



Geographies of science: histories, localities, practices, futures

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Abstract: This paper examines the recent attempts to develop geographies of science both within, and beyond, the discipline of geography. Such efforts have been most successful in work by historical geographers and historians of geography. Investigating the ‘geographical turn’ evident across science studies more widely, this paper considers a broad range of engagements with spatiality by historians, sociologists, anthropologists and posthumanist practice theorists. In doing so, the paper thus argues that *different* geographies of science are emerging. Indeed, bringing together variegated conceptions of the spatiality of scientific activity will allow for wider audiences for geographies of science, inspire further investigation of the geographical sciences, and provide resources for debates regarding disciplinary histories and futures.

Key words: actor-network theory, geographies of science, history of science, hybrid geographies, indigenous knowledges, scientific practices, science studies.

I Introduction

It is a remarkable fact, that, in the recent literature of geography, researches on the method and limits of that science occupy a prominent place. Almost every distinguished geographer has felt the necessity of expressing his views on its aim and scope, and of defending it from being disintegrated and swallowed up by geology, botany, history, and other sciences treating on subjects similar to or identical with those of geography. If the representatives of a science as young as geography spend a great part of their time in discussions of this kind, though the material for investigations is still unlimited; if they feel compelled to defend their field of research against assaults of their fellow-workers

and outsiders, – the reason for this fact must be looked for in a deep discrepancy between their fundamental views of science and those of their adversaries. (Boas, 1887: 137)

It has long been acknowledged that philosophy of science has found it hard to accommodate the supposed exceptional status of geographical science. For much of the nineteenth- and twentieth-century history of the discipline, distinguished geographers thus attempted to defend or define the scientific status of geography. Many practitioners of human geography would recognize that this project had become enervated by the closing

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decades of the twentieth century. Indeed, the debilitation of attempts to defend the intellectual provenance of geographical science could be attributed directly to the widespread disassembling of philosophy of science inaugurated by Thomas Kuhn's *The structure of scientific revolutions* in 1962 (Kuhn, 1996). As Kuhn and his interlocutors began to dismantle universalist aspirations to indicate that scientific knowledges might instead be plural in gestation and local in character, so geographers became less interested in arguing for the exceptionalism of geographical science. Recent interventions by some distinguished historical geographers have thus been less about the need for geography to define itself as a scientific enterprise or otherwise, and more about the development of a nascent field of *geographies of science* (Withers, 1999a; 1999b; 2001; 2002; 2005; Livingstone, 2002; 2003; 2005; Naylor, 2005a; 2005b). These calls for attention to the geographies of science have resulted from long conversations with historians of science and have been characterized by attempts at the *institutionalization* of such approaches. As historical geographer Charles Withers puts it:

if we can have a history of science, a philosophy of science and a sociology of science, why not a geography and, even, a historical geography of science? (Withers, 2002: 9)

As this paper will discuss, however, the interest in the historical and social contexts of scientific knowledge that Kuhn's work precipitated has resulted in a diverse literature now entitled 'science studies' (Hess, 1997; Latour, 1999c; Law, 2004; Golinski, 2005; Fuller, 2006). And much recent literature within science studies has *also* focused on the geographies of science (Harris, 1998a; 1998b; Clark *et al.*, 1999a; 1999b; Pyenson, 2002; Golinski, 2005). Sociologist David Turnbull has referred to this as the 'embracing of the spatial' by science studies (Turnbull, 2002: 273).

Franz Boas thus remains prophetic. Approaches to the geographies of science are in evidence across social inquiry, inspired by soci-

ologists and anthropologists as well as historians. As the representatives of a research enterprise as young as geography of science have been reluctant to acknowledge these broad roots, perhaps due to desires to emphasize an *exceptional* (historical) geography of science, there is a need to take stock of the beginnings of the project and to outline some potential future directions. More importantly, the various proponents of this 'geographical turn' in science studies have themselves utilized particular, and competing, notions of spatiality.

This essay therefore attempts to convene discussion between the different interlocutors of geographies of science by highlighting the competing intellectual traditions coalescing in this new area of geographical pursuits. I begin with the argument that, due to a concern for the credibility of truth-claims and truth-claimants, science studies *necessarily* had to confront questions of spatiality. The paper then examines the geographical approaches that have been evident in science studies: among those who conceive of the sites of scientific practice as social arenas; in accounts inspired, in the main, by architectural studies; in ethnographic and ethnomethodological studies of laboratory spaces; in posthumanist theories of practice, such as actor-network theory; and in discussions about normative proposals, such as postcolonial science studies. This allows, in the final sections of the paper, a more developed appreciation of the possible resources available to geographers of science. The essay thus concludes by highlighting some of the existing and potential contributions by geographers of science to the history of the geographical sciences.

It should be noted at the outset that science studies is used in this paper to refer to the plurality of historical, sociological and anthropological approaches to scientific claims and practices. In consequence, this essay does not aspire to be comprehensive. The paper instead aims to bring some clarification to the expanse of spatial approaches within the 'mysterious field' of science studies (Latour, 1999c: 2).

II Placing studies of science

1 Historians of geography and historians of science

Around two decades ago, David Livingstone bemoaned the lack of historiographical sophistication in work by historians of geography, arguing that redemption could be found through rapprochement with historians of science (1979; 1984). At the same time, those historians of science interested specifically in topics of geographical import, such as biogeography, had neglected the history of geography (Livingstone, 1984). Although Livingstone's desire for conversation was focused on a particular historical issue and a specific historiographic stance, namely linking cognitive and contextual approaches in studies of the relationship between science and religion in the late nineteenth and early twentieth centuries, his arguments were nevertheless prescient. The relationship between the history of science and the history of geography has since formed the locus of various theoretical debates over the intervening period.

The sum of these various historiographical developments was greater enthusiasm for, and erudition in, histories of geography (Barnett, 1995; 1998; Driver, 1995a; 1995b; 1996; Matless, 1995; Heffernan, 1996; 2001; Gregory, 1998; 2000; Withers, 2001; Lorimer and Spedding, 2002a; 2002b; Withers and Mayhew, 2002; Lorimer, 2003a; 2003b). It is almost a decade now since Livingstone returned to the relations between historians of science and historians of geography and affirmed that 'the prospects for insightful conversation between these two scholarly communities are bright indeed' (1996: 360). The engagement between geographers and studies of scientific practice was, however, never limited to readings of historians of science. Indeed, even Livingstone, at around the same time as celebrating this conversation between the respective historians, was claiming, together with geographers Felix Driver and Nigel Thrift, that:

If it were necessary to choose the most vibrant and exciting areas of research in the social sciences and humanities today, then surely the study of science as a social construction would figure large. (Thrift *et al.*, 1995: 1)

In consequence of this multidisciplinary enthusiasm, various studies of what cultural historian Peter Burke terms the 'geography of truth' have become evident (Burke, 2000: 55). That the relationship between these two communities of historians remained problematic should therefore be of little surprise. Interest in the spatiality of knowledge has been of significant magnitude in the past 20 years and competing conceptualizations of spatiality have consequently skewed diverse studies of scientific practice. This reflects, in part, the disciplinary backgrounds of practitioners in a self-consciously interdisciplinary intellectual field: 'the use of "space" is contingent on the mode of analysis adopted' (Smith and Agar, 1998: 2). That confusion results, then, is hardly remarkable – the itinerant conceptual valence of the term has been well noted in geography (Driver, 1994b; Simonsen, 1996). As Doreen Massey engagingly puts it, space 'is one of those most obvious of things which is mobilized as a term in a thousand different contexts, but whose potential meanings are all too rarely explicated or addressed' (Massey, 1999: 27).

The following section discusses the inspiration for this examination of the place of knowledge before delineating how practitioners have utilized notions of spatiality in conflicting ways.

2 Embracing the spatial: emerging localisms in science studies

According to historical sociologist Steven Shapin, the western tradition has generally been unconcerned by the place of knowledge in accounts of truth (Shapin, 1991). There have been exceptions, of course, such as William Whewell's nineteenth-century interests in 'the geography of the sciences' (Schaffer, 1991b: 202). But universal knowledge, by definition, does not belong to any particular social place:

'Truth is – and, arguably, always has been – the "view from nowhere"' (Shapin, 1998: 5). These conceptions have held significant support. Attempts to argue for the situatedness, or historico-geographical contingency, of knowledge are therefore viewed as *political* challenges because they are 'likely to be regarded as denigration: what we thought was genuine knowledge was, in fact, just local lore' (Shapin, 1991: 209). As some criticisms of Shapin have indicated, simply claiming 'that knowledge is geographically located is widely taken as a way of saying that the knowledge in question is not authentically true at all' (Shapin, 1998: 5; 1999). But for those, like Shapin, who believe that knowledge is made and sustained through situated practical activity, then knowledge-claims must achieve universality, and this argument has resulted in a geographical turn in science studies.

In contrast to the *Theaetetus* view of propositional knowledge as 'justified, true belief', then, this embracing of the spatial has taken an expressly *sociological* view of knowledge as 'whatever people take to be knowledge' (Bloor, 1991: 5). In so doing, practitioners 'attempted to disengage the problem of truth and validity from the problem of credibility' (Ophir and Shapin, 1991: 6). The goal of the theorist was to avoid commitment to the truth-status of the beliefs in question, and instead describe and explain what is *in fact* believed within a community.

The famous 'symmetry postulate' is generally taken as the main feature of this programme. This *sine qua non* of much early work in science studies is neatly summarized in the words of sociologist David Bloor:

Both true and false, and rational and irrational ideas, in as far as they are collectively held, should all equally be the object of sociological curiosity, and should all be explained by reference to the same kinds of cause. In all cases the analyst must identify the *local*, contingent, causes of belief. (Bloor, 1999: 84, my emphasis)

The group of historical and sociological studies which coalesced around these assumptions

has been named the 'strong programme' of the sociology of scientific knowledge, or the 'Edinburgh School' of Bloor, Shapin and Barry Barnes. This approach was recently formulated more widely by Shapin as involving the study of *credibility* (Shapin, 1995a). Expanding on the symmetry postulate, Shapin adopts the term *truthing* 'to describe the mundane processes by which credibility is established' (1995a: 259). These processes are inchoate, and may even be unformalizable, but those at work in each separate case should be identified, because 'scientific knowledge is made by and through mundane – and locally varying – modes of social and cultural interaction' (Shapin, 1998: 6). As historian Peter Dear (1995) argues, perhaps the best way to conceptualize these projects is as part of a social history of trust rather than Shapin's (1994) ostensible focus on 'truth'.¹

Following Ophir and Shapin (1991), I want to emphasize the importance granted to spatiality in this formulation: for the proponents of this contextual history and sociology of science, the object of analysis became the grounds of *local* credibility. The place of knowledge is fully imbricated within networks of power relations, and in perceptions of the validity and legitimacy of that knowledge. The identification of the place of knowledge therefore becomes 'part of any inquiry concerning the ontological status of scientific objects and the epistemological standing of scientific statements' (Ophir and Shapin, 1991: 15; Shapin, 1995b).

The emergence of this localist genre in science studies has been criticized for failing to develop 'a systematic focus on the relationships between *thought* and its social setting' (Ophir and Shapin, 1991: 9, original emphasis). What I want to stress is that the conception of this localism remains undertheorized and there are questions to be addressed regarding how *some* knowledge 'spreads from one context to many, how is that spread achieved, and what is the cause of its movement?' (Ophir and Shapin, 1991: 16; Shapin, 1995b). As I will argue, this formulation, dating from this epochal 1991 paper, remains fundamental to

the geography of science. During the same year, historian Simon Schaffer made a similar argument that more attention had to be granted to the distinction 'between the processes of "localization", through which local techniques get to work at sites like labs via the concentration of widely distributed resources' and "'spatialization", through which techniques which are efficacious within the lab, manage to travel beyond it' (1991a: 190). James Secord returned to this issue in his Keynote Address to the joint meeting of BSHS/CSHPS/HSS in 2004, stressing that it remains the 'central question in our field. How and why does knowledge circulate?' (Secord, 2004: 655).² For Secord, "'knowledge in transit" should become the governing concern for future historical studies of science' (Secord, 2004: 664). As he puts it:

It is not so much a question of seeing how knowledge transcends the local circumstances of its production but instead of seeing how every local situation has within it connections with and possibilities for interaction with other settings. (Secord, 2004: 664)

As geographers have long been aware, the operative question then becomes how to theorize what is involved in the terms of 'local' or 'place' (Matless, 2003). Attempts to deal with this localism have been met in different ways among students of science studies. I will now consider these in turn, before addressing those made more explicitly by geographers.

III Localist histories of science

1 The socio-spatial school and communities of knowledge

The threshold of the experimental laboratory was constructed out of stone *and* social convention. (Shapin, 1988: 383, my emphasis)

I begin with the body of work that has generally argued for the *mutual constitution* of the spatial and the social. The new philosophy of the early English Restoration, embodied in Robert Boyle's experimental investigations

into pneumatics, has been an archetypal historical case for such sociologists of science. Within such empiricist schemes of knowledge, the ultimate authentication for a knowledge-claim was the act of *witnessing* (Shapin, 1988). The veracity of an experimental report depended on the original experience of a specified person on a particular occasion (Dear, 1985). But this had to be witnessed. Boyle and the experimental philosophers thus sought through witnesses to establish *matters of fact*, items of knowledge regarding the operation of the air-pump 'about which it was legitimate for a witness to be "morally certain"' (Shapin, 1984: 483; Shapin and Schaffer, 1985). The seventeenth-century matter of fact was, then, 'at once an epistemological and a social category', establishing direct and virtual witnesses by three mutually embedded technologies: the material, the literary, and the social (Shapin, 1984: 484; Shapin and Schaffer, 1985). As such, Boyle and his supporters had to establish both social solidarity and collective agreement upon the foundations of knowledge (Shapin, 1984; Shapin and Schaffer, 1985).

Although Boyle sometimes wished to study and perform his experiments in solitude, this alone could not result in the establishment of matters of fact. This support could only be gained from the public sphere. Yet this approval could only be sought from a very specific 'public'. An incomplete solution to this dilemma involved the creation of spaces where witnessing was to occur, and ultimately, could be guaranteed. Experimental sites were to be public and of easy access, but at the same time they were in the residences of gentlemen. Consequently, the 'house of experiment' relied on the invocation of the correct gentlemanly behaviour required when an individual was a visitor in the house of another (Shapin, 1988). More than simply containing those activities involving the air-pump, the site of experiment invoked in any visitor the tacit acceptance that they were a witness and should behave accordingly (Shapin, 1988). The establishment of matters

of fact was in this way spatialized, as certain observations could only be made by the 'geographically privileged' (Shapin, 1988: 375). Precisely because an individual's gentlemanly identity depended on his trustworthiness, participation as a witness in the house of another gentleman assured credibility for that particular experiment to become a matter of fact. In other words, 'gentlemen in, genuine knowledge out' (Shapin, 1988: 397).

The *place* of the experiment thus offered 'a partial answer to the fundamental question, Why ought one to give one's assent to experimental knowledge claims?' (Shapin, 1988: 374). Resembling the sociology of Anthony Giddens by stressing the mutual constitution of spatial arrangement and social practice, Shapin argues that the 'siting of knowledge-making practices contributed toward a practical solution of [these] epistemological problems' (Shapin, 1988: 373).

Epistemologists have become increasingly interested in the social capacities of knowledge in the past two decades (Kusch and Lipton, 2002). By so formulating the question of trust, philosopher of science Peter Lipton argues that Shapin's project 'encourages us to see the epistemology of testimony as a problem in applied ethics' (Lipton, 1998: 7). Indeed, testimony, what we know from what others tell us or 'an epistemic source outside the individual, or between individuals' has been granted widespread attention (Kusch and Lipton, 2002: 209; Plantinga, 1993; Kusch, 2001; 2002). Alvin Plantinga, in a sophisticated elaboration of Scottish philosopher Thomas Reid's eighteenth-century studies of *warrant*, argues that we are 'dependent upon testimony for most of what we know' (Plantinga, 1993: 78). Such discussions of testimony have been developed by Martin Kusch as 'communitarian epistemology' (Kusch, 2002: 341; see also Kusch, 1999; 2001). This approach facilitates an upscaling, and consequent spatialization, of epistemic studies by claiming that '*communities* rather than *individuals* are the primary bearers of knowledge' (Kusch, 2002: 335, original emphases). For

Kusch, testimony is in part performative and, crucially, 'is involved in the constitution of social institutions' (Kusch, 2002: 349).

Such elaborations of the philosophical consequences of investigations of locality, trust and truthing have not been much addressed by Shapin. In reply to philosophical criticisms of his epistemological ductility, Shapin playfully argues:

I am not a philosopher; I have no professional interest in sorting or evaluating philosophers' theories about truth; I do not have another formal theory of truth to propose in opposition to any of theirs; and my part of the academic world is not in the epistemology business. That is to say, the sorting of my subjects' knowledge-claims into proper-stuff and pseudo-stuff is not my job. I am a historian and sociologist of science. As such, I try to describe what various peoples in various temporal and cultural settings have counted as natural knowledge – knowledge that corresponds, or coheres, or that is in some other way deemed the right stuff. When I am feeling particularly ambitious, I sometimes venture explanations of why people come to the judgments they do. (Shapin, 1999: 1–2)

This passage is of further interest because Shapin's position directly corresponds to that of Robert Boyle's experimental philosophy. Just as the 'job of the experimental philosopher was to speak of experimentally-produced matters of fact, not to conjecture further than that', and yet at the same time establish matters of fact as the foundational category of the new experimental community, Shapin is concerned with creating the terms of contemporary debate for science studies (Shapin, 1984: 501). In short, Shapin wants to establish the mundane practices of knowledge-production, or truthing, as matters of fact. This explains why Shapin, in direct emulation of Boyle, uses the lexicon of an older philosophy (such as in his use of the terms 'truth' and 'truthing') to overcome the practical problems posed by the new historical sociologies of knowledge. As Shapin concludes a recent essay, we *still* 'live in a world of knowledge that is good enough, adequate to the case in hand, as

reliable as it can practically be, morally certain' (Shapin, 1999: 13).

2 *The architectures of science*

This socio-spatial school has inspired many excellent historical accounts of laboratory spaces and their epistemic consequences (Galison, 1985; Hannaway, 1986; Schaffer, 1998). There has been a related interest in the importance of the arrangement of *physical* space in science studies, galvanized more by *architectural* studies. This internalist position rests on the argument that spatial arrangements determine degrees of visibility and social interaction within architectural structures. Most notably, this has been pursued by students of architecture Bill Hillier and Alan Penn. Influenced by Claude Lévi-Strauss, whose structural anthropology sought to analyse social and mental processes through their supposed objective expression in spatial configurations, Hillier and Penn argue that at some rudimentary level every society has a 'spatial logic' which 'links the *frequency* of encounters with the *type* of encounters' (Hillier and Penn, 1991: 27, original emphases). By this, Hillier and Penn mean that everyday social interactions rely on informality and are concentrated in the quotidian spatial zone, whereas rarer, formal interactions tend to be more geographically segregated (Hillier and Penn, 1991). This argument attempts to move beyond a naïve causal relationship between environment and behaviour, to illustrate that specific designs of laboratory space can exacerbate social interactions by forming Granovetterian 'weak ties' which may be *generative* of scientific knowledge (Granovetter, 1973; Hillier and Penn, 1991).³

Many more historians and sociologists of science, however, have focused on the architectural emplacement of scientific practice. Peter Galison, interested in how 'the buildings of science literally and figuratively configure the identity of the scientist and scientific fields', explicitly calls for greater interaction between science studies and architectural studies (1999: 1). Such work has been concerned

with how the architectural structures housing scientific research imbue both scientists *and* non-scientists with meaning and identity (Foran, 1998; Gieryn, 1998; 1999b; Rentetzi, 2005). As historian Maria Rentetzi argues, '[a]rchitecture is more than the trace of these practices' (2005: 305). In designing a space for radioactivity research, for example, architects and scientists collaborated in the production of the Radium Institute of the Austrian Academy of Sciences in early twentieth-century Vienna (Rentetzi, 2005). For Rentetzi (2005), the gender assumptions of this new discipline were *written in* to both architectural plans and the built environment.

The spatial imaginary involved in architectural studies of scientific practice is perhaps best illustrated through the work of sociologist Thomas Gieryn. As Gieryn bluntly puts it:

What is sociologically interesting about buildings for science? Possibly this: Built places materialize identities for the people, organizations and practices they house. (Gieryn, 1999b: 423)

Gieryn's (1983) early research concentrated on the pragmatic problem for scientists of the *demarcation* of science as an intellectual pursuit. This 'boundary work' is consistently undertaken by scientists and, for Gieryn, is an ideological and rhetorical pursuit which is historically and geographically contingent, allowing scientists to contrast their pursuits favourably with 'non-science'. Given such interest in intellectual boundaries, it is no surprise that Gieryn's later work has focused on the material and semiotic embeddedness of these rhetorical borders (1998; 1999a; 1999b; 2000). Indeed, rhetorical boundary work is explicitly rendered *cartographically* as a heuristic tool in further considering the epistemic authority of scientists (Gieryn, 1999a). In arguing for the importance of 'truth-spots', Gieryn (2000) shows how these boundary disputes become architecturally emplaced. By considering the design of the places where science is practised, Gieryn argues that '[k]nowledge-making projects are situated not just by

the gender, race, class and nationality of the knower, but also by the material and architectural surroundings of inquiry' (1998: 310).

In a recent review of theories of 'place' for a sociological audience, Gieryn illustrates his captivation with some of the geographical literature (2000; 2002). As I will argue, however, this architectural school, although similar to the socio-spatial school, is different to the stance proffered by most geographers of science.

IV Practicing science

The focus on the local aspects of truth-making has been made possible through concentrating analytical attention towards practices: upon technologies, texts and quotidian activities. To return to Secord again, this 'move to study *practice* has . . . been the single most significant transformation in our field during the past twenty years' (2004: 658, my emphasis). It is worth stating that in recent theoretical discussions across the social sciences and humanities notions of 'practice' have dominated (Schatzki, 2001). And this is no less the case in cultural geography (Lorimer, 2005). Among students of science studies, theories of practice have most often been used to argue for the continuing importance of empirical inquiry. As Lynch and Woolgar (1990) outline, it is incumbent upon analysts to investigate what counts as scientific practice in *particular* cases. This section will discuss the spatial conceptions behind the different localist studies of scientific practices.

1 Laboratory ethnographies and ethnomethodologies

Attention to particular scientific practices has been most successfully developed in laboratory ethnographies (Knorr-Cetina, 1983; 1992; 1995; 1999; Latour and Woolgar, 1986; Traweek, 1988; 1992; 1996; Collins, 1992; Law, 1994; Franklin, 1995; Gusterson, 1996; Nader, 1996; Nothnagel, 1996; Mol, 2002; Doing, 2004).

This literature, sometimes termed the '“anthropology” of science', was inspired by the classic ethnography of Roger Guillemin's

laboratory at the Salk Institute written by sociologists Bruno Latour and Steve Woolgar (1986: 27). By treating the laboratory spaces of endocrinologists as an anthropological *culture*, Latour and Woolgar revolutionized the study of scientific practices. Conceptualizing the laboratory as cultural space is significant for ethnographers due to the epistemic baggage of 'science' in their home communities. It is the quintessential example of what Laura Nader terms 'studying up' – examining the cultural practices of the powerful (Nader, 1969). In conducting ethnographies of laboratory spaces, analysts thus have to work particularly hard with 'the problem of maintaining analytic distance' (Latour and Woolgar, 1986: 275).

Many laboratory ethnographies were henceforth constructivist in viewing science as constituted through practices (Law, 2004). Such studies commonly involve participant observation of laboratories deployed together with elements of discourse analysis (Knorr-Cetina, 1995). Anthropologist Sharon Traweek undertook contemporaneous fieldwork to Latour in the mid-1970s at the National Laboratory for High Energy Physics in Tsubuka, Japan, the Stanford Linear Accelerator (SLAC), near San Francisco, and the Fermi National Accelerator Laboratory (Fermilab), Chicago (Traweek, 1988). In writing 'a description, as thick as I could make it, of an extreme culture of objectivity', Traweek is perhaps most successful in outlining how high-energy physicists construct their lives through quotidian activities in laboratory spaces (1988: 162). Traweek shows how communities of physicists are constituted through practices such as the inculcation of novices and the decisions governing individual research careers (1988).

Ethnographers have also studied the development of enskillment by laboratory researchers in gravitational radiation (Collins, 1992), as well as nuclear weapons research at the Lawrence Livermore National Laboratory, San Francisco Bay Area (Gusterson, 1996). More recent ethnographies have taken Traweek's insights further and have begun to

develop discussion of the politics of labour in scientific laboratories (Doing, 2004). Karin Knorr-Cetina (1999) has developed comparative ethnographies of experimental high-energy physics and molecular biology to show the contrasting *epistemic cultures* of the technical, social, and symbolic machineries of knowing that develop across different scientific communities. In doing so, Knorr-Cetina has shown how scientific enterprise 'has a geography of its own. In fact, it is not one enterprise but many, a whole landscape – or market – of independent epistemic monopolies producing vastly different products' (Knorr-Cetina, 1999: 4). Interestingly, of the various approaches to science studies, it is these substantive laboratory ethnographies that have received the least consideration from geographers of science.

A related approach to have been developed, most notably by Michael Lynch, is the *ethnomethodological* study of scientific practice (Lynch, 1991; 1993; 1999; 2001). Ethnomethodology is defined as 'the study of practical action and practical reasoning', and is most associated with the work of Harold Garfinkel and his students (Lynch, 2001: 131; see also Garfinkel, 1967). The main concern of ethnomethodology is with 'the relation between practices and *accounts* of those practices' (Laurier, 2003: 1521, my emphasis; see also Lynch, 1993). Ethnomethodology treats rationality as a 'situated accomplishment by the parties whose local practices 'assemble' the recurrent scenes of action that make up a stable society' (Lynch, 2001: 131). Proponents of ethnomethodology therefore study 'micro' social phenomena, such as face-to-face interactions (Lynch, 1993). The aim, defined tautologically, is to provide descriptions of practice 'good enough to count as adequate descriptions of what they claim to be describing' (Laurier, 2003: 1522; see also Laurier and Philo, 2004). In outlining observations of practices and what are usually taken to be 'common sense' accounts, ethnomethodologists *resist* the urge to *interpret* such descriptions (Laurier, 2003). Eric Laurier and Chris

Philo have pioneered this approach in cultural geography (Laurier, 2001; Laurier and Philo, 2003; Lorimer, 2005).

In a decidedly geographical idiom, Lynch argues that ethnomethodological approaches to laboratory science require 'a revised understanding of what 'place' includes. It is all too easy to assume that laboratory floor plans provide access to the 'place' where scientific activity is generated' (Lynch, 1991: 73). For Lynch, the enclosures and partitions in the architecture and lived space of the laboratory are 'surface features of the phenomenal fields investigated by the scientists inhabiting such a "place"' (Lynch, 1991: 74). Technology, human bodies, and disciplinary practices are, for Lynch, mutually constitutive of the 'place' of knowledge. Sociologist Andrew Barry similarly argues that while space has a physical dimension 'it does not have an existence or an effectivity independent of its constitution with specific technological apparatuses' (Barry, 1993: 463).

2 Posthumanist theories of scientific practice

Laboratory ethnographies and ethnomethodology have inspired a *posthumanist* strand of practice theory. Despite resistance from historians, anthropologists, and sociologists of science (Strathern, 1996; Secord, 2004; Bourdieu, 2004), these arguments are much more familiar to human geographers. For sociologist Andrew Pickering, they form part of an attempt to construct a '*posthumanist* social theory: one that recognizes from the start that the contours of material and human agency reciprocally constitute one another' (Pickering, 2001: 164, original emphasis). Under this schema, both material and human agency 'are continually made, unmade, and remade' through practice (Pickering, 1992: 21). Through his agrestic metaphor of the '*mangle of practice*', Pickering argues that agency and culture are temporally emergent through scientific activity (Pickering, 1995: 23, original emphasis).

The most successful posthumanist approach, sometimes termed 'actor-network

theory' (ANT), finds its origins in the *following* of the activities of scientists in laboratory ethnography (Latour and Woolgar, 1986). Similar to Pickering's understanding of practice, ANT extends the symmetry principle of David Bloor to a second order, whereby humans and *non*-humans are to be discussed symmetrically (Latour, 1987; 1999a; 2005). Put simply, thinkers such as Bruno Latour and John Law 'impute' notions commonly taken as fundamentally human, such as 'agency', 'intention' and 'purpose', to other material objects and organisms. Agency is therefore *not* to be wielded by any sort of entity, but is understood as a *relational effect which is generated and performed through material configurations* (Law, 1994; 1999; 2004). This approach is inspired by semiotics, in that it argues that 'entities take their form and acquire their attributes as a result of their relations with other entities', but extends the argument from language to *all* materials (Law, 1999: 3). ANT is thus a '*semiotics of materiality*' (Law, 1999: 4, original emphasis). It was these various attributes that led Lynch to argue that a better designation for ANT would be 'actant-rhizome ontology' (1995: 169).

Initially, Latour argued that this relational effect of agency was performed through networks of heterogeneous materials, and that actors-in-networks strove to create 'allies' through the production of '*immutable and combinable mobiles*' that could remain invariant as they travelled through a complex of networks (Latour, 1987: 227, original emphases; see also Latour, 1983). More recently, advocates have bemoaned the 'fixing' of ANT as an increasingly popular 'theory', which has eroded the ability of scholars to use such insights productively and had political consequences in failing to recognize *difference* beyond networks (Latour, 1999b; 1999c; Law, 1999; 2000; 2002a; Hetherington and Law, 2000). In consequence, attempts have been made to develop beyond ANT, such as the introduction of '*fluid objects*' that are able to shift and adapt

to local circumstances (Law and Mol, 2001: 614, original emphases; Law, 2002a; 2002b). The idea here is that such objects allow theorists to encompass what lies 'beyond the conditions of network possibility', and thus allow Law to counter the criticism that many mobiles are actually *mutable* in different cultural contexts (Law, 2002b: 101).

The standard example of a fluid object is the Zimbabwe bush pump (Law and Mol, 2001). This water pump was rapidly accepted across a range of circumstances in different Zimbabwean villages. As Law and Mol expand:

So why is this? The answer is: *because it changes shape*. Of this pump and everything that allows it to work, nothing in particular necessarily holds in place. Bits break off the device and are replaced with bits which do not seem to fit. And other components – we are talking here both of parts of the 'machine itself', and of the social relations embedded in it – are added to it, components which were not in the original design itself . . . It is a *mutable mobile*. (Law and Mol, 2001: 613, original emphases)

With such modifications, it increasingly appears that ANT is becoming the kind of 'theory' that Isaiah Berlin warned scholars about over four decades ago: 'A theory festooned with *ad hoc* hypotheses to account for each specific deviation from the norm will, like Ptolemy's epicycles, soon cease to be useful' (Berlin, 1960: 14).

There has been a conspicuous geography to the scholarly reception of ANT. As Pierre Bourdieu puts it with no little reticence, ANT is a branch of science studies that was 'developed mainly in France, but which has enjoyed some success on the campuses of English-speaking universities' (Bourdieu, 2004: 26). At the same time, the insights of ANT have proved immensely productive across a diverse range of studies. Anthropologists, such as Marilyn Strathern and Philippe Descola, for example, have stressed that studies of non-humans might be best facilitated through extensive ethnographic experience (Strathern, 1996; Descola, 1996; 2005).

Of all the approaches in science studies that have embraced the spatial, ANT has proved most successful in attracting expositions from geographers to the expense of substantive elaborations (Murdoch, 1997; 1998; Kortelainen, 1999; Laurier and Philo, 1999; 2004; Bingham and Thrift, 2000; Philo and Wilbert, 2000; Whatmore, 1999a; 1999b; 2002; Hinchliffe, 2001). In his histories of economic geography during the 1960s and 1970s, Trevor Barnes has used ANT to document the enrolment of allies, objects, and texts during the adoption of locational analysis and statistical methods (Barnes, 1996; 2001; 2003). Yet, critical engagement with ANT has been rather sparse in geography, notwithstanding a couple of recent exceptions (Castree, 2002; Kirsch and Mitchell, 2004). Interests in the interconnecting geographies of objects, commodities, and responsibilities, for example, have yet to move forward debates about posthumanist practice theories (Cook, 2004; Massey, 2004). Likewise, Knorr-Cetina's and Bruegger's (2000) work on postsocial relations in investment banking has yet to engage seriously with geographical research on economic cultures (Thrift and Olds, 1996; McDowell, 1997; Thrift, 2000).

3 Normative and postcolonial science studies

Students of science studies have often been criticized for apolitical accounts of scientific practice (Latour, 2004; 2005). Following the so-called 'science wars' of the 1990s, various scholars sought pathways for a more publicly orientated research that would attempt to contribute to contemporary contestations over scientific knowledge and practice (Labinger and Collins, 2001; Powell, 2002). Such discussions were formulated by Harry Collins and Robert Evans as a programme for science studies which would be focused on investigation of *expertise* and *experience* (Collins and Evans, 2002; 2003). For Collins and Evans, this 'Third Wave of Science Studies' would aim to develop a 'normative theory of expertise. The aim is to approach

the question of who should and who should not be contributing to decision-making in virtue of their expertise' (Collins and Evans, 2002: 238, 249). By constructing a typology of expertise, Collins and Evans intend to adjudicate between relevant capacities in particular cases of scientific dispute (2002; 2003).

Directing future research towards the analysis of expertise has met some resistance among scholars of science studies, especially around issues of the degree to which Collins and Evans acknowledge previous work on the interface between science and policy (Gorman, 2002; Jasanoff, 2003; Rip, 2003; Wynne, 2003). There is, however, broad support for a '*reconstructivist STS . . . that is normative in orientation and activist in sympathies*' (Woodhouse *et al.*, 2002: 299, my emphasis). Such reinvigorated interest in scientific expertise might be seen as part of a wider normative turn across the social sciences and humanities (eg, Burawoy, 2005). Recent pronouncements in science studies have encouraged activism, the cultivation of public intellectuals, and addressing poverty and inequality (Woodhouse *et al.*, 2002; Bijker, 2003; Senker, 2003). As Wiebe Bijker put it in his Pre-Presidential Address to the 2001 annual meeting of the Society for Social Studies of Science, '[c]urrent societal problems urge a broadening of the STS agenda' (Bijker, 2003: 445).

This normative dimension of science studies has been pursued most vigorously in work encountering 'postcolonial moments' (Verran, 2002: 729). Although feminist critic Kavita Philip (1998: 302) was to argue as late as 1998 that '[p]ostcolonial theorizations of marginalized epistemologies are conspicuous by their absence' in science studies, a growing literature on the place of indigenous knowledges has emerged over the past few years (Bravo, 1996; 1999; Turnbull, 1997; 2000; 2002; Philip, 1998; MacLeod, 2001; Anderson, 2002; Bravo and Sörlin, 2002).

The general inspiration for these spatial studies of cross-cultural science has been the replacement of the dominant conception of universal rationality with notions of the *local*

geographies of knowledge. For Helen Watson-Verran and David Turnbull, it was inevitable that students of science studies would confront postcolonial literatures, because:

recognizing the localness of science subsumes many of the previously supposed limitations of other knowledge systems compared with Western science. Though knowledge systems may differ in their epistemologies, methodologies, logics, cognitive structures, or socio-economic contexts, *a characteristic they all share is localness*. (Watson-Verran and Turnbull, 1995: 116, my emphases)

The importance of these arguments emerges in the contemporary political discussions surrounding indigenous knowledges. The gradual expansion of a localist historiography, which 'slough[s] off a paradigm of cultural *deficit*, replacing it with a paradigm of cultural *difference*', has been celebrated among historians of colonial science (Chambers and Gillespie, 2001: 222–23, original emphasis; MacLeod, 2001). Although an incomplete project, it is envisaged that eventually such science studies will contribute to the construction of 'a new, more responsive, democratic, and self-questioning global discourse' (Chambers and Gillespie, 2001: 229).

As Turnbull argues, ways of addressing such deficiencies become evident if knowledge systems are allowed to interrogate one another (2000; 2002). Historical studies of local science 'must allow the voice of the colonized and subjugated cultures in their own terms' (Chambers and Gillespie, 2001: 233). Such interrogations, for Turnbull (1997; 2000), should be conducted in *third space*, a specially constructed knowledge space where ontological assumptions and power relations in knowledge traditions are made evident (Bhabha, 1994; Soja, 1996):

The future for local knowledge traditions is, I believe, dependent on the creation of a third space, an interstitial space, a space in which local knowledge traditions can be reframed, decentred and the social organization of trust can be negotiated. (Turnbull, 1997: 560)

In her investigations of the commonalities between Aboriginal and scientific approaches to firing regimes in Northern Australia, Verran argues that postcolonial moments in science studies may 'be fruitful as a way of re-conceiving ecology, and other sciences' (2002: 758). Attempts to convene such interrogations are obviously fraught with difficulty but they are critical for more complete histories of humanity, to allow cultural reconciliation, and for practical utility in contemporary environmental disputes (Chambers and Gillespie, 2001).

There are connections to be forged by geographers in investigation of the role of scientific practices in processes of postcolonial development and through attempts to support the political claims of indigenous peoples to 'differential geographies' (Fairhead and Leach, 2003; Castree, 2004). Given the ethical dimensions of discussions in geographies of commodities, human geographers have much to contribute to debates about normative science studies. Moreover, there are striking parallels between these discussions in science studies and those of geographers around academic activism and the production of public intellectuals (Murphy *et al.*, 2005; Castree, 2006).

V Futures: beyond *historical geographies of science*

Having commented widely on various approaches that are engaging with the spatialities of scientific practice, it is important now to return to the project that has emerged from *within* the discipline, that is, the *historical geography of science* of David Livingstone and Charles Withers (Livingstone, 1994; 1995; 2000a; 2000b; 2002; 2003; 2005; Withers, 1999a; 1999b; 2001; 2002).

For Livingstone, the 'geography of science' states 'that space matters in the conduct of scientific inquiry' (2002: 8). As such, Livingstone constructs a provisional classification of such spaces, such as those of the laboratory, the field, the museum, and those 'spaces of circulation' (whereby those spaces become nodes in

larger networks) (Livingstone, 2002: 24; 2003). Put starkly, for Livingstone:

Location at once facilitates and legitimates facticity. Exactly *how* this is so, of course, is another question, and one which, in my judgement, is the central item on the agenda of the 'geography of scientific knowledge'. (Livingstone, 2000a: 295, original emphasis)

This ultimate question left unanswered by Livingstone, namely, that of exactly *how* location matters, is essentially the same as that of Ophir and Shapin (1991), Schaffer (1991a) and Secord (2004). It should now be evident, from the preceding discussion, that there are numerous theoretical resources available to this project. As yet, historical geographers of science, such as they exist, have generally emphasized the particular spatial understandings of accounts presented by historical sociologists and cultural historians, often neglecting ethnographic and normative approaches to science studies. Despite a general injunction for studies of the local circumstances of particular cases, historical geographers of science have only begun to produce empirical studies of scientific activity (Naylor, 2002; Finnegan, 2004; 2005; Lorimer and Spedding, 2005; Powell, 2007; Vasudevan, 2007; Powell and Vasudevan, 2007).

However, there are promising signs of interest from historians in the geographies of scientific knowledge (Harris, 1998a; 1998b; Lux and Cook, 1998; Clark *et al.*, 1999; Rupke, 2002; Schaffer, 2002). Other historians of science, such as Robert Kohler, deploy a resolutely geographical vocabulary of 'place', 'nature', and 'cultural geography' (Kohler, 2002a; 2002b; 2002c). It is disappointing therefore that Kohler's interest in 'practices of place', like that of Lorraine Daston in the 'new geography', does not appear to have emerged from any serious engagement with *geographical* literature and instead from readings in environmental history and history of cartography (Kohler, 2002c: 204; Daston, 2001: 3).

An arena in which there is great potential for future contributions by geographers of

science is around discussions of travel, instrumentation, and metrology. It is in this work that the competing understandings of the spatiality of science have undertaken fecund interaction. Recent work has stressed the importance of practices of instrumentation in constructing scientific knowledges that 'can travel beyond the limits of laboratory' through standardization, calibration and reproduction (Bourguet *et al.*, 2002: 3). Turnbull refers to this as 'the "travelling turn" in the social history of science' (Turnbull, 2002: 273). The circulation of instruments is dependent upon embodied practices of motion, appropriation, and learning. The spatial imaginaries involved here collapse simplistic renderings of metropolitan/marginal or global/local to instead argue that practices of precision, value, and error are intertwined with bodies, objects, and cultures (Raj, 2002; Schaffer, 2002). Such geographies are central to the scientific enterprise. Indeed, as Bourguet *et al.* argue, there 'is no science which does not involve some form of displacement' (2002: 4).

Moreover, geography has always been, in part, a field science and has begun to re-examine critically its histories and practices of travel (Driver, 2000; 2001; Powell, 2002; 2007; Lorimer, 2003a; 2003b). It is here, in geographies of the *geographical* sciences, that distinctive and important contributions to science studies might also be made by geographers. The traditions of the discipline provide incredible opportunities for the serious study of practices in the field sciences and thus for histories of geography. As Livingstone argues, a 'social history of physical geography is a real *desideratum*' (Livingstone, 2002: 79, original emphasis).⁴ Moreover:

geography, as a discipline, is ideally placed to make a significant contribution here. Cultural geographers find themselves side-by-side, in the same department, with physical geographers working in the natural science tradition. And a golden opportunity is thus provided for them to examine what could be called the cultural geographies of the bench-scientists and the field-worker . . . Regardless, connecting historical work on the production and circulation of geographical knowledge with

ethnographic investigations of the cultures of contemporary geographical practices, seems to me a very fertile and exciting line of research. (Livingstone, 2002: 85)

Attention to the practices of fieldwork in physical geography not only greatly enriches science studies and histories of geography, but also presents an opportunity for the discipline of geography. As Derek Gregory put it a decade ago, 'we desperately need studies of what physical and human geographers *do* – in the field, the archive, the laboratory, the lecture-room, and in all those other sites where we spend so much of our working and thinking lives – because I suspect we would then discover that there is more common ground between our working practices and habits of mind than most of us presently recognize' (1995: 184, original emphasis). Cultural geographer Mike Crang sees such studies as helping 'bring the two tribes of geography into dialogue more fruitfully than an endlessly touted environmental synthesis' (1998: 1973). Sarah Whatmore's calls for *hybrid geographies* gesture towards what I am after here (1999a; 1999b; 2002; 2003). For Whatmore, science studies provide 'fertile grounds' for geographers in 'fashioning new divisions of labour and allying different and distinctive social and natural science skills and imaginations together' (Whatmore, 1999a: 260).

VI Conclusion

This essay has argued that while conceptions of space are variegated within science studies, Livingstone's pleas for conversation between historians of science and historians of geography are beginning to be met (Livingstone, 1984; 1996; 2005; Shapin, 1998). As this essay has argued, it is certainly to be accepted that the 'geography of science . . . implies more than an acknowledgement of the locational context of science' (Driver, 1994a: 338).

At the same time, by reviewing the differential embracing of the spatial in science studies, I have attempted to offer a caution to calls for any *prescriptive* attempt to institutionalize an

exceptional geography of science. That these variegated areas are not always acknowledged is a reason for this intervention. There has only been limited opportunity here to hint at some of the future directions of work for geographers of science, but it should be evident that the project attracts practitioners from across usual subdisciplinary confines. Geographers of science are attempting to make distinctive contributions in science studies, perhaps especially in studies of the geographical sciences through investigations of technologies of circulation, movement, and assemblage. These scholars also remember that their expertise provides resources for arbitrating over what scientific practice *ought* to be in contexts of environmental, social, and cultural contestation.

This undertaking should not be construed as a simple disciplinary excursion. But this work also raises important consequences for disciplinary futures, as Boas would have reminded us, in terms of histories, research practices and divisions of geographical labour. A number of discussions between sociologists of scientific knowledge and historians of science have often foundered on issues of method or epistemological assumptions (Kohler, 1999; Jasanoff, 2000; Eddy, 2004; Golinski, 2005). In review essays of science studies, anthropologists and sociologists have often complained about the perceived remoteness of investigations of scientific practices from central disciplinary concerns (Traweek, 1992; Franklin, 1995; Shapin, 1995b). Thinking about geographies of science thus connects directly to arguments about the aims and scope of our discipline and, indeed, to what geography ought to be. Importantly, the traditional diversity of geography and the uncertain status of geographical science, so feared by Boas, should not be bemoaned in these times. Instead, connections across geographical traditions are providing for some of the most innovative work in contemporary geography.

James Secord concedes that because of the diversity of theoretical perspectives in science studies it seems that 'history of science, compared to any time since its foundation in

the 1950s, has experienced a loss of direction' (2004: 671). It would appear that, to the contrary, the outlook for *geographers* of science is much more enthusiastic and exciting. Geographers have always been able to accept competing inspirations and interlocutors. Rather than aim for the institutionalization of geography of science, then, practitioners should draw strength from the intellectual energy of competing *geographies*. The connections between debates across geography and conceptions of the spatial within science studies suggest that the prospectus for geographers of science is bright indeed.

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Notes

1. Dear (1995: 454) thus calls for 'a social history of conviction, rather than "just" a social history of publicly accredited truth'.
2. This keynote was delivered as a plenary lecture on 6 August 2004 at the fifth joint meeting of the British Society for the History of Science, the Canadian Society for the History and Philosophy of Science/La Société Canadienne d'Histoire et de Philosophie des Sciences, and the History of Science Society (of the USA) at Halifax, Nova Scotia (Secord, 2004). Given the combined audiences of the three scholarly associations, and that these joint meetings are held only every four years, Secord's argument carries significant weight for historical studies of science.
3. For Granovetter (1973), 'weak ties' are important social connections with acquaintances that are not based on family or close friendship. For an analysis which extends the generation of 'weak ties' across larger travel networks in the seventeenth century, see Lux and Cook (1998).
4. Neil Smith (1992) has made much the same point. Geomorphologist Chris Keylock (2003) has recently used science studies to reflect on

the impact of Mark Melton on the history of quantitative geomorphology.

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