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Geographical traditions, science studies, and biography: a conversation with David N. Livingstone*

MICHAEL HOYLER, TIM FREYTAG and HEIKE JÖNS

La Nature est un temple où de vivants piliers
Laisser parfois sortir de confuses paroles;
L'homme y passe à travers des forêts de symboles
Qui l'observent avec des regards familiers.

from Charles Baudelaire, 'Correspondances', *Les fleurs du mal* (1857)

Historiographies of geography, or, bringing science studies into geography

The geographical tradition?

It is almost ten years since the publication of your seminal book 'The geographical tradition'.¹ Your account of geography's story from the 'Age of Reconnaissance' to the late 20th century has received overwhelming acclaim, notwithstanding some critical voices about the title's use of the singular.² Why did you choose to refer to 'the' geographical tradition in this groundbreaking intellectual history of geography?

DL: *'The' geographical tradition?* Well, a number of people have queried that title because of their sense that we ought to speak of 'geographical traditions' in the plural. In some ways I think of this as a postmodern inclination to expunge the definite article. 'The' is out. Pluralising is in. So we talk about 'geographical imaginations', 'geopolitical traditions', 'histories of cartographies', and such like. To people with these inclinations, 'the' geographical tradition seemed hopelessly *jejeune*. At least that's how it seems to me when they insist on putting 'sic' after the 'the'. Still,

* Most of this conversation took place in the Heidelberg Castle gardens on 27th June 2001; it was continued two days later in the Studio of the Villa Bosch. The interview situation exposed us to a pivotal puzzle of science studies (and *Wissenschaft* more generally): the correspondence between world and words. To be turned into text, this exchange had to undergo several subsequent transformations: recording (contra stubborn technical devices), transcribing (with the help of Katharine Hoyler and Hinrich Helms) and editorial sculpting (of hyper-mobile pieces). However, all transformation is transient: it continues through your – the reader's – encounter with this text.

¹ David N. Livingstone, *The geographical tradition: episodes in the history of a contested enterprise* (Oxford and Cambridge, MA: Blackwell, 1992).

² For a brief summary of book reviews in over 20 geographical and history of science journals, see Charles W.J. Withers, 'Conversations in review', *Ecumene* 3 (1996) pp. 351-60, which includes two critical appraisals, by Jane Camerini and Michael Heffernan, as well as a reply to both by David Livingstone. See also the discussion under the general title 'Geographical traditions: rethinking the history of geography', in *Transactions of the Institute of British Geographers*, NS, 20 (1995) pp. 403-22.

at one level, I guess, such criticisms are fair enough insofar as they are seeking to forestall closure, to let other voices be heard, and to prevent some hegemonic and ‘invented tradition’ monopolising the conversation. But I was never claiming that my story of geography’s history was *the* history of the *only* geographical tradition. In fact I said this quite explicitly in the first chapter.³ There are many geographical traditions and heritages, not least in parts of the world to which I do not have linguistic access.

So, why ‘the’ geographical tradition? Several things, I think, need saying here. What I was trying to argue in the book is, first, that geographers belong to a tradition of enquiry, and second, that that tradition is an essentially contested one. On the first point it seems to me that Alasdair MacIntyre is exactly right when he observes that any tradition is ‘an historically extended, socially embodied argument’, and an argument precisely about what constitutes that tradition itself.⁴ As a means of conducting intellectual history, MacIntyre sets off his concern with tradition against both Enlightenment encyclopedism and neo-Nietzschean genealogy, and I think there’s a lot to that. As for the contestation element, I think it was W.B. Gallie’s famous article on ‘essentially contested concepts’ that laid the philosophical groundwork for the cogency of speaking of ideas at whose very core was definitional ambiguity.⁵ You might profitably think of ‘science’ as such a concept, or ‘Enlightenment’, or ‘art’, or ‘the Scientific Revolution’. These practices or epochs simply defy any singular denotation. ‘Geography’, it seems to me, is yet another. Of course you can adopt a stipulative definition and say, when I used the word ‘geography’ I mean this or that. But that doesn’t do any real historical work for us.

By using the idea of geography as a temporally extended and culturally incarnated tradition of enquiry, I thought that I was able to get some purchase on at least the histories of academic projects in geography. The aim was to try and get a handle on geography as a more or less incoherent, contested, disparate, pluralised, tradition into which generations have been socialised. To be sure, the subject’s practitioners have queried many of the traditional methods they were taught and many of the traditional concepts that have been in use. We have, for example, had telling interrogations of

³ For example: ‘Let me emphasise at the outset that this book is not in any sense ‘the’, or even ‘a’, history of geography. Rather, I will proceed by identifying what I take to be some of the key chapters in the history of geography since 1400’ (p. 3). And again: ‘my account is biased. It is biased towards what I think of as important epochs. It is biased towards English-language geography, not because other traditions are unimportant, but precisely because they are too important to continue to be parodied in traditional textbook fashion. I simply do not know enough about these traditions to begin the task. For they too must be contextually interrogated and retold’ (pp. 30-31).

⁴ Alasdair MacIntyre, *After virtue: a study in moral theory* (London: Duckworth, 1985) p. 222.

⁵ Originally published by the Aristotelian Society in 1956, ‘Essentially contested concepts’ is reprinted in W.B. Gallie, *Philosophy and the historical understanding* (London: Chatto and Windus, 1964) pp. 157-91.

the seeming value neutrality of quantification, of the installation of a gendered epistemology in fieldwork, of the necessarily fictionalised character of all cartographic projects, and so on. But it seems to me that any critical transformations these reconceptualisations induce are only possible from *inside* a tradition of enquiry. And that is precisely what a living tradition is. It is an argument over the intellectual and ethical ‘goods’ that are enshrined in the tradition itself. *The geographical tradition* was not an attempt at deliberate exclusion or silencing, though of course one is always engaged in an unconscious silencing just by virtue of one’s own lack of knowledge or perception or insight. What I was really trying to argue was that any tradition of enquiry which is vital, including geography, is a tradition which is in dialogue with itself, about its cognitive content, its practices, its mode of operation, its assumptions, and indeed its exclusions. That is why I call it a contested enterprise or a contested tradition – there is no essential core that remains unchanging or inviolate across periods of time.

Another thing I need to say about the book is that my focus was pretty largely on the history of geography as an academic or quasi-disciplinary project. Perhaps this was too narrowly conceived. Of course there are many things that a fully-orbed history of geographical sensibility should embrace that never really surface in *The geographical tradition*. For example, while my account was largely in the mode of intellectual history, others have begun to unravel remarkable social histories of popular geography, of geographical textbooks, of the entangled webs of geographical knowledge, power and travel. Similarly we are in need of more work on geography and the practices of government, on the intimate connections between the growth of geographical knowledge and missionary enterprises of many sorts, on the deployment of geography in theatrical displays. All of these, and many many more, are important and significant lines of enquiry. They simply did not come within my purview when I was writing the book in the late 1980s, but I wasn’t trying to exclude them in any way at all. And maybe I should add that I did not treat physical geography as seriously as I should have done. Perhaps I am trying to begin to make up for this by the sort of work I talked about in my Hettner lecture on tropical climate. Anyhow, a social history of physical geography is a real *desideratum* and I hope someone will take it forward. It’s time that Gordon Davies’ wonderful history of early geomorphology, *The earth in decay*, was redone in the light of the sociology of scientific knowledge.⁶

There are clearly also histories of non-western geographical traditions to be registered, and some of my critics have not hesitated to point out that European and American geography has never been insulated from a wider global context. To be sure, medieval Islamic geography and cartography fed their way into the west; Jewish

⁶ Gordon Davies, *The earth in decay: a history of British geomorphology 1578 to 1878* (London: MacDonald, n.d.).

mathematical practices constituted a very significant intervention; and of course there were rich Chinese geographical traditions. I definitely should have acknowledged all of these much more than I did, and if I ever do a second edition I shall want to pay special attention to them. But there is the question of language and the problems of getting to grips with a culture without serious linguistic competences. So I was, and remain, hesitant about succumbing to the gestural politics of mere tokenism.

As for geographical traditions in the modern world, I think there are important stories to be heard about contemporary Asia and Latin America, for example; but it was a big enough task for me to try and say something sensible about five hundred years of geography in the west without risking yet further problems. Does this begin to answer your question about the definite article?

Critical historiography vs. hagiography

You have just focused on the critique of your book, but what seems to us to be far more relevant is the new way in which you have tried to do a historiography of geography. So how would you distinguish your approach from more traditional ways of writing the historiography of geography and why is it necessary in your opinion to write different accounts from those that had previously been available?

DL: Most of the histories of the subject that were available at the time I started thinking about this project in the mid-to-late 1980s were internal disciplinary narratives which did not place geographical work in any broader context. They were internalist and they were also typically some form of geographical apologetics: the people writing these histories had a very definite conception of what they felt geography was, or what geography ought to be, and history became a means of legitimating a very particular stance on the nature of the subject. So if people thought of geography as fundamentally a science of regions or the study of occupied space or the elucidation of the cultural landscape, then their histories were written in a self-justificatory way. They talked of blind alleys or deviations from the true course of history, and such like. Of course these histories were themselves located – for example, in the midst of struggles over academic turf at a time when geography was seeking some distinct intellectual space in the disciplinary division of labour. Whichever, all of these started with the very thing that I wanted to query, namely, that there is a clear, transcendent, definition of what constitutes ‘geography’. Coming up with a definition and then writing a history that justified it just seemed to me the wrong way of going about the job. Better to let the past have its own definitions of what it understood geography to be and work from there. My project, then, was fundamentally an anti-essentialist one.

By this time historians of science had long departed from hagiography and had broadened their concerns beyond the ‘canonical’ high points in scientific history – the Copernican revolution, Cartesian mechanics, the Newtonian world system and so on. Such progressivist history, driven as it was by epistemological preoccupations, had largely been played out, and as the history of science became professionalised, the perceived need to mobilise history to shore up scientific culture itself began to fragment. Insights from sociology and anthropology broadened the range of questions to be asked, and the subjects that historians of science scrutinised spiralled well beyond what early practitioners thought of as classical science. A lot of my own historiographical inspiration was derived from this source, and I spent a good deal of time reading how historians and sociologists of science contextualised scientific practices and domesticated them in the cultural, political and intellectual frameworks within which they had developed. At the same time, the social historians of science were querying the seemingly obvious distinction between *internal* cognitive content and *external* social context. It had become clear that the effort to segregate ‘internal’ from ‘external’ matters was misapplied effort. It was much more a question of mutual constitution.⁷ By now, of course, there were related moves taking place within geography itself. Geographers were becoming interested in social theory, for example, and these engagements raised similar philosophical questions about the status of knowledge. These resonated with my own concerns even though the discipline’s social theorists were not particularly concerned with the history of geographical ideas and practices; they were probably more preoccupied with issues in geopolitics, matters of reflexive political commitment in the subject, and the theoretical groundwork involved in reconceptualising spatiality. Anyhow, my initial inclination was to try and bring the kinds of questions that intellectual historians were asking into the project of writing the history of geographical knowledge and practice. For me this revolved around the need to connect the geographical tradition to the prevailing cultural circumstances at particular points in time and in particular spaces. I wanted to argue that the question ‘what is geography?’ was incoherent, and that one always had to add temporal and spatial modifiers. Namely, ‘what was geography in Elizabethan England, or Jeffersonian America, or Enlightenment France, or Weimar Germany, or inter-war Britain?’ So, in a sense, I was trying to write up episodes that would contribute to a historical geography of geographical knowledge.

⁷ See, for example, the work associated with two major schools in the history and sociology of science, the Edinburgh school (e.g. Barry Barnes, David Bloor, Donald A. MacKenzie and Steven Shapin), and the Bath school (e.g. Harry Collins and Trevor Pinch). Important case studies are included in Barry Barnes and Steven Shapin (eds.) *Natural order: historical studies of scientific culture* (Beverly Hills and London: Sage, 1979); and Karin Knorr Cetina and Michael Mulkay (eds.) *Science observed: perspectives on the social study of science* (London: Sage, 1983). See also Steven Shapin and Simon Schaffer, *Leviathan and the air-pump: Hobbes, Boyle, and the experimental life* (Princeton: Princeton University Press, 1985).

When you look back now on the past ten years, how has the historiography of geography developed during that period?

DL: Dramatically, I think. And in some very exciting ways. In fact I think I would go so far to say that some of the most interesting and stimulating and fruitful ideas within human geography in general have actually arisen from those interested in the nature of geographical knowledge and its deployment for particular interests. These have helped to make the discipline much more self-questioning about its practices, whether they are to do with cartography or field work, whether they are to do with quantification or surveying, whether they are to do with classification or representation. In all of these, taken-for-granted assumptions about their capacity to deliver value-neutral, objective knowledge have been shattered. So here is a case where historical work turns out to raise significant epistemic questions of contemporary relevance. It is a mark of the subject's maturity, I think, that remarkably sophisticated histories are now available of, for instance, the reciprocal connections between geographical knowledge and institutional setting, the role of geographical texts in a school environment as a vehicle for promulgating a colonial mentalité, the deployment of geography in the cause of national identity, the mobilisation of geography in the courtroom, on the stage, in the exhibition hall – indeed in performances of all kinds, the extent of geography's complicity in the politics of race. These studies, it seems to me, and others like them, have delivered new and challenging histories of the practices that geographers have for so long simply taken for granted. And this body of work can't be relegated to the archive or the ivory tower or just to those with an antiquarian interest in the history of ideas. For it necessarily raises questions about our own disciplinary identity. It renders unstable the practices we have canonised. And it demands reflexivity on the part of each one of us. I consider all these to be good things. So the changes in geography's historiography, I would argue, have profound implications for the conduct of the whole discipline.

Writing historiography

The fact that most of these new studies have emerged in the 1990s, do you think that has to do with a kind of distance in time to what is being studied, colonialism, for example, imperialism, in the German case the Nazi period?

DL: It is always easier, at least relatively easier, to try and take the temperature of a past era than to get the pulse of the period in which you are enmeshed right now. I suppose there are simple things here: the availability of archival resources and accessibility to documentary materials. There is somehow less 'noise', as it were, in the past, and therefore the possibility of making some sense of what William James

(referring, if memory serves, to the infant mind) once called the ‘blooming buzzing confusion’. But also, and I think this is actually quite important, a little temporal distance can, as it were, reduce political heat. What I mean is that some subjects in the contemporary world are so biting controversial that it is difficult to engage in those without temperatures rising very quickly. Passions can easily be aroused. But when you address such subjects historically, you can begin to discern something of their complexity, ambiguity, and contradictions. And you can examine controversial subjects rather more dispassionately. If, in moments of self-reflection, it suddenly dawns on you that the same sorts of things actually might be happening right now, that’s all to the good. In this connection, I think that geography, like anthropology, for a long time was embarrassed by its profoundly imperialist genealogy and the ways in which it was used to bolster a variety of racial schemes and colonial policies. With historical distance, it becomes possible to look our own past square in the face, and then to raise questions that are of immediate contemporary relevance. I suspect the same can be said for accounts of German geography under the National Socialists.

But having said that, a historiographical dilemma lies waiting in the wings. It is presentism and I have been critical of it as a cardinal historiographical error. Basically it means writing history backwards by reading current debates into the past or by using history to justify some contemporary state of affairs. Many conventional histories of geography presented the story of the discipline which revealed it as leading inevitably to some perceived present-day orthodoxy. Clearly this is a basic mistake. And yet there *is* a sense in which presentism is unavoidable. For a start, we live in the present and its horizon inevitably informs how we engage the past. Gadamer, it seems to me, is absolutely right here, for in our hermeneutic conversations with the historical or spatial ‘other’ we just can’t exit the present. Imposing ‘our’ present on the other is, of course, hermeneutic bad manners, and that’s what much presentism really is. But escaping the present is impossible. We speak the language of today, and that is the only language that is available to us. So in one way or the other, the present always impacts itself on the histories we write. So there is a sense, then, in which revisiting and re-envisioning historical episodes is a kind of history of the present. Histories of disciplinary practices, for example, are also of contemporary relevance. When we begin to unpack, say, the social history of statistical methods – the sort of thing that Ted Porter has tellingly done⁸ – that makes us less gung-ho about the deliverances of quantification and more cautious about the use of numbers. The same can be said about field techniques, the generation of data sets, map production, and so on. So, in one sense, historical work is necessarily presentist. It shows that normative practices in a discipline’s intellectual arsenal have

⁸ Theodore M. Porter, *The rise of statistical thinking 1820-1900* (Princeton, N.J.: Princeton University Press, 1986). For an application to geography see Trevor J. Barnes, ‘A history of regression: actors, networks, machines, and numbers’, *Environment and Planning D: Society and Space* 30 (1998) pp. 203-24.

a history – that they are the product of historical negotiations – and that has inevitable implications for current practice.

Traditional historiographies, often in a hagiographic format, have perhaps contributed to a stabilisation of the discipline, have provided a sense of professionalism. To what extent is a critical historiography caught up in the tension between de-stabilising the subject's credibility and creating a – albeit radically different – sense of disciplinary identity?

DL: That is a very interesting point because it raises questions about the political positioning of the historian within any profession. And it identifies a significant difference, generally speaking, between historians of geography and historians of science. Nowadays, though much less so a generation ago, much history of science scholarship is produced by historians who are not practising scientists, who are not actively engaged in doing physics or chemistry or medicine. They are not usually members of university science departments. But the history of geography is still mostly carried out by members of the geography profession. And I think it's the same with the history of anthropology and sociology. So people like myself are positioned differently with respect to the history of the things that we write about than a historian of science who is in a philosophy, or history, or history of science department. Now this location in the academic division of labour leads to a set of questions about precisely what one is doing by interrogating, perhaps subverting, the 'icons' of a tradition – the idols of the tribe as Bacon put it. And by 'icons', I don't just mean the discipline's perceived 'heroes'; I also mean the procedures and techniques that have been installed as customary disciplinary hardware. This may be exciting; but also de-stabilising. And this is what your question is driving at. Some doubtless think that the new critical historiography, unlike the old celebratory hagiography, is really undermining the nature of the entire enterprise. I just think this is mistaken. In my view it is a mark of any tradition's intellectual maturity and vitality (to return to MacIntyre's earlier remarks) to be able to take seriously its own history. Rather than being worried that these ventures damage the enterprise, I think they actually enrich it by fostering critical dialogue, self-consciousness, and a greater sense of responsibility for the geographical knowledge we promulgate.

Would it make sense to write about contemporary practices within the discipline, interview the 'icons', for example, or should one focus on the use of archival material?

DL: Of course there is no necessary conceptual tension between doing both of these kinds of activities. Indeed I think that one of the lessons we have learnt from science studies is that one can look at a contemporary practice and treat it in the same way as anthropologists, entering an unfamiliar cultural group, record and try to make sense

of the practices and behaviours they encounter.⁹ Some of these studies of ‘the tribes of scientists’, as they have been called, have been extraordinarily illuminating. And I am sure that this is because Clifford Geertz was entirely right when he observed that if you want to know what a particular science is all about, you should turn first not to its theoretical declarations or its empirical materials, but to what its practitioners actually do.¹⁰ Having said that, I suspect that ten years or twenty years later, if a historian then came to write the histories of the practices that the anthropologist of science had investigated, that account would be very different. Indeed conducting such a ‘longitudinal’ study would be a very interesting exercise. As it is, I think 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-scientist and the field-worker. If such studies were carried out now, these analyses could later be compared with how a future generation of historians interpret those self-same practices. I suspect that the different temporal spaces of enquiry are going to make a profound difference to the very meaning of ‘practice’, to the questions that are posed, and to what passes as interesting answers. 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.

Spaces of science, or, bringing geography into science studies

Regional cultures of geographical traditions and intellectual life

We have talked about The geographical tradition, and from that moved on to the question of historiography versus hagiography and how we can write disciplinary histories today. You have stressed the multiplicity of traditions and voices, and that – to some degree – finds expression in your

⁹ Comprehensive anthropological studies of daily scientific practice were conducted by Bruno Latour and Steve Woolgar, *Laboratory life: the social construction of scientific facts* (Princeton: Princeton University Press, 1979) and by Karin Knorr Cetina, *Die Fabrikation von Erkenntnis. Zur Anthropologie der Naturwissenschaft* (Frankfurt am Main: Suhrkamp, 1984). For different approaches of post-Kuhnian science studies, see Andrew Pickering (ed.) *Science as practice and culture* (Chicago: The University of Chicago Press, 1992) and Mario Biagioli (ed.) *The science studies reader* (New York: Routledge, 1999).

¹⁰ Geertz put it this way: ‘if you want to understand what a science is, you should look in the first instance not at its theories or its findings, and certainly not at what its apologists say about it; you should look at what the practitioners of it do.’ Clifford Geertz, ‘Thick description: an interpretive theory of culture’, in *The interpretation of cultures: selected essays* (New York: Basic Books, 1973) pp. 3-30, on p. 5.

interest in regional cultures of science and regional cultures of geographical traditions. What kind of geographical traditions have entered into your own way of doing historiography – different national traditions like the German perhaps, people like Hettner, how has this strand come into your perspective, how is it taught, reproduced in the Anglo-American context?

DL: I think we are talking about two different things here, though they can be related in important ways. One is about the influence of regional structure on intellectual projects. The other is about regional geographical knowledges and how these have come into the Anglo-American tradition. Let me say something about the second one first. And it is – frankly – a confession. My impression is that in English-speaking geography, what is taught in courses on the history and philosophy of geography (however it is described) is highly Anglo-American centred. And they often only deal with very recent history. Even where such courses with a longer time-frame are delivered, I suspect that only a few emblematic cases from outside the Anglophone world are surveyed. So there might be a lecture on Hettner as a field scientist, say, though probably not on his later work on the history of geography. There might also be symbolic lectures on Vidal and the Vidalian tradition; there might be something on Ratzel and a little bit about Richthofen or Penck. But mostly I think where histories of geography continue to be taught, it is pretty much in the Anglo-American tradition. Indeed I have reason to suspect that even these emblematic cases are increasingly excluded from courses on the history and theory of geography as English-speaking curricula increasingly excise historical sensibility in the interests of transferable skills and claims to equip students for the market-place. Ironically, at the very moment when the cultural politics of the discipline are impressing on us the needs of greater cross-cultural engagement, perceived economic imperatives are pushing geographers in the direction of technique and technology.

It is interesting that even when you develop a critical stance within your own culture, there is the danger of being hagiographic about others.

DL: I think that is absolutely right, but again I would then urge that there are critical histories of non-English language geographical traditions to be written.

Some critical accounts have recently been published in Germany.¹¹

¹¹ See, for example, Ute Wardenga, *Geographie als Chorologie: zur Genese und Struktur von Alfred Hettners Konstrukt der Geographie* (Stuttgart: Steiner, 1995). For a collection of studies on the history of geography in Germany, see Ute Wardenga and Ingrid Hönsch (eds.) *Kontinuität und Diskontinuität der deutschen Geographie in Umbruchphasen: Studien zur Geschichte der Geographie* (Münster: Institut für Geographie, 1995). Information about other recent publications and ongoing research can be found at the website of the study group *Geschichte der Geographie* at <http://www.giub.uni-bonn.de/geschichte/homepage.htm>.

DL: Indeed. And there are, it has to be said, some comparable analyses by French and Spanish geographers using sociological perspectives. I've also seen some very stimulating work on Argentinian geography, and on other national traditions as well.¹² But the problem with the mono-lingual Anglophone world is that we are not as familiar with these works as we ought to be. So I think that you are absolutely right; generally speaking we are much more sophisticated, subtle and anti-hagiographic about our own culture than we are about others, at least in our teaching programmes. That's my impression anyhow though of course I may be quite wrong and my experience entirely idiosyncratic.

There is another point though about regional culture and intellectual life that I'd like to make. What I have become interested in recently is ascertaining how intellectual pursuits – and those of a scientific variety in particular – bear the stamp of regional culture. How this operates can take many forms. It might be to do with different channels and networks of communication, with different discursive styles or pedagogic customs or different religious traditions. Of course the lines of influence here are not all one way. Regional culture shapes science, even as scientific pursuits condition regional culture. This means, I think, that it makes sense to think of scientific rationality having a geography as well as a history. So, on the one hand, I am using the fact that science discloses regional particularities to query the idea that there is a single thing called 'science'; there are many scientific traditions and they manifest themselves differently in different regional situations. On the other hand, scientific enterprises have been actively engaged in shaping regional identities. Geography itself has been a crucial tool here – through its concern to compile resource inventories, through its interest in mapping the land, and through its promotion of surveying practices. Often it was performances of this class that produced in regions or nations a sense of their own identity. So there is a reciprocal relationship between region and science, and it seems strange to me that while geographers have long displayed an interest in regional patterns of agriculture or employment or disease or transport, and so on, they have shown little concern to cultivate what we might call regional geographies of science.

Locating science

We have talked about the historiography of geography and how science studies have influenced your contributions to the field. Your second project in some way has evolved from this encounter, actually turning it round, bringing a geographical approach into science studies.

¹² Some of this work appears in English in Anne Buttimer, Stanley D. Brunn and Ute Wardenga (eds.) *Text and image: social construction of regional knowledges* (Leipzig: Institut für Länderkunde, 1999).

DL: You're right. My early aim was to bring something of the historiographical subtlety, complexity and sophistication that operated among historians of science to the writing of geography's history. The direction of intellectual influence, in other words, was *from* science studies *to* geography. But then it struck me that there might be some potential in seeing if intellectual traffic might move the other way. That is, were there geographical perspectives and analytical tools that could be interestingly and appropriately applied to scientific enquiries?¹³ At the same time there were signs that historians and sociologists of science themselves were beginning to ask questions about the role of territory, sites, and spaces in the production and consumption of scientific knowledge. Several practitioners of science studies had already examined the role of particular sites in the generation of scientific claims. I am thinking here, for example, of Harry Collins' early work with the TEA laser.¹⁴ Later, Steven Shapin and Simon Schaffer published their remarkable account of Boyle's air-pump which included a map of the European distribution of air-pumps in the 1660s as part of a sophisticated analysis of the problems of experimental replication.¹⁵ And then Shapin himself published a couple of extraordinarily stimulating analyses of the laboratory as a scientific site.¹⁶ Work of this sort found reinforcement from the philosophical writings of people like Joseph Rouse.¹⁷ More recently Tom Gieryn's, Emily Thomson's and Peter Galison's interest in the architecture of science has opened up intriguing questions about the reciprocal relations between the buildings of science and the building of scientific knowledge, so to speak.¹⁸ And of course Bruno Latour's actor network theory and his ideas about 'immutable mobiles' and 'centres of calculation' display profoundly geographical motifs. There was also a conference some years ago specifically on territorial themes in the history of science¹⁹ and I was instrumental in bringing Shapin to speak on this general theme at the annual

¹³ David N. Livingstone, 'The spaces of knowledge: contributions towards a historical geography of science', *Environment and Planning D: Society and Space* 13 (1995) pp. 5-34.

¹⁴ Harry M. Collins, 'The TEA set: tacit knowledge and scientific networks', *Social Studies of Science* 4 (1974) pp. 165-86.

¹⁵ See Shapin and Schaffer, *Leviathan and the air-pump* *op. cit.*, p. 228.

¹⁶ Steven Shapin, 'The house of experiment in seventeenth-century England', *Isis* 79 (1988) pp. 373-404; Steven Shapin, "'The mind is its own place": science and solitude in seventeenth-century England', *Science in Context* 4 (1990) pp. 191-218.

¹⁷ Joseph L. Rouse, *Knowledge and power: toward a political philosophy of science* (Ithaca: Cornell University Press, 1987) and *Engaging science: how to understand its practices philosophically* (Ithaca: Cornell University Press, 1996).

¹⁸ Thomas F. Gieryn, *Cultural boundaries of science: credibility on the line* (Chicago: University of Chicago Press, 1999); Peter Galison and Emily Thompson (eds.) *The architecture of science* (Cambridge, Mass.: MIT Press, 1999).

¹⁹ Selected papers from the conference appeared in Crosbie Smith and Jon Agar (eds.) *Making space for science: territorial themes in the shaping of knowledge* (Basingstoke: Macmillan, 1998).

conference of British geographers in 1996.²⁰ That same year, in the published version of his History of Science Society Distinguished Lecture, A.I. Sabra – although operating with a different epistemological agenda – rehearsed the significance of localism for understanding the history of Arabic science.²¹ All these register a spatial moment in science studies and perhaps even something of a spatial turn.

In the light of all these moves, it seemed to me that the time was right to examine rather more systematically what a ‘geography of science’ might begin to look like, and this is the theme of my forthcoming book.²² The obvious thing to do was to take some very traditional geographical motifs – the sorts of things that conventionally geographers have been interested in – location, site, region, diffusion, circulation, and to ask the question, ‘do these matter in the practice of science?’ So I began to read in the historical and sociological literature on sites of scientific enquiry and to ask the question ‘How does the site of enquiry influence the cognitive content of scientific enterprises?’ I looked at those traditional sites that we routinely associate with science: observatories, laboratories, museums, botanical gardens. But I also give some attention to other locations like the tent, the public house, the ship, the court, the cathedral, the stock farm. For these have also been significant settings for scientific endeavour. And I have come to the conclusion that the sites of science and the social spaces of enquiry are indeed crucially important elements in answering the question ‘Why do any of us believe what a scientific community has to say?’²³ In other words, *where* scientific investigation is carried out constitutes at least a partial answer to why we are expected to give our assent to any particular claim. That is, we are supposed to believe in science because its findings have been delivered by appropriate people in appropriate places. A moment’s reflection, of course, makes clear that this is a massive exercise in social trust. And nowhere are the cultural politics of trust more clearly disclosed than in those enterprises concerned with generating knowledge of distant things. In order to ensure that data were correctly gathered, that observations were properly recorded, that instruments were accurately operated, what John Law once referred to as ‘drilling’ – that is the training of operators to carry out repetitive tasks – was put in place to try and warrant trust.²⁴ This is an inherently geographical problem, of course. The tyranny of distance means that we have to trust in the

²⁰ Shapin’s paper was later published: ‘Placing the view from nowhere: historical and sociological problems in the location of science’, *Transactions of the Institute of British Geographers*, NS, 23 (1998) pp. 1-8.

²¹ A.I. Sabra, ‘Situating Arabic science: locality versus essence’, *Isis* 87 (1996) pp. 654-70.

²² David N. Livingstone, *The spaces of science* (Chicago: University of Chicago Press), in press.

²³ See David N. Livingstone, ‘Making space for science’, *Erdkunde* 54 (2000) pp. 285-96.

²⁴ John Law, ‘On the methods of long-distance control: vessels, navigation, and the Portuguese route to India’, in John Law (ed.) *Power, action and belief: a new sociology of knowledge?* (London: Routledge & Kegan Paul, 1986) pp. 234-63.

physical senses and moral integrity of other people to glean knowledge of far-away phenomena. So the epistemology of trust is an inherently socio-spatial problem, and it redraws attention to the importance of site in scientific investigations.²⁵

Second, as I have already indicated, I am interested in elucidating regional cultures of scientific enquiry. This involves trying to figure out what shapes these cultures and in what way scientific enquiry bears the stamp of the region within which it is located. If we look at the early emergence of what we now call ‘science’ in the fifteenth and sixteenth centuries, for example, we find hugely different practices in the Iberian peninsula compared with the Italian court; and the practices of natural philosophy that were cultivated in both were dramatically different from the procedures that the Royal Society in London and the Royal Academy of Sciences in Paris installed. In all of these cultural spaces, the means of securing natural knowledge bear the stamp of the regional settings within which they take place. Yet all of these activities are still described as ‘science’. It is an interesting question why that should be, why that collective label should be used to gather together such a wide range of discrete practices. It certainly says something about the cultural role that science has come to play in the modern world.

My third interest is in circulation: how do ideas, instruments, institutions, theories move from one place to another, what kind of mechanisms are involved, and what is epistemically at stake in the processes of replication, calibration, and translocation? It is not simply a matter of straightforward diffusion across the surface of the earth. Rather a whole suite of negotiations are involved in the transmission of information, theory, and data from one place to another. At the same time, scientific theories are differently encountered in different spaces. And this opens up the whole matter of what Jim Secord has recently called the geographies of reading.²⁶ Texts are read in different ways in different settings and a spatialised hermeneutics is always engaged in the moment of textual encounter.

Anyhow, these are some of the questions I have been trying to address in my attempt to cultivate an historical geography of science. In due course, who knows, the ‘geography of science’ as a field of endeavour might take its place alongside the ‘history of science’ and the ‘sociology of science’ and the ‘anthropology of science’.

²⁵ For a recent geographical study of the problem of trust, see Michael Heffernan, “‘A dream as frail as those of ancient time’”: the incredible geographies of Timbuctoo’, *Environment and Planning D: Society and Space* 19 (2001) pp. 203-25.

²⁶ James A. Secord, *Victorian sensation: the extraordinary publication, reception, and secret authorship of Vestiges of the natural history of creation* (Chicago: University of Chicago Press, 2001).

Situating David Livingstone, or, connecting biography and geography

Autobiographical notes

You have stressed the importance of space and social networks for the creation of knowledge generally. If you think of the place where you live, Belfast, probably that has had a profound influence on your way of thinking, perhaps even on the topics you have chosen for enquiry. You have written books on the relationship between science and religion,²⁷ for example, and that might originate out of some kind of biographical background in the context of Belfast as a divided city.

DL: Yes. Growing up in a place like Northern Ireland you cannot help but see – for good or ill – the importance of religion in society. Autobiographically I can be very clear about exactly what led me in the direction of studying the historical relations between science and religion. First of all, when I read geography at the Queen's University of Belfast back in the early 1970s, the professor of geography, Bill Kirk, was very interested in intellectual history, and even though he didn't write much on the subject, he was always very keen on trying to place concepts and ideas in an historical context. He also had a personal interest in the geographical significance of various works of ethnographic history on religion and myth – like J.G. Frazer's *Golden bough* for example. So that was important. But I also remember clearly that when I was doing my final examinations in 1976, I read a book (or a least a fair amount of it) by the Dutch historian of science Reijer Hooykaas, called *Religion and the rise of modern science*,²⁸ and it suddenly struck me that what he was trying to do very generally for the emergence of European science in the seventeenth century might be done for the development of the geographical enterprise more particularly. The formative role that Hooykaas attributed to religious thinking in the seventeenth century as fundamental to the growth of science in the period, of course, resonated with the society in which I was living, where religion also continues to be a major cultural force. Now that I look back on it I can see a lot of drawbacks in Hooykaas's analysis, and it has been the subject of a fair bit of revisionism. But it was influential on me at the time, and I owe a lot to it.

So, initially I was interested in investigating the role of religious presuppositions in the development of geographical thought since the seventeenth century, and I proposed this to Bill Kirk as a dissertation project. He very gladly took it on – and so

²⁷ David N. Livingstone, *The preadamite theory and the marriage of science and religion* (Philadelphia: American Philosophical Society, 1992); David N. Livingstone, *Darwin's forgotten defenders* (Edinburgh: Scottish Academic Press, 1987); David N. Livingstone, D.G. Hart and Mark A. Noll (eds.) *Evangelicals and science in historical perspective* (New York: Oxford University Press, 1999); David N. Livingstone and Ronald Wells, *Ulster-American religion: episodes in the history of a cultural connection* (Notre Dame, Indiana: University of Notre Dame Press, 1999).

²⁸ Reijer Hooykaas, *Religion and the rise of modern science* (Edinburgh: Scottish Academic Press, 1972).

I stayed on at Queen's for my doctoral work. It was, I suspect, one of the few places in British geography where a thesis of this sort could have been written at the time. Of course I quickly realised that the project, as I had conceived it, was far too big for a doctoral degree, so what effectively happened was that I narrowed it down to look much more specifically at the birth of geography as a discipline in late nineteenth-century America. I focused on Harvard and the succession of geographical and geological practitioners from Louis Agassiz to William Morris Davis, with the main focus on Nathaniel Southgate Shaler – Agassiz's student and Davis's teacher.²⁹ How I went about that project owed a good deal to a colleague who was lecturing in Queen's at the time, John Campbell, who subsequently went back to Oxford. He encouraged me to place Shaler in the widest possible context, so I worked on his role in the American history of Darwinism, geology, educational theory, environmental conservation, racial politics, and much else besides. But Hooykaas' book also stood me in very good stead because you cannot write about the history of scientific culture in the nineteenth century anywhere in the west without taking religion really seriously. Religion continued to be extraordinarily important as a 'common context' for science both in America and Britain right up to the end of the nineteenth century. But, strangely, historians of geography (by and large) had virtually ignored this framework, and it seemed to me that their work was, for that reason, seriously compromised. There were, to be sure, exceptions; but as a generalisation I think I'd still stand by that early conviction. Anyhow, these ideas provided me with a context, the context of the development of religious thought, within which the evolution of geography could be domiciled. The only historian that I knew of within geography at the time who took the natural theology tradition seriously (though it was becoming the subject of much sophisticated analysis by historians of science like John Brooke, Bob Young, Martin Rudwick, and my colleague in Belfast, Peter Bowler) was Clarence Glacken,³⁰ and I corresponded a little bit with him. I remember as a young graduate student getting a letter from Glacken which said something like, 'Look, your enquires soon will lead you way beyond geography texts. They will lead you into geology, theology, zoology, and a whole host of other subjects. So just read widely.' That was very good advice and it led me to writing a different kind of history of geography from what was available, say, from Walter Freeman or Preston James or a number of others.³¹

²⁹ Eventually a reworked version of the thesis appeared as *Nathaniel Southgate Shaler and the culture of American science* (Tuscaloosa and London: University of Alabama Press, 1987).

³⁰ Clarence J. Glacken, *Traces on the Rhodian shore: nature and culture in Western thought from ancient times to the end of the eighteenth century* (Berkeley: University of California Press, 1967).

³¹ See, for example, Walter T. Freeman, *A history of modern British geography* (Harlow, Essex: Longman, 1980); Preston E. James, *All possible worlds: a history of geographical ideas* (Indianapolis, Ind.: Odyssey Press, 1972).

My interests then have always been in the history of geographical ideas and practices broadly conceived. I have never been attracted to debates about the ‘essence’ of geography or about the right definition of it. These seem to me to be misconceived questions. And I have also worked more generally on the history of the relationship between religious discourses and scientific endeavours. Indeed some of these interests are now coming together for me in another way. I am beginning to work on the reception of Darwinian evolution in a number of nineteenth-century cities. The intellectual elites in these cities very largely shared a similar religious outlook but they all reacted in very different ways to what they understood Darwinism to be saying. The obvious question is ‘Why?’ It turns out that local cultural, social, and political factors shaped the specific encounter between science and religion in these different locales. So trying to unravel something of the story of science and religion in the context of local circumstances is a way in which I hope to tie together several of the themes that I have been working on disparately over the past number of years. It’s clear that this is fundamentally an exercise in the historical geography of science and belief.

Science and religion / knowledge and belief

Science and religion perhaps historically fostered each other, but there were also big tensions, linked to the question of what constitutes knowledge and belief, and whether they can be separated or not.

DL: I have two things to say about this. First, I think that the historical relationship between whatever we mean by ‘science’ and whatever we mean by ‘religion’ is infinitely more complex than any of the historiographical models that are currently on offer. The language of conflict works for certain parts of the story but it is unquestionably not universally the case. You only have to look at Newton or Boyle in the seventeenth century, never mind going any further, to know that the language of an inevitable warfare between science and religion just doesn’t do the job. It’s too crude. It doesn’t slice history finely enough. So even if the language of conflict is right for the Galileo story (and, actually, I don’t know that it is, even here), or for current rows in the United States over the teaching of evolution in schools, it is certainly not widely applicable as a general historical model. The opposite of this – that science has always benefited from religion – is equally problematic because there were many occasions when there really was genuine struggle. Certainly there have been those who have argued that the growth of science – whether in the Christian west or in the Islamic middle ages – materially benefited from religious convictions. But the thesis just doesn’t hold up as a universal theory. A more interesting suggestion is Frank Turner’s argument that what *looks* like an intellectual conflict between science and theology is really (historically speaking) a *social* struggle between

two competing elites – scientists and clergy.³² That moves the debate away from transcendental ideas to the all-too-mundane world of power politics and struggles over cultural authority. And certainly for the Victorian period in England, there's a lot to it. In order to wrest cultural kudos from the established clerical elite, the new professionalising scientists, like the set rotating around figures like Huxley and Tyndall, did all they could to outmanoeuvre the old guard. More radical still is Bob Young's essentially Marxist account.³³ Basically (though I am simplifying it) the argument goes something like this. Religion has – fundamentally – been about the business of justifying the status quo through appealing to God as the ground of the social order. It's a theodicy – justifying the ways of God to human society. In the latter decades of the nineteenth century, science took over that role and justified the social order by appealing to things like the laws of natural selection or the conservation of energy or some such. Science, in this scenario, is merely the continuation of the self-same repressive ideology that had characterised religion. It becomes a kind of secular theodicy – justifying the ways of nature to society. So there is no *conflict* between religion and science. Instead, because both are socially sanctioned ideologies, there is a radical *continuity* between them. I think Young's analysis has a lot going for it, at least for the English-speaking world in the Victorian era. Whether it's applicable elsewhere is a different story, though I've found something similar in late nineteenth century America. Anyhow, these are four of the main stories that are told about the relationships between science and religion. I think none of them works as a universal historiographical model. They don't take place and space seriously enough. They don't allow enough for the messy contingencies of history and geography. We need to get more particular and less general in our talk about 'the relationship between science and religion'. Hence some of the work I was talking about a minute or two ago.

Secondly, I am not sure it is easy to map the lines between knowledge and belief anyway. What passes as scientific knowledge is a judgment that is made by a culture. It is the outcome of historical negotiations. Knowledge has traditionally been defined as 'true and justified belief' and I think that we have come to realise the difficulties of putting flesh on that definitional skeleton. Many defining lines have been proposed: a century or more ago Charles Peirce and William Whewell believed predictive success was the undeniable hallmark separating science from non-science; the logical positivists thought the verification principle would work; Popper used the idea of falsifiability; inductivists in general have turned to testability. But none of these have

³² Frank Miller Turner, 'The Victorian conflict between science and religion: a professional dimension', *Isis* 69 (1978) pp. 356-76.

³³ See Robert M. Young, *Darwin's metaphor: nature's place in Victorian culture* (Cambridge: Cambridge University Press, 1985); Robert M. Young, 'Evolutionary biology and ideology: then and now', *Science Studies* 1 (1971) pp. 442-503.

really delivered the goods. Such proposals have fallen foul of a thousand qualifying counter-examples. And this has led philosophers like Larry Laudan to speak of the ‘demise of the demarcation problem.’³⁴ In my view it is better to use the word ‘knowledge’ (at least for historical purposes) in a more conventionalist sense, namely, to refer to what any cultural group decides – for whatever reasons – to give the label ‘knowledge’ to. And that means that what is accounted ‘knowledge’ will change from period to period, from culture to culture, from place to place. This doesn’t mean that I want to discard the idea of truth or to imply that it has no realist locution as Mary Hesse puts it. But it does mean that what passes as the ‘justification’ of a claim is relative to cultural circumstances. As I’ve said before, what is reasonable for a sixteenth century sailor to believe about cosmology, isn’t what a twenty-first century astrophysicist would be justified in believing. Justification is a relative concept. Rationality is always situated rationality. And this means that there is an historical geography of rationalising practices to be uncovered. So, I think it is mistaken to try to map a clear, essential line between knowledge and belief. In my view that is a misconceived enterprise. This means that I am more interested in looking at knowledge-making enterprises and belief-sustaining cultures. And when we engage in such work, we shouldn’t start out with too many assumptions about where various cultural items fall on the map. What our culture now thinks of as ‘scientific knowledge’ and ‘religious belief’, or ‘real science’ and ‘metaphysics’, too neatly separates out what are far more integrated and overlapping discourses than are commonly acknowledged. In fact that’s not even the right way to put it for it implies that there are two realms that intertwine. And I don’t want to give that impression. To me, the interesting question is to determine why the lines get drawn where they do, what interests are at stake in the drawing of those lines, and which groups have the power to declare on what is a society’s official ‘knowledge and belief cartography’.

Let me add just one other comment. Shapin’s work on the social history of truth is a most interesting book.³⁵ But it is gloriously mis-titled. It is not a social history of truth; it is a social history of *what has been taken* to be truth. It is a social history of the making of warrant, of *what has passed* as truth. Societies have proposed all sorts of criteria that have to be fulfilled for a claim to receive the accolade of being denominated ‘truthful’. These simply change over time and from place to place, and there are unquestionably social histories and cultural geographies of what people have considered the appropriate practices you have to perform, or what epistemic obligations you have to fulfil, in order to arrive at something called ‘truth’. I like that way of operating, because I think that it breaks down any simplistic distinction

³⁴ Larry Laudan, ‘The demise of the demarcation problem’, in Michael Ruse, *But is it science?* (Buffalo, New York: Prometheus Books, 1988) pp. 337-50.

³⁵ Steven Shapin, *A social history of truth: civility and science in seventeenth century England* (Chicago: University of Chicago Press, 1994).

between knowledge and belief. It helps us refocus. For what we are really talking about are historical geographies of systems of warrant, historical geographies of schemes of credibility, historical geographies of the moral economies of epistemic justification.

Social constructivism

We have talked a little about your autobiography but I could sense some reluctance in associating this with your historiographical writing. This is interesting, given the importance of spaces and networks in your account of scientific practices.

DL: One of the drawbacks of a radical social constructivism, or indeed of what might be called constitutive spatiality, is that it can disable you from making substantive claims of any sort. Every proposition that you utter becomes so self-reflexive that you cannot really say anything that even begins to look like a truth claim. I don't want to accept that. If you detect any hesitancy in my reflections about locating my own historiography, this could be the source. Let me explain. If social constructivism is applicable to scientific practice, it is also applicable to historical writing. If you can – as it were – sociologise scientific practice you can sociologise historical knowledge too. You can't exempt historians – or sociologists or human geographers for that matter – from the same kind of treatment that you apply to scientists. This is a tricky issue, and something of the dilemma it raises surfaces when we ask what sort of claim a social constructivist is making when they tell the story of some particular episode in the making of natural knowledge. Take Steven Shapin's remarkable book, to which I have just referred, on what he calls the 'social history of truth.' I'm not sure it's exactly right to call Steve a social constructivist – that label carries a lot of baggage – and it might need to be finessed in one way or the other. But for the sake of argument, let's assume that he is. So, he recounts for us the social forces and cultural practices that went into the making of knowledge in Robert Boyle's England. The story has to do with the conditions for achieving credibility, how warrant was stabilised, which social groups were expected to be truth-tellers and which were situationally prone to exaggeration, the role played by conventions of civility in genteel discourse, and so on. It's a resolutely socialised account of what came to be accorded as scientific truth.

So far so good. But what about Steve's story itself? Is it also simply the product of social forces, say, late twentieth century conventions of making historical knowledge, the current practices of establishing credibility, the resolute display of sources in footnotes, and so forth? Well, yes, partly so. But can we leave it just there? I don't think so. Why? Because that would surely disable his own account. *A social history of truth* wouldn't really be about how ideas of truth were settled in seventeenth century

England; it would be about Steve's own autobiography and his socialisation into the culture of twentieth century sociology. Indeed if we were social constructivists we couldn't even make *that* claim as an assertion about what's really going on in his book, for any claim we make would be about ourselves too. You get the point? And significantly Steve, detecting I think potential incoherences of this infinite regress, makes a comment on the very first page of the volume something to the effect that the book can be read in pretty straightforward realist fashion. Indeed, if I recall it right, he adds something along the lines that his autobiography is less interesting than the subjects he writes about.³⁶ I'm pretty sure I endorse that claim for myself too! For my perspective, I believe Steve has given us a *truer* account of how knowledge was made, and how claims to truth were established, in seventeenth century England than we have had before. And that's what makes the book's argument so compelling. But then there's a problem. Steve himself is now making a substantive truth claim about the history of truth-making, isn't he? Now if this is right, then social constructivism can't be the whole story. And, ironically, it's because Steve's story is so compelling that I think the universalisation of social constructivism must be mistaken. He's given us a truer account of what goes on in the practice of science than we have hitherto had.

Now you see what I am trying to do when you ask autobiographical questions. I am resisting the inclination towards socio-spatial reductionism for that amounts to self-referential incoherence. Of course I can be contextualised. I was brought up and live in Northern Ireland. I continue to belong to one of the religious traditions there. I share convictions that are a part of that heritage. Certainly I am in critical dialogue with that tradition. But I still affirm key aspects of it. No doubt that helps explain my continuing interest in the role of religion in scientific culture. No doubt these convictions also make me keen to retain the idea of truth as a normative principle. No doubt they shape my belief that religious belief is not simply reducible to Durkheimian society worshipping itself, or to Freudian psychoses, or to Marx's opiate of the masses, or to Dawkins's cultural memes. They doubtless help explain why I think that conceptions of the human subject as nothing but (if I might caricature it) Marx's flotsam-and-jetsam of history, or Darwin's trousered ape, or Freud's bundle of drives, or Dawkins's genes-with-an-attitude, or Dennett's reducing us to something like a cross between a mango and a microwave, are all inadequate. I suspect, to return to an earlier point, they also help explain my sense that Gadamer is correct when he speaks of the need to re-appropriate prejudice, or pre-judgment,

³⁶ The exact wording is: 'Unlike some postmodernist and reflexivist friends, I write in the confident conviction that I am less interesting than the subjects I write "about," and, accordingly, much of this book can be read in the mode of old-fashioned historical realism'. Shapin, *A social history of truth op. cit.*, p. xv.

because these are constitutive of our being.³⁷ The idea here, I take it, is that we are all inescapably located in a situation, a tradition, a set of circumstances. And it is only through the traditions in which we stand – not by bracketing them – that we can have access to ‘the other’ at all. Only by bringing together the horizons of the world in which we are situated, and those of the other, can hermeneutic rendezvous take place. Prejudices do not cut us off from the past, or from ‘the other’; they are the way in which, if only initially, other realms are opened to us. All this is, for me, indubitable. And yet, I think it would be mistaken to reduce my historiographical predilections to *nothing but* the crystallisation of autobiography. That would amount to a kind of socio-spatial determinism. Perhaps that is why I am inclined to think of myself as a *modest* social constructivist or, perhaps better, a *chastened* historical realist.

³⁷ Gadamer puts it this way: ‘It is not so much our judgments as it is our prejudices that constitute our being. This is a provocative formulation, for I am using it to restore to its rightful place a positive concept of prejudice that was driven out of our linguistic usage by the French and the English Enlightenment... Prejudices are not necessarily unjustified and erroneous, so that they inevitably distort the truth. In fact, the historicity of our existence entails that prejudices, in the literal sense of the word, constitute the initial directedness of our whole ability to experience. Prejudices are biases of our openness to the world. They are simply conditions whereby we experience something – whereby what we encounter says something to us.’ Hans-Georg Gadamer, *Philosophical hermeneutics*, translated and edited by David E. Linge (Berkeley, California: University of California Press, 1977) p. 9.