Landscapes of Knowledge

David N. Livingstone

Space is rapidly becoming a central organizing principle for making sense of scientific knowledge. The recently published third volume of *The Cambridge History of Science*, which deals with "Early Modern Science" (Park & Daston, 2006b), is indicative. Its editors have chosen to devote nine chapters to such subjects as markets, piazzas, and villages; houses and households; libraries and lecture halls; courts and academies; anatomy theaters and botanical gardens; and coffeehouses and print shops. All are interrogated as critical sites of scientific knowledge. This emphasis, standing in marked contrast to earlier heroic narratives of scientific progress and great-name history, enables the editors to speak of the ways in which what they call the "geography of changes in natural knowledge closely tracked that of religious, military, and economic developments" (Park & Daston, 2006a, p. 7). And it raises profound questions that go beyond the mere charting of place-based activities. Eamon (2006), for example, notes that the emergence of the marketplace "as a site of natural knowledge" where goldsmiths, herbalists, apothecaries, dyers, and many other craftsmen procured fruit, leaves, seeds, ointments, and other natural objects "signaled important shifts in the definition of knowledge and of who might qualify as natural knowers. It also raised questions . . . about whose knowledge was considered valid and authoritative" (p. 207). Cooper (2006) uses her interrogation of the home as a space of knowledge to show how "natural inquiry in early modern Europe . . . often constituted a *family* project" (p. 225). Household recipe books are just as useful as scribbled laboratory notes in gleaning a sense of domestic space as a setting for scientific knowledge. English women of means, for example, sometimes had stills and alembics in their kitchens which they used to tinker "with and write down medical recipes" (Cooper, 2006, p. 227). For his part, Johns (2006) begins his analysis of coffeehouses and print shops with the arresting claim that "experimental philosophy came to prominence on a wave of coffee" (p. 320).

D.N. Livingstone (✉)
School of Geography, Queen's University Belfast, Belfast BT7 1NN, Northern Ireland
e-mail: d.livingstone@qub.ac.uk

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This particular probing of the sites of scientific knowledge production during the early modern period is only one of the recent expressions of a more general geographical turn in science studies. Withers's (2007) remarkable geographical scrutiny of the so-called Enlightenment is another. The spatiality of knowledge has thus become a focal point of conversation amongst a range of historians, sociologists, geographers, and anthropologists interested in the nature of scientific culture. Taken in the round, it is an enterprise operating with the conviction that science "is indelibly marked by the local and the spatial circumstances of its making; that scientific knowledge is embodied, residing in people and in such material objects as books and instruments . . . and, finally, that scientific knowledge is made by and through mundane—and locally varying—modes of social and cultural interaction" (Shapin, 1998, p. 6). Perspectives have differed, of course, and strategies for elucidating just how space matters have been far from uniform. Some scholars have been motivated by the ethnographic lure of thick description; some have retained the epistemological preoccupations of traditional philosophy of science but have sought to spatialize the conventional distinction between the context of discovery and the context of justification; others have dwelt on the reciprocal connections between the practices of science and the production of space.

Mapping Scientific Space

In pursuing this enterprise, a variety of ways of thinking about the spatiality of scientific knowledge and practice have emerged (Meusburger, 2000). Some of them have discriminated between the production and consumption, or construction and reception, phases of the scientific knowledge circuit. What animates this line of inquiry is the recognition that very specific kinds of spaces have to be made for the conduct of scientific inquiry. They include sites like laboratories and museums, observatories and dissecting rooms, survey ships and census bureaus, and botanical and zoological gardens. But they also embrace "natural" locations that are delimited in certain kinds of ways so as to be constituted as field sites. In each case there are protocols for the management of the space, and various mechanisms are installed to police the site and control its human occupation. In recent years, a good deal of attention has been directed to these locations and to how their microgeographies have shaped the practices that go on in these cognitively privileged sites (see Livingstone, 2003a). And there are, of course, other less dedicated spaces where scientific knowledge has been generated, including public houses, royal courts, cathedrals, tents, stock farms, and specific cities—like Chicago (Gieryn, 2006). On the consumption side of the equation, attention has been directed to how knowledge moves from its point of construction and out into the world of general intellectual commerce. A whole suite of proposals has been advanced to get a handle on the processes involved. One of the most popular has been Bruno Latour's account in which centers of calculation and immutable mobiles have held pride of place. Just how distributed knowledge and information is brought together in dedicated sites and reassembled

has been at the forefront of his inquiries. Indeed, the interventions of Latour and others scrutinizing how knowledge travels and transforms have rendered troublesome the seemingly clear boundary between production and consumption, and several writers have called for the demolition of that convenient taxonomy. The reason is that knowledge is produced in the moment of encounter with new theory as it is shaped, taken up, and put to use in different intellectual and social spaces. Secord (2004) has recently given voice to this sentiment by his proposal to shift focus and think about knowledge-making as a form of communicative action (p. 661) and thereby "recognize that questions of 'what' is being said can be answered only through a simultaneous understanding of 'how', 'where', 'when', and 'for whom'" (p. 664).

Another way of thinking about the geography of science is to approach the problem through scales of spatial analysis. On one scale, it makes sense to look at very specific venues of the sort I have itemized already. On another scale, one might profitably inquire into the significance of regions (ranging from the provincial to the continental) in the conduct of scientific enterprises. Just how a scientific culture is shaped by its regional setting is likely to be a reflection of forces such as local styles of patronage, pedagogic traditions, circuits of communication, networks of social organization, and expressions of religious devotion. Historically, scientific subcultures have taken form in response to the dictates of urban politics and industrial pollution, the demands of civic pride, and radical protest. The traffic should not be thought of as one-way. It is not just that scientific practice is molded by regional setting; it is also that regional identity itself has often been conditioned by scientific projects. Applied astronomy, precision mapping, resource inventory, and geodetic survey are just a few of the scientific practices that states have mobilized for the purpose of defining the bounds of its territory and providing a register of its natural assets. Such activities at once impose rational order on the seeming chaos of nature, deliver to governments a sense of territorial coherence, and supply servants of the state with geographical data essential for fixing tax, stimulating economic growth, exploiting resources, and maintaining military defense.

A third means of thinking in a structured way about the location of science is to discriminate between different kinds of scientific spaces—such as spaces of experiment, spaces of exhibition, and spaces of expedition—and choreograph their differing geographies. Consider how the natural world is differently encountered in these different venues. In experimental laboratories, nature is subject to a variety of manipulations. In exhibitionary arenas—such as botanical or zoological gardens, or museums—the overriding concern is to arrange plants or animals or objects through some classification system and exhibit them appropriately, to put them in their place as it were. In expeditionary settings, the aim is to encounter unmanipulated nature, to literally keep it in site. Of course, there are problems with this tripartite arrangement, not the least of which is that they overlap and intersect in all sorts of promiscuous ways. As Kohler (2002), for example, has compellingly shown, the distinction between laboratory and field does not work well for major strands of biology during the early twentieth century. His inquiry into what he terms the "landscapes and labscapes" of science subverts the standard distinction. What he calls the "lab-field

frontier" that "in 1900 was a defensive boundary demarcating the different worlds of field and laboratory biology" had changed by midcentury into "a broad zone of scientific subcultures that were neither pure lab nor pure field but mixtures of the two" (p. 293; for Germany, see Cittadino, 1990). It is in this third kind of scientific space that the labscape emerges as a critical site of biological inquiry and Kohler (2002) conducts a "transect" (p. 293) of this terrain elucidating the shifts in the zone's practices of place. And it is no surprise that Kohler resorts to geographical vernacular to describe his self-appointed task. He thus speaks of the "cultural geography of border biology" (p. 294), the value of a "cultural-geographic approach" (p. 295), and the blurred spaces between nature and artifice as "a patchy cultural landscape" (Kohler, 2002, p. 308).

My aim so far has not been to defend any particular systematization but rather to advertise just a few of the ways in which the geographies of science have been schematized. Various surveys of this general terrain now exist, so further review would be redundant in this chapter (see Finnegan, 2008; Naylor, 2005; Powell, 2007; Withers, 2002).

Recurrent Signals

A number of themes snaking their way through these endeavors can be extracted as focal points of recent thinking about the spatiality of science. I want now to consider them in a fairly impressionistic way. For convenience, I will briefly tackle them under two labels: *polarities* and *materialities*.

One collective consequence of the turn to the spaces of science has been to render problematic a series of polarities that have frequently been taken for granted. Let me just name three: the natural and the social; the local and the global; and the scientific and the political. First, the distinction between the natural and the social has been disturbed in different ways, not least by the recognition that in so many scientific arenas the "natural" is stage-managed by human artifice. Collins (1988), writing on the modern public experiments of the nuclear industry, observes that the demonstrations are effective precisely because the smooth public display conceals "the untidy craft" of the scientist; demonstrations work—as he superbly catches the character of the circuit from private to public—by "caging Nature's caprices in thick walls of faultless display" (p. 728). Whether in the laboratory, in the field, or in the museum, the natural is constituted by the social. Reflecting even for a moment on terms like *species*, *race*, *matter*, even *nature* itself, not to mention *selfish genes,* brings one face to face with labels that have been freighted with cultural baggage.

Second, the standard juxtaposition of the local and the global has been undermined by work on the processes of knowledge-making. It is not that universal knowledge is simply true, whereas local knowledge is at best partial or at worst pathological. Rather, the question is how locally generated scientific knowledge achieves universality (Shapin, 1998). What are the mechanisms by which it spreads—and spreads unevenly—across space and time? What role is played

by the standardization of measurement, the calibration of equipment, and the disciplining of observers in global circulation? The insight that the global is local at every point has troubled the ease with which local and global may be disaggregated.

Third, the comfortable distinction between the scientific and the political has been progressively undermined in critically significant ways. Whether one is inspecting the imperial vocabulary of Darwinian biogeography (Browne, 1996); the reliance of Victorian scientific travelers on the infrastructures of colonialism (Camerini, 1996); the operations of pharmaceutical corporations in the global trade in genetic resources (Parry, 2004, 2006); the colonial networks that facilitated the transfer of botanical specimens to and from metropolitan centers like Kew Gardens (Schiebinger & Swan, 2005); the ways in which ethologists like Karl Vogt read animal behavior through the lens of the emergence of the nation-state, the decline of monarchy, and the rise of republicanism (Rupke, 2009); or the role of different regulatory regimes for the conduct of clinical trials—such as in Cuba—in the evolution of the biotech industry (Reid-Henry, in press), one sees plainly what might be called the geopolitics of scientific knowledge and practice.

Another suite of concerns grounding scientific knowledge in the places of its making congregates around what, for convenience, I call materialities. What I am after in this context is the move away from thinking about scientific knowledge as free-floating and transcendental to thinking about it in a way that roots such undertakings in material entities—like bodies, buildings, and other physical objects. In contrast to the image of scientific knowledge as disembodied, abstracted truth, a set of arguments is now in place emphasizing the corporeality of knowledge and its incarnation in human subjects. In part, this development builds on the insights of Michael Polanyi, who perceived scientific instruments as extensions of the body in the acquisition of knowledge. As he observed: "Our body is always in use as the basic instrument of our intellectual and practical control over our surroundings" (Polanyi, 1959, p. 31). In part, it draws on work directing attention to the human body as itself a site of knowledge acquisition. Alexander von Humboldt, for example, used his own body as a recording instrument on his expedition to South America between 1799 and 1804, when he applied electrodes to himself in the attempt to ascertain the effects of an electric current. Given the fact that bodies are resolutely located in space, there are grounds for suggesting that scientific knowledge is always positioned knowledge; rationality, always situated rationality; inquiry, always located inquiry (Lawrence & Shapin, 1988).

Scientific knowledge, of course, is not just incarnated in bodies; it is also located in buildings. And the role of scientific buildings in the building of science has attracted considerable interest (Gieryn, 2002). There is, for example, compelling work on such themes as the struggles over the arrangement of exhibition space, the Gothic revival style of architecture used for Victorian natural history museums as part of a cultural struggle to professionalize science, the layout of laboratories to facilitate preferred social interaction. Whatever the particulars, these concerns have served to underscore the importance of place in what might be called the architecture of science (Galison & Thompson, 1999).

Finally, the tracing out of the geographies of science has fostered increased interest in another set of material entities—objects. Whether it is human tissue, fossil specimens, geological samples, cultural artifacts, or plant species, scientific knowledge is bound up with objects that move around the world from site to site. The cartography of these dynamics at once maps physical *and* conceptual movement. Hill (2006a), for example, has traced the shifting meaning of medical objects gathered by Henry Solomon Wellcome as they traveled from their point of origin in local indigenous cultures via a medical collection to the Fowler Museum in Los Angeles. In transit, they shifted from being everyday articles to exemplars of ethnological history to works of primitive art (Hill, 2006a, 2006b). Similarly, Dritsas (2005) followed the trail of the freshwater mussel shells collected by John Kirk during the Zambezi Expedition of 1858–1864 from southeastern Africa to Philadelphia via London and thereby disclosed how the movement of these objects was critical to the warranting of zoological knowledge. This geographical trajectory was both a physical migration *and* a conceptual journey from "the farthest empirical and geographical peripheries into the metropolitan knowledge system" (Dritsas, 2005, p. 49).

Having noted some of these recurring themes in recent work on the spatiality of science, I shall now try to move the debate forward a little more by reflecting on four particular spatial themes that, in my view, might be more prominently integrated into the geography-of-knowledge project than they are at the moment.

Landscape Agency

As a consequence of my expressed unease at the deterministic cast of Dorn's *Geography of Science* (1991), some commentators have concluded that the geography of knowledge that I have promulgated is too resolutely culturalist and consequently fails to accord any role to the natural world in the shaping of cognitive claims about it. Dorn's account, let me remind you, very largely congregated around a Wittfogel-style narrative that attributed the development of science to the effects of those societies requiring hydraulic management and, thus, technoscientific initiative. To explain the development of science, Dorn looked to "soil, climate, hydrology, and topographical relief, and to demographic fluctuations, latitude, and the differences between sown fields, steppe, and desert" (p. xi). Not surprisingly he found that the writings of the U.S. geographer Ellen Churchill Semple still "remained fresh" (p. xii) and insisted that Ellsworth Huntington's environmental determinist "thesis was never really refuted" (p. xix). Now it is indeed the case that I remain profoundly uneasy with this form of ecological constructivism. But this stance should not be taken to mean that I believe physical landscapes exert *no* influence on the production of scientific knowledge. Keeling (2007) contends that my use of place has been overdetermined by a focus on the "cultural, social, and even textual spaces of scientific endeavour" (p. 405). "This emphasis on the cultural and social geographies of knowledge production," Keeling goes on, "privileges the

representations and practices of scientists, devoting rather less attention to the natural spaces and phenomena which they study and engage" (p. 405). There may well be something to this claim, but I do want to allow for the agency of landscape in the production of at least some forms of scientific knowledge.

Of course, before pushing the matter of the agency of nature very far, one needs to consider in a much more sustained way the nature of agency. Traditional conceptions of agency have tended to converge on the critical importance of intentionality as the motor of history. Essentially, this formulation has restricted agency to the operation of the human. Resisting this understanding, various efforts have been made to liberate agency from human captivity. Latour (1999), for example, speaks of dispersing agency across the human *and* nonhuman worlds in networks of actants, thereby democratizing it in a radical sense. Ingold (2000) has sought to recast the human agent as already inescapably embedded in the world and not abstractable from it (see also Nash, 2005). I do not intend to adjudicate here on the ontological status of Latourian actants or on the persuasiveness of Heideggerian-sounding proposals about being-in-the-world. Philosophical interrogation is not my quarry. My concern is decidedly more modest. It is simply to allocate to landscape some role in setting limits on what observers can coherently say about it. I want to allow space for the thought that nature has some part to play in the theories that are constructed about it.

Such moves open up the possibility of reflecting in one way or another on the role of landscape in the generation and circulation of scientific knowledge claims. In this connection, it is suggestive to consider the ways in which natural historians who conducted research in Arctic landscapes responded to Darwin's theory—which was born of field observations in the temperate and tropical worlds. For Darwin the abundance and hyperfecundity of tropical nature was so overwhelming that he felt that describing it to an untraveled European was like trying to convey the experience of color to a blind man (Martins, 2000). Writing to a friend in August 1832 on the luxuriance of the Brazilian vegetation, he mused: "it was realizing the visions in the Arabian nights—The brilliancy of the Scenery throws one into a delirium of delight" (Darwin to Frederick Watkins, 18 August 1832, as quoted in Burkhardt & Smith, 1985, p. 260). As for the temperate world, it was his field experiments in the landscapes around Down House that furnished him with critical data. "My observations," he told the botanist Joseph Dalton Hooker in June 1857, "though on so infinitely a small scale, on the struggle for existence begin to make me a little clearer how the fight goes on"[1]. Recall, too, Wallace's (1878) observation that it was only in the equatorial latitudes that "a comparatively continuous and unchecked development of organic forms" had taken place and thus that in those regions "evolution has had a fair chance" (p. 122).

In the world of the Russian Arctic, things were different. Working in conditions where nature displayed no plenitude, no superabundance, no swarming life forms, the vocabulary of overpopulation and struggle between species just did not seem right. In that environment there developed a tradition of evolutionary zoology emphasizing cooperation in which the Malthusian components of natural selection

were systematically expunged. As Peter Kropotkin later summarized the work of the St. Petersburg naturalists who carried out their inquiries in the Siberian wilderness and the Russian steppes, "*We see a great deal of mutual aid,* where Darwin and Wallace see *only struggle*" (as quoted in Todes, 1989, p. 104).

The Canadian north provides a useful comparator. Although there was a signal absence of response to Darwin among Canadian practitioners of geology, several botanists broached the subject during the early 1860s (Berger, 1983). The dominant motif in their endeavors was the fundamental significance of struggle against the vicissitudes of a harsh landscape. But, as in Russia, it was not struggle between species; instead it was struggle against an unyielding Precambrian shield. Success required inherited modification, and the idea of environmentally induced adaptation and acclimatization was resorted to so as to account for vegetational patterns. Such circumstances encouraged the agricultural reformer William McDougall to suggest in 1854 that the Lamarckian principle of the inheritance of acquired characteristics provided a viable explanation. Yet Darwinian language was embraced to a much greater degree in Canada than it was in Russia. George Lawson, for example, found J. D. Hooker's Darwinian account of arctic biogeography compelling, and George Dawson happily resorted to Darwinian vocabulary in his 1878 anthropological studies of the Haida people of Queen Charlotte Islands (Zeller, 1999).

Both landscapes, I venture to suggest, had some role to play in the production of the scientific theories that were constructed about them.

Political Ecology

Of course, the agency of landscape, as I have hinted, is neither monocausal nor unmediated. Rather, it is inflected in complex ways by what I call the political ecology of science, the view that nature is inescapably read through the lens of cultural politics and that the knowledge claims that manifest themselves in particular settings are the compound product of nature's agency *and* cultural hermeneutics. Latour (2004) has promoted the term 'political ecology' to argue against the conventional bifurcation between nature and culture and to urge their recasting into a new "collective" that "accumulates the old powers of nature and society in a single enclosure before it is differentiated once again into distinct powers" (p. 238). The ontological rearrangements that Latour envisages are intended to decompose nature as a specific sphere of reality—whose existence was always in any case a political constitution—and to reconceive of humans and nonhumans alike as members of "an assembly of beings *capable of speaking*" (Latour, 2004, p. 62). This simultaneous dissolution of "the social" and "the natural" and the surfacing of mediators through which forces act are, as I understand I, what Latour means by the "sociology of translation" (Latour, 2005, p. 106)—his preferred designation for what has become known as actor-network-theory (p. 106). These proposals clearly resonate with those who have been engaged in the task of complicating the assumed boundary

lines between human and nonhuman. As Sarah Whatmore (2002) characterized it, as she inaugurated her project on "hybrid geographies," the "forays" that practitioners in the social sciences and humanities make "into the domain of natural sciences have swelled, so a plethora of 'things' has been trespassing into the company of the social[,] unsettling the conduct of its study" (p. 1).

The political ecology of science that I conceive is less metaphysically ambitious, less concerned with the epistemology of agency, less oriented toward taxonomic rearrangement than these proposals. Rather, it is intended to highlight the ways in which scientific knowledge of the natural world is politically constituted in different ways in different settings. In the Russian case that I have just reviewed, for example, the particular construction of Darwinism that crystallized there was at least in part the product of a landscape hermeneutic shaped in dialogue with Malthusian demographics. Both on the political left and right in Russia, Malthus's atomistic conception of society had already been castigated, mostly since the 1840s, as a cold, soulless, and mechanistic product of English political economy. Malthus may have rationalized poverty and inequity in England, but his commentators were certain that his theory would not apply in a harmonious Russia. It ran foul of Russian visions of a cohesive society that would jeopardize the cherished peasant commune (Todes, 1989). Indeed, according to Shaw and Oldfield (2007), the very understanding of what landscape is was construed in Russia in very particular ways reflecting political and ideological preoccupations.

In Canada the relative lack of response to Darwinism during the 1860s, 1870s, and 1880s sprang in part from an ingrained Baconianism that prioritized collecting and classification over theoretical speculation. The absence of public controversy as indifference turned into advocacy had much to do with the ways in which religious leaders found it possible to incorporate evolutionary thinking into their progressivist conceptions of historical change. In addition, its later adoption was bound up with romantic nationalist notions of "the north as a source of liberty, physical strength, and hardiness of spirit" (Zeller, 1999, p. 99). Darwin delivered a scientific framework that could feed the nation's resolve to overcome its harsh environment and mold a race fitted for survival. As Zeller (1998) puts it, "inhabitants of northern lands somehow acquired the mental and physical hardiness that destined them to thrive there. Biogeographical theories anthropomorphized northern forms ... that successfully 'invaded' southerly lands and moved in as 'denizens'" (p. 27).

The political ecologies of science manifest themselves in other ways, too, not least through the role that cultural politics play in shaping scientific knowledge and its circulation. Let me illustrate. In the decades around 1800, scientific speculations about human origins were massively freighted with political cargo. The debate between the Scottish jurist Lord Kames and the American moral philosopher Samuel Stanhope Smith is illustrative. Kames (1774) brought the full weight of his scholarship to bear on the question of the role climate played in racial differentiation. In his view, Montesquieu's resort to climate as the explanation for human variation was simply mistaken. As he read the record of the human past, he readily came to the conclusion that climate did not make human varieties; rather, human

varieties were made for different climates. The implications—hesitant though he seemingly was to adopt them—were plain: human diversity was primitive, not derived, and different races were fitted for particular places. Such a conclusion was deeply troubling to Smith, whose *Essay on the Causes of the Variety of Complexion and Figure in the Human Species*, which first appeared in 1787 and then in an expanded form in 1810, had Kames in its crosshairs from the start. To Smith, polygenism was obnoxious scientifically, religiously, and—most critically—politically. Why? He spelled it out clearly in the 1810 edition of the work: "that the denial of the unity of the human species tends to impair, if not entirely destroy, the foundations of duty and morals, and, in a word, of the whole science of human nature. No general principles of conduct, or religion, or even of civil policy could be derived from natures originally and essentially different from one another" (Smith, 1810/1965, p. 149). In a setting where the new nation had overturned monarchy, inherited privilege, and established religion as the grounds of civic authority, universal human nature was the only foundation on which an orderly polity could be erected. In the early days of the new American Republic, a confidence in a common human constitution was precisely the philosophy that was needed if public virtue was not to dissipate. The realities of American geopolitics thus profoundly informed Smith's response to Kames's palaeoanthropological proposals (see Livingstone, 2008).

Print Culture

In the wake of the great flurry of interest in the history of the book, evidenced particularly in Johns's monumental *The Nature of the Book* (1998), there has been a growing recognition that there is a spatiality as well as temporality to textual productions of all kinds. The task of "bringing geography to book," to use James Ryan's neat phrasing, is opening up a host of critical questions revolving around print culture and the production, consumption, and circulation of knowledge (Ryan, 2003). As I have pointed out elsewhere (Livingstone, 2005), a suite of multidimensional geographies potentially manifest themselves, including the charting of the material spaces of book production, the distributional networks of mass print, the cultural topography of book buying, and the social morphology of lending libraries. And, of course, print culture extends far beyond the history and geography of the book. As Ogborn (2007) shows, many different modes of writing were implicated in the making and maintaining of the English East India Company. Heraldic manuscripts, political pamphlets, stock listings, official regulations, and many more were implicated in the construction of economic, governmental, and trading knowledges (Ogborn, 2007). Scientific enterprises are no less characterized by textual mulitiplicity; conventional published findings take their place alongside what Jardine (2000) calls "routinely authored works—instrument handbooks, instruction manuals, observatory and laboratory protocols," all of which are basic to the regulation of empirical practices (p. 401; see also Topham, 2000).

My own interest in print culture has tended to revolve around the located nature of hermeneutics and what I call the geographies of reading (Livingstone, 2003b). The basic thought is that texts are differently encountered in different settings and, thus, as they travel their meaning is transformed by the venues in which readers find themselves. Consider first an example from the sphere in which the practice of hermeneutics originally emerged, the interpretation of religious texts. To a great many slaveholders in the antebellum American South, a plain, unadorned reading of the Bible seemed to sanction the slave system, and there was no need to turn to secular science or unorthodox readings of Genesis to support it. To them, such perfidious projects would only defile a laudable religiously sanctioned institution and weaken the foundations of southern patriarchal communalism. Scriptural arguments in support of slavery abounded (Genovese & Fox-Genovese, 2005; Stout, 2006). The clergyman George Armstrong pronounced in *The Christian Doctrine of Slavery* (1857) that abolitionism had sprouted from the infidel breed of philosophy that had inspired the French Revolution. The theology professor George Howe, in an unrestrained attack on the polygenist lectures of Josiah Nott, repudiated efforts to justify slavery in the language of biology, preferring instead the vocabulary of the Bible, under which ancient and modern slaveholders "lived, protected and unrebuked" (Howe, 1850, p. 487). Similar cases could readily be elaborated.

In the northern states, by contrast, many readers interrogated the Bible for its abolitionist possibilities. Some ferreted out passages mandating the release of slaves after a set number of years; others pointed out that proslavery theologies conveniently ignored practices like polygamy, which enjoyed Old Testament sanction every bit as much as slavery. Still others dwelt less on specifics than on making a generalized moral case against slavery's inhumanity.

What became clear was that there simply was no such thing as a politically neutral, straightforward reading of the text. Hermeneutics just *were* shaped by the cultural conditions and political stance of commentators. As Holifield (2003) argues, the slave issue precipitated a move away from what he calls a Baconian hermeneutic by introducing a historical consciousness that insisted on the need to locate biblical texts in the time and place of their writing (pp. 494–504). The American Civil War—as Noll (2006) perceptively suggests—precipitated, and was precipitated by, a hermeneutic crisis.

The reading of scientific texts, theories, and reputations is no less susceptible to located interpretations, as recent research has amply disclosed. Let me illustrate. Much of the inspiration for this work has come from Secord's (2001) analysis of the reading of the early Victorian "sensation," Chambers's (1844) anonymously published *Vestiges of the Natural History of Creation*. What emerges from this remarkable work is that the meaning of the text was shaped in profound ways by where readers were located. In Liverpool, for example, the text's potential for inspiring urban reform was seized upon by the Mechanics' Institution with its anticlerical leadership, whereas in the Anglican-dominated space of the City's Literary and Philosophical Society it was met with alarm. In other urban settings other microgeographies of reading surfaced (Secord, 2001).

Rupke's (2005) "metabiography" of Alexander von Humboldt demonstrates how the reputation of even a single scientific figure could be differently forged in different venues. At various points in German political history from late-Prussian times through the Empire Period, the Weimar Republic, the Third Reich, and the divided Germany of post-1949, Rupke shows how the identity of Humboldt was recreated to suit the political sensibilities of the moment. Hence, he pauses to consider Humboldt the liberal democrat, Humboldt the Weimar *Kultur* chauvinist, Humboldt the Aryan supremacist, Humboldt the antislavery radical, and Humboldt the pioneer of globalization. All these projections press Rupke to the conclusion that the reading of an author's reputation is a located enterprise. Humboldt "has become a man with several lives," Rupke writes, "products of appropriation on behalf of geographically separate and chronologically successive socio-political cultures" (p. 16).

I myself have sought to map out something of the geography of reading Darwin. The meaning of Darwinism, I contend, was locally constituted in very different ways in different places. South Carolina naturalists read its monogenist anthropology as subversive of the racial basis of old southern culture, whereas many New Zealanders welcomed it because of its potential to underwrite the runaway triumphs of a settler society happily wiping out the Maori. In Scotland the high profile public controversy over the biblical criticism unleashed by William Robertson Smith (1875) cast any threat from Darwin in the shade, and few oppositional readings are evident. In Ireland Darwin's *On the Origin of Species by Means of Natural Selection; or, The Preservation of Favoured Races in the Struggle for Life* (1859) was differently read by Catholics and Protestants, by reviewers in Dublin, Belfast, and Derry. The meaning of Darwin's text was differently construed, depending on attitudes to the controversial Belfast address of John Tyndall at the 1874 meeting of the British Association for the Advancement of Science, to Catholic stipulations about the university curriculum, and to what was thought to be the implications of Darwin's theory for the management of population. In every case what Darwinism *was,* was locally constituted (Livingstone, 2005).

Yet more recently, Keighren (2006) has sought to trace out a book geography through his examination of the reception of Semple's *Influences of Geographic Environment* (1911). While disclosing something of its differential reading in different sites, Keighren also attends to the conditions that shaped encounters with this key text—notably, how it was read in the early twentieth century for the ways in which it could deliver a scientific methodology for a subject seeking disciplinary identity and institutional esteem. There are hints, too, that how Semple's book was read was shaped by reactions readers had to hearing her speak. What is also clear is that, although Semple herself shunned the term "geographic determinant" and claimed to speak "with extreme caution of geographic control" (Keighren, 2006, p. 530), she was routinely cast as an environmental determinist. Whatever she intended to communicate, readers persistently took her to be saying something else. As Fischer (2003) has tellingly remarked, "The wonder in reading is that the writer is never in control" (p. 344).

All of these instances serve to highlight the instability of scientific meaning and to demonstrate that, although texts may be immutable mobiles (though that proposal

is itself doubtful), their meanings are entirely mutable. No single uncontested meaning can be distilled from them. The implication is that scholars seeking to come to terms with the spatiality of scientific knowledge must engage more intensively with the geographies of print culture than they have in the past. This reorientation will also involve taking seriously *literal* translation as well as the *metaphorical* translation of meaning that goes on across "contact zones" of one sort or another. As Elshakry (2008) points out in an analysis of the cultural politics of late nineteenth-century scientific translations into Arabic, the whole notion of knowledge in motion is rendered yet more problematic when translation and transliteration are involved. As she puts it, "the specific problem of finding appropriate and meaningful lexical equivalents in cross-lingual scientific discourse" (p. 702) is a dilemma of very considerable proportions "for understanding the geography of knowledge" (p. 703) because it involves "questions of linguistic tradition, cultural purity and modernity itself" (p. 702) all played out "against the background of colonial rule and its resentments" (p. 702). Translations into Arabic of the very term *science* were profoundly implicated in the politics of language because they abutted on matters of "cultural authority, social change, and literary tradition" (p. 705).[2] When one realizes that there were no immediate Arabic equivalents for terms like *race*, *species,* and *evolution*, the dilemmas multiply alarmingly for translating a work like Darwin's *Origin of Species* and dramatically bring to the fore what Clifford (1988) pertinently refers to as the "politics of neologism" (p. 175).

Speech Space

I want finally to turn to a fourth theme that has, in my view, been only patchily developed in accounts of the geographies of scientific knowledge—spaces of speech. My own interest here lies in the connections between what I call location and locution, that is, on the ways in which settings both enable and constrain spoken communication (Livingstone, 2007). But there are other routes into this inviting zone. The remarkable range of sites of scientific conversation is itself worthy of scrutiny. Alongside learned societies like the Royal Society or the laboratories that gentlemen naturalists had constructed in their own homes, the new scientific conversation of the seventeenth and eighteenth centuries spilled out into public houses and drawing rooms, coffeehouses, and parlors (Fara, 2004; Terrall, 1996; Walters, 1997). In the kitchen Joseph Addison observed during a visit to one particular household, he found women discoursing on the usefulness of mathematical learning (Meyer, 1995, p. 24). For the twentieth century, a good deal of work has focused on the discourse of lab technicians in an effort to show how science is made in particular settings (Latour & Woolgar, 1979; Traweek, 1988).

As for the Victorian period, Secord (2007) has begun the task of depicting the rich array of venues in which scientific conversation took place. He calls to mind the fundamental importance of speech in an oral culture where verbal presentation took precedence over scientific print, and of the rich array of sites where scientific

conversations could take place—at high table, bedsides, scientific societies, dining clubs, soirees, and tea rooms. He provides rich description of what might be called conversation management as hosts consulted etiquette manuals for suitable topics for polite discussion and ways to prevent fashionable table talk from degenerating into vulgar shoptalk. Some subjects, like botany, were "in"; others, like mathematics or phrenology, were "out." Indeed, even in elite scientific spaces, Secord insists that verbal communication "remained central to the presentation of new scientific work" (p. 30).

In this vein, I want to dwell on the ways in which social spaces both shape, and are shaped by, speech. What can and cannot be said in particular venues, how things are said, and the way they are heard are all implicated in the production of knowledge spaces. In different arenas there are protocols for speech management; there are subjects that are trendy and subjects that are taboo. In public spaces and in camera, in formal gatherings and in private salons, in conferences and consultations, in courtrooms and churches, in clinics and clubs—in all these venues different things are speakable (and unspeakable) about scientific claims. In every case the setting sets limits on what can be spoken; the social space conditions what is heard. And individuals moving between these spaces adjust their speech—code-switching I believe it is called—to suit the setting. In so doing, as Burke (2004) points out, they are "performing different 'acts of identity' according to the situation in which they find themselves" (p. 6). In other words, the control of speech space is intimately connected with the maintenance of identity. Spaces of speech, of course, are also spaces of silence. There are always voices that are absent, or are not allowed to speak, or are denied access. In colonial societies, as Scott (1985) powerfully reminds his readers, the oppressed can rarely let their voices be heard. No doubt for different reasons, but with not dissimilar effects, those people marginalized in scientific debates find their voices unwelcome in science's privileged sites.

Let me just touch on two kinds of speech space and the ways in which their elucidation might illuminate the geographies of scientific circulation. First, family space. A sensitivity about what could be spoken at home was something about which leading scientists sometimes reflected with their close associates. Thus J. D. Hooker mused in a letter to Charles Darwin in which, as Brooke (2007) puts it, "the social pressures for conformity were perfectly explicit" (p. 16):

> It is all very well for Wallace to wonder at scientific men being afraid of saying what they think . . .. Had he as many kind and good relations as I have, who would be grieved and pained to hear me say what I think, and had he children who would be placed in predicaments most detrimental to children's minds . . . he would not wonder so much. (J. D. Hooker to C. Darwin, October 6, 1865, as quoted in Burkhardt, 2002, pp. 261–265)

Such concerns easily spilled over into anxieties about how a wider public might set constraints on what one was comfortable saying. Darwin certainly felt such moral pressure. He found it "a fearfully difficult moral problem about the speaking out on religion" (Brooke, 1985, p. 40). This circumstance, of course, makes it extraordinarily difficult to ascertain just precisely what he *did* think about certain

subjects. For, as Day (2008) has observed, the "penchant for deliberate and sometimes dissembling cultural self-fashioning could be particularly conspicuous when religion was the subject of conversation" (p. 55).

In yet more public institutional spaces, how scientific claims were talked about required care. Alexander Winchell, who lost his chair at Vanderbilt University over evolution, had mused in an explanatory letter to the readers of the Nashville *American* on June 15, 1878: "I have always taken pains, in my lectures at Nashville, to avoid the utterance of opinions which I supposed were disapproved of by the officers of the University" (as quoted in Alberstadt, 1994, p. 108).[3] He evidently did not succeed in his tongue tactics. In Belfast, in the aftermath of the assault that the religious establishment had received from Tyndall's taunting speech at the British Association meeting of 1874, pulpits, platforms, and presbytery meetings talked about little else. The series of evening lectures that they organized in the city that winter marked out the hermeneutic horizon against which the theory was judged for more than a generation. The speech space that Tyndall helped crystallize set boundaries on what could be said and heard about Darwinian evolution, and the local almanac for 1875 railed against the "very bad taste" that Thomas Henry Huxley and John Tyndall exhibited in their recent addresses to the British Association. They had infringed oral propriety. As MacIlwaine (1874–1875) was at pains to point out at the Belfast Naturalists' Field Club at its winter session that year, talk of religious belief in a scientific setting was "a violation of the rules of good taste"; Tyndall's "reckless" incursion into theology and metaphysics at the meeting of the British Association was thus nothing short of "reprehensible" (p. 82). In Boston, the students of Louis Agassiz could talk about the new Darwinian theory only in secrecy for fear of their teacher's ire. Years later, Nathaniel Shaler reflected that to be caught in such conversations "was as it is for the faithful to be detected in a careful study of heresy" (as quoted in Livingstone, 1987, p. 28). Such concerns might help explain why, when many of Agassiz's students did become evolutionists, they turned to the Neo-Lamarckian version, which retained notions of inherent progress, rather than to the orthodox Darwinian model.

Indeed, high profile clashes, like the infamous altercation between T. H. Huxley and Samuel Wilberforce, Bishop of Oxford, at the 1860 meeting of the British Association for the Advancement of Science, cannot be understood, in my view, without attending to whether or not decorum was breached during the row. Matters of etiquette and good taste were certainly in the minds of some observers who reflected on the occasion. Frederic William Farrar, theological writer and later Canon of Westminster recalled that what the bishop said was neither vulgar nor insolent, but flippant, particularly when he seemed to degrade the fair sex by pondering whether anyone—whatever they thought about their *grandfather*—would be willing to trace their descent from an ape through their *grandmother*. In Farrar's opinion, everyone recognized that the bishop "had forgotten to behave like a gentleman" and that Huxley "had got a victory in the respect of *manners* and *good breeding*" (as quoted in Lucas, 1979, p. 327). And yet, although later writers placed Huxley on the side of good breeding, at the time both the *Athenaeum* and *Jackson's Oxford Journal* thought him discourteous (Lucas, 1979). The boundaries of civility shifted

over the decades. As White (2003, p. 65) puts it, Huxley's frankness "still seemed unruly and discreditable" in 1860, whereas Wilberforce's ally Richard Owen, who had "once seemed honest and polite, appeared disreputable and ill-mannered" by later standards. So ... did the bulldog bite the bishop, or did the bishop badmouth the bulldog (see Livingstone, 2009)? It all depends on the character of the speech space that summer afternoon.

It does not take much imagination to make the transfer to today. Whether the conversation is about genetically modified crops, global warming, stem cell research, intelligent design, the commercial use of bio-organs, cold fusion, laser-guided weapon systems, or even the social construction of scientific knowledge, interlocutors are usually well aware of the immediate speech space they are occupying and what would constitute a violation of its rhetorical decorum. All these are controversial subjects, of course, but it is not difficult to entertain the thought that partisans for some particular scientific perspective are only too happy to police conversational arenas to outlaw rival theories. To ascertain just what role speech spaces continue to play in the circulation of knowledge seems to me to be a promising line of inquiry.

Conclusion

Scientific knowledge is a geographical phenomenon. It is acquired in specific sites; it circulates from location to location; it transforms the world. As students of the spatiality of science have pursued their inquiries, conventional distinctions between the natural and the social, the local and the global, and the scientific and the political have been rendered more and more troublesome. At the same time, attention to the role of material objects like specimens and samples that trace out their own dynamic geographies as they move around the world is opening up new and fertile lines of investigation. In this chapter I have sought to further supplement the agenda for geographical studies of scientific knowledge and practice by calling attention to the role of landscape in knowledge enterprises, to the political ecology of science, to the critical significance of print culture in the circulation of scientific claims, and to the place—and places—of speech in scientific culture. My reason for doing so is that science shapes and is shaped by the physical world; science produces and is produced by cultural politics; science generates and is generated by textual encounters; science is made and remade by how it is talked about. Landscape agency, political ecology, print culture, and speech space, I contend, are fundamental to the ongoing task of illuminating the geographies of scientific knowledge.

Notes

1. Retrieved August 10, 2009 from http://www.darwinproject.ac.uk/darwinletters/calendar/entry-2101.html.
2. I am most grateful to the author for sharing her pre-published analysis with me.
3. Winchell's letter is reproduced *in extenso* in Alberstadt (1994).

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