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Geo-historiographies

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The working title for this chapter was 'monster in a box'. That was its electronic file name at least. I took the title from Spalding Gray's one-man show that I saw in the early 1990s in Vancouver.[1] The stage presentation involved Gray sitting at a desk, performing an hour-and-a-half monologue. The piece was about Gray's struggle to complete a novel that had ballooned to 1900 pages, and which he now lugged around with him in a large cardboard box. That was the monster in a box. It sat prominently on the desk as Gray delivered his monologue, representing both a penance and a reminder of work yet to be done.

I used the title 'monster in a box' because I had one, too. Mine sat on my home office floor for more than a decade. Its contents were also supposed to be turned into a book. That the box remained there for so long was my penance, as well as a guilty reminder of work left to be done. In my case, it was the work of telling stories from geography's past. The box was made from white, corrugated cardboard, by then stained and battered, designed originally to hold a case of de Neuville wine. It was filched from a British Columbian government liquor store in a previous house move. It sat in various places on my home office floor from 1998 when I began filling it.

The box contained the accumulated documentary weight of five years of interviews and archival research undertaken between 1997 and 2002 about the history of American geography from the mid-1940s to the mid-1980s. I conducted 36 interviews, focusing on the discipline's Quantitative Revolution, but frequently spilled over into other topics.[2] I brought back from my meetings cassette tapes, CVs, reprints, unpublished lectures, poison pen letters, triumphal cartoons, subversive diagrams, and rare discontinued journals and series. My most treasured item was Bill Bunge's (1968) 'Fred K. Schaefer and the science of geography', Paper A, Special Papers Series, *Harvard Papers in Theoretical Geography*. It was simultaneously homage to Bunge's hero Kurt Schaefer and a scurrilous, possibly libellous, attack on his nemesis, Richard Hartshorne (Heynen and Barnes 2011). I had known of the paper's existence since 1978 and came close to putting my hands on it several times. Finally, in December 1997 it was slipped into my pigeonhole in a plain brown envelope. I immediately added it to my box.

While I promised the University of British Columbia's Research Ethics Review Board that my research materials would be stored in a locked, fortified filing cabinet in a locked, fortified office and for which I would have the only keys, the reality was that everything went into that box. It was open to anyone who cared to look, but no one ever did.

I initially thought of the material I collected as my 'data'. My earlier work in geography was as a conventional social scientist. My doctoral dissertation consisted primarily of neatly ordered rows of equations and mathematical derivations. On some pages, there were only equations, and on many pages, there were more symbols than words. I knew that I couldn't write the history of the Quantitative Revolution as a series of Greek letters and mathematical operations (although in my more fanciful moments that prospect did cross my mind). But using my data I thought I could construct a systematic, linear, verifiable and causally compelling account: my model of the Quantitative Revolution. The data would line up, self-organize, tell their own story, speak for themselves.

But the more interviews I conducted, the more stuff I loaded into my box, the more it became clear that my data were never going to speak for themselves. Instead, it required *me* to engage in a continual, often a maddening, occasionally sleepless, process of sorting, shuffling, ordering and assembling. I read and reread, cut and recut, pasted and repasted, struggling to craft a lucid, plausible narrative. The very last thing my data did was to self-organize. In the end, I wasn't clear I'd even gathered data. There were dates (which could be hazy), discussions of specific events (which could be contradicted) and bibliographical references (which proved not always to exist). More interesting than the 'facts' were the stories, and how they were told: the tropes used, the pacing, the hesitations, the gaps, the arc of the narrative. They were gripping.

As I conducted interviews, it became obvious that I was part of that history, too. I had started as an undergraduate at University College London (UCL) in 1975, well within the period about which my interviewees spoke. And I continued during the late 1970s and early 1980s as a graduate student attending the University of Minnesota's Geography Department. I was supervised by Eric Sheppard, one of the best mathematicians of his generation, who worked on a theoretical centrepiece of spatial science, spatial interaction modelling.[3] In turn, Eric Sheppard was supervised by Leslie Curry, who was present at the very beginning of the Quantitative Revolution. Curry made formative contributions especially to the probabilistic modelling of 'chance and landscape' (Curry 2002). With this kind of pedigree, and with a doctoral thesis of bone-jarring equations of my own, there was no question that I also was in the box.

The purpose of this chapter is to face down my monster. I do that by telling stories from my research, and stories about how I undertook that research. This approach was directly inspired by Spalding Gray's. He quelled his monster by giving a performance that, in part, was about how he came to make that performance. I follow the same self-reflexive tactic, hoping for the same successful end.[4] I was also later inspired by works of some historians who have recently self-consciously reflected on working in and with an archive (Steedman 2001; Stoler 2009). Hayden Lorimer's (2010) contribution within historical geography has been especially important. Lorimer (2010: 249, 253) calls his project 'archival hermeneutics', and explicitly 'experimental'. Its purpose is to 'extend beyond print culture and the written word' to 'include the context, encounters and events that constitute research practice' (Lorimer 2010: 249). That is precisely my aim in this chapter. I want to report the written and spoken word, both of which ended up in my box, but I also want to provide the 'context, encounters and events' of my 'research practice' that led them to be deposited there in the first place. My desire to write about both stems, I think, from the radical change in my research interests and methods compared to the kind of geographer I was, and the methodological tradition in which I was originally schooled. I have had to retool fundamentally, refashioning and remaking myself. It was not easy, remaining unfinished, likely never to be finished. It was often frustrating, especially writing this chapter. But I couldn't help myself. I had to confront the monster.

In the guise of refashioning and remaking, I have taken up several strategies, some experimental, which are illustrated throughout the chapter. The first is to deploy a less academic form of writing. I aspire to a looser, more colloquial prose that facilitates the narration of vivid and engaging stories that constitute geography's past. Second, as I have done so far, I situate myself within the narrative to remind readers that I am, like many of them, part of the story, too. But unlike them, I am telling the story. The author is me, making a critical difference to how the story is written. Third, I discuss learning and practising methods for acquiring stories about the past. Those stories are derived principally from collecting and recording oral histories, as well as locating and interpreting archival materials found in both public and private holdings. In both cases, researchers at those sites, including me, are never innocent bystanders but have geo-historiographical stories to tell of their own. Fourth, I gather, review and set out distinct bodies of theoretical literature concerned with critically analysing the production of academic disciplinary knowledge and its multiplicitous social entanglements. Traditional accounts of geography's history can be antiquarian, shunning explicit theorization. In contrast, my approach is resolutely theoretical. Finally, I stress the importance of the geography of geography's own knowledge, which is bound inextricably to the history of the discipline. Geography's geography is not mere background colour, a gazetteer-like list of places, but must be conceptualized, theoretically connecting the history of the discipline's ideas with where they were produced and travelled.

The chapter is in two main sections. The first asks a set of general conceptual and methodological questions about writing geography's history. Why write that history? How should we theorize geography's past? How should we conceive the geography of geography's past? What sources should we use and how? And finally, how should we write disciplinary histories? The second section tries to answer those questions substantively by using the research materials I've gathered about American geography's recent past – the monster in the box.

QUESTIONING GEOGRAPHY'S HISTORY

Why Write Geography's History?

Why bother? Why do we need to know about the history of the discipline? There is a strong belief associated especially with modernism that the past is irrelevant. As Henry Ford said to a *Chicago Tribune* journalist in 1916, 'We want to live in the present, and the only history that is worth a tinker's damn is the history we make today.' 'History is bunk' (Ford 1916).

Clive Barnett (1995: 417) may not literally say the history of geography is bunk, but, clothed in academic prose, that is, in effect, what he says: 'I want to articulate some doubts I have about the value and relevance of expending energy studying the history of geography as a means of throwing light upon the state of the discipline today.' Later in the same essay, he drops the academic clothing altogether: 'let the dead bury their dead', he (1995: 419) bluntly writes and, directly echoing Henry Ford, he directs us to 'forget about the past and … act instead with no regard at all for what has gone before' (Barnett 1995:

419). '[T]he only context that really matters [is] the contemporary one' (Barnett 1995: 417). That sounds a lot like, 'the only history that is worth a tinker's damn is the history we make today.'

Barnett believes that the history of geography is irrelevant to understanding the contemporary discipline because geography has changed so radically over time (Barnett 1995: 418). For Barnett, geography's past is like a foreign land. The earlier form of the discipline is so different from any present incarnation that no connection between the two holds. They are incommensurable. Further, many histories of the discipline, and Barnett focuses on David Livingstone's (1992) *The Geographical Tradition*, end their account before they reach the present (in Livingstone's case by about 25 years). The consequence is that they have nothing to say about the only thing worth saying anything about: now (Barnett 1995: 417). For Barnett, geography's histories stop at exactly the point that they should begin.

For Barnett, geography's history is a dead end, but what about those geographers who have written or edited books about the history of geography, what is their justification? Don't they provide a rebuttal? Unfortunately, in many cases they don't, their rationales are weak or poorly articulated or not consistently carried through.[5] There are some exceptions, however.

Richard Hartshorne's justification found in his 1939 monograph *The Nature of Geography* is maybe the best-known, providing a robust defence for studying the history of geography (Entrikin and Brunn 1989). Hartshorne believed that it was the discipline's past, and particularly its origins, that defined the subject (Mayhew 2011). Hartshorne (1939: 8) wrote, 'If we wish to keep on the track – or return to the proper track – ... we must first look back of us to see in what direction that track has led.' But while Hartshorne provided this argument for studying the history of the discipline, he failed to provide warrant for his assumption that a discipline is defined by its historical origins (Mayhew 2011). Without that warrant, Hartshorne's mandate for historical study possessed no basis, turning geography, as Neil Smith (1989) argued, into only 'a museum'.

Brian Berry's (1978) edited *The Nature of Change in Geographical Ideas* suggested that geography's history should be examined to document the various hints, anticipations and precursors of the true scientific geography that finally fully emerged around the time Berry published his collection. Driving geography's history, Berry believed, was a scientific rationality that struggled to assert itself. At first, its appearance was intermittent and partial, but over time it increasingly revealed itself, eventually enabling geography to become a fully fledged science during the discipline's 1960s Quantitative Revolution. Berry's approach to history is known as 'presentism', or 'Whig history', or 'internalism'. This is the idea that disciplinary change is guided by the progressive working out of a deep-seated principle (here scientific rationality) that becomes finally realized at precisely that point in time at which the author of the history writes. Unlike in Hartshorne's account where the past defines how we judge the present, in Berry's conception, the present defines how we judge the past. As a justification for historical research, though, presentism rests on a teleological fallacy that the present reaches back to shape its past (Fischer 1970). Consequently, as Roy Porter jibed, history only 'becomes a pack of tricks we play on the dead' (quoted in Livingstone 1992: 7).

A third instance is provided by Johnston and Sidaway (2004) in their popular textbook, *Geography and Geographers*. They argue we should study 'the history of geography ... for the light it sheds on what geographers are and do' (Johnston and Sidaway 2004: xi). What is missing in their statement is any link connecting the past to the present. If their goal is to know what geographers 'are and do' – that is, their work now – why do we need to study history? Wouldn't it be better to learn about geographers as they are currently, focusing on what they do in the present?

Finally, there is David Livingstone's (1992) history, against which Barnett was specifically reacting. For Livingstone (1992: 28) there is no 'eternal metaphysical core to geography independent of historical circumstances.' There is only history, and the reason to study it is 'to teach us to be humble about any claims to knowledge .... [T]he history of geography has always been contested and negotiated' (Livingstone 1992: 3). For Livingstone, the reason to study the past is not because it bears directly on the present but because it teaches us an important general lesson that can be applied to the present: that all definitions of knowledge are historically contingent. This is an important point, but in itself, it is not a convincing justification for studying the history of geography. The contextual and negotiated character of knowledge can be learned other than from studying geography's history. Science studies has made the same argument about the contextual and negotiated character of knowledge for more than 40 years, but to be as convincing as possible, it has focused on the most difficult cases – 'hard' sciences like physics and chemistry, rather than softer cases like the history of geography.

The larger point here is that, to demonstrate that studying the history of geography is necessary for understanding the contemporary discipline, one must show an indissolvable relation between geography's past and present knowledge. That crucial connection is denied by Barnett and Ford. For them, the past is severed from the present, where nothing that once occurred constrains what occurs now. That is why history is irrelevant. In contrast, what needs to be shown, as William Faulkner famously put it, is that 'the past is never dead. It's not even past'.[6] For Faulkner – and this also will be the central theme in the remainder of the chapter – past and present are inextricably joined. We cannot escape history because the past never fully passes. Instead, we carry the burden of the past into the present, bringing with us what went before.

Such a position was grasped, perhaps better than anyone else, by Michel Foucault. For Foucault (1970: 219) knowledge is profoundly historical, it is 'the unavoidable element in our thought'. We never begin from scratch. The best we can do, the only thing we can do, is to provide a 'history of the present' (Foucault 1977: 31), a historical analysis of 'what today is' (Foucault 1994). While we can never remove ourselves from the present – it forms our perspective for everything else – we have no choice but to be historical, to practise what Foucault called archaeology or genealogy.[7] Archaeology and genealogy do many things as techniques for excavating the past, but for my purposes the most important is to show that present knowledge necessarily rests on past knowledge. While history for Foucault is contingent (the present could always be otherwise), the relationship between past and present knowledge is necessary. The past never passes, implying that studying the history of geography is indispensible.

How Should We Theorize Geography's Past?

My research in the history of geography draws theoretically on science studies. As an approach, it first emerged in the mid-1960s, associated with the Science Studies Unit at the University of Edinburgh, and taking the form of the so-called 'Strong Programme' (Bloor 1976). As a project it quickly fragmented, becoming a sometimes fractious collection of schools and approaches (well reviewed by Hess 1997, and in geography by Powell 2007). Continuing to tie them together, however, was a common anti-rationalism.

Rationalism defined 'the other' approach to scientific knowledge – that of the conventional philosophy of science. The conventional view averred that scientific truth was revealed by applying human reason or rationality to problems and formally codified as the scientific method. By consistently applying that method, truth was exposed; science progressed. Furthermore, within this conventional view rationality required no justification. Its invariant universal logic was justification enough, with no reference needed to bolstering external factors.

But for the (anti-rationalist) science studies, external factors were the very bases of the construction of scientific knowledge and truth. Rationality played no part in the mechanism. David Bloor, one of the founders of the Strong Programme, even suggested that rationality was just another social construction. 'Something does answer to the term reason, but it has been misdescribed. ... Like all divine and magical forces and queer mental processes, the force of reason ... is the force of society misdirected' wrote Bloor (1988: 69–70).

Under science studies, scientific knowledge is produced only after significant resources and work are expended on constructing it. That includes money, machines, buildings, equipment, institutional support and the labour of trained personnel, including scientists. Because scientific knowledge is an artefact, humanly produced, the processes involved in its manufacture leave their social mark on the product. This argument parallels one that Marx made about commodity production. Commodities for Marx are artefacts, humanly produced from complex social processes, which are then inscribed on the good. But, Marx suggested, consumers often ignore that fact. They treat the commodity as if it were an abstract object, shorn of its messy trailing entanglements. In Marx's lexicon, they 'fetishize'. The same applies to scientific knowledge. Conventional philosophy of science conceives knowledge as an isolated abstraction, fetishizing it. In contrast, science studies knowledge is always scored by an outside world, muddied and materialized.

The Strong Programme at Edinburgh was the original version of science studies, but the best-known variant, certainly the version most commonly found in geography, is actor-network theory (ANT). Associated particularly with the work of Bruno Latour (1987, 1988, 1999, 2005), ANT suggests that knowledge is a relational effect, the result of persuading sometimes very different entities to work together. To use the ANT vocabulary, knowledge is produced by 'enrolling' a heterogeneous set of 'agents' (as varied as books, Bunsen burners and even human beings) within a 'network' of 'alliances'. The important point is to organize, or in ANT-talk, 'translate', the interests of different agents so that they are willing to join and participate in a common project. Only once the interests of an agent are translated – that is, are aligned with those of other agents – will a stable network be achieved and knowledge produced or 'performed'.

'Perform' is an important word. From an ANT perspective, acquiring knowledge is an active process. It emerges only through deliberative ongoing action, by orchestrating an integrated performance among different entities. Performance also implies that things can go wrong, that it might not all go right on the night: notes are omitted, or played badly, or in the wrong place; instruments are not tuned or malfunction (strings break, reeds splinter, valves stick); individual players fail to turn up. A performance is a precarious achievement, reliant on a myriad of different elements playing their role and cooperating. The resulting performance might be brilliant, bringing down the house, like producing a vaccination for polio or the double-helix model of DNA. Or it might never begin, or quickly fizzle out, or finish with an audience's indifferent shrug.

While larger philosophical ideas and a specialized abstract vocabulary spiral through science studies, much of its written work is substantive, concrete and historical. Conceptual issues tend to be tackled by remaining at ground level, tethered to the detailed record of the particular study. Typically the focus is a limited piece of scientific knowledge – Robert Boyle's air pump experiments and the formulation of his famous law (Shapin 1994); Louis Pasteur's war on microbes and the development of pasteurization (Latour 1988); Francis Crick's and James Watson's development of the double-helix model of the gene (Kay 2000). There is less interest within science studies in examining the history of academic disciplines, but there is no reason that its approach and terminology can't be used for such a purpose.

Latour (1999: Chapter 3), in fact, provides a model of scientific performance metaphorically based on the circulation of blood through a vascular system (Figure 9.1) that can be modified to deal explicitly with disciplinary concerns (Barnes 2004). Latour argues that, for a scientific discipline to be healthy, circulation and coursing should occur around four circuits – instruments, colleagues, allies, and the public – that then come together in a fifth as links or knots. Specifically, by 'instruments' he means the various material means by which the world is mobilized and engaged; by 'colleagues' how a discipline or profession becomes independent and forms its own criteria of evaluation and relevance; by 'allies' the institutions, people and things that must be persuaded to join the project – that is, to have their interests translated; by 'the public', the social world outside the academy; and by 'links' and 'knots' the points where the four circuits of objects, people, institutions and the social world come together. For Latour, scientific success follows from unimpeded coursing around the five circuits. For this to happen, the circuits function together, but, just like bodies, things can go wrong. Circuits clog, bits come loose and flow volumes reduce, are erratic or, even, catastrophically stop altogether. Nothing is assured. There is need for periodic check-ups, occasional intervention, sometimes of a major kind: removing blockages, rerouting vascularization, steadying flows, even stopping and restarting the system. And in the end it might be to no avail. It might really be the end, the body of knowledge turning lifeless. Science studies is less some grand philosophy than a toolbox of conceptual instruments and devices used to monitor, track and make sense of disciplinary knowledge both in the present, but also, as I show below, in the past.

What is Geography's Geography?

In his 1977 primer for human geographers, *Quantitative Methods in Geography*, Peter Taylor (1977) provided an unusual map: 'Quant

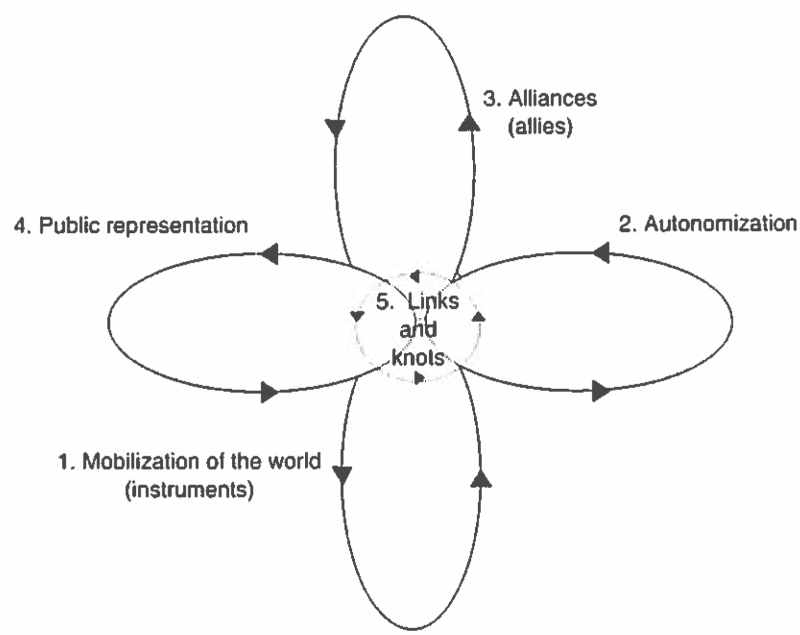

Figure 9.1 The four circuits of disciplinary health (Reprinted by permission of the publisher from Pandora's Hope: Essays on the Reality of Science Studies by Bruno Latour, p. 100, Cambridge, Mass.: Harvard University Press, Copyright © 1999 by the President and Fellows of Harvard College)

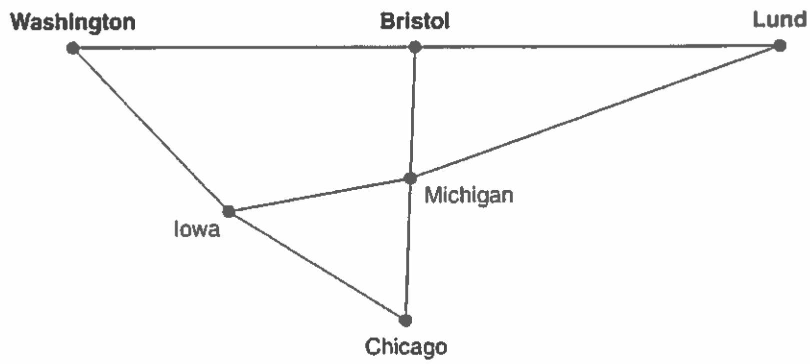

Figure 9.2 Quant Geog airlines flight plan (Redrawn from Taylor 1977: 15). From Taylor. *Quantitative Methods in Geography: Introduction to Spatial Analysis.* © 1977 Brooks/Cole, a part of Cengage Learning, Inc. Reproduced by permission. www.cengage.com/permissions

Geog airlines flight plan' (Figure 9.2). It was unusual because it was not a map of a thing, but of an intellectual idea, one that in this case transformed the discipline – geography's Quantitative Revolution of the 1950s and 1960s. Taylor's map illustrated the places in which the Quantitative Revolution was formulated and practised and between which it travelled, represented by the connecting straight lines linking sites. It was a map of geography's geography. Consequently, it could allow us, as Agnew and Livingstone (2011: 18) put it, to 'think geographically about geography and thereby of "geographizing" geography itself.' Not much 'geographizing' geography has ever happened in the discipline, however, because of the dominance of rationalism. If rationality was defined by an invariable universal logic, then it didn't matter where rationality was applied because the same result held everywhere. Rationalism was 'the view from nowhere' (Nagel 1986).

The emergence of the anti-rationalist science studies, however, has allowed geography to be put back on the map. As Ophir and Shapin (1991: 4) wrote in an early review: 'What if knowledge in general has an irremediably local dimension? What if it possesses its shape, meaning, reference, and domain of application by virtue of the physical, social and cultural circumstances in which it is made, and in which it is used?' More than 20 years later, it is no longer 'What if? Geography is clearly recognized as a constituent element in the production of knowledge, leaving its epistemological mark. It is the view from somewhere (Agnew and Livingstone 2011: 7). Further, knowledge travels and circulates, but at different speeds, serendipitously interacting with ideas from other places, changing form. The geography of ideas is complex, processive not instantaneous, rooted in the stickiness, fallibility and frailty of human interaction at a distance.

Geographers over the last 20 years have increasingly attended to the geography of ideas, and to the geography of their own discipline. David Livingstone's work has been both formative and exemplary, bearing on both the spatialities of science in general (Livingstone 2002, 2003) and the spatialities of the discipline of geography in particular (Livingstone 1992); however, his work often underplays explicit theorization.[8] But theoretical work on the geography of ideas has been carried out by non-geographers and is potentially useful for 'geographizing geography'.

The first set of such theories focus on places of knowledge. What makes a place suitable for generating new knowledge? And, once generated, how can knowledge be made credible so that it is accepted in other places? Hetherington's (1997) Foucault-inspired notion of heterotopia addresses the first question (see also Ophir and Shapin 1991: 13–15). Hetherington argues that some places, which he labels heterotopias (following Foucault preface, 1970, 1986), hold the potential to allow for the emergence of a different form of ordering compared to that found in other places. Heterotopias are more porous, flexible and tolerant of difference. With less rigid rules about what can be said and who can say what to whom, there is scope in a heterotopia for novelty and creativity that is denied in other kinds of places. In turn, that openness allows a heterotopia to become a focus for heterodox interests. In disciplinary terms, heterotopias are places of potential new paradigm formation, sites that spark and contribute to larger disciplinary transformations. That is the significance of Taylor's map. It is a cartography of heterotopias that changed the discipline of geography. There is one other point. Foucault (1986: 26) argued that to gain access to heterotopias 'the individual has to submit to rites and purification. To get in one must have a certain permission and make certain gestures.' Entry into the heterotopias of the Quantitative Revolution, as we will later see, involved just such consecrated acts of purification, a submission to the immaculate logic of mathematics.

The second question about place is addressed by Thomas Gieryn's (2002) notion of a 'truth spot'. A truth spot is a site that gains sufficient credibility that those who claim knowledge from there are able to assert that their claims 'are authentic all over' (Gieryn 2002: 118). As a result, places 'escape place ...; place achieves placelessness' (Gieryn 2002: 113). One of Gieryn's examples is the Princeton Plasma Physics Lab that 'pursues credibility for its claims without recourse to place' (Gieryn 2002: 125). Gieryn argues against this assertion, showing exactly how the trick of making place disappear is done. It is by claiming that the results at the Plasma Lab in Princeton are replicable anywhere else in the world. Not true says Gieryn. They can be replicated provided only that all other labs are constructed as exact replicas of Princeton's. But this is not the same as claiming that results are 'authentic all over', and certainly it does not prove placelessness. It suggests the reverse: that it takes enormous effort to undo geographical difference. It is realizable only by artificially constructing one place as the mirror image of another.

The second kind of geographical theorization is about the movement of ideas over space. Bruno Latour's (1987: Chapter 5) emphasizes the processive geographical character of knowledge acquisition involving ceaseless travel and passage. Knowledge is never instantly true, but becomes true only through the enormous amount of work involved in establishing and maintaining networks of circulation (brilliantly exemplified in Latour's 1999 Chapter 2 essay on the pedofil of Boa Vista). In Latour's vocabulary, Gieryn's truth spots are 'centers of calculation'. They are key nodes in extensive geographical networks enabling them both to receive knowledge and to distribute it, allowing action at a distance. Figure 9.3, taken from Latour's (1987: 220) *Science in Action*, portrays the circulatory process as cumulative, with more and more information and things brought back to the centre as a result of increasingly expansionary geographical crossings and recrossings.

Latour (1987) is mainly concerned with successful centres of calculation, imbuing them with an imperial power both to attract things and to send out decisions that act at a distance. But imperial centres do not get always get their way. For Deleuze and Guattari (1987: 239), the important question is not what makes a centre a 'zone of power' but what escapes its influence and the consequences. They suggest that unordered Otherness outside a centre can

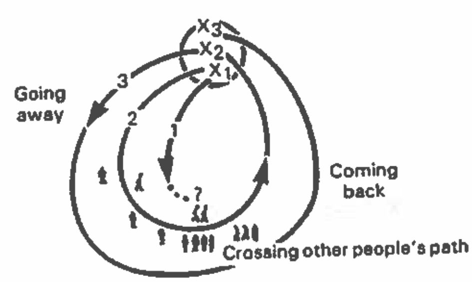

Figure 9.3 Centres of calculation (Reprinted by permission of the publisher from Science in Action: How to Follow Scientists and Engineers through Society by Bruno Latour, p. 220, Cambridge, Mass.: Harvard University Press, Copyright © 1987 by Bruno Latour).

undermine a 'zone of power' if given an opportunity, and exemplified below in the discussion of the history of geography's centres of calculation.

What Sources and Methods Should We Use?

At least for my study of geography's North American Quantitative Revolution there were two main sources of information, requiring quite different methods. Both were well known in historical enquiry, although neither was well known to me: techniques of oral history and archival research.

In 1997, it was still possible to speak to original participants in the Quantitative Revolution to ask them directly about the details of their involvement. A few of the original pioneers were retired although still alive, like Chauncy Harris, Ned Taaffe and Bill Garrison. Second-generation participants were thick on the ground, the majority senior professors in one university or another.

In speaking to those geographers, I initially thought of my method as 'the standard interview'. I knew about interviewing. I had done it for at least a decade before, talking especially to people involved in British Columbia's forest economy. Interviewing meant systematically squeezing information from the interviewee. Speaking to those involved in the Quantitative Revolution would be no different, but it was. At the end of my first interview, I had less than a page of discrete facts, and that more efficiently gathered from a CV than an interview. In the next interview, I allowed the interviewee more space to ruminate and stray, to be less fixated on my prepared questions (seen in Barnes 2012). The interviews became progressively less interview-like, turning into conversations about a life lived, an oral history. People were so enthusiastic to talk. In some cases, it was as if they had waited their whole life to be asked. I gathered material without asking questions. Geography's Quantitative Revolution became inseparable from the biographies of its participants. I recorded life stories and I recorded stories about the history of geography.

The oral histories were rich, compelling, vivid and impassioned, but could they be believed given that they relied for the most part on someone's memory? Memory, after all, is actively constituted, worked and reworked, never fixed and final. Hemingway (1999: 84) once said, 'Memory … is never true.' There was also another issue: the ability of oral histories to convey large-scale socio-institutional shifts – war, for example, or broad governmental changes or severe economic disruption. Such shifts needed to be included, but oral histories gave, at best, partial accounts. An individual could speak about larger macro forces, giving local instances of their effects, but an oral history, by definition, is an account by a single individual, reflecting personal idiosyncrasies, prejudices, gaps in experience, and a specific context. Oral histories highlighted diverse experiences, made concrete abstract movements (like geography's Quantitative Revolution), revealed back-stories not recorded in official accounts and attended to bodies and a range of human emotions, but they were not so good for representing abstract ideas or general conditions or large sweeping historical processes.

Because of both issues, the reliability of memory and the representation of large-scale social change, oral histories required additional triangulation and supplementation – triangulation to better assess the claims to historical truth, supplementation to include necessary explanatory material otherwise omitted. On both counts archival resources became invaluable.

Archival material, 'the grey' as Nietzsche and Foucault called it, were textual documents in the broadest sense, 'which can be really ascertained, which ha[ve] really existed' (Nietzche quoted in Mayhew 2011: 28). Initially, those texts were freely, albeit irregularly, given to me when I attended interviews and duly deposited in my box. I didn't pay them much attention at first. I thought 'real history' would derive from the interviews, with archival material adding only grace notes. But, as I came to recognize limitations with the interview material, I began systematically seeking and using archival collections, particularly personal papers.[9]

My first 'real' archive, the Division of Rare and Manuscript Collections at Cornell University, was a shock, though. Cornell held the Regional Science Archives,[10] including papers of affiliated individuals like Walter Isard and William Warntz. As a space, the archive was clinical, meticulously organized, rationally arranged down to the last decimal place. It had to be. Serving as a prosthetic memory, as Steedman (2001: 67–8) argues, the archive must be scrupulously organized to allow what is stored to be recalled when required. But at first I saw little order, only blooming, buzzing confusion. I didn't know what to ask for or how to ask for it. When a kind archivist kindly gave me the finding aid and I retrieved my first box of material from storage, I then didn't know how to search through it, how to take notes and on what, and how to read the texts inside.[11]

Taking off the lid of my first box was viscerally thrilling. I had 'archive fever' (Derrida 1995), a sense that I had uncovered origins, found 'the beginning of things' (Steedman 2001: 3). Best of all, in spite of the finding aid, you never really knew what you would discover when reaching your hand into an archive box and pulling out a folder. Some documents produced chills (Barnes 2013). Most often I discarded what I retrieved. That doesn't mean it was waste. What I put to one side could well be vital to someone else. Foucault, who worked in archives his whole academic life, recognized that anything within the archive could be potentially useful: 'However banal [the archival material] may be, however unimportant its consequences may appear to be, however quickly it may be forgotten after its appearance, … [it] is always an event … emerg[ing] from its historical irruption' (Foucault 1972: 31). Anne-Marie Stoler (2009: 3) who drew on Foucault in her own archival study of Dutch colonial records similarly wrote what was "left" [in the archives] was not "left behind" or obsolete' (Stoler 2009: 3), but continued to contribute to 'the pulse of the archive' (Stoler 2009: Chapter 2).

Most of the people whose archives I read had shuffled off this mortal coil several decades earlier. I was taking the pulse of the dead, but life still throbbed on the page. Rather than dry as dust (and sometimes literally turning to dust, Steedman 2001), the archival material was as lively as the oral histories. Of course, archival documents contain errors. Collections are spotty and piecemeal, but they were crucial to reading across the grain of the stories I gathered in the oral histories, providing context and correction.

How Should We Write?

Everyone who contributes to the history of geography faces the same prospect of filling a blank page or screen. Yet there is little discussion about how history should be written. It is as if the writing takes care of itself. As John Gregory Dunne (2006, 373) put it, writing is 'manual labor of the mind, a job, like laying a pipe.' From this perspective, writing is mechanical: lining up words in the right sequence, checking off verb and subject agreements, eliminating grammatical horrors like the split infinitive (one of my doctoral thesis examiners spent their time counting mine – 93), and scrutinizing punctuation placement.

Since the mid-1970s, there has been increasing recognition within the human sciences – which has found its way into geography – that writing was more difficult than avoiding the temptation of the split infinitive, or ensuring the correct placement of commas. Writing was judged inherently problematic. Of course, the individual act of writing was long known to be hard, but this was not just about finding le mot juste. It was a general 'crisis of representation' (Marcus and Fisher 1986). There was a structural problem in the very practice of writing, in the intrinsic ability of words to represent the world out there and what we did in it (see McGeachan and Philo, chapter 24 this volume).

Critics, the most well-known of whom was Derrida, developed the literary technique of deconstruction to suggest that the problem stemmed from the fundamental instability of language. On the surface, words appeared fixed and certain, as real and stable as the objects they named. But this was only outward appearance. Underneath they were contradictory, ambiguous and incomplete, a word's meaning having the proclivity to slip, slur and slide. Deconstruction was a technique to demonstrate that instability, showing how an apparent intended meaning was subverted, could be turned even into its opposite. For example, in Derrida's (1986) contrapuntal reading of the American Declaration of Independence, lurking within the 'inalienable rights' of 'life, liberty and the pursuit of happiness' were their monstrous opposites: death, incarceration and misery. You could never mean what you say, or say what you mean. The meaning of words could always be otherwise. But there was no alternative either. Writing was the only game in town.

Out of these realizations, there has been textual loosening and experimentation. I've been drawn to two of its forms in writing the history of geography. The first is a playful prose. Once we drop the conceit that we are able to tell it like it is, the idea that words stick to objects in a one-to-one relation, directly and incontrovertibly, new possibilities open up for how we write. We are no longer constrained by the straitjacket of objective writing. Indeed, it becomes possible to signal that objectivity is impossible by playful prose. Writing about the history of geography would then operate at two levels: it would relay a story, in this case, a history of geography, but the very writing of that story would raise issues about its objectivity as history. This does not paralyse writing disciplinary history, but it becomes more complex (and interesting). What particular writing strategies are available?

Irony is one. At its core, irony implies dissimulation, denying a single meaning, saying one thing, implying something quite different. For example, the central character in Edward St Aubyn's *At Last,* Patrick Melrose, says: 'Just try giving up irony, that deep down need to mean two things at once, to be in two places at once, not to be there for the catastrophe of a fixed meaning' (St Aubyn 2011: 60). Irony can be achieved by an occasional textual wink or nudge or it can frame the whole work (Julian Barnes's) *Flaubert's Parrot*, 1984, does that by ironically enclosing the non-fictional biography of Gustave Flaubert within fiction). Using irony is not giving up on history. But it suggests that it is more difficult than supposed. It needs to involve more than lining up fixed meanings in the 'right' order. It requires multiple registers: to tell the story, but in telling the story to raise questions about it as a story. That is what irony does.

There are other disruptive (playful) strategies. Self-reference is another. While a text may suggest it wrote itself, seemingly a straightforward chronicle of the facts, it didn't, because here I am, the author, directly talking to you. Hi! Or another strategy is humour. Arthur Koestler (1964) argued that a joke works by putting into conflict two competing frames of reference. Laughter is sparked by the gap in between, in trying unsuccessfully to reconcile two irreconcilable frames of references. Take for example the joke, 'Richard Hartshorne thought history was true as long as he believed it.' The humour here (such as it is) derives from the disjuncture between Hartshorne holding to the idea of an independent historical truth, but asserting that truth was only independent when he happened to hold it. The smile is the disruption, trying to hold to objective truth, while at the very same time denying it.

The second disruptive form of writing, and evident especially in the introduction to this chapter, is reference to who I am, and undertaken by situating myself within the text. As a technique, it underlines that writing does not originate from on high, brought down fully formed from the mountain top. It is written within the hurly-burly events of ground level by a particular person, embedded within their own particular geography and history. Bringing the author on to the page can produce charges of self-indulgence and self-aggrandisement (Lorimer 2010: 253). Foucault, for example, never explicitly wanted to be in his books, reluctant to reveal anything publically about himself. He said, 'I am no doubt not the only one who writes in order to have no face. Do not ask who I am' (quoted in Macey 1993: xiii). But he acknowledged that his writings reflected the details of his own life: 'Each of my works is part of my own biography,' he said (quoted in Macey 1993 xii). But if he never reveals his biography, how can his readers fully appreciate his writings? He may have hoped that his writings had no face, but clearly they did. It was his own. Explicitly introducing in this chapter, the author – me – giving facts about my life, is to show precisely how my own biography puts a face on the writing, situating it, undermining claims to objectivity, but still narrating a history of geography.

STORIES FROM GEOGRAPHY'S PAST

I turn now from reflecting on why we should tell geography's history, and how we should tell it, to providing concrete stories from that history. Partly this is to exemplify the chapter's conceptual arguments, to show their relevance, to put them to work. And partly it stems from believing that the best way to demonstrate the importance of knowing geography's history is to tell actual stories from it. Those stories show that geography's past, including that of 70 years ago, my concern, remains vitally alive, compelling and relevant. Geography's past should be studied not from a suffocating sense of disciplinary obligation, but because it tells us striking things about ourselves, as striking as anything produced by contemporary research.

Why Write American Geography's Second World War and Post-War History?

But why focus on the history of American geography from the Second World War? Because it is the history of the present – both mine and, as I will also suggest, the discipline's as well. Let me start with mine. I never did history. I thought it dull and tedious, about lining up past facts in chronological order. It was the present that was important. But in the late 1980s, I began to think about my own present. Why did I study what I did? I concluded it had everything to do with the history of geography. Or more precisely, it had to do with the particular moment in geography's history when I enrolled within it: autumn 1975 at the Department of Geography, UCL. In Foucault's terms, it was then I became a geographical subject, disciplined from my first year by compulsory courses within the prevailing quantitative-mathematical order. That continued in graduate school, resulting in a PhD thesis filled with equations. But I was not a mathematician. Worse, I was a bad mathematician pretending that I was good. I felt a fake, dreading the moment when I would be exposed. But what was the history that produced that present? Initially I thought it was geography's Quantitative Revolution of which I caught the last gasp at UCL, explaining those courses in quantitative methods. I started there, but I quickly moved down the escalator of history. To understand the 1960s, I needed to understand the 1950s, but to understand the 1950s I needed to understand the 1940s, which led me to the Second World War. There I paused.

The science studies writers Andrew Pickering (1995a, 1995b) and Donna Haraway (1997) both portray the Second World War as the beginning of a different form of knowledge production. Pickering (1995a: 5), following Michel Foucault's vocabulary for marking off abrupt discontinuous epochs, applies the term 'World War II regime' to understand the period. Haraway prefers the compound term, 'technoscience'. She likes that word because she thinks the Second World War was marked by a willingness to join and collapse entities, institutions, techniques and forms of knowledge that formerly were held distinct and separate. For

Haraway (1997), and this is also Pickering's (1995b) conclusion, from the Second World War onwards knowledge was produced by new 'cyborg' entities that constituted techno-science or the 'World War II regime'. Ideas, techniques, machines, academic subjects and institutions were brought together in combinations never existing before, undermining old boundaries, creating cyborg hybrids.

Pickering (1995b; Pickering and Guzik 2008) uses the metaphor of 'mangling' to represent the process by which previously separate entities were joined in new combinations. It is an attractive metaphor because it connotes both an openness to history (anything can happen, and will) and an openness to what can be included within history (it was not only humans who enter the mangle but books, machines, organizations, concepts and much more). Haraway (1997) further adds an explicit mechanism for bringing entities into technoscience. It is through 'hailing' and 'interpellating', terms taken from the French Marxist Louis Althusser (1971: 173). Hailing is the ability of an entity to draw people and things to it; interpellation is the force that allows the people and things that have been hailed to stay put, in this case, generating new scientific knowledge. Further, for Haraway, it is not only humans that are converted into subjects by 'hailing' and 'interpellating', but also material entities, institutions and even academic disciplines like geography.

The reason for a history of geography that starts at the Second World War, therefore, is because there are good grounds for seeing that moment as a crucial divide. It represents the beginning of a new form of knowledge and conditions of production. Haraway and Pickering are primarily concerned with changes that occurred in the physical sciences. These include its increasing instrumental character and that it is machine-reliant, multi-disciplinary, collaborative (not only team-work in the laboratory but also in a range of participating partners outside – industry, government, universities), mathematical and model-based, and is undertaken on a large scale ('big science') requiring significant amounts of money, equipment and trained personnel (Barnes and Farish 2006).

But those same (cyborg) characteristics increasingly took hold in the social sciences as well. An example during the Second World War, and particularly germane for American geography, was at the Research and Analysis (R&A) Branch of the Office of Strategic Services (OSS) (Barnes 2006; Barnes and Farish 2006). The OSS, forerunner of the Central Intelligence Agency, was established by order of the US President in July 1941 to gather and analyse information bearing on national security. OSS's intellectual heart was R&A, which employed several hundred American and émigré social scientists including geographers.

Initially, R&A produced its knowledge along traditional disciplinary lines. But in January 1943, it was reorganized, moving toward the cyborg model of the physical sciences. Social scientists began working collaboratively in large teams across disciplines on specific instrumental problems. There was also a limited development of mathematical models and statistical techniques, primarily carried out by economists and psychologists. Increasingly, the Branch strove for rigour, systematicity and explanation, defining its mandate in terms of scientific objectivity and the deployment of a pure and presuppositionless logic (Katz 1989). This wider sensibility especially attracted the younger geographers of R&A – Edward Ackerman, Chauncy Harris, Edward Ullman. It was at such marked odds with the practices and methodological inclinations of the discipline they had known, and codified as ideographic in Hartshorne's (1939: 449) *The Nature of Geography*: 'a descriptive science concerned with and the interpretation of unique cases….'

Once the War was over, and with a general move to make social science more like physical science, Hartshorne's disciplinary definition became increasingly inappropriate. The post-war conviction was that science had won the war. It had delivered the nuclear bomb, radar and even an embryonic form of the computer (the Colossus at Bletchley Park, credited with breaking the German U-boat Enigma code). Comparable achievements could be attained by the social sciences provided they became like the physical sciences. Such a shift was also urgent given that a new war, the Cold War, began to tighten its grip. As Daniel Bell (1982: 13) wrote in his survey of post-war social science: 'If the widespread mobilization of science, and the concentration of some specific objects, could produce scientific and technical breakthroughs, why could not a similar mobilization ... produce similar results in the social sciences? ... The social sciences were [to] becom[e] "hard", like the natural sciences.'

And so they did. Reflecting on changes in the human sciences over the period 1940–1960, Carl Schorske, who was also at R&A, notes the 'passage ... from range to rigor, from loose engagement with a multifaceted reality historically perceived to the creation of sharp analytical tools that could promise certainty where description and speculative explication had prevailed before' (Schorske 1997: 295). Although most obvious in the massive post-war mathematization of economics, the move to analytical rigor was also found in fields as diverse as philosophy, political science, sociology and, my concern, geography. That was what the Quantitative Revolution was about.

In particular, those three R&A young geographers, Edward Ackerman, Chauncy Harris and Edward Ullman, were 'hailed' and 'interpellated' by the emerging Second World War regime while they were at OSS. That regime was heady stuff, promising intellectual respectability, tools for positively changing the world, getting to the bottom of things. Slowly, but surely, to use Pickering's (1995b) metaphor, geography was mangled, squeezed and tangled with other post-war social sciences, including kindred disciplines like planning and regional science. As it did so, geography became more abstract, theoretical, concerned with mathematical and statistical models, mimicking the production of knowledge in other Cold War sciences. This is why I had the education in geography I did.

Theorizing the Quantitative Revolution from Science Studies

I was drawn initially to the Edinburgh School in theorizing the history of geography's Quantitative Revolution. That School made the scientist's social interests central, showing how they ineluctably seeped into and coloured their apparently 'objective' scientific claims. For example, Donald MacKenzie (1981) demonstrated how the different social class positions of two early twentieth-century British statisticians, George Udny Yule (an aristocrat) and Karl Pearson (a member of the Victorian bourgeoisie), produced two quite different but logically consistent forms of the regression equation for nominal data. The class interests of the two men were built into the mathematical architecture of their respective formulae.

Although I tried, I found it very difficult to replicate such studies for geography (Barnes 1996: Chapter 4). The class interests of quantifiers seemed so varied, and in any case difficult to know. Virtually all the early practitioners were male as well and exclusively white. Undoubtedly, the social interests embodied in masculinism (e.g. rationality, certainty, domination, a God's-eye view; Rose 1993; Berg 1994) came out in the wash of the Quantitative Revolution, and possibly whiteness too (although I was often unsure exactly how). But once I made those points about the social identity of geography's quantifiers, an enormous amount of information in my box was still left unused.

Another approach, not based on the social interests of individuals as such, but related, was Allen Scott's (2000) contention that the Quantitative Revolution emerged out of the socio-political interests of a Fordist Keynesian welfare state. His argument that the Quantitative Revolution occurred because it was necessary for the reproduction of the

Fordist regime smacked, however, of some form of functionalism. As with all functionalist arguments, it left no room for the individual and their contingent life experiences. But that was what my box contained, an accumulation of life contingencies.

I therefore moved away from the social interest focus in science studies to a different variant – ANT (Barnes 1998). ANT seemed better suited to representing the often enormous work needed to produce knowledge, as well as to capture its fragility and contingency. It rejected the singular causal role of social interests, emphasizing distributed agency spread across many actors, human and non-human. It encouraged the inclusion within the narrative of material actants – books, machines and devices – recognizing their agency. It wasn't overtly historical, but at least in Bruno Latour's (1987, 1988, 1999, 2005) rendering, the illustrations were historical. Further, ANT emphasizes process, 'science in the making', rather than 'ready-made science' (Latour 1987: 4). And what is an emphasis on the *process* of making knowledge, Ian Hacking (2002: 8) asks, 'if not historical'?

The post-war rise and fall of North American regional science provided a useful case study to illustrate the advantages of using ANT (Barnes 2004). Regional science ran parallel to quantitative geography, but at periods the two intersected. Regional science was the vision of a single man, the American economist Walter Isard (1919–2010). From the 1940s, he tirelessly sought to develop a new discipline that combined orthodox economic theory and quantitative techniques to examine spatial economic, and later social, cultural and political issues. The first meeting of the Regional Science Association was held in Detroit in December 1954, and the next year the founding Department of Regional Science at the University of Pennsylvania was inaugurated (both had Isard at their helm).

Regional science has now exhibited the full trajectory of a discipline, from energetic beginnings to sputtering end. The substantial amount of effort required to produce that trajectory was clear from my first visit to the archives. There were 100 large archive boxes of Isard's papers housed at Cornell's Division of Rare and Manuscript Collections, 28 devoted to his correspondence alone.[12] Isard's correspondence occupied so much space because that was the epistolary work it took to start and maintain a new discipline. It involved him continually putting out fires, trying to unblock blockages, make new contacts, extend the network and exhort members to do better. At first it worked, with strong flows around the Latourian circuits, which included cutting-edge machines (instruments) such as the computer (and entering American universities for the first time during the early- to mid-1950s when the Regional Science was founded); colleagues such as those at the University of Pennsylvania who agreed to underwrite a new discipline; allies such as economic geographers some of whom joined regional science departments, contributed to regional science journals and certainly borrowed regional science theories and analytical techniques; and the public, like government bodies, planning agencies and non-profits, many of which gave to regional science the life blood of funding.

But some time in the 1980s those circuits began to block and the pulse of regional science became fainter. There was no longer cachet from using a computer now because everyone had them. There was not much cachet in regional science either once the founding Department at Penn was closed in 1995, even though the dean who closed it, Rosemary Stevens, recognized 'that our Department of Regional Science … [was] the flagship department for the discipline' (quoted in Bailly and Coffey 1994: 38). Regional science's allies, particularly geographers, also began deserting; lured by a geographical Marxism established in part as anti-regional science. And the public was not so keen either because regional science seemed increasingly set in aspic, 'largely having failed to evolve beyond its 1950s origins' (Bailly et al. 1996: 157).

Because geography's Quantitative Revolution was entangled in a larger disciplinary project, its history cannot be as neatly defined as regional science's. Unlike regional science, which did not institutionally exist until December 1954, academic geography went back at least a hundred years earlier, with claims that its origins were with the Ancient Greeks (Martin 2005). Given the weight of this disciplinary tradition, which is what Hartshorne's argument drew on making his book doubly weighty, the effort to break free to launch the Quantitative Revolution required both propitious circumstances and concerted effort.

One set of propitious circumstances was bound up with war, the Second World War and, later, the Cold War. If in the OSS, some young geographers were exposed to social scientific methods, the Cold War was even more of an impetus, providing money for quantitative projects and training in analytical techniques that together produced Schorske's (1997) 'new rigorism'. Geography was hailed and mangled, at least at some sites, and transformed into a Cold War science. Here, concerted effort was important, although the resulting achievements remained often precarious and contingent. One of the contingencies was the geography of geography's geography. The sites that were first hailed and mangled were either relatively new or marginal in the sense that they were occupied by weak representatives of the earlier dominant (Hartshornian) ideographic tradition. In more established sites, the weight of the regionalist tradition held firm. In some places, it was too firm, becoming crushing as it did at Harvard, which dissolved its department in 1949 because the university's president, James Conant, thought that traditional geography was 'not a university discipline' (quoted in Smith 1987: 159).

The departments of geography at the University of Washington at Seattle and at the University of Iowa at Iowa City were two of the earliest departments to experience the Quantitative Revolution. Seattle's department was never a leading centre in geographical research and, during the first part of the 1950s, it suffered from weak leadership, a husband and wife team, Howard Martin and Francis Earl ('the dinosaurs'; Pitts 2002). They were incapable of resisting the collective force of change represented by the serendipitous arrival from the mid-1950s of a group of energetic, ambitious and intellectually dissatisfied, male graduate students. Later dubbed the 'space cadets', it was this group, funded by Cold War money and supervised, taught and sometime protected by a young professor in the department, William Garrison, who took up quantitative techniques and mathematical models, to launch a revolution. It was gentler at the University of Iowa. The department was established only in 1947, headed by a professor formerly in the Business School, Harold McCarty. There had been no tradition of geography at Iowa and so McCarty began his own. Again drawing on Cold War money, it involved the collection and statistical analysis of empirical data that then would be turned over primarily to economists to explain with their models.

In both cases, the emergence of the Quantitative Revolution was not inevitable. It could not be explained by Berry's (1978) presentist argument about the necessity of science's rise. The Quantitative Revolution was a relational effect, the consequence of many different agents working together to form a stable network. That network consisted partly of graduate students and professors who later, by physically relocating, extended the network to new places. There were many non-human agents, too. They included the 650 IBM computer housed in the attic of the University of Washington chemistry buildings; rows of Marchant electronic calculators; the Gestetner duplicating machine used at the University of Washington for distributing discussion papers around the world; books like August Lösch's *Economics of Location* that one of the space cadets, Brian Berry, brought with him to Seattle from England in 1955; and rooms in buildings like the 'Citadel' in Smith Hall, University of Washington, Seattle, where the space cadets sat, talked, calculated, plotted and wrote. The network of the Quantitative Revolution was heterogeneous and fragile, but for a period it held up, the coursing flowed and pulsated.

The Geography of the Quantitative Revolution

For the network to get off the ground, it needed specific geographical sites at which to form and from which to extend. The University of Washington, Seattle, and the University of Iowa, Iowa City, both exhibited heterotopic qualities. Both were open to new forms of ordering. At neither site was the older form of Hartshornian regionalism hegemonic. Bastion centres of the older form of geographical ordering, such as at the University of Wisconsin where Hartshorne was professor, were not easily contested. But Washington and Iowa, for reasons already given, were. Both were marginal to American academic geography in the sense that the discipline's establishment neither knew much about them nor cared. Consequently, they were potentially ripe for the introduction of new heterotopic forms of ordering represented by the Quantitative Revolution, and utterly different from what went before.

To participate in these heterotopic acts, it was necessary for initiates to engage in purification rites, as well as to gain 'permission and make certain gestures' (Foucault 1986: 26). At the University of Washington, Garrison's new course, Geog 426, Quantitative Methods in Geography, which was offered in the fall term 1955, was the first rite of purification. Indeed, it was the original rite of purification for the entire discipline given that it was the first course in modern quantitative methods in geography ever offered in an American university. Richard Morrill (1998) who was in that first class says, 'it wasn't just the introduction to beginning statistics but the whole gamut from beginning to all that was known in those days. So, it was a ferocious baptism.' But, as with all rites of purification, not everyone was cleansed. Heterotopic spaces remained closed to some people. Richard Preston, a new graduate student at Washington, dropped Garrison's course because he felt unprepared. As Preston remembers, 'When I went in to tell [Garrison] that I was going to drop the stats seminar, he told me that the only way I could flunk that seminar was to demonstrate to him that I didn't belong in graduate school' (Preston 2000).

The other attribute that must hold for a place to participate in the production of knowledge is that they are truth spots (Gieryn 2002); their knowledge claims are 'authentic all over'. What made them that was their peculiar material configuration including bodies, computers, buildings, duplex machines and ink marks on paper. Law and Hetherington (2000: 3) write: 'Material trappings are not just trappings. They are not idle. They are also performative. That is, they act.' In this case, the material trappings of geography's heterotopias acted to perform truth.

The bodies were mostly of white male graduate students who began arriving in large numbers to potential 'truth spots' from the 1950s. *Annus mirablis* at the University of Washington was 1955 when four stars of the Quantitative Revolution serendipitously arrived the same month to begin graduate school: Brian Berry, Dick Morrill, John Nystuen and Waldo Tobler, and to join Duane Marble who was already there.

Some machines were already in place – mechanical calculators like the Monroe or electronic calculators like the Friden or Marchant. But the machine par excellence was the computer. The first commercial computer in North America was the IBM 650 launched in 1954 and bought by Columbia University. Other universities quickly followed, including the University of Washington. Donald Hudson (1955) boasted in a 1955 advertisement for the department about the presence of a computer and, just as important, someone in the department who knew how to operate it, Duane Marble. There was no formal training, though. It was 'bootstrap operations' as Brian Berry (2000) called it. Nor initially were there any formal programming languages. Michael Dacey (1997) remembers you had to 'programme with patch panels, actually plugging in wires'. There was also at least one other important machine at the University of Washington: the duplicator (plus another essential material trapping, unlimited paper). That machine was critical for launching first an internal discussion paper series and from March 1958 an international one that solidified the department as a truth spot. Furthermore, those papers entered into the 'rites' and 'gestures' necessary to gain entry and participate in the heterotopias. Not to have them meant being cast out into the wilderness. Les King (2000) remembers that 'Having earlier been in that milieu in North America with all the discussion papers circulating around, once [I] got down to New Zealand [to take up a position at the University of Canterbury] in 1961 ... it felt like one had fallen off the edge of the world.'

To ensure continued vitality of the place-based network that was the Quantitative Revolution (illustrated by Taylor's map), Latourian alliances needed to hold, mobilities to flow, centres of calculation to calculate and enrolment to extend. Initially bodies flowed, with the revolution brought to new places on the backs of former graduate students as they became assistant professors in such places as Chicago, Ann Arbor or Columbus. Likewise, immutable mobiles flowed first as discussion papers then as specialized journals like *Geographical Analysis* or *Environment and Planning*. Further alliances were made by joining with other organizations such as the Regional Science Association, but also by convening from 1961 National Science Foundation summer schools in quantitative methods that sought to convert the non-numerate to numeracy, and swelling the ranks and sites of the larger network. But as this process unfolded, there were changes in position: Washington and Iowa fell back; Chicago, Northwestern, Michigan State and Ohio forged forward.

From the 1970s onwards, however, these sites within the centre increasingly lost power, their accumulative authority weakening as in quick succession the geography departments at Michigan, Chicago and Northwestern Universities were closed down. In 1993, Penn's Regional Science Department was also shut. What escaped the zone of power was having its revenge as now the old network of quantitative geography unravelled, and new ones emerged based on Marxism, culture and social theory. (Gieryn 2002)

Hearing Voices

Having many of the people still alive who participated in the movement I was studying was both an advantage and a disadvantage. It was an advantage because I was able to talk to many of them, receiving first-hand accounts of what it was like. But that brought its own issues. My procedure was to visit the interviewee, usually in their university office, but occasionally in their hotel room or at their home, and once even in a bar (I learned my lesson). I took notes, but I mainly relied on tape recording the conversation. The tape was then transcribed either partially or fully and sent to the interviewee who amended it as they saw fit.

In all sorts of ways, though, the transcript produced was not a rendering of what it was like. It was often not even a rendering of what the actual interview was like. There were the usual recording problems of machine failure, operator failure and interviewee failure. The result was that the recording was never the mimetic copy promised by the manufacturer of the 'Clear Voice' tapes I used. Then there was the production of the transcript itself. I outsourced some of the transcribing, but because of various acts of fecklessness (both human and non-human), there were textual gaps, ellipses, question marks and fissures in the returned transcripts that required my interpolation, intervention and repair. The transcript was then further transcribed when I sent it back to the interviewee for checking and amendment. Responses varied from none, to removing swear words (leaving in one case almost no adjectives in the text), to the minor correction of names, dates and places, to a complete rewriting of the interview leaving only a coincidental resemblance to the original.

While initially I thought the transcripts were the real thing – the history of quantitative geography – they were not. They provided concreteness, back-stories, funny anecdotes, accounts of real bodies, emotional range and colour, obscure facts, clarification and corroboration. The transcripts were immensely useful texts, but they were not *the* text. No such text existed. As a historical source they suffered from all the usual problems: a fragmentary quality, unreliability, partialness and an inability to speak to larger structural shifts. I do not blame my interviewees. They were conscientious, knowledgeable, interested, unfailingly helpful and enormously generous. After speaking to them, I felt proud to be part of the discipline they helped shape.

But some of them did not think so and were disappointed in my history. This goes to the disadvantage. The archive may well have had a pulse, but at least it did not speak back to me in the forthright terms that some of my interviewees did. One of them would regularly denounce me from the back of the room whenever he heard me speak. 'Why do you hate us?' he would ask. I didn't, but neither did I tell the story exactly in the way my interviewees told it to me. Because they were there, some interviewees thought their voices should be privileged. My history should be exactly as they relayed it. When it wasn't, they felt let down, duped, even betrayed. But that is writing history. My earlier condescension notwithstanding, I came to appreciate that writing history was so much more complicated than lining up events chronologically. It involved going beneath the floorboards of surface accounts, juggling many kinds of texts, often at odds with one another, precariously constructing a narrative that connected heterogeneous parts. I had many motivations for writing the history, but one was to honour my teachers. Now I was told I hated them. No wonder I suffered insomnia.

Fortunately, the archives did not talk back in the same way (although archivists sometimes did: Barnes 2010: 668). But there was a liveliness and emotional resonance to the archival material I did not expect, and found, especially in the letters: Chauncy Harris writing a perfect condolence letter to his dying friend, the Yale geographer, Stephen Jones; Bill Bunge writing an excoriating letter about Richard Hartshone to Andrew Clark (Chair of the Geography Department at Wisconsin), but who was dead by the time the letter was received and it was instead opened by Hartshorne; and a series of wonderfully affectionate letters between an obvious father figure, Derwent Whittlesey, and Edward Ackerman, his student protégé at Harvard and an orphan from age 11.

To appreciate these archival sources, of course, they had to be found, and that was not always easy. That was particularly true for my first archival collection, the papers of William Warntz, which were housed in the Regional Science Collection at Cornell. At the time, that collection was only roughly sorted, the finding aid coarsely organised. Warntz was a hoarder of the worse kind, his materials scattered and unsystematic. Several other personal collections I later examined were undertaken by hoarders, too, like Chauncy Harris. Harris was a meticulous hoarder. He continued to visit the Special Collections Research Center at the University of Chicago, where his papers were kept almost until the day he died (Boxing Day 2003), further sorting, refining and annotating (Barnes 2013). The boxes of Warntz's collection remained virtually untouched since they were packed up and his University of Western Ontario office emptied after his death in 1988 (Janelle 1997). Shopping lists, 'Back in 5 minutes' Post-It notes and retail receipts were mixed up with what most interested me: rusty paper-clipped sheaves of lined sulphur-yellow paper filled with tiny numbers – calculations of potential for Warntz's (1959) social physics-inspired project of 'macrogeography'; Office of Naval Research grant applications; carbon copy letters sent to Bill Bunge telling him, among other things, that Warntz's wife would never allow him in their house again after his last visit and the unfortunate accident with the piano; and a miniature wooden mock-up of a Varignon[13] frame, as well as an inflatable beach ball to accompany a never-completed textbook with Bunge, *Geography the Innocent Science* (there were letters about that, too, in other boxes, along with drafts of chapters).

You never knew what you would pull out next. Other peoples' papers such as Walter Isard's and, in the same Regional Science Collection, were much more orderly. Isard came into the library every day I was there to organize his own papers. They were in such good shape in part, as he later told me, because he had enjoyed past secretarial help, both as Head of Department and as the key administrator for the Regional Science Association (Isard 2000). As I examined other collections, it was then obvious when secretarial help was available, and when it wasn't. When Edward Ackerman was an administrator for the Tennessee Valley Authority in the early 1950s and then at the Resource for the Future Inc. from 1955, his archives burgeoned, but when he was a professor at Harvard in the 1940s and at the University of Chicago for periods during the first half of the 1950s, they shrank. The same was true for John Q. Stewart, the astrophysicist at Princeton.

This speaks to the patchy nature of the archive. In spite of the promise of completeness ('the assumption that the past has deposited all of its traces somewhere', Steedman 2001: 76), things go missing or are never kept in the first place. It produces the 'undeniable ache … for the find that never was', as Lorimer (2010: 261) puts it. I know what Lorimer means. When I was at the National Archives and Record Administration (NARA) in Washington DC, I saw reference to a report written by Edward Ullman, completed at the end of the war, summarizing his contribution to OSS. It was exactly the memo I wanted, but even the brilliant archivist at NARA who helped me couldn't find it. There was another reference to it in Ullman's own papers held in Seattle, but it wasn't there either. I thought I had found it in Edward Ackerman's papers held at the University of Wyoming at Laramie. It was listed in the finding aid, but in spite of the promise of it being in a specific named file, in a specific numbered box, it wasn't there either.

Steedman (2001: 77) interprets the archival urge psychologically: 'searching for … a lost object, which really cannot be found.' That might be true, but clearly much can be located (witness my overflowing box), information from which can then be 'grubbed up and snuffled out' (Lorimer 2010: 258). That which remains unknown should then be seen, as Lorimer (2010: 268) suggests, less as a loss to be grieved over than as an opportunity for experimentation: to gather alternative archival sources and to try out 'more imaginative styles of composition and expression … fusing … poetics and politics.'

Writing Geography's History

Richard Hartshorne (1939) would not have liked that suggestion. His history of geography deliberately shunned imaginative style. Certainly it shunned poetics and politics, presenting itself as stern, objective writing. There was not even a preface to hint at the extraordinary circumstances in which his book was written (discussed below). Later, during the Second World War, when Hartshorne was Chair of R&A Branch's Projects Committee at OSS, he wrote and circulated a detailed memo banning imaginative styles, especially poetics and politics, from any writing carried out within the Branch:

> We should cultivate what might be called a clinical attitude. … The rules of objective writing are presumably familiar. … The most obvious and yet

> most common crime against objectivity is the use of hortatory and value words and phrases. Generally speaking 'should' and 'ought' – not to mention 'must' are taboo. Value adjectives and nouns are to be avoided no matter how much they appear to add literary quality .... Proust, Joyce, or Gertrude Stein would all be equally out of place in R&A.[14]

This attitude will not do in the contemporary writing of the history of geography. Because of the arguments made about the very character of language, it is impossible to be objectivist. We need to write differently.

This is most obvious in the explicit incorporation of me within the text of this chapter. As author I am here the whole time. I don't slope off, the writing then taking over on its own. It is uncomfortable to be so present, going against the grain of everything I formerly believed. But here I am, as I was on the first page, because there is an obligation to make clear that the history of geography I struggled over came from me, the writer. What you write comes from the life you lead and have led, your biography. That's why there are stories about me from my past. They are not meant to be decorative, an alluring lead-in followed by the same old stuff, or an opportunity to trumpet myself. Their primary intent is to show that I am part of the story, that my life shaped the narrative and I cannot be disconnected from it.

The other writing strategy is to shape the style of the prose so that it mirrors the larger argument made about the object of investigation, in this case, the history of the discipline. The approach here is to create a homology between the form of the writing and the form of the argument. There have been several examples within geography of using such tactical prose: Olsson's (1980) hermeneutical poetry, Doel's (1999) postmodern stammering, and most recently Cook's (2004) defetishizing staccato rhythms. Each author uses their style of writing to reflect the logic of their argument. Form and substance merge.

I suggested that the prime issue in writing the history of geography is to convey a history, but at the same time to raise questions about its authority and objectivity. I did that partly through the use of irony, by definition allowing two simultaneous and opposite meanings: both true and not true, both a real story and one made up. The use of the term 'monster', as in 'the monster in the box', was deliberately ironic, signalling both something real and made up. On the one hand, the monster was only too real, unruly piles of paper and other objects, crammed to overflowing in a box sitting on my office floor. It was physically unsightly, emotionally unsettling, scary even. A monster. But at the same time, there is no such thing as a monster. It is made up, fictional, just like the one in Mary Shelley's *Frankenstein*. By playing off the figure of the monster, I tried, in this chapter, both to tell a history of geography and also to ironically raise questions about its telling. Steve Woolgar (1983), writing from within science studies, argued that it was crucial to be an ironist when writing social accounts of science because, while you claimed that there was no such thing as scientific truth, you also claimed that that very statement you just made about scientific truth was true. As a solution to the paradox (known in philosophy as *tu quoque*, 'you too'), Woolgar, in collaboration with Malcolm Ashmore, suggested use of ironic, self-referential writing (Woolgar and Ashmore 1988). My ironic writing is a weak version of what they proposed.

There are other strategies than irony. In more than anything I've professionally written, this chapter emphasizes my role as an author. There were several motivations, but the most important was to embody the writing, to move from an omnipotent and absent narrator (the view from no one) to an author who is situated and present here and now – me. Another strategy was the use of humour – valuable for undermining authority and especially an authority that claims historical truth. Humour can be provoked precisely in the gap that lies between claims to objective historical truth and the specific historical contexts in which that claim is made. I find it funny (maybe tragic, too) that Hartshorne (1939) claimed to have found an objective definition of geography while on sabbatical in Vienna, 1938–1939. What was he thinking? His study was written in the midst of the Nazi campaign of *lebensraum*. Austria was annexed already and Czechoslovakia was next (Hartshorne 1979). If ever there was a time and place in history when it should have been clear to anyone, especially a political geographer, that the definition of geography was not objective, but saturated in politics, it was during 1938–1939 in Vienna where Hartshorne was. You have to laugh.

CONCLUSION

Historiography is usually defined as the history of historical writing, a history of how historians have written about the past, their approaches and methods. To some degree, I took my (given) title for this chapter, geo-historiographies, to be about the history of how geographers have written about their past. But to an even larger degree, I interpreted the title yet more liberally as a basis for a wider discussion of what was possible in writing geography's history. It was within that context that I provided an extended illustration from my own historical research on American geography during the Second World War and the early Cold War period. I realize that the usual point of historiography is not to undertake direct research on history, instead interpreting only what others have written. But the chapter was an experiment, involving rule breaking.

In this, I was inspired in the first instance by Spalding Gray's own rule breaking, an extended performance about why he couldn't complete his performance. He used his stage (and film) act to talk about various events that occurred off stage, preventing him from completing his novel, the monster in the box. It was only by giving a public performance, by unveiling facts on stage about his private life, previously hidden, that Gray was able to write his book, to quell the monster. Similarly, I tried to quell my own monster by disclosing in this chapter events bearing on the contents of my box but which were also previously hidden, kept off stage. By publically setting them out in this chapter, I hope to finish my own project just as Gray finally completed his.

But as I read in preparation for writing this chapter, I found a second source of inspiration, more directly academic, fitting more closely with the concerns of historiography. This was the work of some historians and historical geographers who have begun to rethink the relation of the archive to historical research. Like Gray's performance, these works also involved rule breaking. The traditional view of the archive was as a repository of 'original sources' (Steedman 2001: 9), from which 'facts … could be dislodged' and retrieved (Lorimer 2010: 251). The archive was treated as a passive site, an inert container of facts. Just as the scientist collected their facts by going into the field or the lab and recording objective observations, the historian went into the archive, blew off the dust of ancient volumes and recovered observations of their own, albeit embedded in texts but still treated as objective. I held that same view when I first began my project on the history of geography's Quantitative Revolution. I thought once I collected the historical facts of that revolution that the story would tell itself. But of course it didn't. Work by scholars like Steedman, Stoler and Lorimer partly explains why. There is nothing straightforward about either an archive or the practices of work within it. Both are fraught with political, social and cultural anxieties. These become evident when we start examining the archive not as a fixed passive object, and archive research not as a disembodied activity, but as set of distinctly embodied geographical and historical processes (Lorimer and Philo 2009). That's why stories about these processes need to be told alongside stories derived from the materials held within the archives. I needed to tell stories that came from my box, and stories about my box, and that is what I have done in this chapter.

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NOTES

1 The stage presentation was later made into a film of the same name, *Monster in a Box* (starring Spalding Gray, directed by Nick Broomfield, 1992, 87 minutes, London: Channel 4 film).

2 The full list of interviewees: John S. Adams, Brian J.L. Berry, Larry Bourne, Larry Brown, Patricia Burnett, Ian Burton, William A.V. Clark, Kevin Cox, Michael Dacey, Michael Dear, Roger Downs, William Garrison, Arthur Getis, Reginald Golledge, Michael Goodchild, Peter Gould, Susan Hanson, Chauncy D. Harris, Geoffrey Hewings, John C. Hudson, Walter Isard, Leslie King, James Lindberg, Fred Lukermann, Richard Morrill, Gunnar Olsson, Richard Peet, Forest R. Pitts, Phillip L. Porter, Allan J. Pred, Richard Preston, Gerard Rushton, Allen J. Scott, Edward J. Taaffe, Waldo Tobler and Michael Woldenberg. Only one person unambiguously refused: 'I decline your invitation'. But several put off the interview until 'a later time' that inevitably never arrived.

3 See my 'The historic Eric Sheppard' written for the celebration in 2008 of Eric's University of Minnesota Regent's Professorship. It is available at: www.geog.ubc.ca/~tbarnes/papers.html

4 By giving a performance about why he had not completed his book, Spalding Gray, finished his book, *Impossible Vacation* (1992). He subdued the monster.

5 Volumes like Woolridge and East (1950) and Dickinson (1969) provide no intellectual rationale at all. Freeman (1961: 10) equivocates, but his strongest theoretical argument is that geography's history shows that 'there is no new idea under the sun.' Martin (2005: xv) says something similar: 'We shall learn that many of the ideas of recent times are not new but are ideas of earlier times refurbished.' In both cases, though, such a justification undermines historical enquiry, implying a set of Ur geographical ideas that are repeated over and over again regardless of historical context. Historical enquiry, consequently, becomes pointless because we always know ahead of time what we will find. Stoddart (1986: 25–27) offers a smorgasbord of justifications for historical enquiry. His favourite is that the history of geography shows the force of Gramsci's thesis about hegemony (Stoddart 1986: 18–25), but that thesis is never redeemed in any of his subsequent substantive historical studies. Finally, one would have thought Peet (1998), as a Marxist, would give strong emphasis to history, but while his text is a thorough and clear review of social theory, it is peculiarly flat and atemporal, with historical context all but absent.

6 William Faulkner, act I, scene II, *Requiem for a Nun*, 1951.

7 Both terms refer to Foucault's techniques for writing history by examining discursive traces from the past, directing them to a history of the present. Foucault developed the idea of archaeology in the 1960s and genealogy during the 1970s. Genealogy gives greater emphasis to issues of power than archaeology. Dan Clayton (2000: 291) defines genealogy as 'an historical reconstruction of the relations between power, knowledge and the human subject that aspires to an imminent critique of the present.' I do not carry out here either a disciplinary archaeology or genealogy, but see Gregory (1994: Chapter 2) and Driver (2000) who do.

8 In an essay with Charlie Withers, Livingstone offers a 'spatialized historiography of science' around a threefold classification of 'site, circulation and region' (Withers and Livingstone 2011: 1). The historiography is very useful, but lacking still is a larger explicit theoretical frame.

9 They included the papers of Edward Ackerman (University of Wyoming, Laramie), Chauncy Harris (University of Chicago), Richard Hartshorne (American Geographical Society, University of Wisconsin, Milwaukee) Walter Isard (Cornell University), John Q. Stewart (Princeton University), Edward Ullman (University of Washington, Seattle), William Warntz (Cornell University) and Derwent Whittlesey (Harvard University).

10 Information about the Regional Science Archives at Cornell is found at: www.cornell.edu/search/?q=Regional+science+archives&sa=go&output=xml_no_dtd&client=default_frontend&proxystylesheet=default_frontend&as_sitesearch=http%3A%2F%2Frmc.library.cornell.edu%2F

11 Hayden Lorimer (2010: 251) tells a similar story when he first visited the University Library at Cambridge as a new graduate student. For him, the archive appeared to be 'somewhere between a labyrinth and an impregnable fortress' (Lorimer 2010: 251). Even the seasoned Carolyn Steedman (2001: 75) tells some unusual 'stories about what historians do in the Archive.'

12 The finding aid for the Walter Isard Papers, #3959, Division of Rare and Manuscript Collections,

Cornell University Library, is: http://rmc.library.cornell.edu/EAD/htmldocs/RMM03959.html (Accessed October 23 2013).

13 A Varignon frame was a nineteenth-century mechanical device to locate the most profitable location for a firm based on various weights associated with the costs of its raw materials, labour and transportation. By means of a series of pulleys, lead weights and strings, the Varignon frame identified, for any given type of business, a firm's optimal (cost-minimizing) location site.

14 R. Hartshorne, Draft of proposed guide to preparation of political reports, pp. 7, 8, 10, no date, RG226, entry 37, Box 5, Folder 3, NARA.

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