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Placing ideas: genius loci, heterotopia and geography's quantitative revolution

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Abstract: The paper is concerned with understanding the geography of intellectual creativity and change using as a case study the quantitative revolution in geography. First, I review briefly the sea change occurring over the last 40 years in understanding intellectual production, and made most forcefully in the literature in the sociology of scientific knowledge. I highlight three elements: the nature and persistence of intellectual breaks and ruptures; the embodiedness and material embeddedness of the intellectual process; and the centrality of networks and alliances. Secondly, I take each of these three components of intellectual production, and work them through different theories of place to illuminate the role of the geographical within each. In particular, I argue that the geography of intellectual rupture is clarified by using Michel Foucault's notion of heterotopia, that the place of intellectual embodiment and embeddedness is elucidated by Kevin Hetherington and John Law's work on materiality, and that spaces of network and alliance are enhanced by the recent writings on place by Nigel Thrift. Finally, using these three features I present an interpretive analysis of the place of geography's quantitative revolution drawing upon 36 oral histories I conducted with first- and second-generation pioneers of that movement.

Key words: place, quantitative revolution, science studies.

I Placing ideas: genius loci, heterotopia and geography's quantitative revolution

Maybe it is a peculiar quirk of my personality, or that I am a geographer, but I keenly recall the places where I am first exposed to what become for me important ideas. In spring 1975 on the evening train from Exeter St Davids to St Austell as we were

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passing the red sandstone coastal stacks and arches around Dawlish, I was reading Joan Robinson's (1964) book *Economic Philosophy*. I had reached page 33 where she introduces for the first time the Italian Cambridge economist Piero Sraffa. I was instantly besotted. A man of few words, Sraffa nevertheless solved Marx's transformation problem, skewered neoclassical economics, and found the solution to Ricardo's quest of a will-o'-the-wisp, invariable standard of value. Even though I had not yet entered university as an undergraduate, I knew my doctoral dissertation would be about him. It was (Sheppard and Barnes, 1990). In the dreadfully bitter Minnesota winter of 1979, I was walking back across the Washington Avenue Bridge over the frozen Mississippi. I had just picked up my examination blue book from Fred Lukermann with whom I had taken a graduate seminar in the History of Geographical Thought when it belatedly dawned on me what his course had been about. Culture. It was an epiphany (Barnes, 2003). Everything changed. Well almost. I still wrote a doctoral dissertation about Sraffa, but interpreted now as a cultural economist. In the unusually sunny British autumn of 1989 while on academic leave at Bristol University, squirreled away in Peter Haggett's secretary's office behind the Geography Department lounge, I read with the blinds down David Bloor's (1976) *Knowledge and social imagery*. It was a difficult book. Yet, somewhere around the beginning of Chapter 3, I saw the light: science reflected society. I came out of my room for elevenses a new man, with an idea that would shape my research agenda for the next decade, and which influences this very paper.

My examples, I realize, are idiosyncratic, highly personal and inconsequential for anyone but me, but they indicate the larger question I want to pursue in this paper: the relation of geography to intellectual production. How are ideas related to the places in which they are conceived?

Propelling my question is research consuming me almost for a decade on the history of geography's quantitative revolution, the intellectual movement beginning in the 1950s that explicitly introduced to the discipline scientific forms of theorizing and techniques of empirical verification. That research, which also involved examining crossover movements like regional science, entailed collecting 36 oral histories from first- and second-generation pioneers.[1] Clear to me now is the large degree to which that revolution was place-based. It emerged piecemeal in a select set of sites, and even at its peak was found and perpetuated in only a limited number of places. A diagram first drawn by Peter Taylor (1977: 15) unintentionally, I suspect, illustrates the limited geographical reach (Figure 1). The figure is useful not only for what it shows – that is, the now familiar tied centres of geographical calculation central to the quantitative revolution – but also for what it leaves out. Think of all the schools and departments of geography absent from Taylor's map, not only in North America and Europe but also in the rest of the world. Where is Africa, or Asia, or South America, or Australasia? The point is that the quantitative revolution was punctuated, spotty and peculiar to particular sites. But why those sites, why those places?[2]

Rationalism, the philosophy that in the past dominated explanations of intellectual change, contends that place does not matter in the production of ideas. Defined as the application of rational principles to the object of investigation, rationalism by its very definition as universal, as found everywhere, renders irrelevant the peculiarity of the place in which inquiry occurs. It matters not one whit, for example, that Brian Berry was a graduate student in Seattle at the University of Washington in

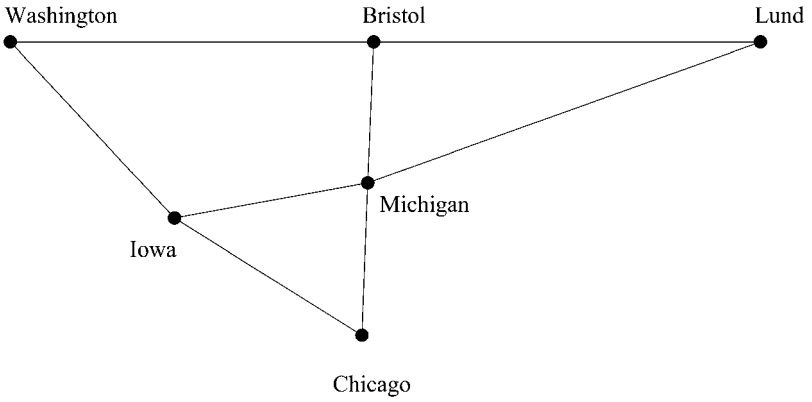

Figure 1 Quantgeog Airlines flight plan
Source: Redrawn from Taylor (1977: 15)
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1958 when he and his graduate supervisor Bill Garrison wrote what became an important paper in the quantitative revolution, 'The functional bases of the central place hierarchy' (Berry and Garrison, 1958; see also Barnes, 2001). That paper used data on the urban hierarchy of Snohomish County, WA, to verify through inferential statistics the abstract spatial logic of central place theory. Under rationalism, critical is only logical impeccability. Where it occurs is unimportant. Berry could have produced comparable results had he remained in London where he was an undergraduate at UCL, or at Minnesota or Wisconsin where he was also accepted for graduate school (Berry, 2002: 26). Of course, Berry had to be somewhere, but in the rationalist view the precise location is immaterial. If not Snohomish county, then the urban hierarchy of Kent or Hertfordshire, or White Bear County in Minnesota, or Dane County in Wisconsin. Key is only the rational mind. Indeed, in the rationalist 'view from nowhere', as Thomas Nagel (1986) labels it, to emphasize place is to undermine the very credibility of inquiry. As one commentator wrote, 'It was the end for cold fusion when people decided it only happened in Salt Lake City' (Kohler, 2002, quoted in Livingstone, 2003: 2).

Against this rationalist view, however, has emerged quite a different one over the last 40 years that says place is utterly critical to the formation of ideas. Ideas are not titrated onto the page drop by drop from a distilled rationality. They are a consequence of grounded social practice. Mao Zedong asks: 'Where do ideas come from? From the sky? No, from practice.' And practice is always undertaken in particular places. Brian Berry (1967: vii) may later say 'that central place theory constitutes a deductive base from which to understand . . . regularities over space and . . . time'. But what he actually did was sit in his fourth-floor office, the Citadel as it was nicknamed, in Smith Hall on the University of Washington campus, Seattle, in the summer of 1956, and rearrange and reinscribe in the form of tables, maps and statistical calculations and coefficients questionnaires originally taken by Duane Marble the year before documenting the number of businesses in the towns and cities of Snohomish County, WA (Barnes, 2001). That Berry carried out such practices stemmed partly from him having enrolled in the first ever quantitative methods

course ever given in an American Department of Geography, Geog 426, offered by Garrison; having taken Edward Ullman's[3] graduate seminar in urban geography in his second term; and having been donated data from Marble from an unrelated project funded by the State of Washington.

Place is not mere background atmospherics, but provides for the very possibility of intellectual innovation. Intellectual inquiry is not the view from nowhere, but the view from somewhere. This anti-rationalist position in which place figures large as an integral component of intellectual production has been worked out theoretically in different ways, often by nongeographers. My purpose is to draw on these literatures to construct a theoretical framework for understanding the role of place within intellectual production that I will exemplify using geography's quantitative revolution.

The paper is Gallic in structure with three sections, each subdivided into three. First, I review briefly the sea change occurring over the last 40 years in understanding intellectual change, and made most forcefully in the literature in the sociology of scientific knowledge. I highlight three elements: the nature and persistence of intellectual breaks and ruptures; the embodiedness and material embeddedness of intellectual production; and the centrality of networks and alliances. Secondly, I take these three components of intellectual production and work them through different theories of place to illuminate the role of the geographical within each; that is, I place them. In particular, I argue that the geography of intellectual rupture is clarified by using Michel Foucault's notion of heterotopia, that the place of intellectual embodiment and embeddedness is elucidated by Kevin Hetherington and John Law's work on materiality, and that spaces of network and alliance are enhanced by the recent writings on place by Nigel Thrift. Finally, using these three features I analyse the places of geography's quantitative revolution drawing upon oral histories.

II Producing ideas

In the standard rationalist account that in the past dominated discussions of epistemology, there is nothing problematic about intellectual production. Rational knowledge is brought under a common set of rules that allows comparison and thereby resolution of conflict or discrepancy (commensurability), and is cumulative in that new knowledge expands and advances old knowledge (progress). As Isaac Newton famously said in a letter to his rival, the vertically challenged Robert Hooke, 'If I have seen further it is by standing on the shoulders of Giants' (quoted by Gleick, 2003: 98). Newton's metaphor speaks to both attributes of rationalism. Because of its rational foundation, Newton assumes that his knowledge is a continuation of, that is, commensurate with, his predecessors (the 'Giants'), such as Copernicus, Kepler and Galileo (and pointedly not Hooke), and the elevated position afforded by rationality allows Newton to gain greater knowledge – 'to see further' – than his predecessors. It is progress.

Against this rationalist view of commensurability and progress that makes production of knowledge appear a technical exercise to be completed by people in white coats, or in Newton's case frock coats, over the last 40 years there has emerged a radical alternative associated with writings in the sociology of scientific knowledge. Arguing against commensurability, and the idea of progress, this body of

work emphasizes the gaps and fissures, the blind alleys and dead ends, the points where rationality breaks down. This alternative approach is not slick or heroic or triumphal – Mary Hesse (1980: 30) says it has been 'a notorious black spot for fatal accidents' – but it begins to make sense of how ideas are closely tethered to the eccentricities, complex interests, materialities and messiness of lives lived at particular times and places, rather than to a polished, distant, universal rationality. Even Newton is not exempt. While he had his *annus mirabilis* when during a 20-month period of prodigious mental concentration he assimilated everything in the world then known in mathematics, and for good measure extended it by inventing calculus, he actually spent far longer in his two-storied wooden 'elaboratory', as he called it, at the bottom of his Fellow's garden at Trinity College, Cambridge, testing witchcraft recipes to transmute base metals into gold (Palter, 1970; Keynes, 1951). Indeed, posthumous forensic evidence suggests that his later, sometimes tenuous, grip on everyday life – Keynes (1951: 321) calls him 'slightly "gaga"' – was a result of mercury poisoning contracted during alchemic experiments (Gleick, 2003: 99). Here, if anyone, is a man confounded by a messy life, materialities, complex interests and eccentricities.

The literature of the sociology of scientific knowledge is vast and sprawling, and there is no single agreed-upon approach (Hess, 1997). In advancing the argument of this paper, let me make three general points that stem from it.

1 Intellectual breaks and ruptures

First, openings, breaches and cracks are normal in intellectual inquiry, not progress. Hence the rise and sometimes decline in the postwar period in geography of the quantitative revolution, or radical geography, or the cultural turn. Or embodied in well-known intellectual about-faces such as by David Harvey (from *Explanation* (Harvey, 1969) to *Social justice* (Harvey, 1973) in four years), or Gunnar Olsson (from *Distance and human interaction* (Olsson, 1965) to *Birds in egg* (Olsson, 1975) in 10 years) or Doreen Massey (1968) who studied Regional Science at Penn and whose first publication at the Centre of Environmental Studies was on linear programming (for her take on what she was doing, see Massey, 1993). This is just the kind of interrupted disciplinary trajectory that we should expect. Thomas Kuhn (1962) was the first to make this point abundantly clear. Disputing both commensurability, because of the value-laden nature of theory, and progress, because different approaches are incommensurable (they are like Gestalt shifts), Kuhn thought science was driven by a series of intellectual revolutions, 'paradigm changes', each of which formed distinct, separate and partly incomparable worlds of inquiry. Key theoretical terms, instruments, measuring devices and metrics, and experimental practices all change from one paradigm to another vitiating rational evaluation.

An elaboration of Kuhn's thesis is Ian Hacking's (1985; 2002a; 2002b) writing on styles of scientific reasoning. It is useful because it clarifies both the nature of the radical discontinuity sometimes found in the production of ideas and the conditions that maintain such separateness. A style of scientific reasoning for Hacking connotes both the historical-cultural nature of intellectual projects – they are 'styles' – and their peculiar nature based upon specialized vocabularies, logics, practices and forms of explanation – 'scientific reasoning'. In particular, Hacking argues that their most interesting feature is 'self-authentication' (Hacking, 2002a: 4); that is,

they both determine the criteria of truth by which they are judged and bring into being the objects that they claim to study.

Briefly, Hacking (1985: 146) writes: '. . . the very candidates for truth and falsehood have no existence independent of the styles of reasoning that settle what it is to be true and false in their domain.' Statistics, for example, one of the styles of reasoning that Hacking lists, is introduced into geography by the quantitative revolution. Papers such as Berry and Garrison's (1958), through the statistical style they prosecute, initiate within the discipline new candidates for truth and falsehood never contemplated before. The criteria of truth and falsehood they deploy, and embodied in statistical coefficients like chi square and regression found in their paper, is given only by the statistical style on which they draw, having no purchase outside of it.

Secondly, styles also bring into being the subject matter that they claim to study. Hacking (2002b: 189) provides a list, which 'include[s] new types of: objects; evidence; sentences; new ways of bringing a candidate for truth and falsehood; laws, or at any rate modalities; and possibilities'. For example, created by the quantitative revolution were: new objects such as computer punch cards stamped with spatial data; new evidence such as the number of service functions found in central places; new sentences for bringing forth candidates for truth and falsehood such as regression lines; new laws such as Tobler's (1970: 236) First Law of Geography that 'everything is related to everything else, but near things are more related than distant things'; and new possibilities such as through Peter Gould's (1979) Augean stables allegory which suggests that after decades of neglect the cleansing power of quantification finally makes geography a legitimate form of intellectual inquiry, at last a social science.

The point is that by its very construction the quantitative revolution was irrevocably marked off from what went before with respect to criteria of truth, and objects of study. The resulting incommensurability undoes any assertion of disciplinary rational progress.

2 Materialization: bodies and things

Secondly, intellectual production is always materialized through human bodies, and nonhuman objects. Neither is present in the rationalist account. Humans in the rationalist account, to use Hilary Putnam's (1981: 7) image, are presented as isolated 'brains in vats', disembodied, disconnected, disembedded. In the sociology of scientific knowledge view, however, knowledge never arrives from pure brainpower, from only sparking synapses. It is the outcome of embodied practice. Scientists are not faceless organs of scientific rationality, but real people with particular kinds of bodies, histories, skills and interests that make a difference to the kind of knowledge produced.

To use the earlier example, while Berry and Garrison were keen not to appear on the same page as their equations and diagrams of functional centrality in Snohomish County, there is a crack in the otherwise faceless façade of the paper where their embodiedness is briefly revealed: on the title page (Berry and Garrison, 1958: 145):

> Mr Berry is a Teaching Associate and Dr Garrison is an Associate Professor in the Department of Geography, University of Washington. The present paper is one of several related to patterns of routes, urban sizes, and land uses stemming from recent research at that university.

This fleeting glimpse is enough. It immediately raises embodiment questions: how was it that Mr Berry, a Teaching Associate, and Dr Garrison, an Associate Professor, came together at Washington to write 'the present paper', and what was it about the historical trajectory of their lives that converged to produce 'the recent research at that university' of which their paper is a consequence?

Just as embodiment is excluded within rationalism, so is its embeddedness in things. To use Bruno Latour's (1993: 79–82) vocabulary, material artifacts in the rationalist view are treated as intermediaries. They are the means to move from abstract hypotheses to equally abstract forms of knowledge that address them. The artifacts themselves are presumed neutral, pure conduits, their materiality never affecting either hypothesis or knowledge. Again, the sociology of science literature would contest such a view. Rather than intermediaries, material artifacts, again to use Latour's (1993: 79–82) vocabulary, are mediators, shaping, channelling and entering into outcomes.

An example is Peter Galison's (1997) study of twentieth-century microphysical detectors in physics laboratories that serve as 'mediators between the production of phenomena and the production of evidence' (Galison, 1997: 3). The metaphor of connection Galison uses is a trading zone. The changing detection devices – cloud chambers, bubble chambers, honeycomb and ball detectors – are not inert pieces of wire, glass and tubing, naïvely responding to the beck and call of the concepts of theoretical physicists, but have some agency of their own, entering into a wider exchange, a larger trading system, in their own right. As a result, the history of the detector machines that Galison tells is not just a technical history – how material bits and pieces are physically joined (although it is that as well) – but it is also 'part labor history, part sociology, part epistemology. It is a history inseparable from individuals' search for a way of working in laboratories sited squarely in a particular culture – here of Victorian Scotland, there of war time Los Alamos. It is a history of twentieth century microphysics written from the machine outward not from the unifying theories inward' (Galison, 1997: 5).

Writing a corresponding history of geography's quantitative revolution from the machine outward is difficult, but machines were used. The Frieden calculating machine, for example, was employed by Berry to carry out statistical testing for the functional central place paper. The point, though, is not necessarily to replicate Galison's study but to recognize how material embeddedness becomes part of the story, not incidental detail.

3 Networks and alliances

Finally, producing ideas involves forging and sustaining networks and alliances. In the rationalist model, once truth is found it shines by its own light. It is instantly recognized for what it is and accepted everywhere. In contrast, in the sociology of scientific knowledge literature, making and maintaining truth is a precarious achievement, involving an enormous amount of work of assembling and keeping on side a series of allies. As long as those alliances hold, truth is maintained. Once severed, things fall apart, the center does not hold.

This broader view of the importance of securing networks and alliances in producing ideas is best associated with the work of Michel Callon (1986) and Bruno Latour

(1987). Their argument is that knowledge is a relational effect, the result of persuading sometimes very different entities to work together to form a larger network of alliances. The task of persuasion is termed 'translation'. It is often a fraught process, with no guarantee of success. This is because, as Latour (1999: 88) writes, translation 'consists of combining two hitherto different interests . . . to form a single goal . . . . Even if the balance is equal, neither of the parties . . . will be able to arrive at exactly [their] original goal. There is a drift, a slippage, a displacement, which, depending on the case, may be tiny or infinitely large.' In Latour's diagram (Figure 2), the drift, slippage and displacements of translation are represented by the no-entry symbols; they are points where attempts to bring together different interests to form alliances come to a halt. Sometimes it is permanent. As Latour's diagram suggests, however, the obstruction might also only be temporary, with the impasse itself later retranslated and the process moving on again.

An example is in persuading the flagship journal of American geography, *Annals of the Association of American Geographers*, to publish writings of quantitative geographers. It was important because the journal was an 'obligatory passage point', to use another of Latour's (1987: 159) terms. Berry and Garrison's paper, however, was rejected from the *Annals* for being 'not geography' (Berry, 2000a: 82). From 1964, under the editorship of the UCLA cultural geographer Joe Spencer, the situation even deteriorated. Spencer was antagonistic to quantitative work from the beginning. It was he who derisively labeled the University of Washington quantifiers, including Berry and Garrison, 'space cadets'[4] at a 1956 Pacific Coast regional meeting. As the *Annals* editor, he hindered publication, suggesting that quantitative writings be confined at best to a 'Research Notes' section of the journal.[5] This was not a permanent blockage, however. Retranslation occurs. It began with the emergence of alternative publication outlets. There was a series of informal publications, beginning with the University of Washington's own Discussion Paper Series and continuing through the more widely circulated MICMOG (Michigan Inter-university Community Of Mathematical Geographers) publications that emerged from the University of Michigan in 1963,[6] which finally became the journal *Geographical Analysis* in 1969. The project was so successful that the discipline itself was retranslated so thoroughly that by 1976 John Hudson, a spatial science acolyte who once described his research speciality as 'dots and lines' (Hudson, 1997), became the

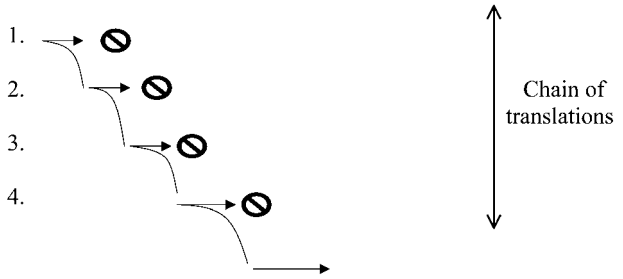

Figure 2 Chain of translations
Source: Redrawn from Latour (1979: 92)
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new editor of the *Annals*, at which point impediments were removed, and Joe Spencer turned over in his grave although he did not die until 1984.

Even from this one story, it is clear that establishing allies and networks through translation is a precarious accomplishment. It takes as much perspiration as inspiration, and continually has the potential to unravel.[7] Knowledge is not universal and fixed from on high, given by transcendental categories like rationality. But it is contingent upon the workings of particular networks of alliances, and the hesitant complex dance of translation.

III Placing ideas

This shift in how intellectual production is conceived within the sociology of science literature offers the potential for the recognition of place, of a view from somewhere. That said, typically only *social historical* context is emphasized (and not surprisingly given that most working in the sociology of scientific knowledge field are sociologists or historians of science or both). Some microgeographical sites, especially particular laboratories, have been examined, such as Stephen Shapin's (1994) investigation of Robert Boyle's in his house on Pall Mall in London, or Bruno Latour's (1988) account of Pasteur's on the Rue d'Ulm in Paris. Yet even these particular geographical sites are rarely cast in a wider geographical vocabulary, and especially that of place.

The vocabulary of place is difficult, however. To define place seems to require one to eff the ineffable. Traditional definitions appear narrow, tedious and pedantic, or hopelessly broad, wild and chaotic. Even Nigel Thrift (1999: 295) says: 'The more you think about place, the less it seems to offer.' But this cannot be true either, and certainly not true for Thrift who in the paper from which this quote is taken devotes himself precisely to thinking more about place. What is required, as Thrift recognizes, is to 'stop looking at things in the usual way' (Thrift, 1999: 296). What I would like to do in this next section of the paper is to examine attempts to conceive of place not in the usual way, including Thrift's own, relating them to the three components of intellectual production I identified earlier.[8]

1 'Monstrous' categories: intellectual rupture and heterotopias

The first derives from Foucault, and in particular his idea of heterotopia, a place defined as outside the norm. I will argue that precisely heterotopia's difference, its incommensurability with its surroundings, marks it as a place of potential intellectual innovation, for reordering the order of things, or, as I will argue later, for reordering the geography of geography.

Foucault first discusses heterotopia in *The order of things* (2002 [1966]). It follows his citation of Borges's well-known fictional entry from 'a certain Chinese encyclopaedia' in which it is written that (quoted in Foucault, 2002: xvi):

> Animals are divided into: (a) belonging to the Emperor. (b) embalmed. (c) tame. (d) sucking pigs. (e) sirens. (f) fabulous. (g) stray dogs. (h) included in the present classification. (i) frenzied. (j) innumerable. (k) drawn with a very fine camelhair brush. (l) *et cetera.* (m) having just broken the water pitcher. (n) from a long way off look like flies.

At the end of this Foucault writes, 'the thing we apprehend in one great leap, . . . is the limitation of our own, the stark impossibility of thinking that' (Foucault, 2002: xvi).

The theme of 'thinking that' is the wider one of Foucault's book, a historical inquiry into what can and cannot be thought that. How can Newton *think that* base metals will be transformed into gold, or *think that* numerological analysis of the Bible will reveal the date of the Second Coming (Gleick, 2003: Chapters 9 and 10).

In trying to understand the paradoxical – Foucault (2002: xviii) calls it 'monstrous' – list of Borges, Foucault says: 'What is impossible is not the propinquity of the things listed, but the very site on which their propinquity would be possible' (Foucault, 2002: xviii). Foucault is asking about the kind of place in which such a classification could hold. He briefly considers two: utopias, although he says 'the road to them is chimerical' (Foucault, 2002: xix), and heterotopias, although these are 'disturbing . . . because they make it impossible to name this *and* that, because they shatter or tangle common names, because they destroy "syntax" in advance, and not only syntax with which we construct sentences but also that less apparent syntax which causes words and things (next to and also opposite one another) to "hold together"' (Foucault, 2002: xix).

Exactly these disturbing qualities of heterotopias make them potent places of intellectual change. They are places where the old order of things is 'shattered', its 'syntax destroyed', where words and things no longer 'hold together', and which is consequently replaced by a new order. That is, heterotopias are places of 'paradigm' change, sites of new 'styles of scientific reasoning.' They are locations where, for example, it is no longer possible to see place as a Hartshornian 'element complex', but easy to see it as the point of intersection of vertices within a hexagonal geometry.

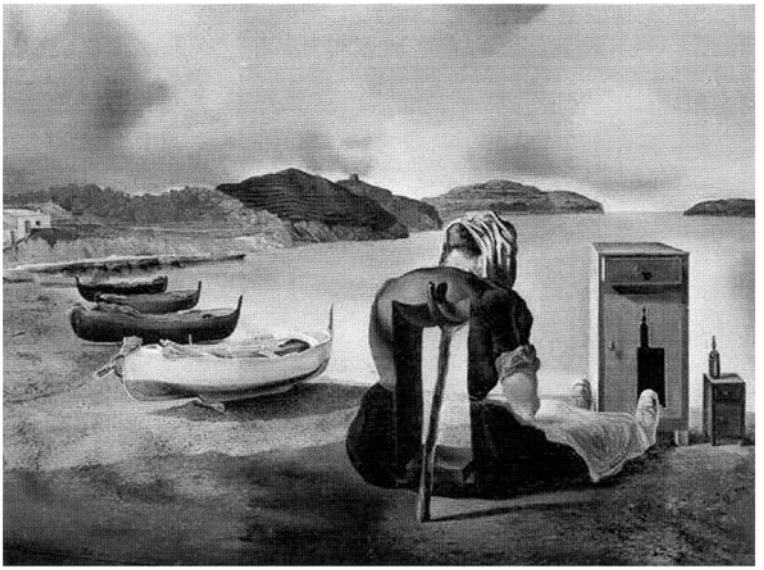
Support for my interpretation is found in Kevin Hetherington (1997a: viii), who defines heterotopias as 'a space of alternative ordering. Heterotopias organize a bit of the social world in a different way to that which surrounds them. That alternative ordering makes them out as Other and allows them to be seen as an example of an alternative way of doing things.' Several points follow.

The first is about the alternative ordering that occurs. It stems from juxtaposing material elements, practices, ideas and texts that have never been juxtaposed, creating the possibility of novelty, innovative leaps and creative frisson. It is like a Magritte or Dali painting in which the unexpected sit cheek by jowl: a man in a bowler hat next to a giant bird next to a standing fish, or fishing boats next to a backless woman next to a night table and a baby's bottle (Figure 3). The result is a space that 'claws and gnaws at us' as Foucault (1986: 23) puts it. As Hetherington (1997a: 42) explains:

> The term heterotopia originally comes from the study of anatomy. It is used to refer to parts of the body that are either out of place, missing, extra, or, like tumours, alien. For Foucault places of Otherness are spaces whose existence sets up unsettling juxtapositions of incommensurate 'objects' which challenge the way we think, especially the way our thinking is ordered. Heterotopias have a shock effect that derives from their different mode of ordering.

For this reason Foucault (1986: 24) calls heterotopias 'counter sites'. They 'suspect, neutralize, or invert the set of relations that they happen to designate, mirror or reflect' (Foucault, 1986: 24). From the Citadel, Snohomish County is no longer a region but a diagram of hexagons, computed values of functional centrality, a regression line.

The second is that heterotopias 'presuppose a system of opening and closing that both isolates them and makes them penetrable. In general, the heterotopic site is not freely accessible like a public space. Either the entry is compulsory . . . or else the



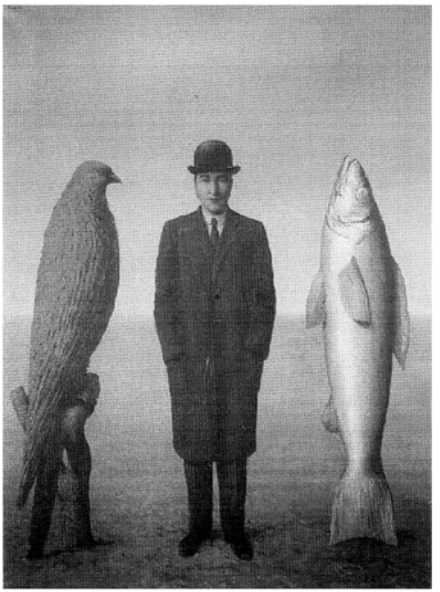


Figure 3 Alternative orderings. Above: Salvador Dali, 'The weaning of furniture-nutrition' (1934). Below: René Magritte, 'Presence of mind' (1960)
© Salvador Dali Foundation Gola-Salvador Dali/SODRAC (Montreal) 2004
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individual has to submit to rites and purification. To get in one must have a certain permission and make certain gestures. Moreover, there are even heterotopias that are entirely consecrated to these activities of purification ...' (Foucault, 1986: 26). Entry into the places of the quantitative revolution, as we will see, involved precisely such consecrated acts of purification, submission to the immaculate logic of mathematics.

Finally, heterotopias are always in the process of being made, ordering rather than order. Through their very dynamic, they create themselves as new kinds of places that may later become (at least for a period) obligatory passage points for other places. Isaac Newton's 'elaboratory' set between medieval magic and Enlightenment science eventually makes possible the modern scientific laboratory. In Hetherington's work, the Palais Royale in Paris opens up a heterotopic space against the Ancien Régime allowing it to 'become the focus for other interests and hopes for social change' in a revolutionary France (Hetherington, 1997a: 51). As I will argue later, the Citadel at Smith Hall, University of Washington, set between the old guard of regionalism and the new wave of space cadets, also became the focus for the interests and hopes for intellectual change in a revolutionary geography.

2 The stuff of life: materialization and 'truth spots'

If heterotopias are places that reorder in radically different ways pointing to new possibilities, what is reordered? It is the stuff of life: it is bodies, it is machines, it is buildings, it is ink marks on paper. It is materiality. Law and Hetherington (2000: 3) write: 'Material trappings are not just trappings. They are not idle. They are also performative. That is they act. And they form part of a materially heterogeneous network of bits and pieces of all kinds that participate in the generation of information, or power relations, of subjectivities and objectivities.'

The broader theoretical view underlying this claim is what Law and Hetherington (2000: 3) call 'material semiotics'. It is the idea that 'entities take their form and acquire their attributes as a result of their relations with other entities' (Law, 1999: 3). In semiotics, those entities are intangible signs, but in material semiotics they can be anything–IBM 650 computers, slide rules, the latest copy of *Economic Geography*, after-lunch soporific lectures in econometrics, or rooms in buildings like the Citadel in Smith Hall. There are no limits. The important point is only that the meaning of an entity is a relational effect of its association with other entities.

Within this conception, places themselves are a relational effect. This is a different conception of place than traditionally found in the geographical literature. There, as Hetherington (1997b: 183–84) writes, places are often 'defined by humanist discourses. It is assumed that place is about agency, and that agency is invariably defined as human agency, even when places are seen to be contested and open to multiple interpretations ....' Instead, as Hetherington (1997b: 184) writes, material semiotics 'aim[s] to bring materiality back in and to see places as generated by the placing, arranging and naming the spatial ordering of materials and the system of difference that they perform ....' Through that placing, naming and arranging it is possible to create what Thomas Gieryn (2002) calls 'a truth spot', that is, a place that through the representation of its very materiality offers ostensibly truthful accounts 'authentic all over' (Gieryn, 2002: 118). Clearly, this occurred with respect

to the places of the quantitative revolution. Small in number, they represented themselves as speaking for everywhere.

For Gieryn, as well as Hetherington and Law, the claim to a truth that is 'authentic all over' is an effect of the particular arrangements of the complex materialities of specific places. Consequently, Law (1994: 15) writes, one needs to 'burrow into them, taking them apart, seeing how [truth was] achieved, and exploring the hurts that were done along the way'. Law does this by 'tell[ing] stories about these materials' (Law, 1994: 23): about people, about things and about texts. It is not just stories that are important, though, but geographical stories – stories about how material bodies, machines and texts perform within particular places, and in doing so make those places at the same time.

3 Making and breaking connections: networks and 'an ecology of place'

The final literature on networks, alliances and translations even in its sociological and historical incarnation is implicitly geographical. Latour's last chapter in *Science in action* (Latour, 1987: Chapter 6), a key text within this literature, is titled 'Centres of calculation'. It is about the strategies and methods found at particular places that allow the gathering, accumulating, organizing and disseminating of information. Such centers for him form key nodes in a larger network of alliances that extend geographically, permitting action at a distance. In Figure 4 the process is portrayed as cumulative, with more and more information and things brought back to the center as the rest of the world is increasingly colonized by expansionary geographical crossings and recrossings (Latour, 1987: 220).

The problem with this conception, though, is its narrow imperialistic template. Indeed, the examples used in Latour's chapter are precisely those of European expeditions of empire: Portugal's explorations to the Indies financed by King John II, or Lapérouse's expeditions to the Pacific made on behalf of Louis XVI, or the various voyages of the British Royal Navy Captains Cook, Vancouver and Bligh that provided plant species for King George III's Kew Gardens. As a result, Latour's centers of calculation are imperial cities like Lisbon, Paris or London, sinks for knowledge

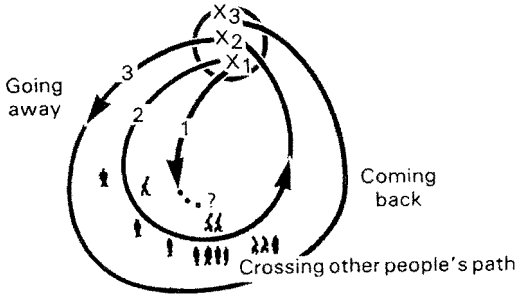

Figure 4 Centers of calculation
Source: Latour (1987: 220)
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and materials drained from the rest of the world. Of course, imperialism was and is a ferocious centralizing and colonizing force, but it is only one model of the place of networks.

It is here that recent writings on place especially by Nigel Thrift are useful. While accepting the importance of networks of the kind recognized by Latour, he offers a more expansive conception that he calls 'an ecology of place' (Thrift, 1999). Whereas for Latour those alliances and networks lead to a militaristic spatial logic, centers of calculation, Thrift argues for an open-ended, dynamic notion. He says he wants 'to forge a dynamic sense of place, a sense of place in which "nothing signifies" and is able to shove off to whatever's next' (Thrift, 1999: 296).

Places are incomplete and therefore dynamic precisely because of their associational nature. As alliances change, and new networks are forged, novel translations are completed, and places and their relation to other places alter. The Geography Department at Bristol hired David Harvey in 1961, Barry Garner in 1963 and Michael Chisholm in 1964, and in 1965 a new chair was established for Peter Haggett who moved down from Cambridge where he was a lecturer, and in the same year he published the critically important *Locational analysis in human geography* (Haggett, 1965). Add Bristol to the Quantgeog Airline flight plan. In 1976 Brian Berry left the University of Chicago for Harvard, in 1985 Gordon Clark who came to Chicago in 1983 from Harvard was denied tenure, and in 1987 Edward Laumann, previously Dean at the University of Michigan, who oversaw the closure of its Department of Geography, disbanded Chicago's own Department. As Laumann said, 'we eliminated it [in Michigan], and we'll do it here. That's my decision' (quoted by Harris, 1997). Take Chicago off the Quantgeog Airline flight plan.

The point is, as Thrift suggests, places are porous and incomplete, shifting in relation to the various changing associations out of which they are constituted. 'Like societies, places can be made durable but they cannot last' (Thrift, 1999: 317). Nor are they necessarily always colonizing, expanding their geographical influence. Imperial cities decline and fall, and so do places forming the itinerary of Quantgeog Airline.

IV Placing geography's quantitative revolution

In a wonderful geographical turn of phrase, Althusser (1972: 85) says, 'Thales discovered the continent of mathematics'. The same process happened from the mid-1950s in human geography with the quantitative revolution. Paradigms and styles of scientific reasoning open up territory not seen before, producing possibilities not yet imagined. The quantitative revolution revealed a new continent to geographers, a world not delineated to them before; a world marked by nested hexagons, functional centrality, bid-rent curves, isodapanes, trend surface coefficients and computers larger than living rooms. Before its 'discovery', this world was unknown. Yet, afterwards, it was the basis of fresh promise, and for some becoming the only world worth exploring. My argument is that the promise and exploration of this new landmass of ideas was revealed not by a universal viewpoint, and not even by a continental one, but by very particular places.

1 'This kind of thing has got to stop': heterotopias and the quantitative revolution

Those first places in North America are perhaps not ones that most immediately jump to mind as arch revolutionary centers: the University of Iowa, Iowa City, and the University of Washington, Seattle, with Northwestern University, Evanston, in the background as a kind of *eminence grise*.[9] The first two, though, fell back within a decade of their rise, and were joined and surpassed by other places. By Northwestern University, which quickly dropped its ghostly role, and became full-bloodedly involved. By the University of Chicago, which rapidly gained prominence through the work of Brian Berry and his students, often at the Center of Urban Studies. By Ohio State University (OSU), which remade itself following the 1963 appointment of Ned Taaffe as chair. By Michigan State, East Lansing, and perhaps more importantly by the University of Michigan, Ann Arbor, where John Nystuen and Waldo Tobler, two of the original Washington space cadets, were hired, and later joined in 1966 by Gunnar Olsson.

Marking these places is their heterotopic quality. They juxtaposed things never juxtaposed before, engaged in purified acts of consecration, and became obligatory passage points for change. In particular, the previous orthodoxy was regional geography involving applying sometimes leaden classification schemes to geographical facts. Michael Woldenberg (2000) remembers Glen Trewartha's graduate Asian course at the University of Wisconsin in spring 1960:

> [Trewartha] developed an outline of how to analyse the region – categories, you know. He had this huge outline, and then he would take China, go through the whole outline – population, resources, etc. – but with a lot of subdivisions. . . . Then he would do northern China, same outline, southern China, same outline, eastern, western. . . . Compare this area with that area, same outline. . . . At the time, I knew it cold. But I've never used it since.

It was against this view that the quantifiers did something completely different. Rather than describing the region in terms of the categories of 'population, resources, etc.', it was described as regression equations, indices of centrality and hexagonal diagrams. For regionalists this was undoubtedly as shocking as dividing animals into 'having just broken the water pitcher', and 'from a long way off look like flies'. How could you *think that* a region is ordered in such terms? This seemed to be Joe Spencer's view at least, and indicative of a series of local cleavages between the old and the new guard. At the University of Washington, the former chair, Howard Martin, and his wife, Francis Earle ('the dinosaurs'; Pitts, 2002), represented old-style regionalists pitted against the new juxtapositions of Garrison's 'space cadets' ('the modernists'; Pitts, 2002). Roger Downs (1998), at the time a graduate student at Bristol University in the mid-1960s, remembers that, following a talk by David Harvey at Exeter University, the Head wrote a letter 'that was put up on the [Departmental notice] board saying essentially we're never going to do that sort of geography here'. Alfred Steers, Head of the Geography Department at Cambridge University, upbraided Peter Haggett in 1964 for showing a multiple regression equation at an RGS conference, saying to him, 'This kind of thing has got to stop' (quoted by Thrift, 1995: 381–82; for the general context see Chorley, 1995; Stoddart, 1997).

Even for those who carried out these new juxtapositions they took time to learn. Arthur Getis (1998), one of the students at the University of Washington in the late 1950s, remembers that:

> When Berry [first] met me [when I arrived at the University of Washington] he said, 'We do some theoretical work here but one of our main interests is to empirically test hypotheses that we propose.' Believe it or not, words like empiricism, theory, hypothesis testing, ... [although] they were not new to me, hit me like a ton of bricks. 'Have you read Christaller?' 'Who?' 'The Löschian approach is very interesting isn't it?' 'Is it?' ... I said I know all the capitals of all the countries in the world, and all the states, and I know all about the mountains and the streams. Ask me.

Dick Peet (2002), who in 1961 went from an undergraduate at LSE to undertaking a Masters degree at UBC, similarly recalls struggling over new vocabulary and practices:

> I remember Walter Hardwick [at UBC] had just come back from Minnesota and he was the apostle of the quantitative revolution, and fairly effective. I remember he started talking about models. While I was at LSE, we used to do this thing with map reading. We had these maps that had been made into three-dimensional models, like mountains and shapes like that, and there was a guy there named Harrison Church, and he used to say to us, 'Don't lean on the models boys! Don't put new valleys where God had not created them!' And I thought, when Walter was talking about models, he was talking about these maps. And then bit-by-bit, I gradually got to know about what was happening, especially in central place theory, which was Walter's big thing.

Once the juxtaposition was made, though, the ordering reordered, the transformation could be epiphanal. Allen Scott (1998), who as an undergraduate studied under the intellectual shadow of A.J. Herbertson's scheme of natural regions at Oxford, recollects:

> I arrived in Northwestern University [for a PhD] in September 1961. I had a three-piece suit, a mac, a scarf, and the temperature was about 90 degrees. I arrived in Ed Espenshade's office [the Chair of the Geography Department]. So, he said, 'Good to see you. Let's talk about your programme.' And he pulled out a calendar and said, 'You will be taking statistics, introductory mathematics, and operational research.' And I looked him and said, 'I came here to do geography.'

Initially 'bitching and moaning about the corruption of geography by these unthinking, unfeeling and incomprehensible statistical procedures', Scott (1998), following a discussion with Ned Taaffe, had his Pauline experience:

> ... it was like the veils falling from eyes. People talk about these moments of illumination; this was my moment of illumination. ... It is like looking at the hexagons of Christaller. From an Oxonian point of view, the hexagons are just in somebody's imagination, they have no relationship to the reality. From a Northwestern point of view, they are a deep structure lying behind the phenomenal appearance of things. And you need to put on another set of spectacles to see that, and to see how that can have explanatory and theoretical consequences.

And so a new continent was discovered. That continent was discovered through numbers and a particular conception of theory. As Scott (1998) continues:

> I remember being in a frame of mind where I thought that anything and everything useful to be said in academic, scientific terms was going to be said mathematically. That there was the whole other world that always interested me of humanistic values, of art, music, and literature. But that was ... another world; that was not the world of scholarship as I saw it. That was the world of one's personal cultivation and enjoyment. But the scientific world was the work of eventually mathematizing every statement we could make about the earthly condition.

To do so, though, to get 'permission' to enter the heterotopia where such acts of mathematizing occur, requires as Foucault suggests undertaking particular 'rites' and forms of 'purification', to 'make certain gestures'. Those begin, as they did for Allen Scott, with courses in statistics and mathematics. At the University of Washington, Garrison's new course, Geog 426, Quantitative Methods in Geography,

was the first rite of purification; indeed, it was the original rite of purification for the entire discipline given that it was first offered in Fall term, 1955. 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.' As with all rites of purification, however, not everyone is cleansed. Heterotopic spaces remain closed to some people. For also in that inaugural class of Geography 426 was Richard Preston who had come to the University of Washington from a very traditional programme in geography at the Central Washington College of Education. He felt 'completely unprepared', and dropped Garrison's course after only two weeks (Preston, 2000). Garrison was not sympathetic. 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). Preston remained in the programme, however, but outside the inner circle, excluded from other rites and gestures associated with the space cadets such as late-night discussions at the Red Robin or attendance at Garrison's lunchtime 'Mass communication séance' (Getis, 1998; Morrill, 1998).

A version of this same kind of disdain for those not sufficiently purified is found later at the University of Iowa. A new faculty member was brought into the Department in 1962 to provide mathematical muscle following the departure of Ed Thomas. As Gerry Rushton (1997) remembers, this faculty member was 'trumped up to us as a . . . hotshot quantitative person. But poor fellow, it was like he walked into a minefield'. The graduate students quickly discovered they knew more math than him, and played a cruel trick. They began talking in earshot of Grammian matrices and their incomparable virtues. But no such mathematical entities existed. As Rushton (1997) continues:

> It became a big joke. Then someone brought in a big pack of computer programming cards which said Grammian matrix programme on the top. We all knew that if we left it around he would want to copy it. So, one day it was left lying around, and sure enough, he copied it. And then when he ran it through it told him what a shit he was. Isn't that awful? . . . But it does say something about what we were like as students.

What I would also like to suggest is that says something about heterotopias and the rites of purification associated with them, and the consequences of transgression, and defilement. Such rites, though, began changing Anglo-American geography.

Often this occured piecemeal as Departments hired quantifiers, who then began to make their new Department in their image. The Washington graduate diaspora did this successfully in places like Northwestern (Garrison, Morrill, Marble, Dacey), Chicago (Berry), Michigan (Tobler and Nystuen) and Michigan State (Getis). In other places, there was resistance. Berkeley's Geography Department was a holdout, but as Allan Pred (1998) recalls 'they had been reviewed and they had been told that they had to hire someone who practiced the new economic geography, and do all the things that the department wasn't doing'. Philip Wagner, a former student of Carl Sauer, and a cultural geographer at Chicago in the early 1960s, told James Parsons, then chair at Berkeley, 'You have got to swallow the pill' (quoted by Pred, 1998). So, reluctantly, Parsons looked for a quantifier. Wagner recommended Allan Pred who was finishing his PhD at Chicago with Brian Berry. Wagner did not tell Pred about that conversation, though, nor did Pred apply for a job at Berkeley; in fact,

no job was advertised. But, as Pred recounts, when he and Parsons ran into one another at the AAG meetings at Miami in April 1962, Parsons said, '"I want you to have dinner with me tomorrow evening." The next day the entire faculty is at the dinner. . . . And after the dinner, I'm told that they are going to put my name forward for a job. It is so otherworldly' (Pred, 1998). So, second-generation Washington entered Berkeley.

Rather than the forced tokenism at Berkeley, the reassembling could also be enthusiastically wholesale in which an entire department was given an extreme makeover. The best example is at Ohio State. In its earlier incarnation, it was primarily a teaching department, steeped in regional geography. But in 1963 OSU hired Ned Taaffe from Northwestern University as the new chair, whom the Dean, John McCoy, charged to reinvigorate it (Taaffe, 1997). Taaffe was given five appointments which because of departures became 10. His appointments were exclusively in spatial science, and included Larry Brown, Emilio Casetti, Kevin Cox, George Demko, Howard Gauthier, Reginald Golledge, Leslie King and John Rayner. Kevin Cox (1997) remembers that: 'There was quite a struggle in the department with the old guard. The way [Taaffe] did it was not to take appointments to the full faculty. He had a small personnel committee who made recommendations.' Perhaps more than at any other place, OSU self-consciously remade itself into a site of quantitative geography. To solidify farther that end, the Department established the journal *Geographical Analysis*, partly funded by the University and published by OSU Press. Kevin Cox (1997) recalls: 'There had been talk about the establishment of *Geographical Analysis*. And there was a faculty meeting called to discuss it. . . . There is no doubt that the journal was seen as something which would be associated with this department, and that would link fortunes of the department with the fortunes of the quantitative revolution.'

2 'We're objective': truth spot material

As a material artifact, the journal *Geographical Analysis* was one of the physical elements that helped constitute the Geography Department at OSU as a 'truth spot'. Certainly, most of its faculty members thought that the statements they made were 'authentic all over', the view from nowhere. Larry Brown (1997) says: 'I remember Kevin [Cox] saying back in the late 1970s that everyone has an attitude and which colors how you see things. But people said, "Oh no we don't, we're objective."' Following Law (1994), I want to understand how that objectivity was manufactured by telling stories about the materials that made up the 'truth spots' of geography's quantitative revolution, certainly by texts like *Geographical Analysis*, but also by things, and bodies.

Let me begin with bodies. Perhaps their most overwhelming characteristic is that they are the bodies of young, white men (Figure 5). The literature in feminist and postcolonial science studies would link in a general way the European-origin male bodies that congregated in Departments of Geography in places like Seattle, Iowa City, Evanston and Columbus during the 1950s and 1960s with the type of 'God trick', 'vision . . . from nowhere' knowledge associated with 'truth spots' (Haraway, 1991: 191; see also Berg, 1994, and the response by Berry, 1994). While this is an important general linkage, I am also interested in specific, concrete ones.

Figure 5 IGU conference, Lund, Sweden, August 1960 (from left to right, and top to bottom: Brian Berry, Hans Carol, Leslie Curry, Michael Dacey, Robert Dickinson, William Garrison, Chauncy Harris, Richard Hartshorne, Walter Isard, Duane Marble, Harold Mayer, Robert Mayfield, Richard Morrill, Raymond Murphy, Edwin Thomas, Edward Ullman, Jay Vance and Torsten Hägerstrand)
Source: *Annals of the Association of American Geographers* 69(1), 122 (March 1979)
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Bodies arrived at the different places of the quantitative revolution for quite different reasons. Brian Berry arrived in Seattle in 1955 because Donald Hudson, the recently appointed chair, jumped the interuniversity agreed deadline for graduate acceptance, and offered him an early Teaching Assistantship (Berry, 2002: 26). Allen Scott (1998) arrived at Northwestern University in 1961 because it required neither an application fee nor an essay, unlike the other US graduate schools he considered. Michael Dear (2000) arrived in Philadelphia in 1971 to attend the Regional Science programme at Penn because he heard an earlier 'mesmerizing' talk by Walter Isard at the British Section of the Regional Science Association at UCL where Dear was undertaking a Master's degree in planning while working at the Greater London Council. Likewise, intellectually motivating people to get on side with the quantitative revolution were very different impulses. For Berry (2000) it was because it was 'new', 'interesting' and 'different'. As he put it, 'You had a bunch of students not a bunch of grand philosophers' (Berry, 2000). Yet for Allen Scott (1998) what appealed was precisely the philosophical underpinning: 'I

knew . . . that positivism would be the light that would guide us ever onwards.' For Michael Dear (2000), it was his left-wing political sympathies. Knowing how to carry out, say, large-scale urban modelling was important because it could help to improve the everyday lot of ordinary people. In fact, even before entering Penn, Dear (2000) taught himself 'linear programming in the off hours at work . . . because it was self evident to me that there were logical statements that you could make, say, about transportation and land use systems. It made a hell of a difference to be able to make measured statements about the ways those systems worked'.

In claiming this variety, I am not denying that there were commonalities among the bodies. Certainly, masculinism was a strong element of the culture. The practical jokes, competitiveness and ambition all speak to it. Following Law, it is clear that it produced various 'hurts . . . along the way'. Certainly to people like Richard Preston, and the new faculty member at Iowa, but also to women both as graduate students and as faculty who were excluded because of their gender. Susan Hanson (2002) entered Northwestern University in 1967 and remembers Ed Espenshade's utter bafflements as to 'why you would be in graduate school if you were female and already had a child. He just couldn't understand it. . . . When he spoke, he only asked how the family were, but never asked about scholarly work. Realistically as a woman in grad school at that time, one did not expect anything different! We knew very well that we were entering male turf'. Just over a decade later not much seemed to have changed when Pat Burnett became a faculty member at Northwestern in 1978. She recalls, 'I had a very difficult time personally at Northwestern with the guys,' and within three years she had 'sued [Northwestern for] breach of contract, with climate of sex discrimination as cause' (although she later dropped her suit) (Burnett, 2002).

Of the things that helped put truth into geography's 'truth spots', perhaps the most important were calculating machines. They were the physical embodiment of the quantitative revolution, numbers 'thingified'. Initially, much of the numerical work was carried out either on mechanical calculators like the Monroe or electric calculators like the Frieden (Figure 6). Brian Berry (2000) remembers 'in his first semester [Fall, 1955] learning statistics on these great big desk calculators that groaned'. Even in the early 1960s when Bill Clark (2000) was a graduate student at the University of Illinois, desktop calculating machines were still the norm. He recalls: 'It was only when I started to write the dissertation that I wanted to run correlation and regression. You ran by hand, . . . you did them with calculators – huge desktop machines. We used those in one or two courses. In the statistics class, we had lots of exercises where you had 30–50 cases. You did them all by hand. Plugged all the numbers in and solved the equation.'

This began to change with the advent of the computer. The first commercially sold computer in North America was the IBM 650 launched in 1954, and bought by Columbia University (Figure 7). Other universities quickly followed, including the University of Washington. Donald Hudson (1955) boasted in a 1955 advertisement for the Department of the presence of a computer, and 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) calls it. Nor initially were there any formal programming languages. Michael Dacey (1997) remembers you had to 'programme with patch panels, actually plugging in wires'. Waldo Tobler (1998) who used the same IBM 650 machine at the University of Washington remembers:

Figure 6 Monroe calculating machine, *c*. mid-1950s
Source: Vintage calculators www.vintagecalculators.com
Reprinted by kind permission of Nigel Trout

We had to go up to the attic of the Chemistry building at 2 am so we could run the computer by ourselves. They didn't have any computer operators in those days, and that was before computer languages like FORTRAN. . . . To cover programming on the 650 you had to pick up two bytes of information on one rotation of the drum. It had a 2 K memory which rotated real fast. And if you were clever, you could pick up two pieces of information in one rotation.

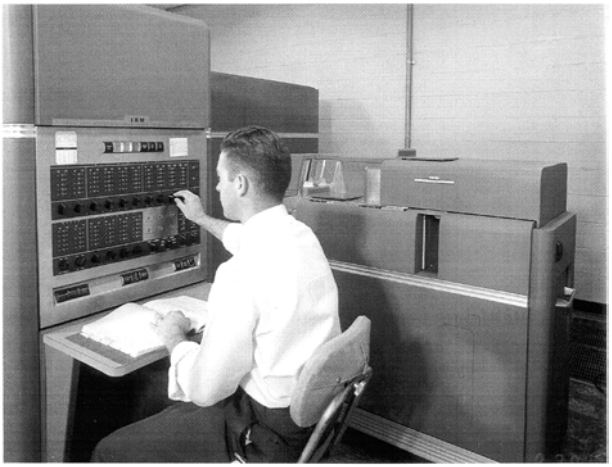


Figure 7 IBM 650 console, Columbia University, *c*. mid-1950s
Source: www.computerhistory.info
Reprinted by kind permission of the Lawrence Livermore National Laboratory

Around the same time, Les King (2000) also remembers doing battle with the IBM 650 at Iowa:

> We spent a lot of time on that damn machine. I was doing multivariate analysis, including multiple covariance analysis for which there was no canned program available then. I recall writing to Donald Bogue, the demographer who had used this analysis, asking how he had computed some of the terms. He wrote back a delightful letter that I wish I'd kept, and he said essentially, 'Well, I just played around with the terms until they looked right.'

While the computer was the machine par excellence for generating truth statements, other machines were also important: slide rules (Pitts, 2002), card sorters (Clark, 2000) and later line printers (Woldenberg, 2000), and early Xerox machines (Downs, 1998). At the University of Washington critical was the duplicating machine. It allowed the Citadel's students to engage in informal discussions that later proved so important. John Nystuen remembers:

> The way we argued was to write about it, and make mimeographs. . . . That was one of the things Donald Hudson contributed to. . . . [H]e made available the mimeograph machine for the graduate students. And he spent a lot of money on paper to allow the graduate students to supply that material. You would type the paper, then make purple mimeographs, pass it on, and argue about it. These were not formal presentations. We would get together in . . . the Citadel up on the 4th floor of Smith Hall.

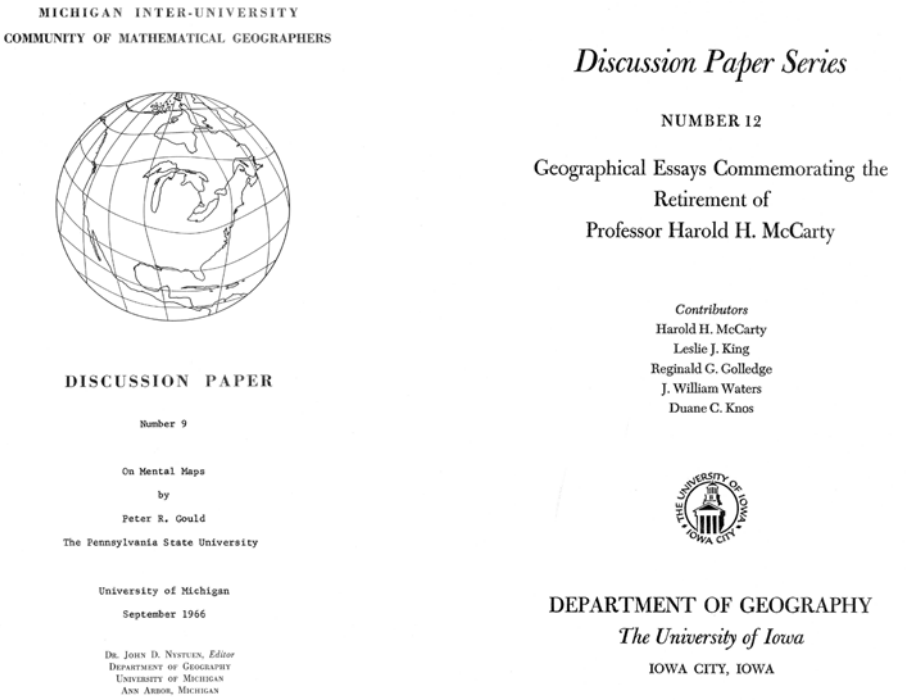
From March 1958, that duplicating machine was used for cranking out a more formal set of papers, the University of Washington Discussion Paper Series, and sent across the world (King, 1979: 125). This leads to the third important materiality, texts, allowing one place to connect with another. Brian Berry (2000) explains:

> There was a sense of the importance of getting things down on paper and dissemination and getting feedback. . . . But it was a time when travel was expensive and difficult, communications were difficult, and so on. Things were not coming out in the main journals. So, [the discussion papers were] a way of providing communication, getting the word out, creating and interacting within an emerging community.

The University of Washington texts were crucial. Peter Haggett (1965: preface) writes about their importance for him in launching a similar project in Britain, and Leslie King (1979: 125) writes about the eager anticipation around their arrival in Iowa. More generally, the discussion papers, as well as others that began to be produced in different places, became the central outlets of research and forums of debate (Figure 8). Arthur Getis (1998) recalls:

> Each center was eager to get all the discussion papers from the other centers. During the early 1960s, most chroniclers of the discipline completely misunderstand what was going on. . . . All of the quantitative-theoretical work that was going on could not get in the journals. What was going on at that stage was a massive trade in departmental discussion papers – the MICMOG papers in Michigan, the Northwestern papers, the Iowa papers, eventually the Ohio State papers, the Washington papers. These were the most valuable commodities that you could get. You didn't care what the hell was being written or produced anywhere else in the discipline. These were the things.

Furthermore, the 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: '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.' There was also a hierar-


MICHIGAN INTER-UNIVERSITY
COMMUNITY OF MATHEMATICAL GEOGRAPHERS

DISCUSSION PAPER

Number 9

On Mental Maps
by
Peter R. Gould
The Pennsylvania State University

University of Michigan
September 1966

Dr. John D. Nystuen, *Editor*
Department of Geography
University of Michigan
Ann Arbor, Michigan

Discussion Paper Series

NUMBER 12

Geographical Essays Commemorating the
Retirement of
Professor Harold H. McCarty

Contributors
Harold H. McCarty
Leslie J. King
Reginald G. Golledge
J. William Waters
Duane C. Knos

DEPARTMENT OF GEOGRAPHY
The University of Iowa
IOWA CITY, IOWA

Figure 8 Discussion Paper Series: MICMOG (left), University of Iowa (right)

chy of receipt. Roger Downs (1998), David Harvey's graduate student at Bristol, remembers: 'You wanted to get hold of those things [the discussion papers] very quickly in the British system. . . . As soon as one arrived it was to be passed around and read and talked about. . . . [T]hey came to faculty and then as graduate students we got them as hand-me-downs.'

Once, though, more formal publication outlets like the published journals *Geographical Analysis* and *Environment and Planning* in the UK (initiated in 1968; see *Environment and Planning A*, Anniversary Issue, 1993) emerged, the need for the Discussion Paper Series was mitigated, producing their demise.[10] Early on, however, they were key materialities for upholding truth claims.

3 'It's more complicated than simple patterns of association': rise and fall

Also key for upholding truth claims were the formation of networks and alliances, and persuading others to enrol in the project. Because of blockages, and impasses, that process is often complex and uneven. Certain sites resisted, such as Berkeley. Others that began strongly, such as Iowa and Washington, later fell back. Other places, such as Harvard, where first William Warntz joined in 1966, and was replaced by Brian Berry in 1973, suggested the promise of enrolment, but in the end never did (Woldenberg, 2000). Yet again, other places, such as OSU, seemed to emerge Zeus-like, fully formed from nowhere full of titanic vim and vigour. As Bill Clark (2000)

said in reflecting on the changing geography of the quantitative revolution: 'I suspect that it's like everything in life – it's more complicated than simple patterns of association.'

While the patterns of association may not be simple, for the rise of such places there needs to be at least some associations (alliances). While, first, they are highly contingent and serendipitous, later they are more intentional and directed. Important, for example, were the National Science Foundation (NSF) funded summer schools in quantitative methods. As Ned Taaffe (1997) recalls: 'The idea came up in John Borchert's house [in Minnesota] when I was at a West Lakes meeting [in 1960]. Espenshade and I and Dick Morrill had driven up there. It came out in a conversation. Why not have an institute for practicing geographers who haven't had formal training. Give them a period of six weeks to retool. It was primarily faculty, but there would be a few graduate students as well.' The first summer institute was held at Northwestern University in the summer of 1961, organized by Taaffe and Berry, and funded by NSF. There were 30 participants, taught by a who's who of quantitative geographers including Berry, Garrison, Marble, Nystuen and Thomas. Serving as a boot camp for nonquantitative inductees – Taaffe (1997) recalls 'many worried looks that first week' – it also functioned as a place for making new converts, creating alliances and extending the network geographically. For example, Henry Hunker, a faculty member at OSU, who attended the first institute, told his Dean, John McCoy, about the benefits of the experience, which, in turn, led to McCoy contacting Taaffe and eventually charging him to refresh the OSU Geography Department (Taaffe, 1997; King, 2000).

Another example was the alliance with regional science. The brainchild of the Harvard-trained economist Walter Isard, regional science enjoyed phenomenal growth through the second half of the 1950s and throughout the 1960s (Barnes, 2004). Formally recognized as a Department at Penn in 1958, and headed by Isard, regional science maintained close links to quantitative geography. Indeed, attending the very first meeting of Regional Science held at the Hotel Tuller, Detroit, in December 1954, were Bill Garrison and Ed Ullman. The links were solidified further as two of the original space cadets were appointed faculty members at Regional Science, Duane Marble and Michael Dacey, and later joined by Allen Scott. In fact, even in the early 1970s, regional science was still attracting graduate students from geography such as Michael Dear, who flew out to Philadelphia in August 1971 on the same plane as Doreen Massey, also to begin her graduate studies in regional science at Penn. Dear was on that plane because of that 'mesmerizing' lecture given by Isard at UCL. In turn, Isard spoke there because he had earlier sent Allen Scott to London in January 1967 as regional science's 'ambassador' to Europe (Barnes, 2004). More generally, through these associational relations the network of quantitative geography was extended and made durable.

But that geography lasted only as long as the alliances associated with it, and as they faltered so did the concomitant geography. From 1980, a lot of faltering went on. The geography departments at the Universities of Chicago and Michigan, as well as Northwestern University, all closed during the 1980s, and the Department of Regional Science was shut down in 1993. Such changes speak precisely to Thrift's dynamism of place, its open character.

That places of intellectual production change quickly is seen early on. Geoff Hewings (1997) who graduated from Birmingham University (UK) went to the

University of Washington in Fall 1965. But once he arrived, as he puts it, 'the polite way to say it was that the bloom was off the rose. . . . The real energy level had probably moved by then to Northwestern and to Michigan, and perhaps a lesser extent to Ohio State. There was still a commitment to that area. . . . But when I hear Getis or Morrill talk about the experience at graduate school, mine was a notch below.' It was similar at Iowa. Because of Harold McCarty becoming less active, and a series of appointees who never stayed long, including Ed Thomas, Ron Boyce, Bill Bunge and Larry Brown, the department lost momentum. Larry Brown (1997) who taught there between 1965 and 1968 says: 'Iowa was interesting because it consisted mainly of old men who weren't doing much research. It was a department that was depleted. . . . It was prominent more for its students like Reg Golledge, and Les King.'

Later, of course, this decline became more general, as former associations became frayed and broken, and by those who previously were at the heart of the project. Indeed, those who were previously at the heart of the project were the most effective at fraying and breaking those associations, creating the greatest disturbances. How they occured, their particular timing and geographical effects vary, though.

For Allen Scott (1998) it was a second Road to Damascus experience but in reverse.

> I started in England a book called *Combinatorial programming: spatial analysis and planning* . . . a winning book title if there ever was one. . . . I finished it in Toronto, and it came out in 1971. Then the sky fell in. Quantification just suddenly imploded for me. It was incredible. Whereas I had illumination and uplift, I now had a sense of the lights going out and depression. God I was depressed. Lost. That faith that mathematics is going to transform the social sciences you discover is mistaken.

As a result, the quantitative revolution lost an ally as Scott moved away intellectually from regional science, and then physically moved from Toronto, where he had gone after London, and one of the second wave centers of quantitative geography in North America.

Likewise, Gunnar Olsson remembers a similar reverse epiphanal experience. At a gathering one evening at his home in Ann Arbor in 1968 that included Reg Golledge, David Harvey, John Hudson and Les King – Hudson (1997) says Olsson's house during that period was 'a salon for theoretical geography' – there was a realization of what he calls the 'inference problem'. That is, the recognition that spatial patterns are overdetermined, 'which means that for a given [spatial] form, you cannot say anything specific about how it was created. And that of course pulls the rug away from convention' (Olsson, 2000). As Olsson continues, 'I think it was that problem that blew the discipline apart. I usually describe it as a torpedo that hit the disciplinary ship below the water. . . . And I think it was the inference problem that had a great impact on David becoming a Marxist, John Hudson becoming a historical geographer, King becoming an administrator, and Reg a behavioural geographer' (Olsson, 2000). If Olsson is right, a whole series of important alliances were broken that evening, with concomitant implications for the geography of geography's intellectual production that eventually came home to roost when Geography at Michigan itself was closed down in 1982, four years after Olsson returned home to Sweden.

Perhaps the most pivotal figure, though, also in Olsson's living room that evening, was David Harvey. Initially, Harvey was the consummate insider within quantitative geography. As a graduate student at Cambridge, he was a Demonstrator for Chorley and Haggett's first-year undergraduate course begun in 1958 that featured, for the first time in the history of the Department's offerings, 'statistical methods, matrices,

set theory, trend surface analysis, and network analysis' (Chorley, 1995: 361). He participated in the Maddingly Hall lectures in 1965, and wrote an essay for *Models in geography* (Harvey, 1967). In 1969, he published *Explanation in geography* (Harvey, 1969), the most complete and philosophically rigorous justification of positivism ever written within the discipline.

Only two years later, though, at the 1971 Boston AAG meeting in a session on poverty organized by Dick Peet, Harvey (1972: 6) announced that:

> [Geography's] quantitative revolution has run its course and diminishing marginal returns are apparently setting in as ... [it] serve[s] to tell us less and less about anything of great relevance. ... There is a clear disparity between the sophisticated theoretical and methodological framework which we are using and our ability to say anything really meaningful about events as they unfold around us. ... In short, our paradigm is not coping well.

Peet (2002) vividly recalls the session:

> ... the room was full, like hundreds of people. ... And it was clear then what was happening. I remember David said he had ten copies of his paper. It was the first mimeographed version of [what would become] *Social justice*. He said he had ten copies, and it was like a dogfight to get them. Maybe 70 people rushed the stage at the end and grabbed these things. I thought, 'God, we've arrived.'

What had arrived was radical geography charting the contours of quite a new continent, geographical Marxism, and the anti-world to quantitative geography.

In doing so, Harvey broke and remade alliances, changing the geography of intellectual production. It starts with his own changing geography, moving from an iconic center of quantification, Bristol, to Johns Hopkins in Baltimore in 1969.[11] Once there he began to interact with some faculty and graduate students at Clark University, Worcester, who in that same year, 1969, collectively began publishing the journal *Antipode*. Emerging, then, was the embryonic beginning of an alternative geography of an alternative intellectual production. But the kinds of general processes producing and sustaining the places of radical geography were the same ones that produced and sustained the places of quantitative geography: the destruction of an old syntax and the establishment of a new one based on novel juxtapositions; the development of new truth spots materially produced, and the creation of new alliances.

V Conclusion

It was here that I entered the story. After my railway line to St Austell experience, I studied geography and economics at UCL. Although still secretly pursuing Sraffa, I became interested in the discussions around radical geography. They seemed so much more compelling than anything we did in economics. Milton Friedman on the quantity theory of money in the *Lloyd's Bank Review* versus David Harvey on theories of capitalist accumulation in *Antipode* was no contest. The fact that *Antipode* was kept behind the library counter under lock and key only added to its allure. For graduate school, therefore, I applied to the places of radical geography. I wanted to be a mobile body that helped to produce new 'truth spots'. But Clark rejected me outright, and Johns Hopkins offered me $3,000 a year, a sum even then that 'Reds' Wolman, the Chair, told me would be 'barely, and I mean barely' enough to live on.

Fortunately, the University of Minnesota was more generous. It explains why I was in Fred Lukermann's seminar, and trudging across the Washington Avenue Bridge in

the winter of 1979. Lukermann often talked in that seminar about *genius loci*, the spirit of place. A miserably failed O-level Latin student, I didn't know what the term meant. I translated it in my head as the location of geniuses. After all, we were always talking in that class about geniuses and their places – Plato and Aristotle in Athens, Kant in Könnesberg, Wittgenstein in Vienna. For the purposes of this paper, my mistranslation was only half-wrong. I'm claiming that even seemingly abstract, rarefied and disembedded intellectual projects, the projects of geniuses, even projects of the genius's genius, Newton, are made in place. The same applies to geography's quantitative revolution. It was produced, set, shaped, grounded in places that made a difference.

Lying behind my argument is the anti-rationalist, science studies literature that has become such a potent force over the last 40 years. Missing from such work is often a geographical sensibility, which I tried to rectify by drawing upon recent theoretical conceptions of place, albeit propounded frequently by nongeographers. Those conceptions, while different, have important commonalities. They reject a humanist view, emphasizing institutional and material disjunction, material flows and practices, and change and porousness (Massey and Thrift, 2003). Places are not abstract, static, self-contained sites, sealed by rigid boundaries, but material, dynamic, open, and defined by their interrelationships with other places. If they were not, they could not function as sites of intellectual production.

Intellectual production is the name of our particular game, and necessarily geographical. Certainly, my own intellectual productions are a result of roads taken, and not taken. For example, it is unlikely that I would have written this paper had I gone as a graduate student to Clark, or had I been willing to take up a bare bones life, and I may well have been reduced to bones by the end, at Johns Hopkins. There is also another geography bearing on this paper, the location where it was first presented, Philadelphia. Given at the centennial meeting of the Association of American Geographers (AAG), an institution founded in Philadelphia, the 2004 AAG was a celebration both of geography's intellectual production and geography's geography. The two are necessarily twinned. The argument of this paper is that this is not only the Philadelphia Story, but quantitative geography's too, and, in fact, that of everyone who places ideas.

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Notes

1. I have interviewed the following geographers since October 1997: 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. The interviews were taped and transcribed. The transcripts were then sent back to the interviewees for amendments (except for Peter Gould who died before the transcript was returned to him). Only the amended transcripts are used for quotation in the paper.

2. Even among the limited number of places where the quantitative revolution occurred my empirical focus in the paper is even narrower, the sites within only the United States. Clearly there are more stories to be told about other places. Ron Johnston (2003a; 2003b), for example, has written two excellent essays about the British scene.

3. Ullman taught in Seattle from 1954, and was one of the very few American geographers at the time to have read, and indeed to have met, August Lösch, one of the originators of central place theory. It was at Harvard in 1938, just before Ullman was to return to Chicago to undertake a PhD that he had his first contact with Lösch. Lösch was there on a Rockefeller fellowship, and suggested to Ullman he read Walter Christaller's doctoral thesis on central places written in 1933 (Ullman, 1980: 221).

4. The original 'cadets' were: Brian Berry, Ronald Boyce, Duane Marble, Richard Morrill and John Nystuen. William Bunge, Michael Dacey, Arthur Getis and Waldo Tobler later joined them.

5. Ron Johnston (personal communication, 2004) recalls sending two quantitative papers to the *Annals of the AAG* when Spencer was editor, and having both accepted, but he was later told 'Spencer thought the second one was anti-quantitative, which it certainly wasn't!'

6. The MICMOG publication series (now available on line at www.arlinghaus.net/image/micmog.html) emerged from a three-way seminar involving participants from the Departments of Geography at Michigan State (East Lansing), University of Michigan (Ann Arbor) and Wayne State University (Detroit), who met at the point of minimum distance between the three places, a town called Brighton at the intersection of Highways 23 and 96. Beginning in 1962, they met on a Friday once a month, initially in the backroom of a local tavern, then later in a high school (Nystuen, 1997).

7. A recent parallel is the 'journal wars' as John Paul Jones, a former editor of the *Annals* calls them (Jones, personal communication, 2001). The wide-scale changes that occurred at the *Annals* – a shift from one editor to four, each with a particular area of expertise and independent ability to accept papers – were directly a result of some interests who felt neglected and wanted stronger alliances with that journal.

8. I recognize that the sociology of scientific literature has recently taken up the project of the place of ideas (Shapin, 1998), as well as some geographers, notably David Livingstone (2002; 2003). I draw on this work. I see my particular contribution as, first, providing an explicitly theoretical account of place and, secondly, relating that theory to the recent history of geography.

9. William Garrison and Marion Marts, both of whom were key to the quantitative revolution at the University of Washington, previously completed their PhD degrees at Northwestern University. Likewise, Edwin Thomas, the person who initially provided the mathematical muscle at the University of Iowa when he arrived in 1958, was a Northwestern graduate.

10. In negotiating the founding of *Geographical Analysis*, Reg Golledge and Les King won assurance from John Nystuen that the MICMOG series would end. In fact, Nystuen handed over to *Geographical Analysis* the MICMOG mailing list that consisted of 700 names and addresses (Nystuen, 1997).

11. The decision by Harvey to join Johns Hopkins's Department of Geography and Environmental Engineering even now seems slightly eccentric given that the Department's main research specialty was waste management. It was also perhaps not the best place to become a Marxist, which Harvey did shortly after arriving. During the 1950s, George Carter, the Chair of the then Department of Geography at Hopkins, turned in his colleague Owen Lattimore to Senator Joseph McCarthy who immediately put him on his list of 205 names of Communist Party members (Harvey, 1983).

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