dynamics, as evidence in large-scale project framings, models have also become important in the imagination of how the Amazon should be governed (Monteiro 2015).

Doing Ethnography of Science-Policy Interfaces

The interface of science and policy has been a central topic in STS since its inception (Irwin 2008; Jasanoff 1987; Nelkin 1975, 1992), but the theme of ethnographic studies of science and technology has only come to the foreground recently (Hess 2001). Policy has gained greater attention from anthropologists (Mosse 2005; Shore and Wright 2003), but most of those studies do not tackle the central place of scientific knowledge in policy-making and policy implementation. This opens interesting and little charted territories for research at the intersection of anthropology and STS. Such studies have the potential to point to new areas of enquiry and for intervention in science and policy.

Beyond the experience of fieldwork itself, ethnography enables us to reconsider the place for STS in (re)creating technoscience. This includes discussions on governance (Guston and Sarewitz 2002) and "responsible innovation" (Stilgoe et al. 2013), discourses which call for a more active and constitutive role for social science and reflexive STS approaches in the production of new technologies (Macnaghten et al. 2005). The anthropological take on STS allows one, for example, to analyse the performative nature of scientific framings of environmental problems; beyond deconstructing traditional frames, this enables us to reconstruct practices and even attempt to participate in constructing different, more relevant (responsible, inclusive, democratic, sustainable) frames and policy approaches (Fortun and Fortun 2005). Ethnography can thus have a critical place at the nexus of scientific knowledge, technological systems and public policies.

As projects such as Amazalert and others build knowledge that relates to pressing environmental problems, they help to co-construct specific policies and transform local, national and sometimes global frames and practices related to issues such as climate change. Deforestation takes on new meaning, as it becomes part not only of destroying a rich and massively biodiverse biome, but of irreversibly changing local (and potentially global) environments and climates. As their frames of meaning change, the policies set up to monitor and mitigate deforestation also change, involving science policy (setting up programmes to produce more and "better" climate knowledge), rural and urban policies (as land use changes in the Amazon are major drivers for large scale deforestation) and development policies (as the environment is perceived as enabling services one can pay for in various ways or attempt to conserve, and economic activities are re-organized along those principles as well).

In terms of science and technology policies aimed at the region, the central place that the Amazon has taken in global climate science has enabled the implementation of ever larger projects to study all aspects of the biome, its relation to the atmosphere, the particles that help cause rain, land use changes, among many other topics. This involves setting up complex technological infrastructures, which drive scientific projects but also create problems for the STS-oriented ethnographer. How does one follow such diverse actors? How does one produce a coherent narrative that analyses, interprets and maps the issues at hand?[4]

One needs to follow not only scientific concepts (i.e. savannization, deforestation, climate change, tipping points) and their transformations, but also the technological infrastructures that allow such concepts to emerge and proliferate—such as the monitoring towers being built all over the Amazon—as well as the remote sensing infrastructure so crucial to deforestation research (Rajão and Vurdubakis 2013). As infrastructure studies have shown (Edwards 2010), climate science has participated in the construction of a global governance that goes beyond science itself. In order for this to unfold, one needs to understand such infrastructures as complex social arrangements (Edwards 2003). These involve distributed systems spread across many sites, institutions, dealing with a multitude of different disciplines and scholarly traditions. The ethnographic sensibility in STS helps us see such infrastructures as more than machines or objects, but as emergent systems enacted through the practical achievements of multiple actors (Morita 2013; Pickering 1993). Scientists, satellites, monitoring towers, supercomputers, laptops, smartphones and laws are all enmeshed in producing these associations and also in creating lock-ins that constrain future developments.

In the case of Amazalert, understanding the idea of a tipping point in Amazonian deforestation is crucial for reflecting on how the whole project was set up and legitimized. According to recent debates on deforestation, strong evidence suggests that if continued beyond a certain point irreversible changes will create a chain reaction that would alter the ecological aspects of the biome as well as the local and regional climates that interact with them (Nepstad et al. 2008). This is related to a possible savannization of the Amazon (Nobre et al. 1991), which is also a marked point of debate in current research on deforestation and climate in the region. Some authors suggest that one of the possible futures of the Amazon, as it is pushed beyond a tipping point after which a chain of irreversible changes occur, is to become more of a savannah-like biome, drier and more prone to fires (Malhi et al. 2009; Vergara and Scholz 2011).

This leads to the idea of constructing an early alert system: if there is such a tipping point, and if this is related to deforestation dynamics which include complex natural and social variables (Amazalert 2015), then understanding these dynamics and producing better models could enable us to enact policies now that will avoid irreversible changes in the future. Modelling is central to this effort as it is thought to be capable of producing reliable information about future trends, which in turn can help to lead to actions that avoid future damage. Such damage includes the loss of valuable “environmental services” related to carbon emissions, water and biodiversity, among many others.

While this idea is interesting and rich in research possibilities, constructing such a system carries a touch of optimism about technological potentials that helped create some tensions in the participatory workshops developed within Amazalert. Different conceptions of the possibilities of modelling socio-natural dynamics were one of the sources of friction and controversy that were observed in collective debates, and such controversies were very hard to mitigate and resolve. Such misunderstandings have been observed in research with similar groups developing multidisciplinary research (Monteiro and Keating 2009) and it remains an understudied aspect of such multidisciplinary collaborations.

Following the Amazalert Project

The Amazalert project,[5] where the ethnographic research that informs this chapter was conducted, sought to produce knowledge about possible tipping points in the Amazon. It lasted from 2011 to 2014 and aimed to produce “early alerts” of possible points of no return in order to provide policy-makers with reliable information about what to do to avoid irreversible damage to the forest. Publicity materials lay out the project objectives in a way that suggests certainty (i.e. by stating that models will be improved by bringing in stakeholders to participate in creating scenarios for the future), but also that make explicit how the project leaders are keenly aware of the uncertainties involved.

Modelling was a central aspect of the work, as the project intended to develop a blueprint for an early alert system based on better predictions. However, this objective was premised on the admission that predictions vary according to what kind of model is used to make them. In interviews and conversations, project members were clear that they hoped to increase robustness in the model by incorporating “social” variables, which included the local stakeholders’ perspectives on scenarios for the Amazon.[6]

The strategy outlined by the scientists, as assessed by analysing both project materials and interviews, was to produce innovative computer models that included the “human elements” of deforestation to obtain more robust predictions. Robustness in this context means a model that can make better, more accurate predictions regarding the dynamics of deforestation. This involved organizing workshops with local stakeholders and policy-makers to produce the scenarios of deforestation and point out the more relevant variables, which would in turn be incorporated into the models. This led to interesting dynamics within the project, creating frictions that were not fully dealt with by the end of the activities, but which can become rich data for ethnographic reflection.

Amazalert held two workshops in order to achieve the task of incorporating socio-economic data into the model. The first workshop was held in Belém between 24 and 26 of June 2013, involving non-governmental organizations (NGOs) located in the Amazon region. This workshop developed two scenarios, a pessimistic and an optimistic about the Amazon, environmental degradation due to land use change and other economic activities. I attended the second workshop on 23 November 2013 in Brasília. The scenarios developed in the first workshop were discussed in Brasília by people from policy-related areas. These discussions occurred in separate groups, involving representatives from many government and policy institutions (Ministry of Defence, Ministry of Fisheries, Ministry of the Environment, Ministry of Agriculture, scientific institutions such as Embrapa[7] and INPE, among others).

Groups were divided into broad topics: Economic Issues, Social Issues and Natural Resources. Interestingly, the Economic Issues group was the largest one, while the Social Issues group the smallest. This last group included two anthropologists, including myself, and was both spatially and thematically marginal in the whole discussion. While the reasons for separating "Economics" and "Social" issues remained unclear to me, it seemed that "social" in this workshop referred to problems related to marginalized populations, politics and identities, while economic referred to more recognizable indicators such as income, or production chains (in agriculture, cattle farming, etc.), which were more readily available for quantification.

The duality between natural phenomena and human/social processes remained problematic in different instances. The native representations of the scientists, while attempting to suggest an inclusion of social data into models, seemed at the same time to reinforce the ontological division between these realms, leading to irreducible differences that were seen as challenges to modelling in the terms set up by the project leaders.

Reducing Society into Models

In the interviews conducted before and after the workshop, the leading scientists expressed the complex and problematic nature of the undertaking. Their wish to incorporate socio-economic data into models, while theoretically a tool to increase the model's predictive power, raised several dilemmas, including the question of how one "reduces" such social data into quantitative variables that would be amenable to become "input" in a model. This was not completely clear to them and it became less clear during the workshop as some participants, including myself, questioned this more directly.

> Well that's difficult. It's very easily said, we do modelling and we do policy advising, but to actually really couple the two of them… Very often it happens in these projects is that there is a modelling component and a policy component, and the modellers do the modelling, and the policy people, they do the policy studies, and then at some point they start saying to each other 'we should really integrate', but then, it's very difficult to talk to each other.
>
> (Interview with one of the PIs for the project during the second workshop)

The dilemma can be roughly glossed as a duality between two resistances: that of the social scientists, who resisted reductionist framings of the social drivers of deforestation; and that of modellers, who resisted the concept of "irreducible complexity" while seeking to actively build models that incorporate those data. Irreducibility would be a challenge to the task of modelling, as it would mean a problem impossible to solve: if social variables are irreducible to numbers or values they could never be modelled, even in approximate terms, and that would make models irremediably limited. But it would also mean a challenge to policies, which tend to favour clear objectives that are minimally measurable and trackable, even though the potential of indicators to actually drive the science–policy interface is problematic (Sébastien et al. 2014).

This dilemma became more explicit in relation to other areas that might be considered "social", including policy. The difficulty in communication between areas was implicit during the Brasília workshop, as the whole dynamics was geared towards producing usable data for modelling. But other issues emerged during the process, including questions of violence in the region, the absence of the state, and conflicts around land (including but not exclusively native Amerindian populations being driven out of their traditional lands). These were suggested in the "social issues" group by people with extensive experience on the ground, but were very hard to capture in terms of "drivers", or incorporate into the scenarios built in the previous workshop in Belém that were structuring the debate in Brasília.

Several discussions (which were filmed and transcribed) related to how technology could enable better, cleaner and more sustainable economic activities in the Amazon, which were preconditions for the more optimistic scenarios to come into being–that is, less deforestation while the region would still achieve relative development.

> But my comment is this, that this is exactly the natural tendency. We want the small, the medium producer, also the family farm, to make use of a better technology, because by doing this, they will be saving space and making that production better in relation to the natural resources that exist in that region.
>
> (intervention of the representative of a ministry in the economic issues group)

In this example, we see a kind of perception that the use of more intensive technologies would save land and thus prevent deforestation. As cattle in Brazil is still raised extensively, relying on open grasslands, the tendency as production grows is for deforestation to expand correspondingly. A more intensive use of technology, according to many, would curb that expansion while still enabling growing outputs. This same view was not restricted to cattle or farming, but also appeared during a debate around natural resources in general:

> I think another important aspect of the long-term mining would be the incorporation of technology in the minerals, so that we don't just keep selling ore without any work or processing in Brazil. I think this adds value, because otherwise we will always be a colony, right!?
>
> (intervention of the representative of a public university during discussions in the natural resources group)

The incorporation of technology was seen as a positive aspect in all sectors: it would enable more productive agriculture and potentially result in more productive uses of Brazil's natural resources. For example, more technology would, according to some, enable the country to reduce exports of raw minerals and increase exports of industrialized goods such as steel or even machinery and capital goods. More technology in farms would also mean broader use of confined cattle, which in theory would need less land and promote less deforestation. These debates went on as the members of the economic issues group debated different scenarios. But the pervasiveness of "technology" as a value, almost in itself, was prominent. The idea that some generic form of technology would necessarily promote more sustainability was not problematized or reflected upon. Which technologies? Embedded in/impacting which social relations? Controlled by which groups?

I was the one elected by my group (social issues) to present our debates to the other groups. The collective sharing happened at the end of the workshop, and my presentation aimed to sum up the topics we covered collectively:

> We did a pre-discussion on the issues that were perhaps missing [*from the other groups*], and we talked about the social responsibility of companies, being debatable that this is on the rise. And also on royalties [*from petroleum, mining, etc.*]. Where do these royalties go? How are they used? There are city governments that will build a palace or a fancy plaza, but won't invest in sewage. Also, large infrastructure works, we discussed compensations and how to mitigate social stress and if, possibly, impact studies were focusing too much on the physical environment and not on the broader social issues. We talked about political culture, which relates to royalties since, for example, corruption and 'clientelismo'[8] have a large impact … if you have a context of corruption and impunity, the money will come in and will not be well invested.
>
> (excerpt from my presentation summarizing the topics covered in the social issues group)

The discussion went much further and deeper than this, especially in the social issues group, in which I participated. We discussed inequality, citizenship, responsibility and topics that are apparently unrelated to environment or economy. However, they help us to understand problems related to violence and local politics, and also explain why environmental laws are not followed, why land keeps being cleared illegally, and why traditional groups are driven out of their lands by large-scale farming, among many other variables that can help to explain deforestation dynamics. We discussed how science and technology could interact with and create a dialogue with local knowledges, while also debating how large infrastructure projects can help create new production chains in the Amazon region that would rely less on deforestation.

A central concern throughout was finding ways of transforming data on socio-economic processes into input that could be incorporated into the model. This meant, in the discussions, developing indicators that would quantify some relevant information about key aspects of interest (e.g. deforestation rates, rainfall, forest fires, etc.). Natural indicators would be mostly related to climate such as sea surface temperatures, precipitation and droughts. But social indicators remained an unanswered question, which the project leaders hoped would be better defined in the next stages of the project. My interviews with the leaders revealed this to be a central concern and a central blind spot of modelling for environmental research, or for using modelling as a source of data for policy: which social indicators should be measured and monitored? How could the measurements and the monitoring become feasible? Which institutions should get involved and why? How can such knowledge inform policies that enable positive outcomes in terms of preservation of the Amazon? The dilemma was addressed by an anthropologist present at the discussions:

> […] the social sciences, although they also work with statistics, […], they don't test, and there are no laws that describe society in the same way that you find regularities and hypotheses, through the way natural systems function. So, social tipping points, that's a complicated thing.
>
> (intervention from an anthropologist during open debates in the Brasilia workshop)

He suggested that indicators for violence could be a starting point, as they are connected to governance and citizenship, among other social issues that are deeply interrelated to environmental governance and possibly climate change mitigation efforts (or their chances of being implemented). However, the issue of social and political "tipping points" remains fascinating: as he put it, there are no limits to social problems as historically the human capacity to create violence and exclusion is very high. Others present argued that tipping points may not be an adequate language to talk about social issues, but that thresholds could be imagined that would delimit situations that we would not want to reach involving violence, poverty and income, among other indicators available.

In the end, there was no solution to this conundrum, even though it revealed very relevant blind spots in the project strategy. This controversy is an example of the kinds of complexities that can emerge in such highly diverse debates, which are very hard to close or resolve in the time allotted for them in the one-day workshop. Results had to be written up at the end of the day, when exhausted participants tried to condense the very rich discussions into central points that were to be taken up by modellers afterwards. These controversies were not discussed in the closing debates and were not explicitly addressed in the final results presented. This indicates that such controversies were not seen as "results" to be dealt with, which makes us reflect on why an ethnographer saw them as interesting and how that could be possibly reflected upon as part of such a scientific undertaking.

Potential Contributions from Ethnography

Some days after the workshop, I interviewed one of the leading modellers involved in the project and asked them about the integration of human factors into the model:

> **Anthropologist**: Have you dealt with the human dimension? Is this a recent trend in climate science?
>
> **P1**: This is a very recent thing from what I can see, we don't know how to quantify this yet, it is very hard to translate into numbers, so it is a relatively recent thing.
>
> **A**: I'm asking because I have heard at INPE, when I was interviewing people working at [PRODES and DETER[9]], the remote sensing guys, this [came up] and I was unsure if it came from INPE or from the [project leaders].
>
> **P1**: It is something that has been emerging in the last 5 or 10 years... In the 90s we learned a lot about the climate, I mean this perception in science that human beings alter the climate, many projects were developed in order to understand how this was happening and we learned that in order to understand we had to understand all processes at the same time, so little by little people were seeing that we needed to study the whole process in an integrated way.
>
> (*Skype interview with senior scientist working with climatology and responsible for most of the modelling in Amazalert*)

This answer helps to illustrate the growing relevance of human and social elements in climate science, which also contextualizes the current problems scientists face in effectively promoting dialogue between social and "hard" sciences on deforestation and other environmental problems. The workshops and other activities that were intended as moments to produce social inputs for the model in Amazalert were very short and did not, in my view, allow enough time for those questions to even be adequately posed, let alone dealt with. This is an instance where an ethnographer, attentive to this dilemma from the start, could proactively suggest ways through which non-quantifiable knowledge might be integrated in some way into the models. What this could mean, practically, is that these issues of incommensurability should be dealt with explicitly and as problems in themselves, which did not happen in the project. They were not addressed explicitly (although in corridor talk they came up often); and they were not perceived as part of the problems to be solved by the scientific effort at hand.

This also indicates that the mere fact of having embedded ethnographers or social scientists as active members of such projects is not enough to make these collaborations more open to uncertainty or to their own limitations. As these large-scale projects grow in number and in relevance, many social scientists are brought into their teams. But are the projects attentive to how these different epistemic cultures are interacting? In the case of Amazalert, social scientists were expected to bring data and insights about the social variables present in deforestation dynamics in order to make the model's performance more robust. This is important as an innovative way of producing models, but the project did not foresee an institutional place for the discussion of incommensurability as a problem in itself that needed to be dealt with; and participants did not take advantage of discussions of these problems that came up in workshops when finalizing the project's mains results.

What could an ethnographer do here in order to address those problems and make them more productive in scientific terms? The first thing would be to explicitly address them in the group setting, with adequate time to process what they could mean to the modelling initiative, for example. The non-quantifiable nature of some crucial data cannot be solved by producing (numerical) indicators, or by just ignoring the issue. On the other hand, innovative modelling could be achieved if the multidisciplinary team, by bringing those problems to the fore and accepting that they were relevant to the whole enterprise and not "problems of communication" or mere lack of shared expertise. The attempt to find solutions to this problem could move scientists to better understand the limits of the model and its applicability, among other unforeseen results.

Making Results Humbler

A noteworthy element in the project's final results as compared with its initial goals was the greater awareness among its participants of the complexities and uncertainties involved in setting up the technology of an early alert system. The results presented at the end of the project were significantly 'humbler' in relation to what the proposal laid out in the beginning. While the project's initial language seemed relatively certain that such a system could be set up and that it could be a useful tool for policy-makers, the language in the final report shows a much more cautious approach:

> Despite its relative resilience if deforestations stays low, AMAZALERT has shown that severe degradation of Amazonia is possible when severe climate change and deforestation progress simultaneously. However, **the type of change can vary strongly and can be difficult to predict**, because signals of change may come only after a biophysical threshold has been crossed and decline will already be rapid or irreversible (…). **Early warning for such change will therefore also have to be approached from a broad perspective**. (…) Thresholds should be defined that account for society's coping capacity as well as with the uncertainty in prediction of natural ecosystem degradation or instability. **In this envisioned early warning system (EWS), new scientific insight and technical capability should be constantly adopted and tested for effectiveness**. (Amazalert 2015: 9, emphasis mine)

This excerpt is taken from the final summary for policy-makers and thus is written in a language directed to decision-makers. It is not a scientific paper, where uncertainties are often openly discussed (Shackley and Wynne 1996). As exposed above, the early warning system proposed is an open system, which would have to be constantly adapted as novel scientific insights and technical capabilities become available. Although the use of models as sources of information for policy is increasing, studies point to the lack of confidence that policy-makers have in using them (Brugnach et al. 2007). This lack of confidence comes from the presence of perceived uncertainty, indicating that policy-makers tend to have high expectations with regard to models output.

Such an open system, "constantly adopted and tested for effectiveness", appears to be open to adjustments due to new knowledge and thus aware of its own limitations. This explicitly partial and incomplete apparatus is humbler than what was proposed at the start of the project in the sense suggested by Jasanoff (2003), being therefore a type of technology more open to participation and questioning than a closed system, locked into a model thought to correctly predict environmental or climate phenomena. It is hard to say why the final summary for policy-makers incorporated the uncertainty in more explicit terms than the initial proposal. Yet it is significant that the results made those limitations explicit and clear, which signals an openness to uncertainty that can be an opportunity for broader collaboration with ethnographers and other social scientists interested in addressing the inherent limitations of modelling as a tool for policy.

Conclusions

Current discussions around science have proposed new forms of producing knowledge that are closely tied to its context of application (Gibbons 2000), and that are open to participation from stakeholders and other actors outside science (Guston and Sarewitz 2002; Owen et al. 2012). Amazalert incorporated many of these elements, internalizing within the project's activities discussions between scientists and groups of stakeholders and policy-makers. And yet it produced results that are more open to uncertainty and complexity than those initially proposed, which makes it an interesting case for debate.

Contemporary societies live in a context of being constantly at risk (Beck 1996), and in such an uncertain environment science has been called to do more than speak "truth to power". Jasanoff's concept of technologies of humility becomes thus an intellectual answer to the challenges of enacting science and technologies that are more participatory, accountable and aware of their own limitations.

While it would perhaps be too ambitious to call Amazalert a fully realized example of a "humble" way of producing science and technology for environmental problems, it is a good example to reflect on in terms of the hopes and challenges of doing such types of "other" sciences. The project effectively worked with scientists and experts from a myriad of disciplinary backgrounds, which is a dynamic of scientific work that increases uncertainty and noise in communication. The project did not, however, make explicit the problems that arose in this kind of interdisciplinary work, problems which were here made visible through ethnographic analysis. Perhaps this was so because analysing these problems was not in the scope of the project; but I argue here that to do so, possibly through the incorporation of ethnography in some explicit measure in the dynamics, can be a useful way to explore the potential gains that arise from misunderstanding and noise between disciplines.

One of the more salient misunderstandings is the problem of incommensurability between different ways of perceiving data and its quantifiability, as observed in the participatory workshops. By bringing in professionals from different areas of expertise and disciplinary backgrounds, these workshops made controversies about data harder to close. This raises important issues related to how such incorporation can happen productively in such projects and how the limitations detected can be addressed. The problems raised related to different perceptions about the quantifiable nature of inputs from different sciences, a feature that is central to the possibility of modelling certain variables. While modellers wanted to include social variables to make models more robust and reliable, social scientists were sceptical about this possibility. This scepticism related to the perception of the great difficulty (perhaps impossibility) of quantifying social phenomena.

The project's results are, as discussed above, humbler than initially proposed in the beginning of the activities. But the project did not fully process or take formally into account the complexities and challenges of the participatory workshops. While these topics were being discussed informally, they were not dealt with as part of the scientific agenda of the project, which may have impeded the scientists from fully appreciating the richness of the experience they made possible. It is proposed here, based on the experience of doing this ethnography, that this methodology can be productively incorporated into such projects as a formal and explicit aspect of the science being developed, and that this can promote significant gains in how environmental (and other complex) scientific collaborations are developed.

How this can be achieved is still unclear, but it is a topic that will probably gain growing relevance as large interdisciplinary projects multiply and as the human dimensions of climate change are increasingly believed to be central do climate change research. Moreover, the demands for social scientists to participate in climate research and governance (Victor 2015) will continue to push this issue to the fore of climate and science policies.

The incorporation of "human dimensions" into scientific and political concerns about climate change opens an interesting window of opportunity for such interactions and collaborations among ethnographers and environmental scientists, and should be the object of more reflection and action on the part of STS scholars. Large-scale scientific projects are, as all social environments, a complex and dynamic arena of conflict and interaction. But they are particularly central in our societies as they produce powerful effects on our perceptions of the world and on our political actions in relation to climate and on the environment. Therefore, the effective participation of ethnographers can enable relevant engagement with such transformative practices and can enable more interesting interfaces between science and policy.

Notes

1. Earlier critiques of Latour and the actor–network approach (Bloor 1999; Collins and Yearley 1992) have engaged with different aspects of this debate: Bloor's work attacks Latour's critique of the Strong Programme, SSK and the sociology of knowledge. Collins and Yearley, on the other hand, argue that actor–network approaches, although presented and perceived as radical, end up reinforcing a conservative position through a relativistic view of science and technology.
2. "The LBA Program is managed by the Ministry of Science, Technology and Innovation and coordinated by the National Institute of Amazon Researches/INPA. In the 17 years of research, the program was important in forming human resources, including over 600 masters and PhD researchers in Brazil. Over 150 cutting edge projects, partnering with around 280 national and international institutions, carried out by 1400 Brazilian scientists and 900 scientists from other Amazonian countries, from 8 European nations and North American institutions, sought to study and understand climate and environmental changes underway in order to favor sustainable development in the Amazon" (Source: http://lba2.inpa.gov.br/index.php/lba-apresentacao). All translations done by the author.
3. "The long-term objective of the ATTO is to measure the impact of global climate changes in the Amazon forests through measuring the interactions between the forest and the atmosphere, besides enabling novel research on the chemistry of the atmosphere (gas exchanges, chemical reactions and aerosols), mass and energy transportation processes in the limit of the atmosphere and processes of cloud formation and development" (Source: http://www.brasil.gov.br/meio-ambiente/2015/03/projeto-atto-inpa-uea-e-instituto-max-planck-assinam-contrato).
4. Fischer (2001), Hine (2007).
5. http://www.eu-amazalert.org/home
6. The incorporation of local perspectives was explained as a way of mitigating inherent uncertainties in the modelling as well as a way to make

models more relevant for policy. In interviews, the principal investigators of the project discussed how this was also seen as a new frontier in modelling work, and the incorporation of social variables in such environmental models continues to be seen as a challenge. Because they attempt to model processes that have a lot to do with human action in the environment, such initiatives are becoming more common.

7. The Brazilian Agricultural Research Corporation (Embrapa), founded in 1973, is a research corporation, owned by the state and focused on developing technologies and technical scientific knowledge for Brazilian agriculture.
8. This can be roughly translated as the preponderance of personal relationships and bargaining to the detriment of the public interest in dealings with/through the state apparatus.
9. "Brazil has two systems for tracking deforestation: PRODES (Program to Calculate Deforestation in the Amazon) and DETER (Real-time Detection of Deforestation), which allow to rapidly identify where deforestation is occurring. PRODES, which has a sensitivity of 6.5 hectares, provides Brazil's annual deforestation estimates (measured each August) while DETER, which has a coarser resolution of 25 ha, is a year-round alert system that updates IBAMA, Brazil's environmental protection agency, every two weeks. This gives authorities the technical capacity—although not necessarily the political will—to combat deforestation as it occurs" (source: https://news.mongabay.com/2011/02/monitoring-deforestation-an-interview-with-brazilian-space-researcher-gilberto-camara/).

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CHAPTER 5

From "Climate Sceptic" to "Dendro-Sociologist": Considering the Role of Trust in the Communication of Science in Action

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"Proposals to grant special authority to STS expertise cannot readily use the leverage provided by identification with a singular source of privileged knowledge.
Instead, if proponents of STS are to play by the rules of their own game, they must win their authority on a level playing field; or, worse, on a field that presents them with an uphill struggle."

Michael Lynch (2009: 109)

The Challenge of Communicating Science in Action

Communicating *science as a process* is different from communicating *science as a product*. Harry Collins and Trevor Pinch (1998: 2) clarify this distinction in terms of transmitting information *about* science (as an activity and practice in action) as opposed to informing *of* science (as a body of knowledge about a particular subject). We can find descriptions

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of science as a product—that is, accounts of accepted theories, artefacts and methods developed after years of experimentation and alternative developments—in media, science classrooms, textbooks and museums. In our highly specialised societies, communicating *what scientists and other technical experts know* about the world is an important mechanism of social coordination. For instance, knowing that experts have forecast heavy storms in a city could assist transport authorities in minimising disruption to commuters.

Communicating science as a process involves explaining to interested audiences *how scientists and other technical experts come to know* about the world. Steven Shapin (1992) argues that the purpose of communicating science in action has emerged from historical processes leading to a separation between workplace and residence. As a result of this modern arrangement, Shapin suggests, members of the public currently have limited access to laboratories and other sites of scientific work, which means that they often idealise the way science is done and distrust those products of science that do not conform to such ideals.

In order to provide the public with more realistic accounts of the ways in which science is done, Shapin insists on the "importance of enterprises that seek to make the walls of scientific workplaces less impenetrable and opaque" (1992: 28). He regards the studies undertaken in the 1970s and 1980s by sociologists and historians of science—who, in conjunction with other scholars from anthropology, philosophy, policy, law and cultural studies, subsequently constituted the interdisciplinary field of Science and Technology Studies (STS)—as a potential venue for informing the public about science in action.

The main challenge that STS scholars have historically faced—as illustrated by the so-called Science Wars (Labinger and Collins 2001)—is to convince certain audiences, particularly natural scientists, that sociological accounts about science in action do not question the reliability of science (Latour 1999; Bloor 2008). On the contrary, STS scholars have argued that the communication of science as a process complements and enhances the public understanding of science as a human product (Collins and Pinch 1998; Shapin 1992). In a letter to the curators of a science exhibition and learning centre at the Smithsonian, the STS scholar Thomas Gieryn (1996) articulated this point in the following way:

> I think learning centers have a terribly important role to play in informing visitors about 'how science gets done' (i.e., the process of science). But what role? In my opinion, the goal of hands-on experiments and demonstrations should be to teach visitors about the intensely problematic nature of scientific experiments, to get them to think about replication as a process fraught with uncertainties and contingencies… Instead of giving visitors the impression that experimental science moves like clockwork, why not give them experiences that encourage them to ponder the question: 'How does science manage to get facts out of the messiness, the confusion, the hunches, the subjective judgments, the broken test tube, the leaps of faith that are inherent in experiment? Could you design hands-on experiments that raise doubts about the conclusions that were historically drawn from them instead of experiments or demonstrations that make the conclusion seem obvious or inevitable? (Gieryn 1996: 112)

Eighteen months before the scheduled opening of the exhibition and as a final (failed) attempt to convince museum curators of the overall value of communicating sociological insights about science in action, Gieryn tried to forge an alliance with the chemists in the exhibition board:

> "Chemists want to convey the excitement of the chase, while—as a sociologist—I am equally interested in showing more concretely that 'the social' does not stop at the laboratory door" (11 May 1992). "Here I echo those chemists on the board, who have begged for more on the doing of science (although I refuse to accept that this must come at the expense of material on the social side—indeed, 'the social' is as much inside the lab as out)" (2 December 1992). (Gieryn 1996: 113)

The reasons why many STS scholars like Gieryn have failed to convey their message to outsiders are specific to their contexts of communication, but one factor that is common across many cases of miscommunication seems to be that STS scholars have a different appreciation of the social nature of science than their audiences. In the quotation above, Gieryn suggests disagreements with the chemists at the Smithsonian about the place of "the social" in science. Whilst the chemists possibly thought that the sociability of scientists—the fact that they are educated in their disciplines, they build on the work of trusted colleagues and are members of wider societies—is an "external" factor to intellectual developments within science and has nothing to do with the ways in which scientists go about doing science, Gieryn argued that "'the social is as much inside the lab as out".

In his interpretation of the Science Wars, Donald MacKenzie (2002) noted that "science warriors" cited the "social" when they wished to criticise the "science". Previously, Michael Mulkay and Nigel Gilbert (1982) had also noticed that the "sociology of error" was a widespread feature of the discourses of natural scientists. For MacKenzie, the false dichotomy between the "scientific" or "technical" and the "social" reinforces "a disabling culture divide" between the natural and the social sciences (2002: 22). Therefore, one way to understand the communicative challenge that STS scholars face in trying to overcome this dichotomy is to think of it as a problem in constituting a unified culture with natural scientists and other audiences outside STS.[1]

In other words, the challenge of *communicating* STS knowledge could be seen as the problem of creating a larger *community* that shares a *common* understanding about the processes of doing science, and the constitutive role of social factors within.[2] In addressing this challenge, I echo Michael Lynch's advice in the opening quotes. In Lynch's opinion, if STS scholars want to play by the rules of their own game, they cannot use the now discredited "deficit model" of communication of natural knowledge to address the public's resistance to STS knowledge. Instead, given that the public already have their own (often competing) views of what the STS scholars would aim to inform them about, Lynch suggests that STS scholars should think of this process of communication as requiring mutual adjustments. In this sense, playing by the rules of our own game would also involve considering one of the key insights formulated by STS scholars, namely the role of persuasion and trust in the development of communities of knowledge (see Reyes-Galindo 2014).

In this chapter I employ STS insights about the role of trust in the constitution of communities of scientific knowledge as a starting point for investigating empirically the challenge of communicating science in action. In the next section, I draw upon my research experience to outline three social processes that, I suggest, explain how I managed to build up contingent trust relations with a group of scientists and to generate sociological knowledge about their work. In the conclusion I discuss the limits of trust and the reasons for my modest success in persuading my immediate audience of scientists of the value of STS insights.

Overcoming Suspicions

When, in April 2012, I first approached Rob Wilson (Senior Lecturer at the University of St Andrews) and his then PhD student Miloš Rydval with the idea of studying their work, my concern was that they would perceive me as a "climate sceptic", intending to expose the next "Climategate" scandal. The term Climategate refers to the episode in November 2009 when emails between climate scientists (including Rob's emails) were stolen and published online by an anonymous hacker. These hacked emails prompted criticism from diverse commentators (commonly referred by scientists and others as "climate sceptics") who cast doubts on the methods, claims and members of the climate science community (Ramírez-i-Ollé 2015).

During my first two years of research, Rob and Miloš often commented—jokingly, I subsequently learnt—that I was a "climate sceptic". In particular, they made these jokes when the social aspects of their work became evident, as if the sociability of science was something to be embarrassed about. For instance, when I witnessed Rob and Miloš negotiating their disagreements on a particular course of action or interpretation of data, they would say to me, "As a sceptic, you might be interested in this discussion, aren't you?" Also, when they gossiped in front of me, they would say while giggling, "We have to be cautious because we have a sceptic among us!" When Rob first made these comments, I immediately clarified my intentions: "No, don't worry, I won't publish anything that you don't want me to publish!" Miloš laughed at my reaction, and said, "You have to get used to Rob's sense of humour!"

Over time, I played along with Rob and Miloš' joke, and I took advantage of it. As I explain in section "Encouraging Civil Scepticism", by pretending to be a "climate sceptic" I tested my own understanding of Rob and Miloš' work. By accepting Rob and Miloš' labelling, quite paradoxically, I also dispelled suspicions from unknown people. At a conference in Aviemore (Scotland) in May 2014, Rob introduced me as his "in-house climate sceptic" to a colleague. By then, I was used to Rob's introduction, so I probably rolled my eyes and smiled. Rob's colleague remained silent for a few seconds, probably trying to understand whether Rob was joking or not, and asked me directly, "Are you?" I said, "Well, I don't know. If you tell me who you think a climate sceptic is, then I will be able to answer your question." We then had a long conversation in which, among other things, he told me about his previous encounters with people he regarded as "climate sceptics". During this conversation, Rob, amused, was whispering to his colleague, "I told you, she is not a sceptic."

In the last stages of my research, after getting to know other members of the dendroclimatology community, I became labelled as a "dendro-sociologist". I first heard this label in January 2014 from a reputed dendroclimatologist after participating in a training course in Tasmania (Australia). During that week, I had conducted fieldwork with other students who were training in three areas of specialisation within dendrochronology (these subfields are constructed with the prefix "dendro" and the specific suffix referring to the application, for instance "climatology", "ecology" or "archaeology"). The label "dendro-sociologist" was meant as a joke (in the sense that all dendroclimatologists knew that the subfield of "dendrosociology" would be ridiculous) and also a welcoming gesture into their community.

I regard the contingent development of trust relations with Rob, Miloš and a few other dendroclimatologists as the reason why, in the aftermath of Climategate, I managed to overcome their initial suspicions and conduct my sociological study effectively. Thinking that trust relations with research subjects (or any other form of human relation) could only bias our ability to think sociologically would simply reproduce the kind of sociology of error that STS scholars have long criticised (Bloor 1991).

Instead, here I adopt a naturalistic approach and offer three social processes as sociological explanations of the simultaneous constitution of my trust relations with dendroclimatologists and STS knowledge. There are many definitions of naturalism, but the one that I am espousing here is the most commonly used in STS. Authors within this tradition of naturalism argue that any form of knowledge (including sociological) can be treated as an object of empirical investigation, by describing and explaining its content and style in relation to social processes and influences (Barnes 1974; Bloor 1991). For these scholars, social processes and influences are seen to be constitutive of the community of knowledge (e.g. interactions between members in the form of instructions and negotiations within the community about the application and revisions of communal knowledge). Here I focus on one of the main social processes involved in the generation of sociological knowledge: the reciprocal influence between the investigator and the investigated.

The aims of adopting a naturalistic inquiry into STS knowledge are twofold.[3] First, I seek to contribute to scholarly debates about the role of trust in creating and sustaining communities of knowledge. Second, I aim to offer resources to my STS colleagues for addressing the communicative challenge outlined before.

Performing a Scientific Status

The first explanation of how I managed to engender trust with Rob, Miloš and other dendroclimatologists is that I made an active effort to present myself as a "scientist", particularly a "sociologist", and sought to use these displays to build up a reputable status. One way to understand the social process of status formation is to use Erving Goffman's idea of "performance" (1956). Goffman understood face-to-face interactions in everyday life as dramaturgical performances, in the sense that when an individual meets other people, the individual tries to affect their opinion of her/him and achieve their recognition.[4] Similarly, in my interactions with dendroclimatologists, I sought to be recognised as "an inoffensive PhD student doing serious work and supported by competent people".

With the assistance of my doctoral advisors,[5] I started building up my non-existent reputation as a PhD student by associating myself with reputable academic institutions (university, funding bodies) and the people and symbols (logos) that represented them. As the sociologist Anthony Giddens (1990) pointed out, our trust in abstract expert systems is mediated by "access points" or people who represent these systems. For instance, front office staff at your local bank branch are one type of access point to the banking system. "Access points" effectively reduce social distance (Reyes-Galindo 2014) and "provide the link between personal trust and system trust" (Giddens 1990: 114).

I looked at Rob's publications to see if he had co-authored any papers with climate scientists from my university, with the hope that they could mediate contact between Rob and me. I found one climate scientist and asked her to be a member of my first year doctoral examination panel. She kindly accepted our request, and emailed Rob to let him know about my work.[6] In my first email to Rob, I sent him a "brochure" of my work that included a very strategic layout of the logos of my university and funding body and the names and titles of my doctoral supervisors. I also included these logos in all my presentations to dendroclimatologists, with the hope of giving them an initial basis for trusting me. I realised, however, that these displays did not immediately make me more trustworthy, because non-British people did not recognise the logos. Therefore, I often had to spend the first couple of minutes of my presentations "highlighting" the importance of my institutional affiliations.

Because dendroclimatologists had no idea about what sociologists do (not least STS scholars), I drew upon certain aspects of their professional identity as a proxy for my own. When introducing myself I used the concept of "patterns", which I knew would be familiar to the dendroclimatologists' realist ethos. I would say, "We, sociologists, study *patterns* of human action, in the same way that you, dendroclimatologists, study *patterns* of tree-rings." I also used familiar analogies to explain my methodology. I illustrated the immersive and affective dimensions of my ethnographic research by comparing it to Jane Goodall's approach to the study of chimpanzees, and the dendroclimatologists' fondness for trees. In conversations with dendroclimatologists, I also emphasised the impartiality of my methodological approach as a feature of "our" shared scientific identity. In doing so, I tried to distance myself from outsiders to the dendroclimatology community who have a critical approach (i.e. "climate sceptics"). For instance, when I was asked if I had seen any case of scientific fraud while doing my study, I would say, "My job is not to look for fraud or to say if someone's work should be done differently, my job is to find out what scientists do and believe and why."

I was also careful to make distinctions between our forms of expertise and to avoid completely conflating our distinct scientific identities. For instance, when I was asked if, on the basis of my work, I thought climate change was a real danger, I would say, "I am not a dendroclimatologist, and I can't answer this question as an expert. I trust what most experts tell us, so yes, I believe climate change is dangerous."

My performances were effective insofar as dendroclimatologists often invited me to "switch" into dendroclimatology and to become one of them. I always said that I did not want to become a dendroclimatologist nor a complete outsider, but someone in-between, a "dendro-sociologist". It is important to note that this dual identity emerged as a result of (tacit) negotiations between myself and dendroclimatologists. I emphasised specific traits of my scientific persona and identity as I understood the dendroclimatologists' scientific culture over time, and their acknowledgement of my status as a "scientist" resulted from this adjustment.

Eye-Witnessing Completeness

The second explanation for my trustworthiness in the eyes of Rob, Miloš and their colleagues is that, as a result of my long-term ethnographic engagement, they regarded me as a competent witness who could give a complete, first-hand account of their work. What I refer to as "eye-witnessing completeness" is a social process insofar that in order for my study to become accepted as a credible testimony of dendroclimatologists' work, I had to negotiate their expectations about what it means to "see" and to "know" their work "completely".[7]

At my first meeting with Rob and Miloš in St Andrews, I immediately understood that, for them, "seeing" and "knowing" in dendroclimatology meant working with people, trees and wood in the different settings where epistemic work occurs. After chatting with Rob for half an hour in his office, he asked Milos to show me their laboratory.[8] The laboratory was one small room shared with other students from the university, which had a few computers, and pieces of wood scattered around. I sat with Miloš, and I explained my project to him. He asked me if I knew anything at all about dendroclimatology, and I responded that I had read one textbook. Miloš then stood up and brought over a large piece of wood. He spent more than half an hour lecturing me about dendroclimatology (i.e. how he had obtained this wood, and what he was intending to do with it in the next few months).

By the end of our first meeting, after Miloš mentioned that he needed some help generating data, I agreed with him and Rob that the best way for me to learn about their work was to become their (non-paid) technician. For the first three years of my fieldwork, my relationship with Rob, Miloš and a few other dendroclimatologists developed in the context of face-to-face instructions in laboratories, offices, homes, classrooms, forests, restaurants and pubs. After this training, I was able to perform a few tasks and understand most of the technical conversations between dendroclimatologists, but I never became fully competent in dendroclimatology (i.e. I never created a climate reconstruction). Yet, by accepting their authority as expert teachers, and giving them the opportunity to certify my understanding of their work in different educational settings, I became embedded in their culture.

In other words, my credibility as a liaison between dendroclimatologists and outsiders was not based on my minimal skills and limited knowledge of dendroclimatology, but on the fact that, in the process of learning those as a trainee, I had observed the day-to-day experiences of doing dendroclimatology and I could testify to it in my study. Rob certainly thought that my first-hand understanding of dendroclimatology stemmed from the fact that I had worked in the field and in the laboratory *with them*.[9] On his project website, Rob seemed to make this point while describing me affectionately with my shortened name: “Meri is busy getting her teeth into understanding dendrochronology—helping with both fieldwork and laboratory work. Kudos!” As a result, when Rob introduced me to his colleagues, he often emphasised that “Meri knows the science”. Surely, what Rob meant by “knowing” was not “doing” the science (as I said, I could do very little science). Instead, by saying to his colleagues and others that I “knew” the science, he was (kindly) certifying me as an eye-witness of his work.

Becoming a credible witness not only meant generating close physical observations of Rob and Miloš’ work; I also learnt that it involved observing how this work changed over time. In April 2012, when I started my relationship with Rob and Miloš as complete strangers, I intended to do what I called a “quick ethnography”. Six months later, in October 2012, I wrote in my research diary, “I can’t leave the field now!” My sense of compulsion (“can’t”) came from the fact that I knew that Rob and Miloš would be disappointed if I stopped my fieldwork then, because in their view, I had only observed a few aspects of their work. I decided to extend the period of my observations indefinitely until they considered their work complete. We agreed that I would postpone the submission of my thesis until Miloš submitted his own (so that I could include in my thesis a copy of the “final” scientific object, a temperature reconstruction for Scotland). Eventually, I stopped generating data in September 2015, just a month after Miloš submitted his thesis, and one year after my funding expired.

Extending the period of my observations until Rob and Miloš considered their scientific object complete meant that I could not always be physically present. In the last year of my doctorate, I moved to London. We interacted regularly online and only met face-to-face on two occasions. During this last year, becoming a credible witness involved keeping in touch with Rob and Miloš by email and relying on their accounts of their own work.

In writing my last thesis draft, it was the fact that Rob and Miloš believed that I had developed the necessary understanding of the embodied, contextual, communal and temporal nature of their work that allowed me to transform the messiness of their work into a linear written narrative. In other words, Rob and Miloš accepted my simplified account of their work as a necessary compromise in making it more accessible to outsiders because they thought that I had *seen* (and *felt* as a non-technician) the complexities involved throughout the *entire* process of knowledge production.[10]

Encouraging Civil Scepticism

The third and final explanation I offer for the existence and maintenance of my relations with Rob, Miloš and other dendroclimatologists over the period of my fieldwork is that I generated opportunities for providing honest and tactful criticisms of my work. I refer to the social processes of negotiation about when and how criticism is exercised and dealt with among mutually trusted people as “civil scepticism”. These courteous sceptical exchanges with dendroclimatologists occurred in the last stages of my fieldwork, when, for the reasons I exposed above, they had come to trust me and respect me. This initial trust was a necessary condition for civil scepticism to happen, insofar as it set the tone of our conversations: we were predisposed to perceive each other’s comments as “constructive” rather than offensive, and we had an interest, in the first place, in improving our respective work through mutual criticism.

Throughout my four years of fieldwork, I asked Rob and Miloš to comment on the different drafts of my thesis as a means not only to comply with institutionalised demands in ethnographic research (Plemmons and Barker 2016) but also, and mainly, to reciprocate Rob and Miloš’ trust and negotiate their dissent. On the one hand, I regarded the manuscript as a “token of friendship”. On the other hand, I regarded the drafting of the final manuscript as a mechanism for negotiating potential disagreements with Rob and Miloš.

I structured my thesis into two narratives, partly to draw the boundaries of legitimate criticism. The “epistemic narrative”, as I called it, was a description of Rob and Miloš’ work, and the “sociological narrative” was my sociological interpretation of their work. I invited Rob and Miloš to comment on both narratives. My commitment to them was that I would incorporate their comments into the epistemic narrative but would reserve the right not to modify my sociological narrative, as a means to preserve my autonomy as a sociologist. Rob and Miloš barely commented on my sociological argument, but when they did, they accepted that we could disagree.

I also invited dendroclimatologists, as a community, to interrogate my work in public venues like conferences and workshops where, from their perspective, such communal examinations *should* occur. More precisely, I presented my work at two international dendroclimatology conferences and two workshops organised by Rob in Scotland. Crucially, I gave these presentations in front of relatively sympathetic audiences that either already knew me personally or, presumably, had heard about me. The content of these talks always revolved around my sociological interpretation of one aspect of Rob and Miloš' work. During the Q&A sessions and after my talks, members of the audience made comments and suggestions that helped me to refine my interpretation.

Dendroclimatologists seemed to be thankful to me for these communal examinations of my work. One example of this recognition is the prize that the scientific committee of one of these conferences awarded me. When I asked one of the members of the committee about the reasons for this award, he mentioned my willingness to engage in a conversation with them: "We appreciated that you had the courage to present your work in front of us."

Another example of the fact that Rob and Miloš valued my methodology was related to my use of "breaching experiments" (Garfinkel 1967: 35). In order to test my own understanding of Rob and Miloš' work and elucidate their assumptions, I often asked them challenging questions such as "Why do you use this equipment?" or "How do you know that this result is good?" Sometimes I would attribute these questions to third parties, and I would say, "Imagine that I am asking this question as a climate sceptic…" Initially, as I expected, Rob and Miloš were bewildered by my questions. Over time, they came to see my "breaching questions" as civilised scepticism and thanked me for that. For instance, I found out that in the "Acknowledgement" section of one of Rob's papers he had included my name alongside others "for their comments and discussion on this work". Similarly, in the acknowledgment section of Miloš' thesis, he thanked me "for all of [my] help and endless inquisitiveness".

Overall, the reason why, I think, these different examples of civil scepticism helped me to maintain my trust relations with dendroclimatologists is that they were aligned with their own norms and practices. In other words, they valued my approach in generating sociological knowledge because they also validated their dendroclimatological knowledge through courteous sceptical exchanges of their work.[11] One piece of evidence of the alignment of epistemic and social practices is the fact that dendroclimatologists often used the Q&A of my presentations for discussing their work among themselves. In this way, they built upon and appropriated my strategies for making my account more credible for their own purposes.

The Limits of Trust and Communication

I have explained why, in the context of widespread suspicion towards outsiders, a group of scientists allowed me to study their everyday work and share my findings with others. I have also outlined three social processes as explanations for the development of inter-personal trust between myself, Rob, Miloš and other members of the dendroclimatology community. These processes refer to my efforts at creating a shared (but not complete) scientific identity and professional ethos; my long-term participant observation of epistemic practices and my commitment to generating a temporally exhaustive account of these practices; and my willingness to create opportunities for critical interrogation and discussion of my work.

I now return to the broader issue discussed in the introduction about the challenge of communicating science in action, understood as a problem in constituting a community with audiences outside STS. To what extent has my inter-personal trust with Rob and Miloš enabled me to communicate STS knowledge successfully, in the sense of creating a larger community of people who think that the "social" is part and parcel of the "science"? How can STS scholars make a better informed exercise of communicating science in action based on my experience? At this early stage of dissemination of my work, I can only answer these questions by discussing how my immediate audience (Rob and Miloš) reacted.

One successful outcome from my communicative efforts is that, at this stage, Rob and Miloš do not perceive my work as a threat to their credibility. Shortly after submitting the final thesis draft, I asked them to give me their general opinion, and Rob responded, "A really nice piece of work. I honestly don't know how many others would find your thesis interesting but maybe that is something we can discuss." In a separate email, Miloš also praised my work but refused to engage in a more detailed discussion about my argument: "Think it's a very good piece of work (though of course I can't say much about the sociological side of it)".

On those few occasions when Rob discussed my sociological ideas, however, I felt misunderstood. Commenting on one of my draft articles, Rob complained, "I wonder if you're being way too simplistic here; by its nature, the scientific method is a critical approach that relies on validation of an observation." Rob's general statement about the sceptical nature of the scientific method was painful for me to read because it represented the kind of position I criticised in my article. I responded to Rob, "I agree that scientists are often sceptical of each other, but you also know that you cannot be sceptical all the time if you want to get your work done (…) What I am trying to say here is that doing science involves a necessary balance between trust and scepticism." He interpreted my response as a criticism rather than a descriptive statement and lamented, "So true—I wish we were a little more sceptical/critical, in public at least."

The main reason why, despite my close trust relations with Rob, he is unable to understand my sociological argument is, I suspect, that it conflicts with an existing cultural norm in the scientific community more generally. This communal norm sets trust and scepticism in opposition, and places trust as unscientific. Steven Shapin's historical work provides the basis for such an interpretation. According to Shapin (1995), the reason why the existence of scientists' trust relations has become historically invisible and seen as unacceptable by many people (including natural scientists) is the success of a culture of epistemic individualism, promulgated, among others, by early modern empiricists. This culture is encapsulated in the Royal Society's motto, "*Nullius in verba*", which roughly translates as "take nobody's word for it". As a result, scientists like Rob might act upon the belief that true knowledge derives from being systematically sceptical and rejecting the authority of others. As a result, as Robert K. Merton observed, "the institution of science makes scepticism a virtue" (1976: 62).

The fact that the communication of STS knowledge might be affected by social factors and processes other than inter-personal trust also became evident to me when I asked Rob to reflect upon the value of my work for him. In fact, I invited Rob to share his answer publicly during an anthropology conference panel I co-convened with one of the editors of this volume (Duarte). Rob's response to my question was in the form of another question, "Does being a sociologist's 'case study' provide a new avenue of science communication and outreach?" He responded by outlining what he called "the climate scientist's 'reality'", which he defined in terms of three types of audiences (peers, the public and politicians). Rob argued that my work would only be valuable to him if it allowed him to gain the trust of the public and politicians and to demonstrate the "impact" of our collaboration to funding agencies. Whilst some of my colleagues attending the panel complained to me that Rob's response dismissed the potentially more enlightening value of my work, I thought they had missed the point entirely.

If we are to play by the rules of our own game, as Michael Lynch advises above, STS scholars must expect that the public reception of our knowledge (just as natural scientists' knowledge) will be inevitably fragmented and linked to the cultural dynamics of their audiences and the context of use of such knowledge. In the case of my research, Rob currently evaluates my work in terms of the specific challenges and goals that his community pursues, namely more public credibility and resources.

Does this mean that my efforts to build up trust relations with Rob and his colleagues have been all in vain, and that I should give up the aim of communicating science in action? I do not think so. Whilst my trust relations with Rob, Miloš and their colleagues have not led to a shared understanding about their work, it has generated a situation in which they *tolerate* my account. Whilst tolerance and respect could be seen as insufficient responses, some scholars have argued that our capacity to maintain relationships with people with whom we disagree but whose ideas we value on a par with our own is the basis for a minimal sociability within and between communities.[12] What emerges from my account is, therefore, an optimistic message for STS scholars regarding the role of trust in communicating science in action; to be sure, having trust bonds with our immediate scientific audiences does not generate immediate and complete communication but it cultivates greater tolerance and mutuality and generates further opportunities for negotiating a common understanding about the processes of science.

Notes

1. For those sociologists who share a cognitivist understanding of social order (Mazzoti 2008), the problem of constituting cultures and societies is, in fact, the problem of constituting shared communal knowledge.
2. For a complementary understanding of the communication of science as a process of formation of shared cultures and identities see Maja Horst, Sarah Davies and Alan Irwin's account (2017).
3. The naturalistic analysis I conduct here is inspired by David Bloor's (1991/1976) principle of reflexivity in the sociology of knowledge and Robert K. Merton's idea about "self-exemplification" (1979: 4). He argued "were the sociology of science not self-exemplifying, then either

the general ideas and findings would have to be thought unsound or the field itself is nothing like the scientific speciality that it is commonly supposed to be".

4. Here I ignore one problem with Goffman's dramaturgical model, namely his distinction between the "performing" self (front stage) and the "real self" (back stage). For an extended critique and development of Goffman's ideas by an STS scholar read Mol (2002: 34).
5. I thank Prof Janette Webb, Dr. Sara Parry and Prof Donald MacKenzie for their advice.
6. Huge thanks are due to Prof Gabi Hegerl for her support.
7. Lisa Garforth (2012) points out that the rhetoric of witnessing and knowing used by STS scholars supports a wider culture of accountability. Similarly, my work was possible partly because Rob and Miloš had an interest in being seen as open about their work, especially after the accusations of secretism aimed at climate scientists after Climategate.
8. "Laboratory visits" or "tours" seem to be one of the most common forms of public communication performed by scientists, but unfortunately, as far as I know, we do not seem to have a very good sociological understanding of them.
9. Collins and Evans (2016) and Ribeiro and Lima (2016) have recently discussed the role of "physical contiguity" in communication across groups, including the sociologist's own communication with those investigated. My only contribution to this debate is that visualising dendroclimatologists' work made me, in their eyes, a more credible witness because it reduced the need for intermediaries and testimonies, which fits with the culture of empiricism that prevails in dendroclimatology.
10. This conclusion raises the question about why climate scientists seem to have a systematic distrust of journalists who report their work (for the latest example see Nethery and Vincent 2016). Climate scientists often accuse journalists of producing simplistic and inaccurate accounts of their work. At the core of these accusations is the belief that journalists are unreliable witnesses. This mistrust might partly arise because of journalists' practices (i.e. the fact that they often converse with scientists intermittently).
11. In fact, I came to regard my own epistemic practices in terms of "civil scepticism" after observing similar practices in the way dendroclimatologists produced knowledge.
12. The sociologist Barry Barnes (2001) has argued that tolerance deserves to be recognised as a "primary virtue" and an essential feature of social life. For Barnes, since no rule or social norm has an inherent meaning, members of a community need to be tolerant of each other's different views in order to agree on the meaning of such rules. However, as Barnes himself

notes, this does not resolve the question of whose views one feels compelled to be tolerant of. As Richard H. Dees (2004) suggests, this is when trust comes into play. According to Dees, we find the need to persuade and be persuaded by those people whom we trust and whose opinions we care about.

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PART II

Intercultural Communication Beyond Science's Boundaries

CHAPTER 6

Epistemic Interactions Within and Outside Scientific Communities: Different or Analogous Processes?

Carina G. Cortassa

Introduction

Throughout history, communities have created practices that configure their experiences of knowing/knowledge and promote its creation and dissemination. The seventeenth century witnessed the emergence of a particular culture of knowledge—science—whose claims of cognitive authority are grounded in publicly acknowledged methods for evidence production and communication, as well as in robust reasons that support the differential feasibility of its assertions (Vega-Encabo 2012). The cognitive and practical achievements reached since then conferred on science a privileged status among the different ways in which contemporary communities make reality intelligible and transform it in several ways.

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Since the mid-1960s, the growing strategic value of scientific knowledge and technological developments have highlighted their close links with a number of non-epistemic interests, which helped to bring about their current controversial character along with the decline of the confidence in their beneficial contributions to humanity. These changes brought an unprecedented visibility to the scientific and technological complex in the public sphere, both due to the remarkable nature of certain results and applications and to the apprehension that came with them. In its expanding capacity for discovery, explanation and prediction of events at all levels, science has been led to frontiers further and further apart from people's comprehension while, paradoxically, it pervades their daily lives in several ways.

Following increasing awareness of science's epistemic and political interest, the analysis of the social dissemination of scientific knowledge has generated a fruitful field of research and action. After an initial period strongly influenced by the so-called model of cognitive deficit—and the consequential strategies to enhance people's scientific literacy—the contribution of Science, Technology and Society (STS) approaches during the 1990s came to nourish the incipient discipline's agenda (Trench 2008; Bauer et al. 2007; Miller 2001). The new perspectives arose from the several questionings to the *tandem* deficit/literacy as the epistemological and methodological basis in Public Understanding of Science (PUS) studies drew scholars' attention to less linear, far more complex, aspects of science–society interactions (Einsiedel 2007).

In this chapter, I propose an approach for tackling the socio-epistemic interactions that allow knowledge to circulate among scientists and laypeople, grounded on the way in which it is shared within the formers' specific realm. The mechanisms that shape cognitive exchanges among experts will be examined in view of certain trends in social epistemology, which can make positive contributions to the lively, long-standing STS' debates on the issue. With a focus on the role played by distinctive features such as trust, reliance on others and particular structures such as deference and authority, it will be argued that these factors provide useful keys not only to grasp the process of knowledge-sharing between experts but to understand its broader social circulation as well. The main argument underlying this proposal is that the epistemic interactions between cognitively imbalanced agents do not radically differ from those established at the inner circles of science. The concluding remarks will highlight the potential of this perspective for enhancing the PUS research agenda.

PUS Studies: From the Cognitive Deficit Towards the Cognitive Deference

The Cognitive Deficit

Interest in relations between laypeople and scientific knowledge arose in a context of emerging concern about the potential consequences of science and technological developments that would rapidly become essential for contemporary societies (Sánchez Ron 2000). Successive approaches to the phenomenon—in the form of the surveys described below—tended to confirm the existence of a perceptible alienation between both worlds, progressively outlining the image of a gap that had to be overcome.

The earliest accomplishments in the field of PUS came from empirical research. During the second half of the twentieth century, periodical large-scale studies on the issue were established both in the United States and Europe. These surveys, designed to determine people's levels of knowledge, information and interest in science, soon became a trend which rapidly expanded to a number of national and supra-national contexts.[1] The issue of how to ensure data quality, reliability and comparability led to the standardization of the basic concepts employed in the analysis and of the respective measurement scales. In particular, at the core of the quantitative tradition promptly crystallized a conception of "scientific literacy" as the condition in which people: (a) have basic knowledge of the natural sciences, medicine and technology, and of the process and methods of scientific research; (b) can interpret the meaning of new developments in these areas; (c) can actively react to them when necessary (Thomas and Durant 1987). This normative vision was operationalized for research purposes in a set of indicators turning around two dimensions: the knowledge of certain terms and concepts and the knowledge of some differential features of scientific practices.[2]

Surveys provided more than the basis for the discipline's methodological stabilization. At a substantive level, besides repeatedly confirming the exiguous level of knowledge in different populations, some of their findings led to infer the existence of a linear correlation between the cognitive and attitudinal dimensions of the public perception of science. Progressively, the hard core of the traditional thesis in the field consolidated around assumptions that a higher scientific literacy level would lead to a greater interest in and more positive attitudes towards science. Conversely, people's negative appreciations and lack of interest in science was thought to be directly related to their low level of knowledge (Torres Albero 2005).[3] As a result, solving the cognitive deficit by *educating* people became a central goal for both scientific communities and governments, given that science and technology's social legitimacy—and, therefore, people's support for decisions and investments in the area—would heavily depend on it.

To outline the problem of the gap between people and science in terms of a *deficiency* of the former to understand the latter is a reductionist view of the phenomenon. As described in the following section, this has been the catalyst for the numerous objections challenging the classical paradigm in the field of PUS during the last decades.

The Deficits of the Deficit (And of Its Critics)

According to the deficit model, to uplift the public's scientific literacy through formal education and massive popularization would be sufficient to positively modify their attitudes towards science, increasing interest in and support for its deployment (Thomas and Durant 1987). The appeal of this image lies in its intuitive and encouraging character: on the one hand, it assumes the commonsense feeling that only by knowing something can it be accepted and truly appreciated. On the other hand, it suggest that, no matter how great the obstacles may seem, the alienation between science and society has a clear and achievable solution.

However, the data provided by successive surveys have tended to blur that optimism. First, the public seemed to resist all efforts and investment made to educate them (Miller 2001, 2004). Second, the accumulated evidence from empirical studies gradually made clear the weakness of the supposed correlation between knowledge and attitudes, hitting the paradigm at its heart (Evans and Durant 1995; Durant et al. 2000; Peters Peters 2000). Additional objections regarding the reliability and validity of basic constructs such as "scientific literacy" (Bauer and Schoon 1993) and of the indicators used in the surveys (Pardo and Calvo 2004) led to diminish the confidence in the deficit-based research as well as in the effectiveness of the practical strategies derived from it.

From the beginning of the 1990s, the most radical challenges to the classical paradigm came from STS. The contribution of the diverse perspectives included under this broad label was key in highlighting the limitations of PUS studies under the dominance of a framework that, trapped in its cognitive reductionism, was unable to tackle the huge complexity of the relationship between science and society. [4] By introducing new concepts, debates and epistemic interests, the "ethnographic turn" (Irwin and Michael 2003), the "contextual approach" (Miller 2001) and the "constructivist" view (Wynne 1995) significantly enhanced the PUS agenda, opening up original topics for research. Just as science is contextually anchored, affected by interests, values and practices that go beyond its strictly epistemic dimension, so are its processes of social dissemination and appropriation. Thus, a fair approach to the interactions between scientists and the public should focus on the specific settings in which they meet, each of them bringing personal experiences, aims, rationales, values, skills and criteria on an equal footing. On this basis, laypeople are no longer perceived as passive recipients of knowledge, but as active agents in their interactions with experts; thus, their scientific (il)literacy is insufficient for understanding their role and attitudes in the process of coping with science and scientists. The mere concept of a cognitive gap becomes inadequate.

The extensive body of research carried out under the ethnographic-contextual premises mentioned above made valuable contributions to developing a better understanding of the interactions between experts and the public, enhancing the theoretical basis of PUS studies and broadening its horizons. The new programme introduced a different way of analysing the rationality of public attitudes that cannot be restricted to the cognitive dimension (Wynne 1991: 116) and readdressed the agenda in terms of the situated processes of controversy and negotiation between different types of expertise and how the scopes of epistemic authority are drawn and redrawn in each case.[5]

While generally agreeing with the idea that the cognitive imbalance is not the only dimension shaping people's links with science, my interest here is on those situations in which it *does* play a central role in their relations with experts, particularly in circumstances when issues are framed in such a way that no one without a certain kind of (scientific, technical) knowledge can feasibly enter the discussion in fair conditions. On these occasions, the agents need to share at least a minimum of common concepts and terminology in order to make the dialogue effective. To understand how this can be possible requires exploring further the basic epistemic asymmetry as one of the premises that are part and parcel of the surrounding context framing and influencing their interactions. Just as the public's culture and skills must be acknowledged as substantial mediations of their relations with science and scientists, so, too, must the factual existence of the epistemic asymmetry underlying this process. The point can be summed up in this way: how can laypeople make their voices heard in a public discussion about the monitoring of embryonic stem cells research without at least knowing what embryonic stem cells are? In cases like this, the absence of a basic understanding of certain concepts rules out all the possibility of participating in the dialogue, turning it into something closer to a duet monologue.[6]

The Cognitive Deference

In 2014, the *Public Understanding of Science* journal launched an essay competition around a compelling question: 'In science communication, why does the idea of a public deficit always return?'. In answering this question, I addressed two types of reason, practical and epistemic, both involving arguments covered in previous sections (Cortassa 2016). At a practical level, the persistence of the idea lies in its schematics: by reducing the barriers that hinder science–society relations to the single dimension of the public's scientific illiteracy, the model represents a reassuring way for coping with the causes and consequences of the problem, while simplifying the type of actions that should be taken to solve it. Thus considered, increasing people's levels of knowledge and understanding capacities would lead them to achieve a greater interest in and more positive attitudes towards science; therefore, filling the gap is just a matter of implementing the best mechanisms at hand to properly educate them and minimize their reticence and fears.[7] Nonetheless, from my point of view, one of the main constraints that PUS studies currently face is far more deep-rooted. At an epistemic level, it derives from the insistence on attaching the deficit notion the character of a *problem*—practical or theoretical—and, as a consequence, to pointlessly prolong the debates around it. The controversy about whether *deficit yes or no* is actually responsible for bringing the analysis of the science–society relationships to a virtual standstill.

My proposal for overcoming this situation is to consider the uneven cognitive positions of scientists and publics as an *assumption* underlying their interactions, instead of *as the obstacle* to be surpassed, and to explore how specialized knowledge circulates and is shared under this prior condition. Moreover, I suggest that the imbalance must be more drastically conceived than the image of the deficit allows, because it does not regard the amount of data each of the agents have at their disposal, but the nature of its attainment and justification: the epistemic asymmetry does not lie in the quantitative aspect of what experts know and the general public does not know, but in the qualitative differences between the ways in which each of them access the contents of science. To fully grasp the contexts in which the social dissemination and appropriation of knowledge take place requires understanding the constraints this feature imposes on the process. In this sense, of particularly interest are the role played by trust, reliance on others and specific structures such as deference and the adscription of epistemic authority.

In this framework, the possibilities of laypeople acquiring specialized knowledge depend on trust of scientists' assertions. According to Hardwig (1985: 339), that's because [she] "(1) has not performed the inquiry that would provide the evidence for hers belief that *p*, (2) is not competent and perhaps could not even become competent to perform that inquiry, (3) is not able to assess the merits of the evidence provided by expert A's inquiry, and (4) may not even be able to understand the evidence and how it supports A's belief that *p*." Assuming we are dealing with knowledge about the Earth's motion, what makes us think that a layperson knows or can know that the Earth is in a state of permanent and combined rotation, translation, precession and nutation movement, if she

1. hasn't determined it through her own means (especially if her own faculty of rational perception implies that the Earth is immobile);
2. is not competent to independently determine the Earth's movement of nutation and will probably never be (because she is not and does not want to be an astronomer);
3. cannot appraise whether the proofs offered are good evidence for terrestrial movement[8] (she is not in position to assess the quality of the relation between the premises and the conclusion about the Earth's nutation).[9]

However, according to Blais, even in those conditions it would be "cognitive suicide" (1987: 363) to assume that knowledge independently obtained and justified is the only valid way to know. Any person who lacks first-hand evidences can still affirm that she *knows*—in a powerful sense—that the Earth's axis moves if she cognitively defers to other/s who know it for her/their own reasons and simultaneously communicates both things—the fact and the supporting evidence. In other words, it's considered that a good reason to affirm that something is known is to trust the knowledge and the proofs conveyed by another person's testimony in the course of a dialogue.

Acquiring Knowledge from Others

Epistemic Exchanges Among Scientists

In the scenario outlined in the previous section, the possibilities for the public to acquire and justify a series of facts rested upon what Hardwig defines as the "[weak] general principle of testimony". This principle captures the epistemic structure of authority that characterizes exchange between experts and laypeople: "If *A* [the public] has good reasons to believe that *B* [the scientist] has good reasons to believe *p* [the Earth's motion of nutation] then *A* has good reasons to believe *p*" (1991: 697).

It's clear that appealing to a reliable authority as a source of knowledge appears at its maximum expression in contexts involving agents in uneven cognitive conditions, as though the model would have been posed strictly to explain their plausible epistemic exchanges. However, even if this is a dominant feature of relations between experts and non-experts, it is not unique to this specific type of epistemic interaction: it is also relevant to exchanges established within the realm of other specialized communities, in which case it plays a similar role (see Reyes-Galindo 2014). Notwithstanding, the persistence of the classical images of epistemic individualism makes it difficult to admit the pertinence of deference in the context of science. In other words: appealing to an authority may sound reasonable to describe the way in which the public can acquire knowledge. After all, to trust others could be the sole alternative in knowing anything that transcends the limits of our immediate experience. But would not that be the very opposite of the *scientific spirit*, according to which one of the basic attitudes is Mertonian "organized skepticism", that is, doubt regarding every kind of second-hand evidence, the demand to test and replicate other researcher's experiments before accepting their assertions or results? Science and deference, are they not, by definition, antithetical knowing practices—the former based in the exercise of one's own cognitive faculties; the latter withholding one's own judgement to deliberately rely on others?

In parallel with more sociologically oriented approaches, a series of views currently set under the label of social epistemology can also help to shed light on those questions. Among others, the works of Kitcher (1992), Goldman (1999) and Fricker (2002, 2006) have explored the role of trust as a common mechanism for knowing in the context of socio-epistemic communities built on the basis of specialization, division of cognitive labour and cooperation among its members, including contemporary science. Conceived as a social, collective endeavour, scientific research relies heavily on scientists' interdependence based on mutual trust.[10]

Kitcher points out at least three ways in which deference to authorities influences the cognitive activity within scientific communities. First, broadly, through each subject's dependency on the knowledge achieved throughout the field's development, which permeates its early intellectual ontogeny during training periods. Secondly, more specifically, when novices become part of a particular group and adhere to the general agreements and settled criteria by those regarded as legitimate disciplinary authorities—that is, when they learn to recognize the accredited voices whose judgements are assessed as both valid and valuable. Thirdly, in the course of daily interaction with peers through which knowledge circulates and, as a result, some of the agent's assertions are accepted with no further requirements, others are put under scrutiny and others are plainly rejected.[11] In the context of scientific communities, says Kitcher, the allocation of credit among their members, the recognition of who is an authority worthy of reliance, directly influences the work of the group as a whole (1992: 245). From this viewpoint, to delegate individual judgement to someone else's judgement does not compromise the epistemic value of rationality at all; in any case, what should be called into question is the criteria of rationality held by individualistic epistemologies.

Epistemic individualism fails to take full account of the way in which contemporary science—structured around the division of cognitive work—produces and grounds knowledge. It can no longer be sustained that direct evidence, independently obtained, is the only valid source of justification because, under current conditions, that would imply denying most of the content of science the proper status of knowledge. In their everyday practice, scientists do not systematically replicate others' experiments unless having grounded and deep doubts about them (Hardwig 1985: 345–347); they are not in position to obtain exhaustive evidence to judge by themselves each and every one of the beliefs accepted as truth in a field, but they take as premises their peers' results when assessing them as reliable informants—that is, that their peers are competent and responsible when producing knowledge and honest when informing about it (Fricker 2002; Blais 1987). To assert in a non-trivial way that science is a *social construction* implies admitting that the epistemic interdependence of its agents defines it so deeply that, ultimately, any picture of the scientific process and practices that ignore or exclude this basic feature would be both unrealistic and unreasonable. Therefore, deferential knowledge is not in any way at odd with with rationality values, nor do appeals to authority oppose the scientific spirit. Experts do not need to confirm every piece of evidence to consider they *know* because they are warranted to take that evidence as such by trusting others who have produced it independently. In this sense, as Hardwig convincingly asserts, "the trustworthiness of members of epistemic communities is the ultimate foundation for much of our knowledge" (1991: 694).

To address the question of whether it is possible for the public to acquire factual knowledge despite the limitations of their asymmetrical epistemic condition, in previous sections it was argued that deferring to the authority of experts is the only way people have at their disposal to know anything exceeding the limited scope of their own experience. At this stage, it seems that this mechanism of knowledge acquisition is not very different from the one operating among agents in a more balanced situation concerning their cognitive resources, that is, within science. In both cases, under certain circumstances,[12] a subject can affirm that she knows something when she takes for granted the knowledge offered by interlocutors previously identified as reliable informants. Just as in the broad socio-epistemic community made up of experts and laypeople, where the latter rely upon their trust in the former to gain access to knowledge, so within scientific groups.

The Place of Testimony in the Epistemic Exchange

The publics' epistemic position is, as previously stated, highly vulnerable. Faced with a scientific proposition, they are objectively unable to assess either the truth-value of its contents or the quality of the supporting evidence. Apparently, in such circumstances, asymmetry reduces the options of believing or doubting the experts' claims. But this idea seems to collide with a common topic for PUS scholars and practitioners, namely the need to promote people's *critical judgement of* science. Wouldn't it be contradictory to affirm that their chances of obtaining knowledge through epistemic dialogue rests on admitting the validity of others' word rather than thinking for themselves? It could be objected that the deferential model does not significantly differ from the deficit model, as both endorse an image of laypeople as passive recipients who believe in science as they do in any other type of knowledge. However, as we will see, acknowledging the cognitive asymmetry between agents in no way implies confining laypeople to a position of blind trust, to an overwhelming dependence on experts that excludes any sort of control over the knowledge at play. In that sense, the issue is whether or not people's trust in the authority of scientists is a normative ascription. Under what condition do people have good reason to ascribe that authority? When is and when is it not reasonable to accept scientists' testimony on a particular subject—for instance, about the nutation of the Earth's poles?

This type of question is linked to a far-reaching discussion in the fields of STS and social epistemology about the justification of knowledge acquired on the basis of trusting others. On the latter's approach, the debate was shaped in terms of *reductionist* and *anti-reductionist* views on testimony.[13] A brief examination of their respective arguments can shed light on the process of the public circulation of scientific knowledge.

According to the *anti-reductionist* approach, testimony is in itself a basic form of the justification of knowledge at the same level as, for example, perception, memory or inference: under certain minimal conditions—in the absence of compelling reasons for suspicion—recipients are entitled to accept as knowledge the assertions offered by an informant merely on the basis of her words, without any further positive epistemic work on the former's part (Lackey 2006: 4). In other words, any assertion is a priori creditworthy until shown otherwise. Even when the hearer has scarce or no information available to assess the speaker's reliability, the acceptance of a person's testimony is epistemically grounded because testimony, in general, is credible prima facie (Goldman 2006). Adler (2015) summarizes this position as the "Default Rule" (DR): "If the speaker S asserts that *p* to the hearer H, under normal conditions, then it is proper or correct for H to accept S's assertion, unless H has special reasons to object." Except for specific cases—for instance, when the proposition's content is clearly unlikely or the hearer has good reason to be wary of the teller's credibility—trusting others' words is fully justified without any further procedure. Thus conceived, testimony is innocent—reliable—until proven guilty.

Looked at from this angle, the critical activity of the public during their encounters with science appears, if not completely put aside, reduced to its minimal expression. At best, faced with an expert claim—for instance, about the nutation of the poles—people would screen it almost automatically, following relatively loose requirements. However, there are times in which the content of a scientific assertion plagues a person more acutely than the Earth's motion and, therefore, its acceptance or rejection will certainly involve a more careful assessment. When the issue at stake is, for example, the safety of nuclear waste deposited locally, laypeople are at substantial risk if the experts are wrong or not sincere about the results of radiation measurements and the evidence put out; it might be expected that, in these cases, at least some of them would engage in a rigorous appraisal of the reliability of the interlocutors in the dialogue.

The main arguments of the *reductionist* approach upon the epistemic value of testimony go in this direction. To be warranted, the acceptance of knowledge acquired through testimony demands much stricter conditions than the simple absence of debunking reasons: recipients have to make an effort to gain positive evidence of any kind beyond the given word. Considering people's imbalanced position, it seems clear that this entitlement cannot derive from access to first-hand proofs on the subject for an assessment of the epistemic quality—accuracy, reliability—of the proofs provided by experts. But, as I have stated on another occasion, "the asymmetry regarding *what is said* does not imply that the person is completely unable to judge the qualities of *who says so*" (Cortassa 2016: 456). Premises regarding the reliability and trustworthiness of an informant, or a group of them, supply the basis of any decision on whether or not the informant can reasonably be credited as an epistemic authority worthy of reliance (Fricker 2002). In which circumstances and under which conditions was knowledge obtained? What are the expert's and her institutions' credentials? What do her peers say about her competence and track record? Does she work for anyone who, for a particular advantage, could have impelled them to lie, hide information, manipulate or distort the evidence?

These types of question reflect the responsible epistemic attitude that a layperson should (empirically) and must (normatively) adopt before deferring to experts; they also show that the adoption of a stance of trust does not imply being passive and/or credulous recipients of knowledge but, rather, active agents who strive to obtain reasons to ground the acceptance or rejection of the scientists' testimony. Objections raised against the deference model's so-called omission of the public's critical judgement make no sense once it is acknowledged that *being reasonably trustful entails being critical*.

Assessing the Credibility of Epistemic Authorities

According to the arguments posited above, faced with "X [a scientist] says *p* [the nuclear waste deposited near the neighbourhood is safe]" a layperson would only be warranted in accepting *p* if she has good reasons to believe that X is competent and reliable to affirm *p* (Fricker 2006). Despite her vulnerable condition, this allows the recipient of the testimony to keep a certain dimension of epistemic self-governance: trust is grounded in the exercise of a sagacious and demanding assessment of the informant's virtues. The supporting evidence at hand is not always exhaustive and does not necessarily imply previous knowledge among the agents. In the absence of a direct contact, the recipient of testimony can infer those qualities from alternative sources, for instance, referring to third parts; nevertheless, a generic idea of the role, competences and values presupposed for a subject to be considered an expert can be helpful in this sense (Fricker 2002: 382), as well as social representations about scientific institutions—universities, centres of research, academies—and what it implies for an agent to be linked with them.

Applied to relations between scientists and laypeople, this is relevant for two reasons: first, because the latter are not in the best condition to obtain information and judge the value of the evidence related to the former's competences. Second, because the links between them are rarely close or sustained enough to provide relevant premises about the capacities and moral features of a single informant in order to assess her claims. But it is possible that any average layperson has a general idea of the type of competences and skills required for a person to be considered "a scientist" or can form an opinion on the issue via the contribution of science communication agents. Mediators count among the most relevant sources of information that permit members of the public to judge the reliability of an epistemic authority and, on that basis, to acquire deferential knowledge.

Once again, the way in which the problem of credit attribution is tackled in the realm of scientific communities can shed light on its counterpart in the broad context of science in society. In the first case, Kitcher (1992) says the mechanism is two-fold. On the one hand, the credit assigned to a colleague comes grounded in the quality of her own epistemic merits, whether these are directly perceived or through other experts' opinions; this is called "earned authority". Trust from direct calibration of the scientist's performance is different from that obtained via the agent's social position in a peer community, in a particular well-renowned institution or in her relationship with outstanding personalities in the field. This type of credibility, based in what Kitcher refers to as "unearned authority", stems more from social than from epistemic features.

Direct calibration has certain restrictions. Since being able to assess the epistemic merits of an agent requires a level of competences close to hers, this practice is mainly restricted to the credit allocation processes which occur inside experts' communities, and so unavailable to the public generally. In their stimulating, indeed controversial, "Periodic Table of Expertise", Collins and Evans (2007) reflect that this type of skillful appraisal in the categories of "downward discrimination" and "referred expertise", which only applies in cases where specialists judge other specialists on the basis of appropriate epistemic considerations. Indirect calibration, however, does connect with the type of evidence that a layperson has—or can have—at his or her disposal to appraise the reliability of an epistemic authority: among them, the social recognition derived from an award or distinction, the identification of the informant with a famous or respected institution, the fact of being or having been a disciple of a figure whose own credit, well established in the public opinion, is shared by his successors. In that vein, Collins and Evans (ibid.) propose that the use of certain kinds of "meta-criteria"—such as the credentials, experience and track record—can be helpful in these circumstances.

So far, the arguments have focused primarily on a situation where a person faces a single position on a particular subject and must decide, in the course of the interaction, whether or not the informant's knowledge should be accepted. However, in real-life contexts of the social circulation of science the issue turns out to be harder because, frequently, the public has to deal not with one but with two or more qualified informants whose assertions on the same topic are not always coincident.[14] Nowadays, a paradigmatic case in this sense is the controversy around the anthropogenic and non-anthropogenic perspectives on climate change. This makes the problem of trust and acceptability far more complex, because laypeople have to choose between putative experts making conflicting assertions about a given matter. Goldman (2001: *passim* 93–108) calls this "the novice/2 experts problem". Since the situation strengthens the demand of being overcautious in the examination, he explores five possible sources of empirical evidence that a layperson might have, in these circumstances, for justifiably deciding to trust one expert more than another:

1. The arguments put forward to support one's position and to critique the rivals' approach. Despite the epistemic imbalance, there may be publics that, for different reasons, are more engaged with the topic at hands and, thus, are better able to recognize that some

arguments are of a higher epistemic quality, or at least more plausible or sounder, than others.[15]

2. Agreement of additional experts on one side or the other of the issue—that means, to extend the chain of appeals to authority can be helpful to identify which proposition achieves a greater level of consensus among the scientific community.
3. The opinion of prominent peers', not involved in the controversy, about the reputation of each informant—basically similar in character to the previous source of evidence, in this case not regarding the content but the teller.
4. The evidence of experts' interests and/or potentially distorting biases underlying their statements. If it can be suspected that any of the informants has reasons to lie, manipulate or hide information, that gives grounds to reasonably choose the position nor affected in principle by those kinds of doubt.
5. The experts' past track records. For Goldman, a background of cognitive success regarding the topic at hands constitute the best source of evidence for credit allocation—although that, for laypeople, it entails the same limitations of the direct calibration mechanism proposed by Kitcher or the Collins and Evans' judgment by meta-experts.

The case of the "novice/2 experts" starkly underlines the vulnerability of laypeople's epistemic dependence. It's not just that people are precluded from assessing the differential epistemic value of each statement at stake, it is also that their difficulties in understanding some essential features of science—that knowledge is per se tentative, the interpretive flexibility of evidence, the fact that equally competent and honest experts may reasonably differ on their views about the same issue—add an additional factor of suspicion. In dealing with conflicting claims, a first reaction is likely to be that some of them are wrong or, worst, that some of the experts are trying to deceive because of a hidden agenda. In those conditions, it can be expected that laypeople maximize the scrutiny of the informants, making stronger efforts in achieving additional premises, beyond their words, to assess their credibility. On this basis, it is imperative to acknowledge that a sociologically based approach is the only way to grasp the local application of the rules governing the credit assessment process, always placed in specific situations of social life and observed or not according to the conditions that they impose. The analysis of the interactional context in which epistemic authority/ies is/are recognized, confronted or debunked, provides a rich point of convergence between social epistemology and the STS studies, which will certainly benefit our understanding of the social circulation and appropriation of scientific knowledge.

Concluding Remarks

Once the constraints imposed by the epistemic asymmetry are recognized, how might it be possible to share knowledge between scientists and publics? A great deal of discussion in PUS studies has centred around this question. In this chapter I have argued that the response lies in the idea that their cognitive exchanges must be considered a particular case of the broader type of social practices that allows knowledge to circulate among members of a socio-epistemic community on the grounds of the trust deposited in interlocutors.

Throughout this chapter it has been argued that the problem of laypeople's access to science can be fruitfully described in terms of the mechanisms that, in the course of social interactions, foster knowledge-sharing between participants. The fact that in this realm of encounters the agents are in radically different positions regarding the highly specialized knowledge at hand—concepts such as DNA, stem cells, the Higgs boson, greenhouse gases and the like—implies that some of them must defer their own cognitive competence to others. However, as we have seen, something similar occurs among those situated in the context of scientific research. A reasonable stance of trust in the words of recognized epistemic authorities—informants worthy of reliance—is the mechanism that allows the acquisition of knowledge from others during the course of a dialogue.

This process and its constraints have been deeply analysed from various perspectives in the inner circles of knowledge production, each of them emphasizing different—but not necessarily opposing—dimensions of the phenomenon relevant to their respective domains and interests. Both STS and social epistemology have made valuable contributions to disentangle the complex network of socio-epistemic interactions involved in the cognitive exchanges among scientists. The two approaches could very well cooperate, complementing each other's resources to better understand the peculiarities of those exchanges in the broader social context. In doing so, the field of PUS could gain a stronger theoretical insight into their main concerns, as well as a new assortment of exciting questions that brings a breath of fresh air into its research agenda.

Notes

1. The first milestone of this process is the 1957 survey on science perception held in the USA by Davis (1958) on behalf of the National Association of Scientific Writers, which outlined the dimensions that would later become the hard core of the quantitative tradition in the PUS field: levels of interest and information; information sources; understanding of scientific facts, methods and process; attitudes towards science and images of scientists. For their part, Dierkes and von Grote (2000) regard the work carried out by Jon Miller for the National Science Foundation series of Science Indicators since 1979 as the real starting point of the discipline. Almost a decade later, the cooperation between J. Miller's team in the USA and J. Durant's team in the UK led to the first big-scale comparative surveys implemented in both countries in 1988, providing a model rapidly adopted in several national studies—Canada, China, Japan, Korea, New Zealand, Spain—and cross-national research—the Special Eurobarometers Europeans, Science and Technology (Miller 1998).
2. Among others, the former encompass concepts such as "molecule", "atom", "DNA", "structure and dynamics of the solar system", "radioactivity" and "plate tectonics". Knowledge about the peculiarities of the scientific method is assessed through responses in surveys that include ideas such as "theory building", "test of hypothesis", "experimentation", "observation", "measurement", etc. The measurement model elaborated by Miller and Durant and its indicators had a decisive influence in the design of current scientific public perception surveys globally (Dierkes and von Grote 2000: 344).
3. See Bauer, Allum and Miller (2007) for a comprehensive account of the research in PUS developed under the core idea of the deficit model.
4. Amongst the most influential references, Shapin (1992: 29) includes the works of Barnes, Bloor, Collins, Latour, Law, MacKenzie, Pickering, Pinch and Star.
5. See Durrant (2008) for a sharp critique of these conceptual movements and Wynne's (2008) response in the same volume.
6. As Broncano puts it "a mere *modus vivendi*, in which both social groups and scientific communities tolerate each other" (2006: 223).
7. This conception is particularly noteworthy in the realm of public policies' popularization strategies (Cortassa and Polino 2015).
8. She is not able to judge whether a series of measures on the erratic oscillation of certain stars, along with the observation of the gravitational force the Moon has over the equatorial bulge of the planet, are evidence enough and necessary to accept that terrestrial poles move 9 seconds of arch every 18.6 years.

9. Although Hardwig postulates that these set of constraints almost force *laypeople* to cognitively defer on experts, Reyes-Galindo clearly shows that akin limitations are perceived among experts themselves. In the following excerpt, it is noticeable how a theoretical physicist refers to the epistemic exchange with his experimental colleagues in terms of the "faith" he must have in their words, precisely because of the restrictions posed by Hardwig: "At some point I have to take on faith what experimenters tell me. I know that there are important questions that need to be answered like the cosmological constant, dark matter, the spectrum of Cosmic Microwave Background radiation or fluctuations you can see, problems in fractional quantum Hall effect or high temperature superconductivity. *I've never done any of those experiments, and I don't understand most of the experiments, but you know, I have faith in these problems that need answering*" (Reyes-Galindo 2014: 739. Author's emphasis).
10. Thus considered, "trust is often epistemologically even more basic than empirical data or logical arguments: the data and the arguments are available only through trust" (Hardwig 1991: 694).
11. See Collins (2014) for a good account of empirical evidence in this regard.
12. I shall return to this in the following sections.
13. The outline presented here is basically focused in the arguments posed by Goldman (2006), Adler (2015) and Lackey (2006).
14. Among them there are both experts and non-experts, taking into account that public debates on scientific issues involve different types of social actors, institutions, lobby group and so forth.
15. As the editors of this volume have pointed out, this type of appraisal presents sensitive limitations, especially in strong controversial situations when not even the experts involved have at their disposal sufficiently "closed" arguments to warrant their putative positions and uncertainties prevail above all.

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CHAPTER 7

'The Year of the Gull': Demonisation of Wildlife, Pestilence and Science in the British Press

Lisa Carr and Luis Reyes-Galindo

Introduction: Science Journalism as an Informal Mediator Between Science and the Public

Science and Technology Studies (STS) has long considered the relationship between science and mass media mainly from the perspective of 'science journalism'—the representation of science and technology through mass media, or the communication of scientific and technological knowledge to diverse publics (Lewenstein 1995; Weigold 2001; Stuart 2009; Gregory and Miller 2000). This perspective on science journalism is also commonly echoed in non-STS concerns about science communication for science professionals (Peters 2013). In this chapter, we take a different turn on the science–journalism relationship by considering an episode

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where the inclusion of scientific knowledge or scientific expertise was *not* the primary intent of the journalist, despite science being of importance to the journalistic topic. We also remark that the lack of research into how science enters journalistic practice is not limited to STS. As Boyce (2006: 890) remarks regarding the massive decline of trust in experts seen in contemporary Western society—including a major shift in science/journalism relationships—'There is a paucity of research examining the role expert-sources play and their impact on audience understandings.'

The chapter will show how a stream of journalistic articles published in 2015 framed the existence of an alleged 'seagull problem' by relying on existing cultural imaginaries of wildlife-as-pests. Although the stories' understanding of seagull behaviour, pathology, population dynamics and particularly their scavenging habits—all key components in the negative portrayal of gulls—touched directly on ornithological science, they were mostly written without incorporating science into the editorial lines, and only later were 'gull scientists' assimilated into the stories—and then, in a very limited fashion. This was first brought to attention by Carr, who has a background in zoology, and will serve as an entry point for analysing how scientific expertise was largely ignored and occasionally misrepresented by the media's sensationalist stereotyping of seagulls, mainly by using discourses of pestilence and animal boundary breaching through the anthropomorphisation of seagull behaviour.

As in other episodes where the media played a role in disseminating stories using scandalous and headline-grabbing material while ignoring relevant scientific expertise (Reyes-Galindo 2016), we ponder the role and aims of journalism in which scientific expertise is relevant, but remains largely ignored. As the copious literature on wildlife representations in the media presented throughout the chapter shows, media representations of animals have played a definitive role in shaping not only public attitudes towards species seen as 'problematic' but also in directing public policies relating to animal 'pest' control. We will argue that cultural framing of human–animal relations in the media has had and will continue to have concrete effects on public attitudes towards wildlife. This is a prime motivation for gaining a better understanding of science–journalism relations in practice outside the traditional 'science journalism' sites, as well as for re-thinking the attitudes of science towards problems of public interest that may not be of immediate scientific interest but which have high socio-cultural relevance.

The Seagull in Popular Culture and Literature

According to the Royal Society for the Protection of Birds (RSPB), seven species of gull commonly occur around the British Isles.[1] However, two species in particular nest on rooftops in urban areas: the lesser black-backed gull *Larus fuscus* and the herring gull *Larus argentatus*. Traditionally found mainly around the coast, especially in the summer months, these two species are increasingly moving into urban areas with abundant anthropogenic food source to take advantage of warmer nesting sites away from potential predators. All species of gull are protected under the 1981 Wildlife and Countryside Act, with herring gulls included in the UK Birds of Conservation Concern red list following decades of substantial decline in breeding populations, whilst the lesser black-backed gull is amber listed and both *Larus fuscus* and *Larus argentatus* come under high levels of protection (Ross-Smith et al. 2014).

While the sight and sounds of seagulls are common in many British urban areas, cultural perceptions of these birds are generally negative. Seagull calls are not the most melodic of sounds and many a British pedestrian has been the sad receptor of a seagull's aerially descending faecal matter. Moreover, the scavenging habits of urban seagulls can cause major sanitation problems, but particularly when rubbish is improperly disposed of. The student neighbourhood of Cathays near Cardiff University is an unfortunate example of seagull impact in an area combining poor urban planning, deficient council services and bad citizenship. The pavements of Cathays are often littered with large amounts of decomposing organic trash, the result of seagull activity, as the birds display an uncanny flair for tearing through the plastic rubbish bags left out in the open streets, often for days on end (see Fig. 7.1). Both insufficient and inefficient council-provided bins, a bare-bones rubbish pickup service, along with local negligence, have led to Cathays being described as 'living in a slum', with seagull scavenging playing an obvious part.[2]

While other British species with urban presence, such as badgers and foxes, have been positively portrayed in popular culture (Stewart and Cole 2015; Cassidy and Mills 2012), seagulls have few advocates to represent them as a charismatic and attractive species. In film, literature and culture the gull is generally depicted as wily, grabbing and aggressive. The seagulls in the *Finding Nemo* animated film repeatedly screech, 'Mine!', while attempting to feed on the protagonist fish. Richard Bach's popular *Jonathan Livingstone Seagull*, a popular feel-good novel where a gull is

Fig. 7.1 The Cathays neighborhood in Cardiff. Credit: L. Reyes-Galindo

the central character, positions the seagull protagonist departing from the status quo of his aggressive flocks and its daily squabbles over food.[3]

Seagulls in the British Printed Media: The 2015 'Year of the Gull'

As pointed out above, we will concentrate on the cultural representation of seagulls through a media content analysis of seagull-related articles in the British printed press. Specifically, we focus on the unusually large number of seagull-related articles published in 2015, a period *The Guardian* referred to as 'the year of the gull', in which British newspapers gave overwhelmingly negative attention to subjects such as seagull 'antisocial behaviour', attacks on people and animals, noise disturbance, health scares, amongst others.[4]

The present work stems from a research project in media studies carried out though a Science and Technology Studies (STS) perspective, which aimed to probe two specific topics: the cultural framing of seagulls in the British press and the role of scientific knowledge in shaping these cultural representations (Carr 2016). To this end we follow a tradition in STS of 'boundary work' and the study of cultural classification, in which socio-cultural environments and the connections between these and technical discourse (or as here, their mismatch) is explored to understand how people, objects and indeed cultures come to be demarcated and classified (Gieryn 1983; Bowker and Star 2000). We also resource STS literature on the processes whereby using a combination of scientific, technical and political arguments individuals or groups come to be labelled as deviant (Reyes-Galindo 2016) and how knowledge claims that have technical bearing can/are/should be accepted or rejected for policy purposes based on their 'formative intentions' (Collins and Kusch 1998; Collins 2010; Collins et al. forthcoming). Rather than follow more rigid and orthodox methodologies in content analysis that focus on extracting 'hard' quantitative data from coding (Elo and Kyngäs 2008), we focused on carrying out a dialogue between sociological perspectives on wildlife representation and the empirical material. Our main aim was the extraction of *qualitative* latent content and the associated cultural categories from the empirical material (Hsieh and Shannon 2005).

Articles about gulls in the UK national media published during 2015 were sourced through the comprehensive *Nexis* index. An initial *Nexis* content-query for 'gulls' in national newspaper articles published between 1 January and 31 December 2015 yielded a sample of 1182 results, which were further filtered to exclude non-seagull topics (e.g. 'The Seagulls', Brighton & Hove Albion Football Club). Using a combination search of 'gulls' in the headline and 'seagulls' in the main text body yielded a more manageable 310 hits, which were then manually screened to further exclude non-seagull-related articles and either repeated or too-similar articles, ending with a final sample of 188 articles, which were manually verified to be representative of the total. Finally, two 'gull scientists' were selected for interviews, with time limitations not allowing for a larger sample. Unfortunately, we were unable to secure interviews with journalists, a possible research follow-up.

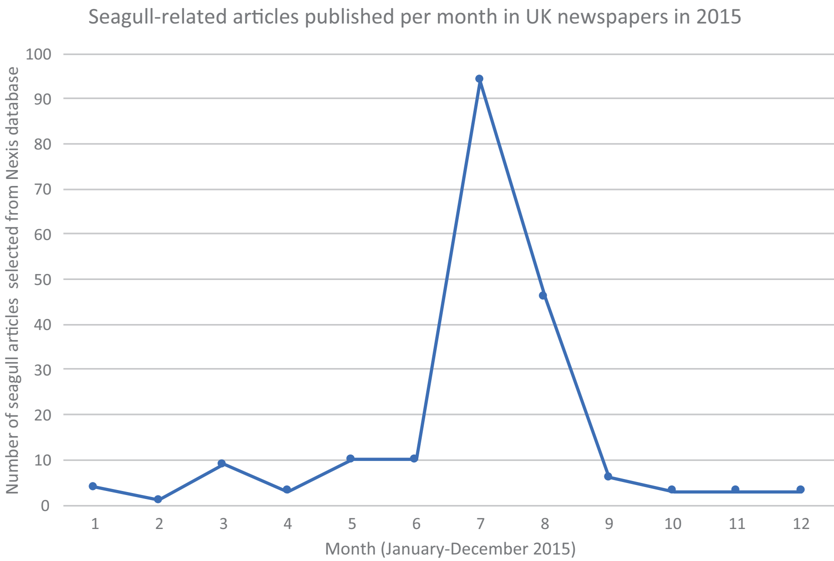

Fig. 7.2 A monthly histogram of sensationalist seagull articles shows a summer spike corresponding to the so-called silly season

Regarding the time-spread of the articles, an outstanding feature of the monthly publication histogram (see Fig. 7.2) was the large spike during the summer months, corresponding to what in communications jargon is referred to as the 'silly season', a period when the cessation of parliamentary activity tends to produce large amounts of trivial articles to fill the void left by a decrease in political news stories (Bowman 2006). Importantly, the same 'silly season' phenomenon was observed by Cassidy and Mills (2012) when analysing negative media discourses on foxes in the British press.[5]

Boundary Breaching and Media Reactions Against 'Transgressions'

Existing studies on animal representation in the media postulate that the major institution that both shapes and reflects attitudes towards wildlife and related management issues is the print media (Wolch et al. 1997; McCrow-Young et al. 2015), with newspapers playing a particularly significant role (Jerolmack 2008). Individual human attitudes towards animals are dependent on personal and contextual idiosyncrasies, but are also importantly shaped by cultural attitudes towards animal visual and behavioural characteristics, such as perceived attractiveness, intelligence, size, predatory nature, skin or fur texture, morphological structure, locomotion features, phylogenetic proximity to human beings, likelihood of inflicting property damage and cultural importance, and these are often synergised by the media and popular culture (Wolch et al. 1997; Daston and Mitman 2005; Cassidy 2012).

Actual physical distance from human to animal is an important parameter in the process of classifying animals as pests, as evidenced by a 'grey area' of pestilence discourse through which an animal residing in two different territories may or may not be considered 'dirty' based solely on proximity (Leach 1964). As we will show, though they are often grouped together as 'seagulls', species around the British Isles are categorised differently as either 'pests' or 'wildlife' depending on their typical contact with human populations. Herring gulls and lesser black-backed gulls are found most commonly residing in urban areas, on rooftops of residential and commercial buildings, so are considered to be nuisances and therefore pests.[6] Yet other species such as the kittiwake are found in coastal and rural colonies and are not widely considered pests and are less affected by proposed control measures and bad press.

A characteristic of media attention towards animals is that wildlife is most likely to make the news when boundaries between humans and untamed animals are breached, whether these be symbolic or physical (Corbett 1995). Knight (2000: 3) points out that people–wildlife conflict is especially marked in human settlements in forest-edge regions, the epitome of the physical boundary between wild and urban, so that '[p]eople-wildlife conflicts are relations of rivalry or antagonism between human beings and wild animals which typically arise from territorial proximity and involve reliance on the same resources or a threat to human well-being or safety'. Even in non-urban settings, proximity will imply conflict when wildlife is perceived to be a threat to resources desired by humans (Goedeke and Rikoon 2008).

Studies by Cassidy and Mills (2012) and Stewart and Cole (2015) on portrayals of urban fox attacks in the written media are particularly relevant to our study given the parallels between cultural contexts and the characteristics of human–seagull and human–fox interaction in contemporary Britain. While both foxes and gulls are animals familiar to most British urban populations, neither foxes nor seagulls are considered 'appropriate' co-residents of intimate human space. Foxes and gulls are not potential 'pets' that are culturally tolerated and indeed positively anthropomorphised within human habitats (Serpel 2005). In examining the media's response to fox attacks on nine-month twin girls in London on 5 June 2010, Cassidy and Mills (2012) showed how the media discourse was centred on the argument that foxes had *breached* and *transgressed* the appropriate societal boundaries drawn between 'human' and 'natural' worlds. The media *Reaktionsbildung* hung onto historically reified anthropomorphic stereotypes of foxes characteristic of European cultures, for as Marvin (2000) demonstrates, the problematic character and image of the fox in European culture developed over centuries in literature and more recently in TV and film: the fox as cowardly, cunning, selfish and murderous with a 'lack of empathy' (Daston and Mitman 2005, Introduction). After the attacks, foxes were portrayed as vermin or pests that ought to be eradicated, should the opportunity arise.

In a similar episode, the print media played a powerful role in framing social meaning around possums in New Zealand, demonising the possum by orchestrating an overwhelmingly negative representation and influencing cultural understanding and public attitudes towards the non-native species (McCrow-Young et al. 2015). 'Warmongering' was encouraged against the species, and documented by a revenge narrative that portrays possums as the perpetrators of native flora and fauna devastations. This conflict between human and possums as a war, justifying retribution, revenge and cruelty against an anthropomorphised 'foreign enemy' is not unusual. In Brazil, government campaigns against the Zika epidemic framed preventive efforts in a war rhetoric against *Aedes aegypti*, repeatedly using the motto, 'A mosquito cannot be stronger than an entire country'.[7]

From Displaced Wildlife to Dangerous Pest: Equivocations

The word *pest* has entirely negative connotations, most notably of plague and infestation. The *Oxford English Dictionary* (*OED*) defines pest as 'a thing which is destructive, noxious, or troublesome; the bane of something' and in later uses, 'an annoying person or thing; a nuisance'. Furthermore, it defines pests as an animal that 'attacks or infests'. Likewise, Knight (2000) locates 'pest' as being part of an extensive English-language vocabulary for inconvenient, bothersome or damage-causing wild animals. Scientific classifications of pests similarly hinge on this anthropocentric dimension, pests being species that negatively impact on human populations (Sorace 2002). The cultural image of a pest, as Lunney and Moon (2008: 56) explain, 'is a powerful one, and it reflects attitudes, danger to humans and economic loss.' As Gingrich and Osterberg (2003: 317–318) explain in a technical report on birds as pests,

> Because of [attractive characteristics], most people value birds in an emotional level … Defining the species of birds that are pests is also a sensitive issue. Generally, any organism that is *out of place* and that interferes with human health or commerce is considered a pest. No matter what species it is, any bird that enters a building is considered a pest.

The *OED* definitions importantly show that there is a tension between a pest as something perceived as *annoying* ('a nuisance'), and that which causes a general type of *harm* ('attack', 'infest', 'destructive', 'noxious') as in most technical definitions. In the seagull episode—as in many other cases studied in the literature—much of the sensationalist negative press surrounding 'transgressions' hinges on an equivocation of these two meanings, or a fallacy based on accidental lexical ambiguity (Powers 1995).

In this sense, Marvin (2000) demonstrates how in the media foxes first become 'pests' when they encroach on and visibly reside in urban spaces and transgress their 'proper' habitat, and how this leads to an increasingly violent portrayal of foxes as 'harmful' predators to justify fox-hunting and extermination. Closer to gulls, pigeons are an example of a bird species widely considered a pest, with feral pigeons in large cities and towns using buildings for nesting and roosting and foraging for food often referred to as 'rats with wings'—rats along with cockroaches being the epitome of urban pests (Birke 2003). Pigeons discharge large and unsightly droppings and cause structural damage to buildings by plugging drains. In the countryside, pigeons are considered to be agricultural pests when they feed on seeds and plants and roost in barns, yet major pest control policies rarely target urban pigeons as they are not considered a major danger to public health (Hoon Song 2000).

Jerolmack (2008) describes how, nevertheless, from the 1960s American public officials began 'spinning' public health concern links between pigeons and disease, even in the face of scientific expertise denying any such link particular to pigeons, as compared with other bird species. Through equivocation, an intrinsically 'dirty' species was therefore framed as not only unsightly but also immediately *dangerous* to human life. Also interesting is that, according to Jerolmack, these categorisations are historically contingent: pigeons only began to be stigmatised with the rise of contemporary urban desiderata of cleanliness and orderliness in the early twentieth century, while other bird species such as sparrows were the bane of public opinion at earlier times.

The portrayal of gulls as pests, vermin and then health risks often emerged across the news stories we examined. As we will illustrate in later sections, though seagulls are not considered by scientific experts to pose a threat to human health, gulls were constantly being referred to as 'pests' and then speculation upon their alleged harm was spun into the writing. The earliest article in 2015 referring to gulls as pests was in the *Daily Mirror* on 13 January. The newspaper refers to gulls as 'winged pests' in Dublin city centre, quoting a minister who said 'they're not seagulls they really are vermin'. On the one level, *pest-as-annoyance* was part of the overarching discourse throughout the articles examined. 'Pesky' was used to describe gulls in a 'seagull survival guide' in *The Mirror* on 5 March, yet 'political campaigns' referenced in certain articles calling to control populations cited 'damage' caused by gulls such as in Northern Irish newspaper *The People* on 22 March. *The Sun* also reported on this story on 13 January and stated that 'pest control experts' were being called in to clear the birds. In several articles, gulls are referred to as 'rats with wings' or 'flying rats' (*The Mirror* 21 August) or even 'grey ratty things' as described in a *The Sun* opinion column on 4 August titled 'I would love to cull my gulls'. On a different level, *pestilence-as-harmful* is insinuated through the use of other language throughout the articles. *The Express* twice used the term 'plague' or 'plagued' in reference to gulls such as on 20 and 21 August. Similarly, on 21 August the *Daily Telegraph* wrote about how a 'license to kill could end plague of seagulls'. Other terms and language used to define pests in Knight's (2000) definition of inconvenient, bothersome or damage-causing wild animals can be seen throughout the articles.

Pestilence as Boundary Breaching

When animals transgress the cultural boundaries of what is perceived as 'appropriate' living spaces, the relevance of cultural stereotyping in the definition of a 'pest' comes to the fore even more clearly. Species introduced to Australia and New Zealand, for example, have had largely damaging effects on local ecosystems and native species and thus could be considered, on scientific grounds, as pests (Dickman 1996). Yet some species that could be defined as pests under these parameters are not, because of their perceived cultural proximity to human spaces. Cats are a species that has been introduced to Australia and their presence has had deleterious impacts on native biodiversity in Australia, yet many are kept as companion animals and are 'naturally' not branded pests (Reddiex and Forsyth 2007). Rabbits in New Zealand and Australia have posed a problem to local habitats, such as grazing on vegetation that has changed the face of the landscape, yet their status as a pet species can prevent them from being including in pest control measures (Loague 1993). Hence, 'whilst some wild animals are celebrated because they are beautiful, rare or useful, many become interpreted as pests' (Jerolmack 2008: 73).

Anthropological and sociological perspectives on dirt and pollution shed further light on the cultural process of species' transition from 'wildlife' to 'pests' and the displaced–dangerous equivocation. Anthropological work by Mary Douglas on classification focused on how 'dirt' is a 'matter out of place' (Douglas 1966/2003: 35), arguing that ambivalence in classification is often repositioned as 'dirty' or 'polluted' within a definition of environmental order. In this way, there are anthropological precursors to how animals that are subjects of pestilence discourse—animals that considered 'vermin' and 'dirty'—are in parallel animals *dislocated* within human environmental/urban order, that is, animals breaching socially constructed boundaries of 'proper' living spaces for wildlife.

The topic of both cultural and spatial seagull boundary breaching was a prevalent one in the articles examined. Of the former example, on 8 February, *The Mirror* published an article regarding British 'seagulls' eating human food in urban areas, favouring human food over their 'natural' food source of fish. For the latter, articles referring to the dog attack in a family's back garden (*Mail Online* 15 July; *Mirror* 15 July) illustrated how gulls nesting on roofs are considered to be residing in human and family areas and are transgressing human habitual living space boundaries. Gulls residing in urban areas are frequently cited as out of place; *The Guardian* on 19 June noted how gulls have migrated inland to build their nests on rooftops rather than cliff tops, with many more articles referring to people being targeted in gardens *because* of these displacements.

Pests as Deviants

Non-classifiably linked to boundary breaching and how this is connected to deviance and punishment is an important topic in contemporary sociology and has also received attention within STS. Becker's (1966) seminal contribution was to note that the category of 'deviant' is a social categorisation isomorphic to that of 'outsider' within an established 'appropriate' socio-cultural space, while Foucault (1999) more specifically linked deviancy to unclassifiability and abnormality. Linking this topic to classification and order, Bowker and Star (2000: 232) argue that, within any classificatory scheme, those entities that are generally invisible to classification become a 'residual category', 'classified' as an unclassifiable 'other'. Reyes-Galindo (2016) has shown how scientists explicitly create outside categories from grey areas of classifiably and then portray the class of unclassifiable/deviant within a naturalised scheme of 'order'. STS studies have also shown how a rhetoric of deviancy is used to stigmatise scientific fields and 'unorthodox' scientific positioning (Collins and Pinch 1979; Collins et al. forthcoming).

A *pest* in this sense is the transgressor of a cultural boundary, an outsider that inhabits a grey area between 'natural' and 'human' spaces, and therefore potentially a deviant. As such, it is not surprising that seagulls should be awarded deviant human agency in media representations. In fact, this deviant anthropomorphising of gull activity was observed in the press articles describing seagull boundary breaching. An article from *The Sun* on 7 July 2015 tells the story of a gull 'invading' a cinema, quoting a cinema worker saying, 'It was just walking around as if it owns the place'. A *Mirror Online* article from 29 July tells the story of Eric, a herring gull, who 'barged' into a kitchen, invading the human home space. The *Daily Mail* reported that 'vicious gulls' were attacking actors at the studio recording popular TV show *Poldark*. Some gull attacks were even, incredibly, hinted as sexist in the *Daily Star* on 21 July, with the use of a quote from lifeguards in Brighton and Hove claiming that gulls target girls as they are 'more likely to drop their food and run'. This was continued in a *Daily Mirror* article on 29 July, which flags up other female issues on top of gull attacks: 'For goodness sake, just when you thought enduring unequal pay, unequal housework, childbirth, menstruation, menopause, cellulite, sleazy men, snagging tights and unwired bras was enough to content with … along comes a flippin' seagull.'

Two Discursive Forms of Seagull Deviance: The Monster and the Individual to Be Corrected

Foucault (1999) establishes two archetypical forms of deviance and how they link to the breaching of social norms which are of relevance to our analysis: the *monster* is a violator of natural, cosmological or social law, the violation being induced by its very existence and is thus incorrigible; and the *individual to be corrected* who is the wilful violator of family or clan law on a local scale, and corrigible. Although Foucault's work focused on human deviance there are two ways in which this classificatory scheme becomes relevant to wildlife demonisation. First, an animal can be said to 'act' viciously and have 'motives' in the same way a human can, thus a petty criminal, anthropomorphised as an individual to be corrected. As regards the figure of the monster, Foucault remarks how many classical monsters include creatures that have both human and the non-human features, but that violate a natural, human or cosmic law of the highest level (i.e. the minotaur).

The news articles mixed elements of both 'monster' and 'lawbreaker' figures and legitimised violence against seagulls based on these representations. The gull as 'antisocial' was a recurring topic in the articles. Although critical of the media response, the 'year of the gull' *Guardian* article's subheading refers to 'the debate over how to deal with the *antisocial* birds that are terrorising Cornwall and beyond'. A 2008 Jersey-based *BBC online* public poll asking if measures should be introduced to reduce the seagull population was entitled 'Anti-Social Seagulls'.[8] Many other articles used anthropomorphic anti-social cue adjectives such as 'boisterous', 'brazen, 'culprit', 'wily', 'crafty', 'jealous', 'rival', 'terrorising', 'rampaging', 'bold', 'rogue', 'public enemy', 'thieving', 'stealing', 'scourge', 'cocky'. More inclined towards the monstrous representation were 'assassin', 'slaughter', 'maniac', 'crazed', 'sadistic', 'evil', 'psychotic', 'cannibal', 'monster', 'nightmare', 'bird from hell', 'plague' (Carr 2016, Appendix 4).

One particular story of a family dog allegedly being attacked in a back garden around 15 July elicited a large number of articles on that day and in the following days. Particular attack events and the stories behind them cater to news values of oddity, and this event was out of the ordinary: relevance and personalisation, as when a family pet was impacted by the attack; and reference to family, the victims of the attack. Other stories catered to controversy regarding the attack, highlighting how the nesting birds from the attack are protected species; such as 'Seagulls pecked my dog to death' from the *Mail Online* on 15 July and *The Mirror*'s 'Seagulls nesting on roof peck Yorkshire Terrier to death—and family are powerless to remove them', on the same day. The subsequent high volume of stories around gulls from mid-July referred back to this story and are often in alignment with news values of *continuity*, *negativity* and *follow-up* (Lee 2009). Consequently, other pet attack events over the summer garnered a high volume of stories, as pets being targeted is a relatable reference to elite species, animals with a close relationship to humans. For example, the *Mail Online*'s story about a family pet's tortoise being pecked to death by gulls focused on the family as victims of the attack and further articles related back to these incidents. Probing user responses to the incident is telling of how sensationalist articles like these can elicit violent, visceral responses from the public:[9]

'Sandy': Sorry, I would take a hose to the nest i am damned if i would live in fear for my children and pets, [*sic*]
'Shelodon69': 'Protected' or not, any nest would be torched & I'd feed them plenty of baking soda.
'youpickone': Feed the gull a plastic fork prong in a hot chip
'teepee': Which Numpty decided that gulls should be protected? They are a dangerous nuisance and a health hazard with droppings everywhere.
'papa smurf': My dog caught a seagull last year on the beach in Cornwall. (Didn't kill it, just caught it) I was amazed that a load of people on the beach suddenly demanded that the dog be kept muzzled & leashed! I thought we all hated seagulls!
'Markoos': I live in a town besieged by these muck-hawks. My attitude is, they are only a protected species if someone in authority is watching.
'Andrea': It isn't true that nothing can be done because they are protected. Whilst wildlife generally is protected by law—under certain circumstances, wildlife can be destroyed or controlled, especially if a danger to humans.

War and Conflict

A rhetoric different from the deviant framing, glimpsed from article online comments and from article headlines, was the use of war metaphors. Descriptions of a direct conflict between humans and gulls appeared frequently in the news texts, of war between humans and 'aggressive gulls'. Stuart Winter in *Express Online* (25 July) sarcastically likened 'seagulls' to Hitler's Luftwaffe swooping into the UK, mocking the public fear of gulls and other articles that declare a so-called war on gulls, as towns with large populations of gulls were often referred to in the press as 'besieged' or 'under siege' (*The Telegraph* 29 June; *The Independent* 21 July; *The Times* 22 July). All these stories depict gulls as the aggressors in conflict.

The media portrayed cases where towns attempted to introduce control measures on gull populations as a declaration of war on the gulls, or a plan in an ongoing war. *The Sun* on 13 January referred to pest control measures being called in for a city in Dublin and says the health minister and senator are declaring a 'war on seagulls'. Similarly, the *Daily Mirror* on 6 March insinuated that a leaflet distributed in Aberdeenshire on how to live with gulls was a 'fledgling plan' in a seagull war. *The Independent* on 10 April discussed the gull problems facing Venice, Italy, and states that the city is declaring a war on gulls. Conversely, in a case of a gull being poisoned in Bridport, more defensive articles where the treatment of the killed gull is considered harsh were written, yet a war on gulls is still alluded to. *The Mirror* website on 19 July discussed how this poisoning of a bird is cruel and senseless, yet still depicts the story as a backlash from residents in a war on 'seagulls'. Similarly, on 20 July the *Daily Telegraph* and the *Daily Mirror* each referred to this same poison story as vindictive action in a 'war on gulls'.

Alternative Media Representations of Seagulls: Beyond the Silly Season

The key argument refuting the more sensationalist gull articles in the press came from a small number of opinion columns and contributed articles. The argument shaped by these articles points out that gulls are a reflection of human actions, their pestilent behaviour derived from human wastefulness and messiness. Patrick Barkham in a *Guardian* comment article on 20 July argued that humans are responsible for the rise of gulls in urban populations by their actions in removing their natural food source, providing alternative food sources in urban areas through litter and food waste and providing a suitable architecture for nesting in the cityscape. Similarly, Janice Turner in a *Times* editorial of 23 July powerfully stated that 'both foxes and gulls live off our greed, squalor and wastefulness. They are a mirror to our flaws.' An opinion column written in *The Sun* by Boris Johnson on 26 July shows an argument of retaliation and comeuppance after the pain humans have inflicted on animals, asking '[w]hat is a dive-bombing gull but a pitiful and ineffectual attempt by the animal kingdom to retaliate after centuries of casual slaughter and cruelty—on a scale we can barely imagine.'

Other references, negative but not all out sensationalist, echoed the positioning of birds as disease vectors described by Jerolmack (2008) in the pigeon case. In 2015, the Rugby World Cup was held in locations across Britain and rugby news garnered media attention throughout the year as fans arrived in the UK. On several occasions gulls were brought up in relation to rugby news, despite tenuous links. The first gull-related article of the year, on 2 January, appeared in the *Daily Star*, calling for a gull cull ahead of the World Cup matches in Ireland. The article talks of a health risk to fans visiting cities in Ireland for the matches due to a presence of gulls, blamed for the spread of diseases and posing a risk to fans. Later in the year, the South African Rugby Team, known as the Springboks, lost in a World Cup match, which was an unexpected event. Yet again turning to sensationalism, on 21 September the *Mail Onlin*e incredibly managed to factor gulls into the loss, saying how the defeat can be blamed on gulls being pests around the training grounds.

Seagulls, Science and the Press

As well as peer-reviewed journal articles, scientific opinion was reflected in only limited fashion, notably outside newspapers in publications such as *BBC Wildlife*, a magazine that writes for nature-inclined readers and people interested in wildlife and ecology, though glimpses of scientific conversation around gulls can be seen in mainstream media articles written by conservationists and those with scientific backgrounds, such as Martin Harper, the Royal Society for the Protection of Birds conservation director writing for *The Telegraph* on 24 July. Outside the printed media, platforms such as the British Trust for Ornithologists' website offer articles on gull science, as do their circulated news publications, which decried that 'it is easy to lose sight of the science amid these headlines' in regard to the seagull media frenzy.[10]

Of particular interest was an article from the October 2015 issue of *BBC Wildlife* magazine, written by the wildlife Features Editor and keen ornithologist Ben Hoare, which pondered on the general status of seagulls from the scientific side, partly framed as an informative reflection on the media scare of the previous months.[11] The British Wildlife special feature included a brief reflection on the British press's portrayal of gulls by scientific 'seagull experts'. Information was supplemented by Viola Ross-Smith, a scientist from the British Trust for Ornithology, Nina O'Hanlon, an Ornithologist at the University of Glasgow, Sam Hobson, a wildlife photographer who is an experienced observer of gull behaviours, Kees Camphuysen, an ornithologist who has conducted long-term studies in the Netherlands and Bristol-based urban gull researcher Peter Rock, offering a platform for scientists and other experts to offer knowledgeable insight on gulls.

Hoare interviewed wildlife experts with experience in seagull behaviour to demonstrate how 'the reality is far removed from the headlines and hype'. Sam Hobson expressed disdain for how the press tends to group gull species together under the term 'seagulls'—coining such practice 'ignorant'. Echoing the few critical newspaper opinion pieces, Viola Ross stated that 'they're just responding to the opportunities we create'. From this sampling of expert opinion, Hoare concluded that '[j]ournalists after a sensational story write wildly inaccurate articles about hordes of belligerent "seagulls" out to get us' and called out journalists for writing 'wildly inaccurate articles' about gull behaviour. Hoare also stated that, though the media has often published wildlife scare stories before as part of the silly season, 'this is now a more sinister practice as gulls are "demonised" through the use of words such as murderous, greedy, crazed, cannibalistic and psychotic'. The overarching opinion was that understanding urban seagull behaviour through science gave a completely different picture to the media scare, and that the 'seagull problem' was a combination of seagull–human urban interaction rather than an intrinsic problem with evil, antisocial seagulls.

As a follow-up to the documentary exploration, phone interviews and email exchanges were carried out by Carr (2016) with two of the BBC experts, Rock and Ross-Smith. They corroborated that, in their opinion, the press' portrayal of seagulls was wildly inaccurate, but that this was in fact entirely intentional, in order to follow what Rock referred to as 'a news agenda'. He described a time when a journalist came to a conference he was giving and quizzed him with a directed question, 'are the seagulls flying rats?', framed in order to obtain that particular quote. When the quote wasn't given, the story was given little space in the press. At other times, he explained, scientific knowledge can be used to lend legitimacy to a contentious or false point. When it was reported that an Irish MP claimed that gulls carried salmonella, Rock was able to offer more elucidation on some particular papers that claimed that few gulls were carriers of salmonella for a limited amount of time with little chance to pass it on to humans (*The Sun* 13 January). In fact, the highest carriage rates found in gulls was argued to come from gulls found near a sewage outpour (Fenlon 1983). As Peter Rock explained,

> What that means is the gulls weren't giving it to us … we were giving it to them. Moreover, he claimed that the argument that gulls carry pathogens is a product of pest control statements with little standing, unfounded inferences of 'look at that shit everywhere, it must be full of pathogens.'

Viola Ross-Smith similarly answered with a resolute 'no' when questioned if she felt the UK mainstream media accurately represents gull science and animal science in the UK, though she did differentiate between various types of broadsheet and tabloid publications and the value of their scientific writing.

> I think papers like *The Guardian* do a better job, unfortunately not necessarily *The Telegraph*.

Using Carr's experience as a zoologist, stemming from the documentary analysis carried out, and using the follow-up interviews to probe seagull experts directly, we concluded that the BBC article could be taken to represent the consensus view of the more specialised scientific community on the seagull problem: that gull boundary-breaching is simply a typical response from gulls that is no different from their normal actions in non-urbanised settings, or as the *BBC Wildlife* magazine discussion of opportunistic feeding in nature of both herring gulls and lesser black-backed gulls puts it, 'the birds may tackle anything from seeds and molluscs to mammals as large as rabbits'. Moreover, given the striking differences in discourse, we also concluded that this scientific consensus was completely misaligned with the journalistic descriptions of seagull behaviour on both factual and discursive terms.

As in the pigeon demonisation case, we stress that the issue has implications outside the immediate confines of journalism. The BBC article described, for example, how the perceived hype had led to policy intervention at the highest level, with then Prime Minister David Cameron declaring the need for an increase in conversation about the 'seagull problem'. Furthermore, in March 2015, a £250,000 research investment into the situation was announced, though cancelled at the last minute due to budget cuts. Moreover, the BBC article pointed out that not only were the media representations inaccurate, but that calls for culls to gull populations had not only been argued to be scientifically inefficient because of seagull breeding patterns, but that actually for certain gull species there was a significant *decrease* in British populations that could lead culls to endanger these populations.

Journalism and the Science of Gulls: Conflicting Formative Intentions

An entry point into analysing the misalignment between gull depictions in the press and scientific understanding of gull behaviour is through the concept of *formative intentions* (Collins and Kusch 1998), or the drivers of actions for members of particular cultural groups which define their 'form of life' (Wittgenstein 1953). Formative intentions in science have been most famously explored by Merton (1942 [1973]) in terms of the 'values' that drive scientific practice within the context of the 'scientific ethos', just as there exist commonly held perspectives on the existence of journalistic values (Weigold 2001; Lee 2009). This view of journalism and science as 'the clash between two distinct cultures' has been copiously echoed in the literature (Ashwell 2016: 380).

To focus on a specific case of value-laden cultural differences, we consider *continuity* as a key journalistic value that is clearly applied in the way gull attack made press, as they gained what scientists considered disproportionate amounts of media attention. Even small events related to gull attacks were likely to make the press sustain a story. The idea of continuity is evidenced in a title found in a 24 July comment in the *Daily Mail*, 'Now gulls attack a pensioner'. The title is indicative of a continuing story, the use of the word 'now' demonstrates how the story has almost picked up mid-sentence in an ongoing matter. We have also described how, for example, certain pet attack stories received continued media attention despite nothing innovative in subsequent articles, that numerous articles were re-writes of previous stories, and generally how repetition was markedly part of the seagull-reporting journalistic practice. Contrast this with the following description of how a senior editor for an important international scientific journal described his perspective on the submission of what he called, 'non-papers', or papers that while trivially true and in some ways 'new', were wholly uninteresting *as* scientific papers and would not even make it past the most preliminary stage of editorial vetting:

> Sometimes, people think that if something is a correct result and has never been published before, this is a good enough reason to publish. Take [the statement] 'three hundred and thirty seven multiplied by seven thousand four hundred and sixty two equals … whatever it does.' This is undoubtedly a correct result and I'm convinced it has never been published, but it's not worth publishing.

A thorough exploration of mismatches between scientific publishing values lies beyond the scope of this text, but the above is a precise example of a clear-cut cultural mismatch between science and journalism as it played out in the seagull attacks. Allan (2009) describes how editorial presuppositions of worthiness of a news story related to science deeply influences editorial decisions, while Weigold (2001) provides a thorough evaluation of journalism–science cultural mismatches such as fairness and balancing. While journalism values establishing a representational balance in controversial subjects and giving voice to dissenting voices, public opinion and 'maverick' science (Dearing 1995; Valenti 1999; Gregory and Miller 2000), scientific development hinges on the balance between 'reluctant revolutionaries' and consensus (Collins et al. forthcoming). There are also important differences in temporality, as journalism is subject to strict time limits in editorial practices (Valenti 1999) while the 'speed of science' tends to be much slower (Collins and Evans 2007). Moreover, while journalists seldom fixate on highly specific reporting fields, scientists are experts in extremely reduced domains of practice (Valenti 1999; Collins and Evans 2007). Other news values that do not reflect on scientific practice include thresholds of interest, meaningfulness, relevance and consonance, co-option/composition, frequency, unexpectedness, continuity, unambiguity, negativity and personalisation, though as other scholars have pointed out, values *will* differ according to the types of journalism being practised and often will be shaped by readership characteristics (Entwistle and Hancock-Beaulieu 1992).

On the other hand, there is also incompatibility flowing the other way as 'science news' may not necessarily match up to such journalistic values so will either get little time in the press or will be tailored to create a more newsworthy element (Gregory and Miller 2000). Working from the above differences, Peters (1995) and Peters et al. (2008) discuss how science and journalism construct knowledge about the world according to different principles, in which breakdowns in accuracy are just one part of the issue, but 'a systematic feature that the meanings of scientific messages change when they are reconstructed by journalism for the public sphere' (Peters et al. 2008: 269). As Weigold (2001) comments, scientists often do not understand the differences between the formative intentions of journalistic and scientific communication cultures, leading to inefficient strategies of linking with journalists and frustration with the values that drive journalism, and a public yearning for stories that do not fit the mould of what is scientifically relevant (West 1986; Boyce 2006).

Conclusions: Epistemic Deference and Science in Journalism

Even journalists reporting on science news are not necessarily scientifically literate, and yet, with little awareness of scientific-communicative normative intentions, they must pass judgement on what science is worthy of reporting. Moreover, journalist perceptions of the relative authority of informational sources to refer to for factual information and details on a story may be skewed, leading to misinformation and misrepresentation of science in the media (Allan 2009). In controversial science news, polarised groups, for example, can contest for representation in the news media in a struggle for legitimacy, posing a problem for journalists on who to refer to for information (Ladle et al. 2005). In general, while it might appear as a truism that one ought to give significant status to expert judgement, it can be difficult for a non-expert to discern between different types of expertise—the external criteria for discrimination between experts that does not depend on understanding of expertise and knowledge itself but an understanding of what expertise is, or what Collins and Evans (2007) refer to as 'meta expertise'.

As Cortassa (2017) explains, from a Public Understanding of Science (PUS) perspective it is tempting and common for misinformation and misalignment of scientific opinion to be framed as a matter of cognitive deficit of non-scientific 'laypersons' or 'publics'. This framing leads to the impasse that the solution is the 'education' of non-scientists in the content of science. Cortassa proposes that, rather than taking the epistemic imbalance as a problem, it should instead be considered as a starting *assumption* in the interactions between expert and non-experts. In tune with this view, in the seagull press, it *ought* to be considered good practice to sustain epistemic imbalance as a starting assumption when describing seagull behaviour, as opposed to specific instances of seagull–human clashes. A similar view is espoused by Boyce (2006) in a prescription which seeks to unify elements of traditional journalism practices alongside scientific hierarchising of expertise: journalist *must* mark the differences between expert and non-expert sources explicitly when 'balancing' a story.

Mass media publics will continue to consume tabloid news, but at no point should it be considered that these forms of journalism can or should lend any sort of *epistemic legitimacy* either to real public opinion or, even worse, to policy leads. At the same time, scientists should not be innocent about the role that the mass media will overwhelmingly play in regard to its choice of worthwhile news. Conveying the scientific viewpoint will then be a matter of entering a cultural landscape with different values and different social, cultural and political agendas. Moreover, this also means that scientists need to *understand* the cultural landscape pervading the topic to be dealt with (Irwin and Wynne 1996). In the seagull case, this would require a social understanding of the origins of animal and wildlife demonisation, pest discourse and how deviance is culturally anthropomorphised into animals, as we have explained here. While science typically ignores these dimensions in favour of simply communicating 'objective knowledge', the media, which relies on its resonance with cultural representations to kindle public favour, exploits these representations to their fullest potential.

We can glimpse the usual prejudices surrounding scientific 'communication' in the *BBC Wildlife* magazine and the science-led editorials in their attempt to 'cure' the deficit of mis-or dis-information through an old-fashioned 'deficit model' approach as criticised by Cortassa. A much more effective model of communication based on increasing mutual cultural understanding between scientific cultures and wider cultural imaginaries is a prerequisite for advancing effective science communication beyond its current dimensions. Indeed, important examples of successful science intervention in policy through a choice of scientifically atypical 'discursive choices' do exist in the literature (Slayton 2007), but these are isolated cases. For the seagull and other animal demonisation episodes, it is important for scientists, policymakers and of course journalists to understand and directly address—informedly, but without prejudice—public fears and attitudes regarding wildlife, boundary breaching, concerns about public health, amongst other intervening factors. While this does not *guarantee* more effective results in policy intervention, it would certainly be an improvement on the science communication side of the episode. We cannot think of a stronger position than this for science coupled with responsible journalism for communication to resonate effectively within society.

Notes

1. 'Seabirds', Royal Society for the Protection of Birds, accessed 4 December 2016. http://www.rspb.org.uk/birds-and-wildlife/bird-and-wildlife-guides/browse-bird-families/gulls.aspx
2. H. Waldram, 'Cathays rubbish blunder', *Guardian Online*, 22 June 2010, accessed 28 December 2016. https://www.theguardian.com/cardiff/2010/jun/22/cathays-rubbish-collection-cardiff
3. Poetry on the other hand has been rather more forgiving with seagulls. George Abbe's poem *Seagull City* describes 'the chalk-white sea-gulls eager for talking'. Perhaps most famously, the fourteenth-century Welsh poet Dafydd ap Gwilym described the seagull as:

   The snow-semblanced, moon-matcher,
   The sun-shard and sea gauntlet,
   Floating, the immaculate loveliness.

4. S. Morris, 'Killer seagulls top the pecking order for a media frenzy', 23 July 2015. *Guardian Online*, accessed 4 December 2016. https://www.theguardian.com/environment/2015/jul/23/killer-seagulls-top-the-pecking-order-for-a-media-frenzy
5. Our analysis is not naïve regarding the 'infotainment' nature of much of news articles in the seagull episode, a phenomenon linked to the unbridle move towards an Americanised, market-driven culture of news reporting that has been widely discussed in academic journalism studies (Thussu 2008). *The Mirror* and *The Sun* are considered paradigmatic examples of the rise of tabloid newspapers in Britain (Rooney 2000). Nevertheless, we have shown that historical studies show precedent exists of how, specifically in the case of animal demonisation via pest-discourse, this type of journalistic practice has been effectively mobilised to produce highly questionable public policy. Thus, even if the connection between journal prestige and writing quality has been explored before (Evans et al. 1990), here we concentrate on making explicit the cultural discrepancies between the pervading journalistic attitude towards a public problem and the science that can intervene in the debate and how these resonate with cultural attitudes.

6. 'Urban Gulls', The Royal Society for the Protection of Birds, accessed 4 December 2016. http://www.rspb.org.uk/get-involved/community-and-advice/garden-advice/unwantedvisitors/gulls/urbangulls.aspx
7. 'Um mosquito não é mais forte que um país inteiro!', News channel from the President's Office, 22 March 2016, accessed 26 December 2016. http://www4.planalto.gov.br/ipcd/noticias/um-mosquito-nao-e-mais-forte-que-um-pais-inteiro
8. http://www.bbc.co.uk/jersey/have_your_say/gull_cull.shtml
9. http://www.dailymail.co.uk/news/article-3162224/Seagulls-pecked-dog-death-Family-devastated-birds-swoop-Yorkshire-Terrier-playing-garden-inflict-fatal-head-injury.html#ixzz4UA2tUffE
10. https://www.bto.org/news-events/news/2015-12/review-year-2015
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CHAPTER 8

From *Trading Zones* to *Buffer Zones*: Art and Metaphor in the Communication of Psychiatric Genetics to Publics

Jamie Lewis and Julia M. Thomas

Introduction

'Scientific communication' is often conceptualised as scientists conveying scientific information to various publics or lay groups (Gregory and Miller 1998; Rowe and Frewer 2005; Holliman et al. 2009; Davies and Horst 2017). This is a notoriously difficult task to accomplish well (Bennett and Jennings 2011). Scientific specialisms consist of esoteric knowledge and vernaculars that are at some remove from everyday life and talk. When the science in question is psychiatric genetics, though, there are added 'communication' barriers to overcome such as the field's problematic socio and political history (Lewis and Bartlett 2015). Compared with other branches of genetics, for example, the specialism is much maligned; it has had to contend with past failures, false promises (Joseph 2006) and, historically, has had a difficult relationship with the public given its unshakeable connection to eugenics (Kerr and Shakespeare 2002). This backdrop is said to continue to hang over the field like Damocles' sword (Propping 2005), despite today's

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promising laboratory developments (Stoltenberg and Burmeister 2000; Burmeister et al. 2008).

Psychiatric genetics' public groups, including those with direct experience of mental health conditions, are multifaceted. Differences include their socio-demographics, their relationship to and experience of the psychiatric profession, and the way in which they receive, and offer in return, information about conditions that are contested, stigmatised, and potentially distressing. The reception of scientific information and their responses can therefore be unpredictable and divergent, which means that forms of communication and those who communicate them need to be creative and flexible. Science communication as a form of public engagement is not a passive, static activity. It is a dynamic, ever-changing, and ongoing process (Lewis et al. 2017). In this chapter, we use our public engagement arts initiative that ran from 2011 to 2015 as a case study to reflect on the ways in which scientific information about the genetic contribution of psychiatric conditions is relayed by scientists to various non-scientific groups. Linked to an internationally renowned laboratory researching the biological underpinnings of psychiatric conditions, which we call *The Centre*, our programme of events engaged a general, non-scientific public, people diagnosed with a mental illness, people with a particular interest in mental health, medical students, schoolchildren, and representatives from within artistic disciplines and communities.

Although art and science initiatives like ours have become more commonplace, psychiatric genetics might resist an alignment with the arts for fear of being perceived as less scientific by its peer disciplines. Also problematic is the desire by scientists to communicate scientific facts and to provide unambiguous and objective answers. This can be seen as going against the grain of an artistic approach more concerned with provoking questions and evoking multiple subjective interpretations and reactions (Costache 2012). It has also been put that "artworks inspire, illustrate and communicate knowledge, but they do not produce it" (Garneau 2008, p. 27). However, the arts can facilitate the production of knowledge through their strong connections with the discursive groups and communities that surround, make sense of, and apply science. Furthermore, the arts can encourage a questioning approach, exploring the ethical ramifications of developments in knowledge, as well as providing a social commentary on scientific practices.

In this respect, engagement through the arts can provide the necessary socio-cultural context for scientific endeavours, whilst also enabling public groups to be part of, and remain in, the conversation. We therefore reflect on the value of art as a method of participatory engagement with science, especially when the science is controversial and highly emotive, as is the case with psychiatric genetics.

Communicating Science: Disciplines and Trading Zones

Today, more than ever, science is a complex and corrugated map, partitioned into various territories and sub-territories (Gieryn 1999). Most commonly, these colonies are identified as disciplines (e.g. biology, sociology), although others talk of epistemic cultures (Knorr Cetina 1999), or communities of practice (Lave and Wenger 1991). These divisions are not a mirror on the nature they seek to explain; they are not natural kinds that can map exactly to phenomena in the world (Lewis et al. 2016), nor are they pure breeds (Galison 2010). Rather, their making takes considerable effort since over time they are landscaped, shaped, and accomplished by those inhabiting the space. In this regard, disciplines have both practical and symbolic functions.

Practically, disciplines promote specialisation, provide a home for particular forms of techniques and practices, nurture particular ways of thinking and seeing the world (see Fleck 1935 on thought collectives, or Hacking 1994 on styles of reasoning), and constitute the modern social order of academic knowledge (Weingart and Stehr 2000). Symbolically, disciplines help to characterise experts as cognisant, providing them with the epistemic capital to speak authoritatively over certain matters. But whilst, for example, medics have a stake in the understandings of the human brain, so too do biologists, psychologists, sociologists, and even artists. To this end, most agree that there is some form of hierarchy of esteem in academic scientific disciplines and, ever since a burgeoning science began to splinter and fragment, each specialism has gone through its own struggle for recognition (Rheinberger 2016). Physics has often been positioned at the peak of the academic pyramid, followed by the other hard sciences of chemistry and biology, and these are separated from the softer, human sciences such as psychology, geography, and sociology (Storer 1967; Cole 1983; Pinar et al. 2008), which are themselves considered to be kept apart from the more artistic and literary subjects. This is not to say the human sciences or artistic subjects are subordinate to the 'harder' life sciences, or that they are necessarily less authoritative on matters, but that their internal disciplinary integrity is not as strong (Holmwood 2010), their methods not as standardised, their shared practice languages not as uniform, and their coherence not as secured.[1]

Each of these scientific collectives also has its own specialised language, its own concepts and neologisms—often tacitly understood by its members—and rarely has too much in common with other specialisms (see Galison 1997; Collins 2011; Duarte 2013). Disciplinary borders therefore have very real effects for those who find themselves inside and outside the boundary, for those who walk on the verges, and for those who seek to find ways of travelling between the territorial lands. All told, whilst Rheinberger (2016, p. 173) claims that the "significance of rigidly fixed disciplines has waned", the map of science has created distinctive affinities that are much more than just surface-level differences in subject matter. Significantly, scientists are also socialised into the values of their disciplinary communities, the result of which means that the wandering scientist faces a considerable amount of reorientation, re-evaluation, and negotiation when she travels across disciplinary borders, making the task of doing interstitial work a formidable one (Lewis et al. 2016). This is what Galison and Stump (1996) refer to as the 'problem of disunity', and resolving the ways in which knowledge and those that produce it can travel between different fields of enquiry, despite deep-rooted linguistic (and cultural) differences, has been one of the main focuses of Science and Technology Studies (STS) (Galison 2010; Reyes-Galindo 2014).

Nonetheless, nowadays, working between and across disciplines is seen as a good in the academy (Strathern 2006). So, how do scientists from apparently incommensurable and unconnected epistemes work together? Galison (1997) has provided us with one possible conceptual toolkit to understand interdisciplinary and interlinguistic communication. He proposes the metaphor of *trading zones* to show how interdisciplinary communication and exchange is accomplished within science. The very idea of *trading* often supposes an underlying notion of capital (Galison 2010). Anthropology, though, where work on the practices of trade has been assiduously explored, has shown how there is no universal currency of exchange. Groups of people can trade even if they attribute different meanings to that which is circulated. And yet, as Marcel Mauss ([1969, p. 31] 1925) wrote in the early twentieth century, "objects are never completely separated from the men [sic] who exchange them; the communion and alliance they establish are well-nigh indissoluble". For Galison (1997, p. 783), who uses physics as an example, a similar, and yet different, arrangement is true in science. He points to the way that "two groups can agree on rules of exchange even if they ascribe utterly different significance to the objects being exchanged; they may even disagree on the meaning of the exchange process itself. Nonetheless, the trading partners can hammer out a local coordination, despite vast global differences". It is therefore in these metaphorical *trading zones*, according to Galison, where objects are exchanged, ideas are shared, interdisciplinary work instigated and interstitial languages formed. "What is exchange work today", Galison (2010, p. 33) continues, "may well become the disciplinary pillars of tomorrow: science is forever in flux, not just in its results but [also] in the contours of its disciplines".

However, science communication is not solely a matter of scientist-to-scientist interaction and exchange. Historically, one of the more impassable borders in the communication of science is between science and the non-scientific public who are faced with a torrent of technical scientific terms. Indeed, the languages of science are said to be incongruous with normal, everyday speak. Despite this, scientists are expected to communicate, and to engage with the general public. It is, as Gregory and Miller (1998, p. 1) state, as if "scientists have been delivered a new commandment from on high: thou shall communicate" the work of science to the masses.[2] Of course, when we consider science communication, we talk of science multiple as we speak of publics plural. When scientists—the travelling disciples spreading the scientific word—communicate their work to the public they are presenting much more than just their own views and values, and yet they rarely talk for the whole of science. They represent, instead, the view of their particular disciplinary specialism or collective (see Horst 2013).[3]

In what follows, we consider the particular scientific specialism of psychiatric genetics. As an intellectual field, psychiatric genetics is rather promiscuous, appropriating ideas from both psychiatry and genetics, as well as other academic domains, in order to try to identify the genetic mechanisms underlying susceptibility to common psychiatric conditions such as schizophrenia and bipolar disorder.[4] We report on a five-year public engagement programme that emerged out of *The Centre* and use it to reflect on the ways in which psychiatric genetics engages and encounters different public groups within the topic of current developments in mental health (and genetics). Specifically, we examine several public engagement arts events and activities that endeavoured to provide both physical and metaphorical space for various publics and experts to come together in conversation about the mind and mental health. Such roaming between disciplines like art and science can instigate an unsettling, a disturbance that can encourage each disciplinary specialism to look with new eyes at their own familiar, and possibly taken for granted perspective.

Drawing on Galison's *trading zones* concept for describing interdisciplinary communication, we propose the concept of the *buffer zone*: an adaptive space resulting in behavioural dispositions that enable conversations about mental health and genetics between experts from various disciplinary hinterlands and publics to begin and continue. The buffer zone affords members from each collective the freedom to broach issues openly; it helps flatten hierarchy, resists jargon, and protects the discussion of potentially sensitive and threatening topic areas from being aborted early because of conversational conflict. Developing artistic public engagement opportunities within this buffer zone framework nurtures the ideal of an uninhibited platform for encounters and conversations between people with different perspectives on mental health. This encourages an inclusive, dialogical, but questioning engagement with developments in psychiatric genetics. Important to STS is to state that to buffer is not to de-value expertise (Collins and Evans 2007), but is to recognise the ways in which artistic works can illuminate alternative matters of concern (see Holmberg and Ideland 2016), and allow these matters to be raised and diversify but, most importantly, to be conserved.

The Art of Science; The Science of Art

Gregory and Miller (1998) remark that twentieth-century champions of science complained that science in popular culture was an underling to other intellectual practices such as literature and art. The culture and education of science, and its initial post-war neglect of the public, had alienated and distanced many people from the practice (Wynne 1992a). This, they claim, was aggravated, in part, because the custodians of contemporary culture had been trained in subjects other than science. They cite Charles Percy Snow's 1959 Rede lecture as a defining moment. Snow condemned the UK's education system for overvaluing literary and artistic skills at the expense of scientific pursuits. Snow's (1959) now (in)famous 'two cultures' watchword described how he believed fences had been built separating and distancing the two terrains from one another to the point that they had almost nothing in common (Collins 2014). Few travellers crossed over to the other territory since the two cultures were considered impervious to one another. Snow was not the first (nor the last) to describe the dichotomous relationship between artistic endeavours and scientific pursuits, but his public profile brought the debate to public prominence (Collini 1993).

In *Two Cultures: And a Second Look*, Snow (1963) took a more optimistic tone, suggesting that a 'third culture' *would* emerge that would bridge the gap between scientists and literary scholars. This space—or metaphorical trading zone—consisted of so-named social historians, such as sociologists, economists, and historians who were on speaking terms with both scientists and artists (see Shaffer 1998). These boundary crawlers could shuttle between the cultures, instigating conversations, trading ideas, and initiating dialogue. For the most part, though, Snow's positioning of the sciences and the arts as two poles has been criticised to the point that this dichotomous relationship has now become somewhat of a trope.[5] Hall (1999) discusses how framing knowledge production in binary terms, such as between science and art, can simply serve to entrap us. This entrapment undermines the need to focus on a more general and powerful willingness to question any claims to knowledge irrespective of their disciplinary roots.

Notwithstanding the criticism of the 'two cultures' allegory, much work is still required if one wishes to bridge scientific and artistic worlds. Some of the tensions between art and science arise from differences in, what Collins and Evans (2007) describe as, their formative intention in relation to interpretative ambiguity: unlike many contemporary artists, scientists do not explicitly intend to produce ambiguous communications from their discipline. Indeed, many scientists advocate scientific clarity and regard science as a route to some underlying truth through a systematic, analytical approach (Popper 1994).[6] This scientific attitude often extends into how science should be communicated. Societal scientific literacy can be achieved, so the deficit model tells us, if leading scientists recite top-down accounts about truths, facts, and statistics clearly and authoritatively to an uneducated and ignorant public (The Royal Society 1985, see also Wilsdon and Willis (2004) for criticisms of Public Understanding of Science (PUS)' failings on genetically modified crops and bovine spongiform encephalopathy (BSE)).[7] Artists, on the other hand, are not necessarily interested in truth and rational objectivity. They are concerned with exploring and expressing the various ways in which we experience the world, interrupting, reframing, and creatively seeking alternatives (see Baker and Gigliotti 2006), with many artists actively encouraging multiple interpretations of their work (Weintraub 2003). Ede (2005, p. 42) synthesises this difference between art and science:

> Compared with the cool rationalism of science with its material belief in wholeness, the theories employed by thinkers in the arts and humanities seem part of a playful circular game in which the truth is never to be privileged in one direction or another and is always out of reach.

There should be no getting away from the fact that a scientific attitude and an artistic imagination, with its playful, often metaphorical and abstract ways of thinking and communicating, can be very different. This does not mean, however, that they are completely incongruous. As Bright (2000, p. 140) contends within the context of contemporary art practice: "the quest for simplicity on the part of science and the delight in complexity on the part of art are incompatible although each side can learn from the other". Indeed, art may be more successful at engaging with science because of the *expectation* for art to be playful, challenging, and subversive in a way that opens up opportunities for discussion (Calvert and Schyfter 2016). Wilson (2010) even argues that art and science can no longer survive in isolation from one another, either in terms of public support or in the production of knowledge. Whilst art, like science, takes many forms and has many processes, fundamentally it is a way of thinking and making connections, a way of communicating, and a way of challenging. When freedom of expression is used as a political tool, art and artists have "come to occupy a privileged and enduring place in society [...] mobilising ideas and people to support or usurp powerful actors and systems" (Phillips

2008, p. 75). As practice, art—again, in a similar vein to science—can offer answers and pose new questions about how we experience and understand both the world around us and ourselves.

For evidence of their mutual interests, one needs only to examine the emergence of collaborative art and science initiatives since the 1990s as common ground whereby the two cultures *have* come (and worked) together.[8] Creative forms of public engagement with science operate within what Kinchy and Kleinman (2003) describe as the tension between the routine, conforming, and deep-rooted disciplinary desire for scientific purity and the pressure to demonstrate social relevance and utility. Science—the dialogical approach to communication tells us—is not done in a vacuum, it is part of society and scientists need to be open to and respond to the multiple perspectives and various ways in which developments in science are interpreted, accepted, and appropriated (see Irwin and Wynne 1996; Miller 2001; Horlick-Jones et al. 2007). Simply put, publics are not empty vessels ripe for filling. They already have existing beliefs, attitudes, alliances, and ways of knowing. The colonisation therefore of this different type of 'third culture' by artists working *with* scientists to engage with scientific ideas and interests, once seen to be the domain of *just* scientists, is now commonplace (see Webster 2005). Through opening up the scientific process to artists, there is the opportunity to re-frame, reflect, re-imagine, and re-purpose science. This creative wandering can also capture a perspective, or way of thinking, that has been forgotten, overlooked or pushed aside by science: a viewpoint other than that which surfaces solely from the laboratory.

Translating Psychiatric Genetics: From Bench to Brain

Over the past 25 years, UK science communication initiatives and public engagement with science and technology (PEST) programmes have profited from the close relationship that art and artists have with different publics and communities. Formal support and funding, as well as more grass-roots encounters persist throughout the UK. For example, the Wellcome Trust, a major British biosciences funding body, continues to fund such collaborative endeavours, while 2017 sees the opening of Science Gallery London, part of a global initiative that promises to connect the arts, science, and health to "inspire the next generation of creative thinkers" (Science Gallery 2016). This alliance, though, is more problematic when the science in question is psychiatric genetics. While there is a long trail of work in the area of art and mental health, this often comes under the guise of art therapy or the promotion of health and well-being through creativity (Schmid 2005). There is much less work fostering an open dialogue about aspects of the diagnosis, causation, and treatment of psychiatric conditions with public groups. Expressed frankly, psychiatric genetics has had a troubled relationship with the public (Smith 2008). Furthermore, the contested nature of psychiatry (Foucault 1986; Conrad and Barker 2010; Hacking 2000), the historical controversies within genetics (Kerr and Shakespeare 2002), and bad media experiences (Dreaper 2010) have added to scientific researchers' concerns that involvement with the arts might consolidate and perpetuate the perception that psychiatric genetics is a less scientific area of medicine. Nonetheless, psychiatric genetics research critically depends on an increased awareness and support of its research in order to attract funding and to raise its status as a discipline. Publics such as patients and research study controls are also resources necessary to do big population studies such as psychiatric genetics research (Lewis and Bartlett 2015).

It is important to stress here that the history and politics of the specialisms of psychiatry and genetics have produced a very different landscape of opportunities and risk in public engagement imagined by psychiatric geneticists than that anticipated by, say, theoretical physicists. Engaging publics with the topic of mental health clearly involves a great deal more than simply communicating concepts clearly and effectively. Like much work on science communication proposes, it is also about framing, trust, and epistemic hierarchy (Jasanoff 2003; Nisbet 2009). Of particular concern to psychiatric genetics is that public engagement with mental health requires an astute awareness of the complex power dynamics between experts and publics because of the inherent emotional resonance and contested nature of the subject matter. Advice arising from the public engagement of other emotive, and sometimes hostile, socio-scientific issues related to human behaviours, such as climate change, is to recommend that scientists do not avoid engaging with the public but that they should be aware that regaling facts is not enough, and that deeper human traits and feelings contribute to the subjective lens through which information is processed (Revkin 2011; Roeser 2012). Other social attitudes relevant to the communication of mental health research includes aspects often captured by the term stigma and, against this background, psychiatric geneticists feel that public engagement is a way to tackle not only stigma attached to mental illness, but also stigma attached to biological psychiatry itself (Lewis and Bartlett 2015). These issues of expertise, trust, power, diagnostic contestation, and stigma contribute to the tensions that can arise during the communication of psychiatric genetics research.

In order to explore the ways in which art can buffer potential clashes between psychiatric geneticists and publics, we endeavoured to engage with a wide spectrum of public groups. Embarking on our public engagement arts project, we were originally told not to 'scare the horses' and 'not in front of the children' by psychiatric geneticists at *The Centre* acutely aware of these complex dynamics and the ways in which their field is perceived. In 2011, despite some dissenting voices, we began to collaborate as social scientist and artist. Lines of enquiry within the artworks included laboratory practice, metaphors of genetics, gene-environment interaction, the collaborative nature of large-scale genomic research, and the hopes and expectations of psychiatric genetics research. Some of the artworks, for instance, centred on aspects of big biology (Bartlett 2008; Hilgartner 2013), which in the case of psychiatric genetics, is characterised by the necessarily large-scale collaborative patient studies that examine DNA from blood samples in relation to information provided in patient surveys. The large-scale painting *Big Science I*, the art installation *Big Science II* and interactive digital artwork *Disturbing the Blueprint* (see Image 8.1) all incorporated elements that invited the public to participate, interact, and contribute either through the making of small components of the artwork or through direct bodily interaction, for example, by making a sound into a microphone that disturbed an edited image of themselves. A playful and seemingly trivial but surprisingly successful participatory act was the making and contribution of small blood red wire figures, used as the starting point for the painting *Big Science I* and incorporated into the installation *Big Science II*. Over 200 people contributed with figures and this participation stimulated conversations about the scientific content of the artworks.

Although these were primarily object-based artworks, they fulfilled the function of what Kester (2004) refers to as conversation pieces. Whilst one perspective is that these artworks served as icebreakers,[9] encouraging participation and discussion between the artistic work, the viewer and the subject matter, another perspective from socially engaged art practice is that the conversation and social engagement itself becomes

Image 8.1 Artworks of *Big Science I*, *Big Science II* and *Disturbing the Blueprint*

the work of art (Helguera 2011). Careful consideration of the level of public participation is important to enable meaningful experiences and communication between individuals, and also to encourage publics to contribute and not to simply stand back as passive recipients. As Thomas (2012, p. 19) states: "recognising the importance of the public voice is vital for the public engagement of science but creating the right space for the public to feel they can contribute to the conversation can be a challenge". The motivation for this trialogue (see Anderson et al. 2010[10])—the expert, the public, and the artwork—arises from observing that people are much more likely to instigate a conversation or participate in a discussion whilst in the role of creative participant, actively doing, rather than passively observing.

Other artists were invited to contribute to the growing conversation and the work took on a curatorial aspect, seeking out new perspectives and connections but always with the focus on public dialogue. We developed and organised two public engagement exhibitions called *Translation: From Bench to Brain* in 2011 and *How the Light Gets In* in 2013 that moved developments in psychiatric genetics and mental health out of the laboratory and into the public arena. These two exhibitions alone attracted around 1000 attendees. Academic speakers at public talks held within the exhibition spaces were impressed at how engaged and eager those that attended were to ask questions. Combining these well-attended academic presentations with smaller discussion sessions, creative workshops, music events, and poetry sittings, the gallery drew in particular publics who, we noticed, were comfortable with traditional art gallery spaces. Experimenting with different places and spaces of dialogue to attract new publics, the work expanded into empty shops in the centre of town, domestic spaces, a church, online interactions, and a yearlong series of art residencies, exhibitions, and events in a large disused attic of a mental health charity.

Throughout these encounters between various lay public groups, scientists, medical students, social scientists and artists, we constantly sought new, creative and pan-disciplinary ways to both communicate and connect the varied understandings of the mind. Art and its use of metaphor, we maintain, provide the opportunity to re-imagine and interrupt the boundaries and relationships between different (disciplinary) cultures and sources of knowledge and to break away from the familiar and the rehearsed. We now turn to a discussion on metaphor, before discussing our concept of the buffer zone.

The Role and Rule of Metaphor in Science, Art and Communication

The word 'metaphor' originates from the Greek word *metaphora*, meaning 'a transfer' (OED 1989). Classically, it has been widespread in the artistic specialisms, especially within writing and poetry, the interchangeable play on words regarded as a form of ornamental language removed from the everyday. An extension to understanding metaphor as a transfer is that of a puzzling but calculated borrowing. In this sense, two seemingly different concepts interact such that one disturbs the other and it is this disruption that results in the generation of new perceptions, knowledge, and meaning (Black 1962; Ricoeur 2008). Of relevance to this idea of disruption is the work of critical thinker Serres (1982), regarded as a traveller between the arts and the sciences. His thoughts on science communication are particularly interesting in this respect since he described three elements: (1) a message, (2) a channel for transmitting the message, and (3) the noise or interference that accompanies the transmission. The noise may make the reading of the message more difficult but the metaphor, regarded as noise, creates a tension that "calls for decipherment [...] that opens up such a fertile avenue of reflection" (Lechte 2008, p. 348).

Metaphor is therefore a borrowing, a deviation, a transposition, and a perplexity (Ricoeur 2008), but this is no longer considered to be merely linguistic decoration. Rather, it is a ubiquitous way of thinking and reasoning, a cognitive device for our creation of meaning and understanding (Lakoff 1993; Lakoff and Johnson 2003). By a process of surprise and disruption, it allows us to transcend literal thinking and begin to generate meaning through questioning the connections that have been made in the process of developing the metaphor. This questioning then instigates a shift within the process of how we understand concepts by relating them to our own experiences (Lakoff and Johnson 2003).

Science, of course, is not averse to using metaphor both within its ways of working and in how it is communicated.[11] Brown (2003, p. 2), for example, has argued that, "much of what scientists do [...] is governed by metaphorical reasoning". He maintains that metaphor links the language of science to the underlying reality it strives to achieve. According to Ahmad (2006, p. 198): "scientists literally and metaphorically create a world of make-believe through a web of words—some borrowed, some invented, endorsing self-belief here and suppressing the beliefs of others there". Metaphors delivered to a wider public often do this by drawing on objects that are familiar in society (Hellsten 2008). An often-used example of the use of metaphors to make sense of and to communicate complex science is evident in the field of genetics (Keller 1995; Kay 2000; Wolfe 2001; Condit 2009; O'Riordan 2010) where phrases such as the genetic *code*, the gene *machine*, gene *mapping*, *blueprints*, and the *book of life* are frequently used. Similarly, metaphors can be used to communicate genetic risk and susceptibility, as described to us by a leading psychiatric geneticist working at *The Centre*.

> So, you know, you may have a number of risk factors, and then if you develop a certain amount of risk, you will tip the scales. So, you would say you are trying to get this idea of balance and an accumulation of risk actually getting you into the diseased state. I would use visual clues to help me do that, but most people understand it.
>
> [Interview with Professor of Psychiatric Genetics]

Linking to everyday societal objects such as weighing scales enables metaphors to be more persuasive when scientists communicate their ideas of genetic susceptibility to the public, bridging scientific and popular discourse. Likewise, the artist, through the use of metaphor, of making connections and seeing the similar in the dissimilar to suggest and evoke rather than to state facts, may help publics find a way into discussing the science by inviting, rather than eradicating, uncertainty. Thomas's interactive computer artwork *Disturbing the Blueprint* (see Image 8.2), on show in the exhibitions, was a commentary on the use of metaphors within the history of genetics, and how those metaphors have been used to bridge temporary gaps in knowledge. Likening computer-programming code to the metaphor of the genetic 'code', the work also highlighted how the language and discourse of genetics was influenced in the 1950s by the growth in computing and information theory. Visitors to the art-spaces were invited to have their photograph taken to add to the growing collection of images within *Disturbing the Blueprint*. We explained how computer code enabled noise from the surrounding environment, picked up by the microphone, to disrupt and discolour blue and white versions of the contributed images. Following this, visitors were invited to disturb their own image and those of others. This participatory process initiated a conversation about the historical developments in the originally proposed concept of a genetic blueprint and how our understanding of this is constantly evolving.

Image 8.2 Example of interaction with *Disturbing the Blueprint*

Big Science I and *Big Science II* (see Image 8.1) invited various publics to consider the unique contributions of individuals to genetic studies of psychiatric conditions and the collective efforts required to accomplish big population studies. These and other related artworks by Thomas evoked parallels between changes in creative media and developments in scientific technology that enabled reflections on the relationship between science and publics, and the timescale over which the generation of scientific knowledge takes place. By working with and experiencing these different media, public participants began to question, develop, and contribute meaning about the broader scientific references made within the artworks. In this participatory format, visual culture, and art and metaphor, can provide a framework that encourages different groups to feel like they can contribute to the discourse of science, specifically because of the ambiguous nature of metaphors compared with standard forms of communication. Contemporary art, for example, often invokes a purposeful elusiveness, employing tactics such as symbolism and metaphor (Hausman 1989; Collins and Evans 2007) in order to invite a response. Science, on the other hand, is said to resist ambiguity and utilise very technical terminology when transferring and translating knowledge.

From reflecting upon this case study, we suggest that propositions about future scenarios, ethical concerns, queries that go beyond current knowledge and questions aiming to reveal what might currently be concealed are given leverage by the ambiguity of metaphor. Ambiguity, although often the adversary to scientific clarity, provides an opportunity for far-reaching questions and statements to not be considered out of place. However, enabling space, both physical and communicative, for boundaries to be transgressed can be a tempestuous negotiation that we argue art and metaphor has the potential to buffer.

Buffer Zones Between Experts and Publics

Galison's *trading zone* concept is a metaphor taken from the economic transaction of goods between people from different cultures and applied to the sciences to describe a space where interdisciplinary research is instigated. Like a marketplace, a trading zone is a place where merchants from various hinterlands, speaking different languages, come together to form alliances, hammer out deals, and exchange goods.

A *buffer zone*, on the other hand, is an area of land that lies between two (or more) hostile regions. They are often neutral zones, sometimes designed for environmental purposes, and they help to mitigate conflict between regions by keeping them apart or by uniting them. The word *buffer* also has several connotations. According to the *Oxford English Dictionary* (OED 1989), to buffer has at least three actions specific to (1) everyday use, (2) to chemistry and (3) to computing:

verb: *buffer*

- to lessen or moderate the impact of something or form a barrier between incompatible or antagonistic people or things;
- to treat with a solution which resists changes in pH when acid or alkali is added to it;
- to store (data) in a temporary memory area while it is being processed or transferred (OED 1989).

In modern talk, 'to buffer' is used in both computing and railway parlance. It often refers to the display of pre-loaded content to alleviate an interrupted video streaming on the web, allowing the user to continue viewing. It is also used in reference to the buffer-and-chain coupling system on the railway networks. Attached to the end of carriages with shock absorbing pads, trains and wagons are brought safely into contact with one another via this arrangement. Finally, in the same workplace, the term describes a barrier, preventing trains running off track.

Each of these meanings can easily be used to describe the various ways in which artworks have agency within the public engagement of psychiatric genetics. Conversations between psychiatric geneticists and publics can travel in non-linear and divergent directions, moving from topic to topic, from matter of concern to matter of concern, attempting to escape, running out of steam and sometimes de-railing. This requires some repair work, but too much meddling and managing and those that attend may wish to use an alternative platform to express their views or, worse still, retreat back to their original stations. Here, when we talk about the buffer zone in relation to our art initiatives, we are not talking solely about the physical space that the artworks inhabit but also the less tangible space in which conversation turns to matters of concern, and the point at which the conversation begins to break down. Figures 8.1, 8.2, 8.3, 8.4, and 8.5 show how we have developed the concept of the buffer zone beginning with head-on forms of science communication through to the various features of public engagement within either a typical art-space such as a gallery or less mainstream spaces in which artworks take place, both designated by the term (art)space.

Public engagement with science is “an often messy and contradictory business where dilemmas and paradoxes abound” (Irwin 2014, p. 74). For example, publics can come into conflict with scientific experts on issues such as agricultural biotechnology, nanotechnology, or fracking. Issues of authority and power, as well as differences in outlooks, expertise, and life experiences can lead to clashes between different groups and collectives during these more head-on forms of science communication and interactions.[12] Figure 8.1 represents a scenario typical of many public ‘dialogue’ events whereby the scientific experts are positioned and privileged in a way that can create a sense of opposition with those attending the event, such as in a panel talk or a formal presentation.

Interactional and behavioural norms of when and how a public ‘audience’ is allowed to contribute means that agency is privileged to the invited speakers and conflict emerges from the attendees’ struggle to be heard (Davies 2011). Occasionally, this manifests itself as the ignoring of the ceremonial order of turn-taking and other public event etiquettes, signifying a rejection of this kind of format and its inherent power structures. Therefore, apart from clashes arising because of the subject matter of psychiatric genetics, there are generic factors related to the format of an event that can induce skirmishes.

Key to Figs. 8.1 to 8.5

People of various socio-demographic characteristics

Scientists from various disciplines

Artist, curator or facilitator

Boundary of a tangible visible nature, e.g. physical wall

Boundary of an intangible nature, e.g. limit of conversation, imagination

Focus of attention or direction of travel

Head-on forms of science communication can result in clashes between experts and publics who approach the issue from different perspectives. We have observed and organised many of these public events with this deficit-model style format during which there is the potential for collisions between scientific experts and publics. Of course, as foregrounded earlier, experts from different disciplines can also clash on topics.

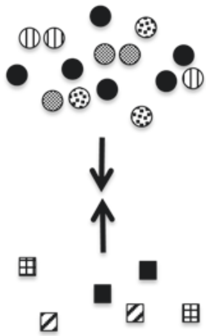

Fig. 8.1 Head-on forms of science communication

Attention can be directed away from the confrontational nature of these head-on forms of science communication through artworks that can open up opportunities for reflection and conversation (see Fig. 8.2). This is not to avoid challenges, but is to mitigate unnecessary confrontation. Whilst a lack of open conflict at public engagement events has been interpreted by some as a submissive but complicit alliance, resulting in friendly unchallenging interactions (Kerr et al. 2007), this does not mean that conflict is always necessary for meaningful and effective dialogue.

As we have found when using artworks to foster dialogue at various public engagement events, the metaphorical references often prompted people to begin a process of questioning, first to make meaning of the artwork, and subsequently the science. In the act of making, contributing to, and experiencing artworks, metaphor offers alternative imaginings in order to understand one experience in terms of another, what Lakoff and Johnson (2003) refer to as *imaginative rationality*. Different artworks within a public exhibition will inevitably instigate various degrees of reflection and conversation depending on a combination of the artistic agency and metaphorical content, the scientific content, aspects of presentation,

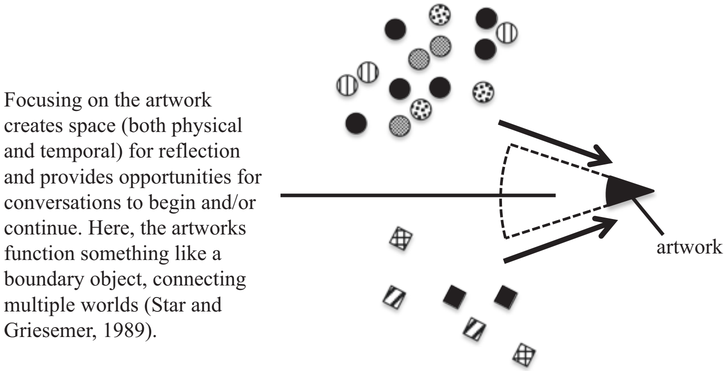

Fig. 8.2 Opportunities for conversation

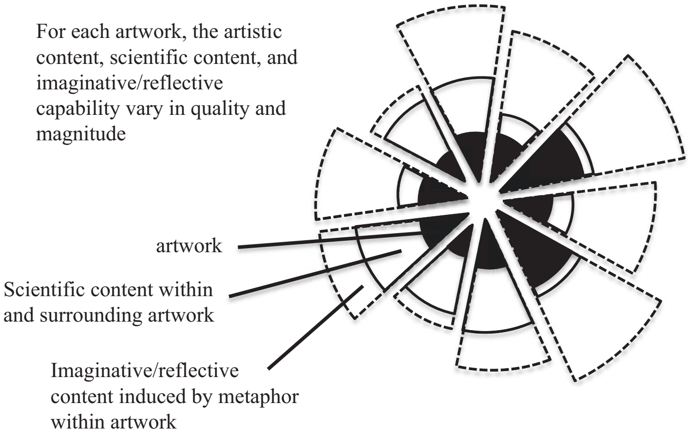

Fig. 8.3 The multifaceted content of artworks

and person-specific sensibilities of the viewer. The amount of imaginative and reflective content will vary according to the interplay between the trialogue of artwork, scientific content, and participatory onlooker (see Fig. 8.3).

The scientific content related to an artwork may be embedded within the piece or it may surround it such as in information panels or accompanying academic talks. As depicted in Fig. 8.4, engagement can be augmented via artist-led or curator-led talks, and this we found facilitated

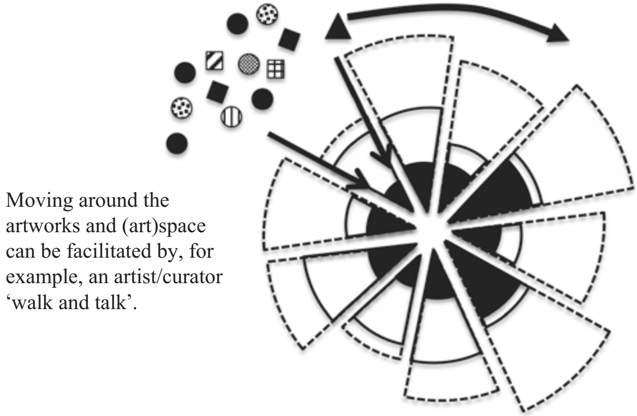

Fig. 8.4 Walking and talking

conversations whereby the mixed audiences, including public groups, scientists, social scientists and so on, were able to respond to the artworks through the lens of their own experiences and expertise.

Walking and talking resonates with the idea of walking as a qualitative social research tool and a means to knowledge whereby "it produces not a conventional interrogative encounter, but a collage of collaboration" (Anderson 2004, p. 260). Rather than walking through place, our 'walk and talk' through the (art)space was a wandering through the scientific disciplines, themes and issues relevant to psychiatric genetics. This journey and its talk is not always a friendly amble. However, potential pitfalls and potholes can be negotiated within the buffer zone (see Fig. 8.5).

Although the architecture and physical limits of the (art)space can serve the purpose of a tangible observable boundary, the buffer zone and its limits also refer to the less tangible conversational and imaginative spaces that we occupy. Rather than viewing our arts activities as simply science communication then, in our public engagement programme we sought to explore how different perspectives and voices could come together within a framework that is at least one step removed from science, unafraid of controversy and multiple interpretations, familiar with evoking emotion, and embedded within ethical enquiry—in other words, art. We continued to examine the ways in which the complexity, troubles, hopes and fears within and surrounding psychiatric genetics might be aired in a less

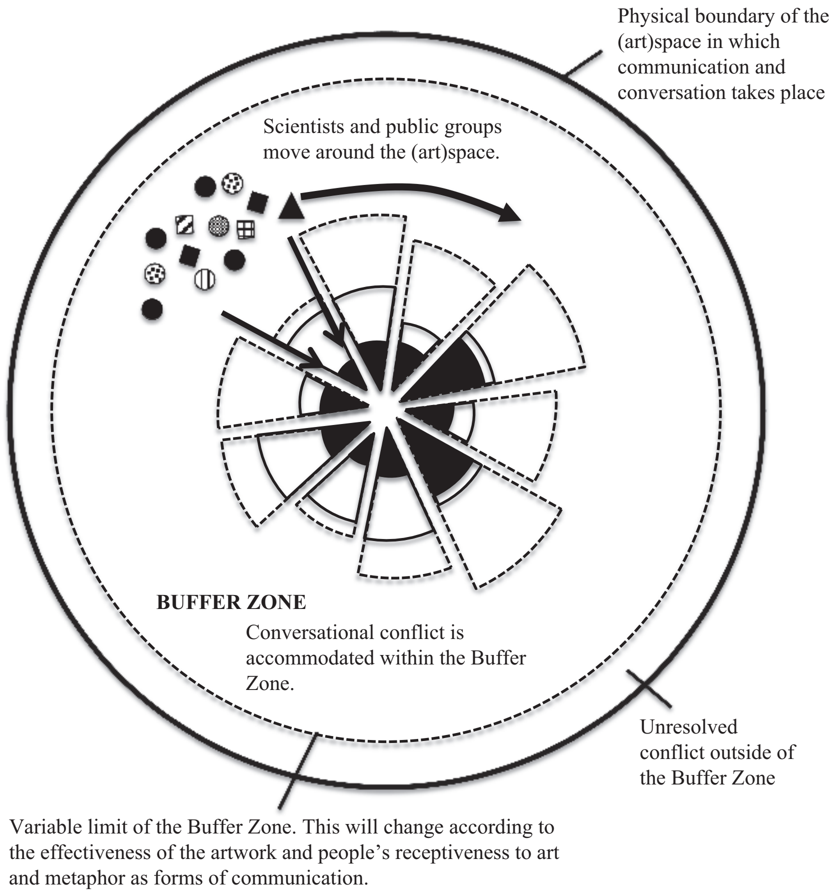

Fig. 8.5 The Buffer Zone

confrontational and more constructive way, aiming to keep people in the conversation, not losing them at the first clash or quarrel. We found that this dialogic and questioning standpoint, facilitated through the use of art and metaphor, afforded new framings in which public groups might feel they could engage with psychiatric genetics. This approach empowered people to air their fears such as to question how the translation of current research into therapeutic tools might 'alter' people with mental illness and how that vision for the future affects the way in which society perceives those people in the present. The idea of a modern-day eugenics remains a considerable concern to some people who feel that others are in control of their lives, while the perception of a knowledge-seeking but emotionless scientific approach was also raised as a worry. To this end, we found that people were not so interested in the science itself, much of it perceived as mundane lab-work and computer work. Instead, publics, and especially those with mental illness, were interested in what the science can do, and what impact it will have on people's lives. They were interested in the use of the science, its value, and how it might be appropriated.

Discussion

The public engagement of psychiatric genetics and genomics is no easy task. Psychiatry's problematic past and genetics' as yet unfulfilled promises for a therapeutic future means there is the potential for conflict during public engagement events. These tensions can further inflame the usual considerations related to issues of trust, power dynamics and disciplinary boundaries of any science communication initiative. As foregrounded earlier, Mauss' observations of many decades previous, that exchange is intimately tied to those involved, highlights that discussions between psychiatric geneticists and publics involve much more than just the delivery of words, more than just sentiment too, what is said comes with the burden of unfinished business. This history (personal, professional, and disciplinary) may originate from social factors related to perceived disparities in expertise and authority, but also past encounters between individuals and mental health professionals and researchers. Such unresolved concerns and transactions can be the root of any conflict and get in the way of what is said.

While, of course, the communication of developments in psychiatric genetics to publics should be much more than conveying information, facts, and opinions clearly, the context for this is likely to be very different to interdisciplinary communication between specialisms and also probably differs from the way other scientific specialisms communicate with their publics. That said, there are some parallels here between the public engagement of psychiatric genetics and other emotive socio-scientific subject matters that are dependent on human behaviour such as climate change. In examples such as these, there is a danger of swinging between the extremes of polite, unchallenging, almost subjugated discussion and dialogically destructive altercations.

We have therefore proposed that contemporary art within an adaptive and participatory public engagement programme provides a framework within which conflict can be mitigated so that constructive and meaningful dialogue takes place. Art can provide a framework that is comfortable with multiple interpretations and unafraid of controversy, provoking a willingness to challenge and question each other's perspectives and claims to knowledge irrespective of disciplinary roots and levels of expertise. In particular, we have extended Galison's concept of the trading zone to consider how an exchange of viewpoints can be facilitated through the use of art and metaphor. A conceptual, cognitive view of metaphor provides a useful mechanism to promote thinking about one thing in terms of another, to disturb the familiar and taken for granted ways of thinking, and to provoke a questioning mentality.

Bringing art and science together as part of a science communication initiative provided the opportunity to alleviate conflict. Crucial to stress is that this mitigation was not to protect the science; it was to protect the conversation. By reflecting on five years of artistic public engagement activities using art and metaphor, both within mainstream and less traditional (art)spaces, we have developed the concept of the buffer zone. The artwork and associated metaphors enable conversations between scientists and publics to begin and to be maintained. It absorbs the head-on collision of aggressive questioning and allows just enough space so that potential conflict is quelled sufficiently to bring the conversation back to one that is constructive. If Galison (1997, 2010) uses the concept of a trading zone to describe a place where scientific cultures and epistemes come together to forge alliances, buffer zones are spaces that recognise the differences in power between two (or more) cultural groups (see also Collins et al. 2010). It is a term that recognises the differences in the understanding of, and relationship with, the subject matter at hand and the importance that conversations do take place between groups that are often uneasy and unsteady in one another's company, enabling all the various positions to be heard, and not losing people at the first hint, or immanent fear, of discord.

Those involved in psychiatric genetics research might feel that engaging with these 'social' issues is to step out of their 'comfort zone', less a foot into a foreign land and more a jump from solid ground into wide, open oceans. Important to stress is that the concept of the buffer zone carries out work in both directions. It helps keep publics in the conversation but also sanctions scientists to work with other disciplines to confront some of the social and ethical concerns and not to shy away from, to use another metaphorical warning from *The Centre*, 'airing their dirty linen in public'.

To conclude, we have reflected on how the scientific specialism of psychiatric genetics can re-connect with society through artistic work. Based on our wide range of experiences and observations within the public engagement of psychiatric genetics, our concept of the buffer zone proposes a way of working with unpredictable and divergent forms of intercultural communication within controversial and emotive science. The buffer zone protects the discussion of potentially sensitive and threatening topic areas from being aborted due to conflict, enabling groups to negotiate perceived battlegrounds rather than erecting old and new barricades.

Notes

1. Braidotti (2013), for one, worries about the future survival of the humanities disciplines.
2. Throughout history, many prominent scientists have been aware of the importance of communicating with 'the public' and creating layperson audiences as a critical component of their scientific work (see Shapin 1984; Shapin and Schaffer 1985). Today, though, we are experiencing an institutional turn for scientists to engage in science communication and for engagement to move upstream (Wilsdon and Willis 2004).
3. A disunified science is more vulnerable to public scrutiny when empirical issues become matters of public interest or are publically aired. A clear example of this was the polarised split between a clinical psychologist and a clinical psychiatrist over the perceived geneticisation of attention deficit hyperactivity disorder (ADHD). This debate peppered the front pages over the UK's broadsheet and tabloid papers in early September 2010 (Dreaper 2010; Lewis and Bartlett 2015).
4. From the 1920s the development of genetic research on complex human behaviours led to the merging of quantitative genetic approaches, such as genetic epidemiology, with the techniques of molecular genetics (Plomin et al. 1994; Owen and Cardno 1999). Relatively recently, this research has also incorporated neuroscience and bioinformatics in order to identify the biological mechanisms and functional pathways that affect behaviour and brain function (Caspi and Moffitt 2006; Burmeister et al. 2008).
5. See, in particular, Gould (2003) who has focused on the commonalities and affinities between the humanities and the sciences.

6. Although, this has for some time been considered to be scientific ideology put to work for the purposes of creating boundaries around science to demarcate itself from non-science (Gieryn 1983).
7. The deficit model assumes that publics' distrust of and disengagement with science was due to a lack of scientific understanding. Until the mid-1990s, filling this deficit with scientific facts was considered by the scientific community to be the answer to improving the public's relationship with science (see Miller 2001).
8. Even so, some sci-art collaborations, as they have been termed, have been criticised as artistically uninteresting (Ede 2005) or inauthentic (Glinkowski and Bamford 2009) precisely because circumstances have meant the artists were seen to be simply illustrating science and not engaging in real and meaningful dialogue with scientists.
9. Perceived in this way, the artwork could also be described as a boundary object (Star and Griesemer 1989).
10. Anderson et al. (2010) uses the concept of the trialogue in the context of walking and talking methods and, in particular, the relationship between the researcher, the participant and the place where they are walking.
11. Nowadays, the language of contemporary science is said to be under stress with words being used to characterise processes that, on the surface, appear indescribable (Hoffmann 2002).
12. See Wynne (1992b), Yearley (2005), and Wilsdon and Willis (2004) for examples of clashes between scientific experts and publics related to radioactive fallout in Cumbria, north England, the BSE crisis, genetically modified crops, and the mumps measles rubella (MMR) vaccine.

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CHAPTER 9

Communication in International Technical Cooperation: An Anthropological Systems Approach

Letícia Cesarino

Introduction

This chapter approaches communication across the multiple scales that typically constitute international development cooperation from an anthropological perspective inspired by systems theory (Bateson 1972; Luhmann 1996). While the arguments I advance here are assumed as valid for development at large, the empirical material on which I draw involves one instance of South–South cooperation: a contemporary modality of aid proposed by countries from the global South that are gradually turning from recipients to donors (Mawdsley 2012). Here I take the case of Brazil's technical cooperation with Africa, and more specifically, a technology transfer project implemented since 2009 by Brazil's national agricultural research institute (Empresa Brasileira de Pesquisa Agropecuária, Embrapa) in four West African countries (Mali, Burkina Faso, Benin and Chad) (Cesarino 2013). I begin by resuming classic Science and Technology Studies (STS) debates on the movement

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of conceptual and material objects across different contexts (Latour 1987; Star and Griesemer 1989; Akrich 1992; Galison 1997; De Laet and Mol 2000), and then seek to extend these further through insights taken from other research fields. The two main bodies of literature on which I draw to this end are ethnographies of international development (Ferguson 1994; Mosse 2005; Li 2007; Cesarino 2012) and contemporary post-representational perspectives in anthropology (Strathern 1991; Wagner 1991; Cesarino 2014). In particular, I suggest that attention to multi-scalarity and self-reference may shed new light on the problem of communication in technology transfer initiatives carried out as part of international development projects, and help to dispel some of the latter's apparent paradoxes.

From Zones and Boundaries to Systems and Scales

The Science and Technology Studies (STS) literature has long dealt with the question of what happens when material and conceptual objects move between different techno-scientific contexts, and between these and lay contexts (e.g., Latour 1987; Star and Griesemer 1989; Galison 1997; Akrich 1992, 1993; De Laet and Mol 2000; Mol 2002). In these debates, two issues generally stand out: how the object has to be flexible enough to allow for translation across contexts, and yet remain rigid enough to maintain a recognizable identity; and how this kind of object operates as a mediator in communication and coalition-building between different groups of actors.

As in Susan Leigh Star's discussion of sites where "boundary objects" arise, international cooperation projects also involve "complex institutional setting[s]" (Star and Griesemer 1989, 387), as well as communication between scientific and non-scientific groups. In the case of Brazilian technical cooperation, project design and implementation requires that researchers in the agricultural sciences such as those in charge of Embrapa's projects regularly interact with partners from other research institutes, diplomacy, and cooperation bureaucracies both in Brazil and in recipient countries.[1] Like Star's actors, whose reciprocal translations sought to articulate diverging interests and conceptions around the stabilization of an institutional project—in her original article, a zoology museum—the Brazilian *cooperantes* also worked to keep their project together (Mosse 2005) by means of conceptual and material objects that circulated within and without the project network. One such conceptual objects was the notion of the *paysan* (the cotton-producing peasant in West Africa), on which I will focus here.

Both my fieldwork and the ethnographic literature on international development, however, have suggested a slightly different analytical route. Besides looking at how such objects articulate different groups through convergence and alliance-building, one should also pay attention at how articulation between different scales may be achieved through disjunctions and gaps. In the case of the project I analyse here, for instance, even if improvisation and attempts at translation were indeed part of its daily routine, the actors did not come up with the kind of intersectional, "pidgin" language that Peter Galison identified across different disciplinary expertises—what he called the trading zone (Galison 1997). And as I have remarked previously (Cesarino 2013, 2014), neither did they establish an all-encompassing, robust "methods standardization" (Star and Griesemer 1989) apparatus across the project's multiple organizational interfaces. Nonetheless, as I will unpack below, the overall assemblage maintained itself in spite of salient discontinuities across its multiple scales.

I will therefore part company with Star, Galison and much of the STS literature to argue that, rather than being obstacles to project success, disjunctions between different groups of actors may in fact be regarded as constitutive of international development's paradoxical efficacy (Ferguson 1994; Li 2007). This paradoxical efficacy relates to the tendency for each of them to operate according to its own references, while self-referentiality itself is rendered invisible. In this sense, it might be useful to introduce other intuitions on relationality and efficacy taken from outside the social sciences' more conventional toolbox. I seek to do this by bringing insights from systems theory to bear on STS debates on technology transfer in international development cooperation.

The influence of systems theory in STS is of course not new, and has been an inspiration for multiple approaches in the field, from classics such as Thomas Hughes's history of technological systems (Hughes 1987) to contemporary topics such as scientific practice in the worlds' peripheries (Neves 2014). From an anthropological perspective, however, this literature has to a large extent deployed the notion of systems in a "weak", sociological sense, thus leaving underexplored its potential for thinking post-representationally. My take on systems theory, on the other hand, comes out of contemporary anthropological concerns with finding an appropriate idiom beyond what Latour and others have called the "modern divides": nature–culture, individual–society, discourse–practice, subject–object, representation–world, micro–macro (Latour 1991; Mol 2002). My main analytical tool in this respect will be Marilyn Strathern's discussions on scale- and context-making (Strathern 1991, 2011; Holbraad and Pedersen 2009), which I will seek to read through insights from systems thinkers such as Niklas Luhmann (1996) and Gregory Bateson (1972).

Although these authors are not commonly associated with Strathern's work,[2] there are multiple resonances among them that may prove useful for the kind of analysis that I wish to advance (and here I deploy the terms scales and systems interchangeably). While Strathern allows me to keep in the foreground the sense of situated comparison entailed in the notion of scaling,[3] Luhmann privileges autopoietic and self-referential elements that are generally underexplored by Strathern. Indications of self-referential processes are found everywhere in the ethnographic literature on international development (Mosse 2005; Rottenburg 2009). However, they are rarely theorized as such, even in those analyses inspired by a functionalist-Foucauldian perspective (Ferguson 1994; Li 2007). The literature on technology transfer, in turn, has generally ignored the problem of self-reference, which is especially fruitful for making sense of how technologies travel across contexts (Cesarino).

This chapter will therefore work with a notion of communication that is less semiotic than cybernetic. From this perspective, communicating "subjects" are not necessarily human individuals or social groups—although they may be, since individuals are themselves systems (Bateson 1972; Luhmann 1996). Moreover, communication in this sense is less about interpreting or translating a language than about producing information—in Bateson's famous formulation, a difference that makes a difference. Thus, rather than a semiotic content transmitted between pre-existing subjects, communication takes the general form of "the processing of selection" (Luhmann 1996, 140) or bifurcations (Strathern 2011) that occur as a "same" object travels across discontinuous scales (or systems). Each time an object crosses systems boundaries, it is reconfigured according to a new relational matrix operating according to its own references, while leaving a trace or remainder that connects it partially (Strathern 1991) with other systems. In operationally closed systems such as organizations and bureaucracies (Luhmann 1996), self-reference means that a system can only relate to its environment, or incorporate information, on its own terms. This means that the efficacy of such systems is to be sought less outside them than in terms of feedback effects on the system itself (Bateson 1972; Luhmann 1996). In this sense, rather than being only a factor of disintegration or schism, self-referential effects may also work to connect scales (or systems).[4] In the case of development cooperation, as the ethnographic literature has repeatedly shown (Ferguson 1994; Li 2007), efficacy is to be found less in terms of the projects' explicit referents (that is, those promoting the "development" of project beneficiaries) than in the self-referential effects fed back into the aid organizations themselves. In other words, while individual projects may fail, and often do so, the development apparatus itself keeps on going—not despite, but precisely because of, such failures (Li 2007).

An analytic framework of systems and scales sketched in these terms has several advantages for approaching the kind of empirical material that I have at hand, while at the same time responding to some of contemporary anthropology's most salient theoretical challenges. On the one hand, long before actor-network theory or the ontological turn championed the problematization of modernity's core divides (Latour 1991; Mol 2002), systems perspectives developed by ignoring the disciplinary boundaries that helped carve them out (Von Bertalanffy 1968; Bateson 1972; Luhmann 1996). In particular, systems thinking supposes no a priori separation between natural and social, discursive and material, subjective and objective domains. At each scale, relations between nature and culture, discourse and practice, time and space are remade, or reshuffled, anew (Strathern 1991; Luhmann 1996). Therefore, systems theory already works with a post-representational idiom that can be readily deployed in lieu of "pluralist" (in Strathern's sense) notions of "social worlds" (Star and Griesemer 1989) or "scientific cultures" (Galison 2011).[5] Finally, this idiom allows for replacing the flat topology implicated in most "network" methodologies (Latour 2005; Mosse 2005) with a fractal topology more akin to systems thinking (Wagner 1991; Strathern 1991). Tracing paths along this topology does entail the kind of "many-to-many mapping" prescribed by Star and Griesemer (1989, 390). But rather than thinking in terms of social groups making alliances, here I consider emerging relations where the object and its context are simultaneously co-produced through scaling operations (Cesarino 2013). Finally, this implies an epistemological attitude somewhat different from actor-network theory's prescription of taking the actors seriously and being "myoptic" in our ethnographic analyses (Latour 2011). Although human and non-human actors are themselves individuated systems, they are also part of broader systems, for instance, those that Luhmann would call "social" (1996). From a systems perspective, it is not only possible, but necessary to both look beyond the actors at the network level, and not take what they say strictly at face value.

Self-Reference: Rethinking Users and Contexts in Technology Transfer

In the anthropological literature, analyses of technology transfer in development projects have typically foregrounded the theme of co-production between the technological artefact and its context—in particular, the user. This is the case of classic accounts, such as Madeleine Akrich (1992) on the "de-scription" of technical objects and De Laet and Mol (2000) on the Zimbabwe bush pump as a "fluid technology". As I have argued elsewhere (Cesarino), although these approaches assume the co-production of technology and context, they are less attentive to the scaling operations involved each time this relation is transformed, especially by holding stable a "micro" versus "macro" binary.[6] I see in this perspective two interrelated asymmetries that are particularly problematic when it comes to understanding technology transfer within international development cooperation. One is a narrow analytical focus on the user-technology interface, with the associated assumption that the user is the one located at a "micro" scale. The other is a bias towards the context of delivery, while the context of origin of the technologies (from where the "macro" perspective is projected) remains outside the anthropologists' analytical gaze.

Here I do not work with the assumption of a micro-macro binary, where the "macro" perspective is supposedly capable of encompassing *more* complexity than the "micro" of a single world that is already given (what Strathern [1991] called the "pluralist" configuration). Following my understanding of Strathern and others, I presume that at each scaling move, complexity is selectively transformed according to a new relational matrix, thus (co-)producing a new context (Strathern 1991; Mol 2002; Holbraad and Pedersen 2009; Cesarino 2014). In this sense, a "macro" perspective only appears as such if we take for granted that one particular perspective is able to perform what Donna Haraway once described as the "God-trick": a gaze that is able to see everything, but is itself situated nowhere (Haraway 1988). In a post-representational topography, on the other hand, every perspective is situated. What may make one of them seem disembedded (and universal) is its efficacy in extending the material infrastructural network that supports it (Latour 1991, 2011), along with the ideological mechanisms that sustain its claims to superiority over other perspectives, which come then to be regarded as local, micro—or, in the Gramscian idiom popularized by postcolonial theory, subaltern.

Approaches in STS such as actor-network theory do recognize and seek to describe how techno-science produces universalizing effects while remaining, in practice, local. A systems perspective brings to light other mechanisms beyond alliance-building, the establishment of obligatory passage points, or the purification of great divides (Callon 1984; Latour 1991). Here I suggest that one important way of achieving the God-trick is by rendering self-reference invisible. In the case of technology transfer in development cooperation, several mechanisms perform such task. One involves the assumption that development projects are *about* developing their supposed beneficiaries. Another is the above-mentioned double bias towards the context of delivery and the user-technology interface. To render self-reference visible therefore entails, in this case, switching our analytical focus to the context of origin of the technologies being transferred (for instance, the development agencies), and extending the notion of technology user beyond emic understandings of it (for example, by regarding providers themselves as users). This is precisely what many anthropologists of development have done in their ethnographic accounts (Ferguson 1994; Mosse 2005; Li 2007). But their insights did not seem to have found enough echo in the STS literature on technology transfer—and neither have they incorporated the advances made in the latter.

In what follows, I will argue that the efficacy of the *paysan* as a boundary object works less by standardizing connections across social groups than by recreating, as it moves across discontinuous scales, a fundamental asymmetry between provider and user, context of origin and context of delivery. While this asymmetry appears to the actors as the ultimate problem to be solved by their development interventions, a systems perspective suggests that it may be best understood as part of the development apparatus' autopoietic mechanisms. In other words, more than providing the conditions for overcoming underdevelopment, agencies involved in the provision of development cooperation advance the conditions for reproducing themselves by continuously (re)creating recipients as deficitary subjects in need of aid.

The *paysan*: Scale- and Context-Making in the C-4 Project

Elsewhere (Cesarino 2013, 2014) I have contended that the overall apparatus of Brazilian cooperation was (and still is) quite fragmented and heterogeneous, even leading some to define it as having a "policy of no policy" (Cabral and Shankland 2013). Different from traditional (Northern) development aid agencies (Mosse 2005; Rottenburg 2009), many of which are well-consolidated organizations operating since after World War II, Brazilian cooperation has involved the sometimes ad hoc assembling of multiple individual and institutional actors that do not have much precedent of working together as providers of cooperation to other countries. This has led to salient discontinuities in its overall organizational assemblage, which I have outlined in terms of three main scales (Table 9.1):

Although this configuration closely follows organizational outlines, it also demarcates clusters of sociality and perspectives across the various individual and institutional actors involved in Brazilian cooperation (keeping in mind the systems-theoretical postulate that agency is not to be restricted neither to humans, nor to individuals in a pluralist sense). While diplomats are the ones in charge of crafting the official discourse about Brazilian cooperation, the sharp discontinuities found between diplomacy and implementation (Cesarino 2017) do not manifest a gap

Table 9.1 Brazilian cooperation's chief scales, for projects implemented by Embrapa (based on Cesarino 2013)

Scale	*Main institutional actors*	*Main individual actors*	*Main domains of action*
I. Diplomacy	Itamaraty (Ministry of Foreign Relations)	Diplomats, politicians	Crafting cooperation principles and rhetoric; high-level negotiations
II. Policy	Brazilian Cooperation Agency (part of Itamaraty)	Project managers, cooperation experts	Crafting and enacting cooperation policy; negotiating and managing projects
III. Implementation	Embrapa (part of the Ministry of Agriculture)	Research scientists, technicians	Conducting experiments and demonstrations; technical training, capacity building

between discourse and practice. Rather, each scale has its own way of articulating discourse and practice, as well as its own references and efficacy conditions. Moreover, this is just one level (albeit an important one) of a broader, dynamic assemblage, which further differentiates both intensively (inside) and extensively (outside). Within Embrapa (scale III), for example, there are differences between actors with a more "technical" versus a more "political" or "managerial" profile, thus bringing them closer to scales I or II. Similarly, although the Brazilian Cooperation Agency is formally a department of Itamaraty and performs functions in project negotiation and principles formulation, especially when it comes to implementing projects, its managerial function brings it closer to Embrapa.

I will explore such discontinuities by providing an ethnographically based account of how the "same" conceptual object—in my interlocutors' terms, the *paysan*—was reconfigured as it moved across one particular actualization of the Brazilian cooperation assemblage outlined above, the C-4 Project. Keeping up with systems perspectives' refusal to think in terms of bounded social groups, I delimited three different ethnographic scales where the notion of the *paysan* played an important role in project practice. I identified these through terms deployed by my field interlocutors (all in French, which was the project's *lingua franca*): *projet* (the project document), *dispositif* (the experimental setup), and *parcelle* (the demonstration plot). As these scales communicated, the *paysan* was pressed into different kinds of service, and rearranged according to changing references. But I will also argue that something remained constant throughout such transformations: not some kind of content attributed to the notion, but a relational pattern whereby the West African peasant farmer was always framed in terms of a deficit. I conclude in the next section by showing how we can make better sense of such recurrence, again, by rendering self-reference visible.

In the world of development at large, whenever peasants are present, they are supposed to be the ultimate users, or beneficiaries, of technologies transferred from donor countries and institutions. They are also at the centre of the contexts to which such technologies are to be adapted. Indeed, the *paysan* lay at the heart of the C-4 Project; and yet, it was one of its most slippery and elusive entities. During my time in Mali and Burkina Faso, peasant farmers were rarely seen around the project's premises, and most of those whom I met were not formally part of it (I will comment on the exceptions to this below). Nonetheless, cotton peasant farmers from the C-4 countries were supposed to be the project's very raison d'être—those that were to be the ultimate beneficiaries of Embrapa's technologies. Their faces, hands, colourful clothes were repeatedly paraded on project brochures, websites, slide presentations, press releases, politicians' speeches, and other public relations materials that are such an important part of international development enterprises. They also figured prominently in the cotton controversy that raged at the World Trade Organization (WTO) in the mid-2000's, which gave origin to the C-4 Project.

On that occasion (strictly limited to scale I), the then presidents of Mali and Burkina Faso (Amadou Toumani Touré and Blaise Compaoré, both ousted from power since then) took the pages of the *New York Times* in order to denounce a scandal: how heavy subsidies lavished on a few thousand American cotton farmers were "strangling" millions of poor peasants in West Africa (Touré and Compaoré 2003). Many non-governmental organizations (NGOs) and activists took this to be an epitome of injustices in global "free" trade. In this editorial, the two presidents were claiming their countries' sole "ticket into the world market", by urging the United States to apply to themselves the free-trade rules they were so eager to impose on others. This was part of a broader controversy at the World Trade Organization (WTO), which in 2002 eventually led Brazil to file a dispute against the USA, out of which it eventually emerged successfully in 2008. The C-4 Project was one outcome of this process, as Brazilian and African diplomats from the so-called Cotton-4 group (Mali, Burkina Faso, Benin and Chad) thought up a joint technical cooperation project during their conversations at the WTO.

But when the agreement between Brazilians and Africans was finally formalized into a cooperation project at scale II, cotton-producing peasants were not included as formal partners, and neither did it include any provision of direct technology transfer to their farms. To a large extent, this had to do with bureaucratic constraints on the way Brazilian cooperation is allowed to operate (Cesarino 2013). Since, until recently, Brazil only received development aid, it does not have an appropriate legal framework for providing international cooperation. Until new legislation was passed in 2011, Embrapa was not even legally allowed to open offices or have bank accounts outside Brazil. Moreover, being a national research institute rather than a development agency or an NGO, it was supposed to partner up with other institutions like itself (in this case, the four national agricultural research institutes in the C-4 countries). Embrapa employees, most of whom worked in the institution as researchers (rather than cooperation consultants or extension agents), had to adapt their expertise and strategies to their newfound role as providers of technical cooperation to countries and institutions about which they had little if any knowledge.

Nonetheless, even if peasant farmers were not included as formal project partners, the *paysan* as a conceptual object continued to play an important role in the project, as the core of the projected context of delivery of Brazilian technologies. What my African interlocutors used to call the *milieu paysan* referred to the overall environment where cotton was produced in their countries: not just how farmers tended their crops, but also rain patterns, land tenure arrangements, availability of agricultural inputs, formal and informal markets for their crops, and so forth. This notion functioned as an "average" model, which reduced the complexity and heterogeneity of "actual" peasant practices to a version utilizable within the project scale. Internally to the project, the *milieu paysan* underwent further differentiation according to where and how it was deployed.

As described above, the C-4 Project was first thought up by diplomats from Brazil and the C-4 countries, who then summoned Itamaraty's technical cooperation arm, the Brazilian Cooperation Agency, to manage its design and implementation. It first came into existence therefore as a bureaucratic entity, a task that involved producing a sequence of documents, from technical mission reports, to diplomatic agreements, to the actual project framework. Here I will focus on one such documents, the C-4 Project in its final form, issued by the Brazilian Cooperation Agency in 2009 (ABC 2009). At this ethnographic scale, the *projet*, the *milieu paysan* figured as the key operator in a process akin to what Tania Li (2007) called "problematization": the construction of local developmental problems according to the solutions already available in the aid agencies, rather than the other way around. Problematization is a basic self-referential mechanism in the world of development, as it actively produces a problem liable to external intervention by selectively deploying certain assumptions about the local context that may turn out to be in fact quite removed from reality (Ferguson 1994). Similarly, cotton production in the C-4 countries was also constructed as a problem liable to a particular kind of intervention—the one offered by the Brazilian agencies. The core problem originally identified in the *projet* which reverberated across other project scales during implementation, was the low productivity of West African cotton. In the world of development at large, low productivity is a common way of framing local agricultural problems, and here as in many other (though not all) instances, Brazilian cooperation followed closely the general trends found in Northern and multilateral aid. But low productivity has many possible causes; here, the explanation selected was the poor investment that these countries have historically made in this crop's technological base:

> The techniques deployed in these countries have not accompanied the level of technological development found in other cotton-producing countries, due to the lack of investment in research. This had far-reaching consequences in a region where the climate poses grave challenges to agriculture. (ABC 2009, 11)

The narrow focus on research and technology is not random: after all, the implementing agency, Embrapa, is essentially an applied research institute. Here we find the kind of inversion identified by Li (2007) according to which the problem is made to fit the solution: formally, this was a technical cooperation project to be implemented by a research institute, and this fact in itself limited the range of available solutions to certain links in the cotton production chain (excluding for instance, other options such as the provision of inputs, credit, or support to fibre processing). The overall project objective was thus accordingly defined as to increase cotton productivity through capacity-building and limited transfer of technology and equipment—precisely the double focus of Brazil's technical cooperation in agriculture at large (Cabral and Shankland 2013).

The reference to "other cotton-producing countries" in the passage quoted above indicates yet other scaling movements. Above all, it makes clear that the productivity problem is not absolute, but relative to a broader frame of global competitiveness. After all, an average productivity of 800–1200 kg/ha (registered among the C-4 peasant farmers) can only emerge as a problem when there are countries (such as Brazil or the USA) producing at 4–5000 kg/ha and selling their cotton in the same world market (ABC 2009). Put in these terms, these figures are enough to justify Brazil's position as a provider and the C-4 countries' role as recipients of development cooperation, even on the face of the diplomats' South–South rhetoric of horizontality, reciprocal knowledge exchange, and so forth.

Just as importantly, this implies a conception of global market cast in neoliberal terms, where the sole way to increase peasant income is to increase productivity in a highly competitive, subsidy-free environment (rather than, for instance, public policies providing direct support for farmers, or encouraging commercialization in a domestic market protected from international competition). We saw above how Touré and Compaoré's complaints did not question the WTO system itself, but the incoherence of having its most active champion (i.e., the USA) lavish heavy subsidies in one economic sector. The cotton problem in the C-4 countries was therefore conceived as a deficit between these countries and other, highly productive cotton suppliers—a lag which is arguably redressable by technical, rather than political, means. Those who wrote the *projet* were therefore not denouncing hemispheric asymmetries, but taking them for granted as a background and justification for a technical cooperation project with a new partner, Brazil. Here, the procedure echoes others identified in the development literature, such as Ferguson's "antipolitics" (Ferguson 1994) or Tania Li's "rendering technical" (Li 2007): a process through which problems that have a political root come to be framed, and approached, as technical problems solvable by technical means. In this case, an entire "social" apparatus disappears to give way to a direct relation between technology and nature ("climate", etc.).

Different from their colleagues at Itamaraty and the Brazilian Cooperation Agency, the Embrapa researchers who specified the project's technical components in dialogue with their African colleagues were not as concerned with global trade or cooperation policy. Rather, they linked the problem of low productivity to serious nutritional deficiencies in West African cotton plants. As their argument went, these deficiencies were caused mostly by poor, leached soils, by the "insignificant amount of fertilizers" deployed by local peasants, and by irregular rain patterns (these two being connected since, without water, plants cannot absorb nutrients properly) (ABC 2009). Even if the agronomists and other researchers were well aware that problems with cotton production in the C-4 countries loomed much larger than soil degradation and insufficient provision of nutrients, from their perspective no other change in the production system would be effective in increasing yields unless this fundamental problem was attacked first. At this scale, too, the *milieu paysan* appeared characterized by a deficit: this time not in relation to other world producers, but in terms of a lag between the productive potential of the improved seeds being sown in their cotton fields, and the nutrients and water necessary for this potential to be fully actualized into high cotton yields. Underlying this technical way of framing the problem lies a deep, albeit invisible, geopolitics. Cotton grown commercially in West Africa today is the product of a long history whereby (less productive, but more resilient) local varieties were displaced by American improved seeds beginning in nineteenth-century colonial farms and tropical research institutes. In other words, by growing improved cotton for export, African peasants became locked into the full Green Revolution apparatus that must necessarily come along with it, especially fertilizers and other agricultural inputs (Cesarino).

The project's agronomic component—centred on a soil conservation and recovery technique perfected by Embrapa and very popular in Brazil called 'no-till'[7]—thus became the project's mainstay. And indeed, in general, agronomists in the partner institutes seemed to me more committed to the project than most of their colleagues from other specialties such as plant breeding or entomology. It could be said that, at least in the project's first stage (2009–2013), the main "users" of the project's technologies were the researchers themselves, especially those in the African institutes that had an interest in the possibility of developing the work they were already doing through this new project. Researchers in these countries are highly dependent on foreign funding for carrying out even their routine work, since their nation-states are not capable of providing national institutes with sufficient or reliable budgets.

From the perspective of researchers working with project implementation, an important technical scale where the *paysan* played a major role was what they called the *dispositif*. This term refers to the experimental setup deployed in the project's field tests, which usually followed a common design in the agronomic sciences called the split-plot. This design allows for the simultaneous testing of several variables within the same, larger plot, by crossing different treatments in a nested structure. A common configuration in the project was, for instance, larger plots with and without no-till, subdivided into treatments comparing two or more cotton varieties (say, a Brazilian, and a local or regional seed) and/or two or more cover plant seeding dates. This was done is such a way that there was always one control sub-plot left (in French, *le témoin*), that is, one that would represent the productive arrangement already deployed by local cotton farmers (precisely, the *milieu paysan*). This allowed for identifying differences in yield and other parameters between the control situation (the way cotton was grown in the C-4 countries) and the control situation plus the new elements introduced by the project (most notably, the management system based on no-till, and some of Embrapa's improved cotton seeds). This scaling operation was central to the project, since it allowed for assembling, within a couple of hectares, a microcosm of what the transfer of Brazilian technologies to actual cotton fields in the C-4 countries would look like. Here, the definition of the *paysan* in terms of low productivity reappears in terms of a concern with yields as the chief variable to be measured in the experiments. When communicated to the scale of management and policy, these and other figures produced at the scale of implementation turned into project "outcomes", and were included in spreadsheets, tables and graphs in reports, brochures, slide presentations at the Brazilian Cooperation Agency and other development organizations.

But where did this version of the control situation come from? Here as in other parts of the project, a more or less stable version of the *milieu paysan* was already available in the local research institutes. This version reflected not what the local peasants actually did in their cotton fields—how could it, since each *dispositif* included only one or a few control plots?—but the management protocols that the African researchers themselves developed and diffused among rural extension agents (normally, employees from the local cotton companies that controlled virtually every link in the cotton production chain). Here it becomes clear how the *milieu paysan* model deployed as experimental control had as much a descriptive as a normative intention towards peasants' practices. But even though the researchers regularly deployed this normative version of the *milieu paysan* in their experiments, they were well aware that it did not reflect actual peasant practice. The problem was not just with the inevitable reductionism implicated in this scaling operation, where the heterogeneity of actual practices was reduced to one model. For the African researchers had not only a version of ideal agricultural practice (encapsulated in extension protocols and experimental controls), but also a well-established version of how the practices of peasants *diverged* from this normative model. Of course, this disjunction only appeared if one left the scale of the *dispositif*, since, in the experiments themselves everything should proceed as if the peasants did follow the management protocols recommended by rural extension agents.

One of these other scales was the *parcelle*, where, besides scientific experiments, the same project plots operated as demonstration windows. This was a hybrid terrain where experimental work and public relations intersected—and where the importance of the latter for the project became especially evident. Here, the plot had an explicit function of rendering visible what was being done in the project. While in the first sense the *parcelle* catered to the interests and references of the researchers and technicians, in the second it aimed at a larger range of "users": politicians, managers, journalists, neighbourhood residents, cooperation workers from other countries, and ethnographers such as myself. Distinguished figures such as state ministers and even the Malian president had visited the C-4 Project's main *parcelle*, located in the outskirts of Mali's capital, Bamako. The purpose here was, on the one hand, to offer feedback to other project scales—such as managers from the Brazilian Cooperation Agency, or diplomats from Itamaraty—and, on the other, to recruit allies and sustain the project network as a whole.[8] This public relations function sometimes prevailed over that of experimental validation, when, for instance, ad hoc procedures (such as extra pesticide spraying) were deployed at critical moments, in order to make sure that the project plot always looked beautiful and neat to visitors' eyes and cameras' lenses. At this scale, the function of the project plot was mostly to look exuberant and under control, regardless of whether or not the Embrapa seeds and other technologies being validated there were indeed making their way into peasant land. In other words, what it communicated was the project's own existence and self-referential efficacy.

Among the visitors to the *parcelle* were African cotton farmers. Some of them were local leaders from cotton-producing areas where the project established a handful of test plots, roughly along the lines of the ones established in the four national research institutes. In experimental terms, these plots seemed secondary to the project, but they allowed for reaching out to peasant farmers without the mediation of powerful links in the local cotton production chain, such as the peasant associations or cotton companies. I do not have much information on how these turned out, but when I did my fieldwork these peasants seemed to me to be typical development "courtiers" (Bierschenk et al. 2000) or "brokers" (Lewis and Mosse 2006). These terms refer to a well-known character in the development literature: individuals who specialize in channelling funds from foreign agencies, thus actively manipulating the development apparatus according to their own interests and references (in a process akin to Bayart's extraversion [2000]). These individuals were a sure presence in the project's photographic and video materials, and usually had demonstration plots from other donors and NGOs on their land.

But other times, random peasant farmers were brought to the *parcelle* outside the project's formal schedule. There, they were introduced to Embrapa's cotton varieties, the no-till system, and whatever other experimentation that was being carried out outside the test plots. In these occasions, some of the problems with the assumptions underlying the *milieu paysan* model encapsulated in the *dispositif* became explicit. Although the control situation in the experimental plots was supposed to represent the cotton plants found in peasant land, they were in fact very different from them. The peasants were always in awe of the size, amount of capsules, vigour of the cotton plants found in the project fields. From this perspective, the difference between control plots (which were supposed to represent the *milieu paysan*) and those containing the new Brazilian technologies was minimal.

The African researchers' account for this gap between the project's control plots and actual peasant farms was based on the abovementioned deficit between what was recommended by the extension protocols and what farmers actually did on their cotton fields. The standard explanation for this deficit was that, even when the recommended fertilizer dosage did reach the peasants, they would divert part of it to their food crops. In West Africa, virtually every cotton farmer also grows food crops (usually cereals such as maize, sorghum or fonio), and frequently also raises a few heads of cattle. But since the only structured production chain in these countries is that of cotton, often they only have access to the fertilizers made available for that crop. "The work in cotton fields is just too hard", the African researchers and technicians told me multiple times. It is as if peasants lived under constant threat of crop shortfall, and their "sufferance threshold", as one agronomist once put it when drawing a comparison with Brazil, is very low. "Their cereal harvest has to last for the entire dry season, otherwise they'll just go hungry. If there is drough, pest or some other misfortune and the peasant loses his crop, he loses everything". In this context, intense risk-aversion would explain their (reasonable) hesitance towards adopting new technologies.

This fundamental discontinuity between research/extension and actual peasant practices rendered whatever improvement in yields eventually detected in the project's experimental grounds virtually meaningless. But overall, project workers did not address this gap because they tended to stick to their own (say, "micro") scale. At the project scale, for instance, the emergence of this version of the *milieu paysan* did not prevent work from advancing. Rather, it enabled further explorations, such as anticipating difficulties with technology transfer to farms, if and when the Brazilian technologies being validated by the project travelled beyond its *parcelle*. For example, besides fertilizer misapplication, in the C-4 Project another serious problem emerged as the *milieu paysan* was put into relation with

Embrapa's technologies, and more specifically, the no-till system: free-ranging cattle. As explained in the project's capacity-building trainings, no-till has three basic "pillars": direct seeding on straw residue; crop rotation; soil cover during fallow. Soil coverage with straw left from the previous season is therefore key to the system's recovery and conservation function, but it could not possibly be done in a context where cattle ranged freely. As the project coordinator at the time once put it, "their plots have one, two hectares each. There isn't even enough barbed wire in the world to fence them all off". This joking comment points indirectly at the problem's true core—the so-called *problème foncier* (the land or agrarian question) in West Africa, where the prevalence of peasant agriculture and customary rights on land is even higher than in other parts of the continent (Moyo 2008). At least in this respect, the *milieu paysan* was not a context liable to intervention by the project, but a given background to be reckoned with. If no-till would stand any chance of diffusing among cotton farmers, it would have to be the one to change, rather than the other way around. As an Embrapa agronomist recognized, "who knows, perhaps here no-till will end up having only two pillars".

Of all project components (which also included plant breeding and biological pest control), no-till seemed indeed to be the most "fluid" in the sense of De Laet and Mol (2000), since it was more easily decomposable and therefore, adaptable. Cover plants (grown in lines intercalated with the main crop, be it cotton or some rotation food crop) were an especially dynamic component in the no-till system. The project researchers were surveying functional equivalents with better adaptation potential to the *milieu paysan*. Thus, if in Brazil soil protection was performed by the cover plants' aerial parts, in the new context greater investment was being made in species with more abundant roots that would perform at least part of the soil cover function. Another example of how the *milieu paysan* was changing the technologies was a proposal to introduce cover plants with a food function (versus Embrapa's preference for agronomic functions), so that the system would offer more immediate benefits for cotton farmers. A third direction sought to replace laboratorial soil analysis (a preliminary step to fertilizer application unavailable to most peasant farmers) with visual modes of recognizing nutrient deficiency in cotton, maize and other crops (Cesarino 2013).

In all cases, however, this was a co-production work that quickly ran into scales over which the actors had little or no intervention power. Due to the vertical and monopolistic character of the cotton production chain in these countries, farmers depended on cotton companies for virtually everything; from provision of agricultural inputs and seeds to production purchase. And for political reasons that are too intricate and delicate to approach here, these cotton companies were not among the project partners. Given such dismal prospects for success, why then pursue a cooperation project with these countries at all? Or have we been looking for efficacy in the wrong place? It is here that an attention to multi-scalarity and self-reference may help dispel these and other apparent paradoxes found not just in South-South cooperation, but in international development as a whole. As communication happened across each scale described here, the notion of the *paysan* was reconfigured to play a slightly different role according to each system's self-referential codes and efficacy conditions, while the overall assemblage kept functioning by recreating, at each step, a fundamental asymmetry that renders possible not just individual projects, but the development apparatus at large.

Concluding Remarks

A systems approach to boundary objects leads us to put differently the question of whether these "maintain a common identity across sites" (Star and Griesemer 1989, 393), or, on the contrary, become something fundamentally different as they travel across "social worlds". This echoes old questions, raised in classic controversies such as those over the incommensurability of Kuhn's paradigms (Kuhn 1962), our capacity to make sense of Zande witchcraft (Wilson 1970), or current anthropological debates on the ontological turn, cosmopolitics, and ethnographic theory (Wagner 1975; Viveiros de Castro 2014). Systems thinking prompts us to raise two points in this respect. One is that, as objects travel across different scales, what is perceived as simplification or reduction is in fact a precondition for communication between systems—or, in other words, for their successful transit across systems boundaries. One of the best-known demonstrations of this point in STS is Latour's laboratory ethnography: the extraction of a scientific fact from "nature" (what the ethnographer called, following the native idiom, "purification") is one such reduction, without which the fact would not exist as such, and much the less circulate outside the laboratory. A second point is that, rather than putting this question in terms of the object's identity, or "being", a systems approach to communication will ask what an object *does* as it moves between scales, or crosses systems boundaries. The term object may be of course misleading, since neither systems nor their components are to be understood in terms of substance or essence, but of relations and effects—as in Luhmann's (1996) definition of a system as the *relation* between system and environment. Finally, taken together, both points drive our attention to the self-referential aspect of communication between systems: in particular, those recursive effects that typically characterize operationally closed systems, of which organizations (such as the ones involved in international cooperation) are prime examples from the "social" realm (Luhmann 1996).

Thus, the *paysan* was approached here as a key boundary object that circulated throughout the multiscalar assemblage formed by the C-4 Project. As communication happened across discontinuous scales—most notably, the ones frames by diplomacy, project management, and experimental work—this object enacted slightly different versions of what West African cotton farmers and farms look like. I have tried to show that these scaling moves functioned as much to connect systems as to respond to each systems' own self-referential processes. For the Brazilian diplomats, the figure of the *paysan* justified a new diplomatic alliance and a claim to leadership in the global South; for the cooperation managers, it provided a problem for which to design and implement a new intervention, and an image to parade in speeches and PR materials; and for the researchers, it provided the experimental foundation for carrying out technology adaptation research. Put in this way, this picture is not too far from what Callon (1984) for instance described in his classic essay on scallops, fishermen and scientists. But in his account, as in actor-network theory at large, the impression is that translations and network-building are always contingent and unpredictable—that the outcome can always be otherwise.

I have sought to underscore, on the other hand, the existence of important recurrences across the multiple scales that make up not just the C-4 Project, but development cooperation at large. Whenever it went and however it changed, the *paysan* was always framed in terms of a deficit. By this I do not mean that the notion of the *paysan* deployed by the actors at a "micro" scale was unilaterally shaped by "structural" asymmetries that lay elsewhere. As a conceptual object, it helped remake such asymmetries at every scaling move, at every communicative event between systems—just as actual African peasants may end up reproducing their subaltern condition by participating as "courtiers" in the development apparatus (Bierschenk et al. 2000).

So why then, do projects such as the C-4 persist, even on the face of often unsurmountable difficulties for making their technologies reach peasant land? Why, after more than half a century, have development agencies and workers not been able to overcome the asymmetries that keep peasants poor, and their countries, underdeveloped? As I already indicated, these questions can be asked as such only if self-referentiality is not recognized as a part of communication processes. Otherwise, a look at the practice of development cooperation leads us to conclude that, far from being a problem to be overcome, asymmetries between the origin and delivery contexts (that is, between providers and recipients, technology and users) are in fact a precondition for the very existence and continuity of individual projects, and, at another level, of the development apparatus at large.

Indeed, the literature on development has long manifested a cunning sense of paradox (Edwards 1989), stemming from a disconnect between development agencies' avowed purpose (that is, to "develop" projects beneficiaries) and their empirical, self-referential effects (mostly, to reproduce themselves). Is it not a paradox, after all, that an organization should have as a goal its own dissolution—which is what would logically happen if recipient countries did actually "develop"? Similarly, we may regard the recurrent sense of deficit encapsulated in the notion of the *paysan*, and the asymmetries that it (re)produces, as part of the development apparatus' autopoietic mechanisms. The abovementioned focus on the "micro" level of the user-technology interface and a restricted notion of the "user" are among the common mechanisms that prevent cooperation workers from seeing self-reference as such. But there is no reason why anthropologists and other STS scholars should reproduce them in our analyses.

Notes

1. Created in the 1970s by the military government, Embrapa is one of the world's leading research institutes in tropical agriculture, and played a key role in providing the technological innovations that enabled the occupation of Brazil's hinterlands by large-scale agriculture.
2. Strathern did, however, reference Luhmann in her more recent work on interdisciplinarity and audit culture (Strathern 2004).
3. 'Situated' in the sense of Haraway (1988) and Strathern (1991). This notion underscores, in this case, the situated character of the ethnographer's mediation between the field and the literature—a methodological concern that is particularly pressing for anthropologists, as opposed to sociologists such as Luhmann.
4. That disconnection may also be a form of connection has been an issue in anthropology at least since Evans-Pritchard's works on Zande witchcraft

and Nuer segmentary systems (Evans-Pritchard 1969, 1976). As Evans-Pritchard's contemporary Gregory Bateson (1958) showed in his 1936 Iatmul ethnography, relations between groups and individuals need not always entail articulation and convergence. What he called for instance symmetric schismogenesis—to stick to some of his own examples, the kind of relation that held between the Soviet and the Western blocs during the Cold War, or in daily quarrels between husband and wife—is based precisely on connecting through disconnection.

5. I am fully aware that neither Galison's nor Star's analyses are strictly representational or pluralist in Strathern's sense (1991). This is, I believe, at the core of Woolgar's qualm with the self-attributed "novelty" of the ontological turn in STS (Woolgar and Lezaun 2013). While I do agree with him that many STS scholars have put forth post-representational analyses in the past without naming them as such, I found it useful to explicitly acknowledge and systematize this perspective—an effort that includes a more careful, less naive deployment of terms such as "culture" or "social".
6. Curiously, Mol (2002) was quite attentive to scaling operations in her ethnography of medical practice in a Dutch hospital, but less so when she approached technology transfer to African villages (De Laet and Mol 2000). This may stem from the fact that in the latter she did not do in-depth ethnography. But it may also be an effect of ignoring self-reference in this case.
7. In the no-till system, the main crop is sown (in this case, cotton or a rotation crop such as maize or sorghum) in lines intercalated with one or more types of cover plant. In Embrapa's routine in Brazil, cover plants normally include grass and/or leguminous species—the first for abundant areal biomass, the second for fixing nitrogen from the air (thus catering to the system's two main functions: protect the soil during fallow and return nutrients to the soil). These plants must be sown at an appropriate date, in order to minimize competition for nutrients with the main crop.
8. A good analysis of cooperation projects along the lines of actor-network theory was carried out by Mosse (2005). There is also a sense in which the C-4 Project could be described as a continuous effort to keep together a socio-technical network that, if left on its own, would be quickly disaggregated by both "natural" and "social" entropic forces.

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