Chapter 2
A Short Cultural History of Anglo-American Economic Geography: Bodies, Books, Machines, and Places

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Economic geography as a discipline doesn't just do culture (and in the various forms illustrated by chapters in this collection). It is culture, produced by culture. Culture goes all the way down, seeping into economic geography's very pores. By culture I mean a set of shared values, judgments, institutional forms, artefacts and embodied practices that while changing over time possess sufficient stability and continuity to make that culture distinctive and capable of transmission.

While I use the singular form of the noun, culture in practice is plural. There are many *cultures* within economic geography. Certainly that is true historically as this chapter will document, but even within a given time period at least many diverse cultures of economic geographical inquiry proliferate. That is no more true than it is right now (Barnes and Sheppard 2010). The current constitution of economic geography is as pluralist as it ever has been, with difference, otherness, and fragmentation breaking out all over. Especially over the last three decades, economic geography has been pulled, twisted and contorted by a torrent of different approaches that just keep on coming (for a review see Scott 2000; Barnes and Sheppard 2010).

And that is only contemporary economic geography. Once one goes back beyond 30 years, many more disciplinary cultures emerge. In setting out a brief cultural history, I begin by elaborating the idea of culture, discussing its relation to the production of scientific knowledge and the shaping of academic disciplines. Here I draw upon science studies that more than any other body of work asserts that culture is woven into the very woof and weave of academic knowledge. Consequently, it is impossible to separate culture from disciplinary knowledge. By unpicking it, one would unravel the very object of investigation. To keep the discussion focussed, I highlight four elements that contribute to the cultural production of academic knowledge: bodies, books, machines and places. The remainder (and bulk) of the

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chapter then uses these four elements as foci to organize selected vignettes of how culture plays out within economic geography's history.

That history, at least in its institutional form, begins in Western Europe in the late nineteenth century, and initially is hand-in-glove with various imperial projects. By the early twentieth century, while still fragile, the discipline begins to stand on its own, becoming increasingly robust and independent over the course of the next 100 years or so. It changes enormously over those 100 years, but constant is the importance of the cultural, and represented by the four elements identified above (Barnes 2000). Specifically, I argue that the blurring of the cultural with the discipline's scientific knowledge prevents clear-cut identification of progress within the history of economic geography. Progress is possible only if scientific knowledge is scraped clean of the cultural, allowing objective knowledge claims to be comparatively assessed, like with like. If cultural residue remains, however, no such comparison is possible, rendering assessments of disciplinary progress unachievable. This is the case in economic geography, I will suggest. The cultural is present throughout the entire corpus of economic geographical knowledge, and explaining, perhaps, why past disciplinary cultures never quite die out, but continue as existing traces. For this reason, economic geography maybe best conceived as a palimpsest (itself a cultural artefact), with partially erased, crossed-out, incomplete, and splintered-off versions of past cultures of inquiry persisting long after they once dominated. This makes the cultural history of economic geography messy and complex, sometimes indecipherable, but for all that no less compelling and powerful.

2.1 Cultures of Knowledge

2.1.1 Kuhn and Science Studies

The standard, rationalist account of the generation of scientific knowledge spurns talk of messy and complex cultures. Instead, it stresses the simple purity of rational thought lodged in a disembodied mind ("brains in vats" to use Putnam's (1981: 7) arresting image). Rationality's universality means that it matters neither where it is applied nor to what it is applied. What's important is only its inexorable pursuit, guaranteeing Progress with an upper-case P. For example, Newton famously could hold an analytical problem in his mind for days, turning it over, trying one logical line of attack, then another, until eventually it succumbed (Gleick 2003). Hence, he was able to invent calculus, or the wave theory of light, or the gravity equation. He did so through sweated brain power. By doggedly following rationality, and only rationality, truth was discovered (literally uncovered). It was as if truth were there all along, pre-existing, but hiding in the shadows, waiting for the searing light of rationality to be illuminated.

Criticisms of rationalism go back literally centuries, and take many forms. The critique I want to pursue is associated with a body of literature known as science

studies. The first canon shots over the bows of scientific rationalism came from Thomas Kuhn's [(1970 (1962))] paradigm-busting book on paradigms, *The Structure of Scientific Revolutions* (reputedly the most academically cited book of the twentieth century). Roughly, a paradigm is the constellation of values, assumptions, techniques and concrete exemplars shared by a given scientific community, making it what it is, and passed down to future practitioners. A paradigm shapes what scientists think about something before they think it. The interpretation I want to draw from this description, and elaborated below, is that a paradigm is like culture. It is a set of institutionalised values and practices, some are formal, others tacit, but both learned and shared within a community, and passed on. In this sense, a Kuhnian paradigm is similar to culture. It is a culture for acquiring knowledge.

Kuhn argues that not only does the paradigm model provide a different account of science compared to rationalism, but provides a critique of it too. Culture makes a difference. Under rationalism, it is only the mental acuity of the individual scientist, his or her firing synapses, that determines whether truth is discovered. In the paradigm view, truth is not discovered but actively made from within a paradigm, shaped by the heterogeneous elements that constitute it. The scientist is not a brain in a vat, but is embodied, undertaking specific corporeal practices using various material objects, and is embedded within a larger cultural system of inquiry (the paradigm). Moreover, Kuhn claimed that the elements that constitute a paradigm are so heterogeneous that comparing one paradigm with another is impossible. They are like apples and oranges. This is a problem because rationality can operate only if the entities to which it is applied are commensurate, comparable. If they are not, the case with paradigms, rationality has no purchase. Choosing a paradigm, therefore, can never be an entirely rational process, but always open to non-rational factors. This view leads to a third point: under the paradigm model of scientific knowledge the assertion of Progress with an upper-case P is impossible. To demonstrate Progress one must be able to show that truths under the current paradigm are truer than truths under the prior paradigm. But if the paradigms generating those truths are incommensurable, no such demonstration can be made. Scientific knowledge, consequently, moves not as linear progress, but as non-comparable, discontinuous transformations.

Kuhn never used the term "culture," and later in his life when he became a professor at Harvard he denied the anti-rationalist implications of his earlier work (Fuller 2000). But those implications were taken up by others a decade or so later after his book was first published under the banner of science studies (for reviews of science studies in geography, see Barnes 2003 and Powell 2007). That literature is now vast, full of disagreement and competing perspectives, but it remains united in upholding Kuhn's anti-rationalism; denying big P scientific progress; conceiving science as a set of learned embodied practices involving a spectrum of material entities (from Bunsen burners to industrial-sized labs); and emphasizing that scientists are always part of a larger *cultural* matrix that doesn't exist only on the outside, stopping at the walls of their lab or at their office door, but seeping inside, getting into their very ideas, theories, objects of investigation, places of work, forms of communication, and the historical course of their disciplinary inquiry.

2.1.2 *Bodies, Books, Machines, and Places*

There are many elements that enter into the cultural production of disciplinary knowledge, but for this chapter, and following the literature of science studies, I focus on four: bodies, books, machines, and places.

Rationalist accounts deal with bodies by excluding them. They follow the seventeenth century French Enlightenment philosopher René Descartes in separating thinking, that is, rational thought, from the body in which it occurs. The body's gender or skin color or sexual orientation is irrelevant. What counts is not what I am, but what I think. In Descartes's famous aphorism, it is because I think that I am ("*cogito ergo sum*"). Thinking comes first, while bodies are secondary, and can be written out of the histories of knowledge. Or when they are included they are there to add mere background color and texture. Contemporary science studies, however, partly as a result of interacting with, and contributing to, cultural studies, feminism, post-colonialism and queer studies, argues that the body should never be in the background but in the foreground. Mind and body cannot be separated. Who I am affects what I think. Bodies are never innocent. There might be the pretence that the body performs as only a "modest witness," to use Donna Haraway's (1997) phrase, rationally and dispassionately recording only the observable facts. Haraway (1997) argues, though, that such modesty is no modesty at all. It is a front, a ruse, hiding and protecting what many of those bodies do. For Haraway, the front of modesty hides the interests of white, Western men. It is their bodies that most produce and benefit from scientific knowledge. "Modesty pays off ... in the coin of epistemological and social power" Haraway (1997: 23) writes. Denying the importance of bodies thus turns out to be a strategy to promulgate a particular kind of knowledge, and for Haraway marked often by masculinism, racism and heteronormativity. It is for this reason that we must be culturally attentive to the kinds of bodies involved in producing economic geography's knowledge. Knowledge is never disembodied, always incarnate.

Second, on the surface books appear as neutral media of knowledge transmission, simply the materialization of rational thought. But like bodies, books have a complex cultural history and role. To use the language of science studies, one purpose of a book is to be a "spokesperson" for a specific culture of inquiry. They sit on the library shelf waiting to be taken to an office or a home, to be opened and read, and to persuade readers of the knowledge between their covers. Bruno Latour (1987) labels books "immutable mobiles." They are mobile in that that they easily travel, but are immutable in that the distance they travel does not physically corrupt the inscriptions they contain. They contribute to the cultural production of economic geography in at least four ways. First, because they are "immutable mobiles" they facilitate connections among geographically dispersed members of the same culture, cementing their relation, but also reaching out to non-members, bringing them into the fold, enlarging the community. Second, a book is a powerful artefact because it brings many varied aspects of the world together on a single page, enabling control, re-ordering, manipulation, and assertion of cultural authority. As Latour (1990: 45)

writes, once inscribed on a book's page, phenomena can be "dominated with the eyes and held by the hands, no matter when and where they come from or what their original size." Third, if they are successful, a book becomes an "obligatory passage point" (Latour 1987: 159), a mandatory cultural reference whether one agrees or disagrees with its position. If you want to do/undo economics then you must read Paul Samuelson's *Foundations of Economics*. If you want to do/undo sociology then you must read Talcott Parson's *Structure of Social Action*. Finally, a book's success derives from the quality of its rhetoric meaning an ability to draw together within its text different allies. Consequently, if you attack the book it means you also attack its allies, thus making the volume stronger, more secure and durable.

Third, it might seem odd to make machines one of the components of the cultural production of a discipline given that generally they are neither thought of possessing culture, nor thought capable of making knowledge (they are tools to produce knowledge, not knowledge producers). Again science studies, suggests a different view. For Latour (1993: 79–82) machines are not just "intermediaries," but "mediators," shaping, channelling, and entering into the form of knowledge that is produced. As Hetherington and Law (2000: 35) write: "Material trappings are not just trappings. They are not idle. They are also performative. That is they act. ... [They] participate in the generation of information, or power relations, of subjectivities and objectivities." For example, software used to run the hardware of geographical information systems brings with a systematic set of cultural biases, hidden assumptions, and *aporias*. The knowledge that is produced by such machines, and found in print-outs and screenshots, are not mirror copies of the world, "the gaze from nowhere" (Haraway 1991: 188), but the view from somewhere. Machines are not inert pieces of wire, plastic, metal, and glass, submissively responding to the beck and call of their operators. But they possess agency, and enter into the cultural production of knowledge. Peter Galison (1997) provides a brilliant illustration in his history of twentieth-century microphysical detectors used in scientific laboratories. His 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" (Galison 1997: 5). Similarly we need a history of economic geography from its machines outward.

Finally, there is place, and increasingly important within science studies (Livingstone 2003; Powell 2007). Initially science studies focussed on the microspaces of just one kind of place, the laboratory (Latour 1988). But it was increasingly recognised that both substantively and theoretically place served more than only a site for the enactment of a specific form of science. How place was an active contributor to the production of knowledge was taken up by several researchers. Kevin Hetherington (1997), for example, argued that some places, which following Foucault (1986) he labelled heterotopias, held the potential to allow a different form of ordering to emerge compared to other places outside. To use my earlier vocabulary, heterotopias, because of their openness, were places of potential new paradigm

formation, sites that permitted disciplinary transformation. Gieryn (2002) suggested that some places through their cultural meaning and materiality became "truth spots;" that is, the knowledge claims made at those sites precisely because they were made at those sites legitimated them as necessarily truthful, "authentic all over" (Gieryn 2002: 118). If it came from Harvard it must be true. Or finally, for Bruno Latour (1987) places were central to the production of knowledge because they functioned as "centers of calculation." That is, they were sites where knowledge from other places was gathered, accumulated, organized, and interpreted, before disseminated and producing "action at a distance" (Latour 1987). The important point for this chapter about these works is that whether as heterotopias, truth spots, or as centres of calculation, places are entwined with the cultural, permeating the resulting production of knowledge.

In sum, following the science studies literature I've argued that the production of knowledge is always infused with culture. It is present at every turn. Any history of economic geography therefore must also be a cultural history, and to which I now turn.

2.2 Cultures of Economic Geography

2.2.1 Bodies

Until even 25 years ago the vast majority of economic geographers were men, and men of Northern European heritage (like me). White men continue to predominate, but over the last quarter of century there has been at least incremental change, with women more numerous as well as non-Caucasians (especially those of Asian heritage). This shift has brought about a change in the intellectual agenda of the discipline, speaking directly to the entanglement of culture and knowledge.

Up until around 1980, economic geography was unabashedly masculinist. That was true first, in terms of the gender of those who practiced it, and second, in terms of the discipline's substantive interest. Economic geography was primarily concerned with what men did at work, and often taken to be producing physical commodities using large machines with other men and frequently in heartland industrial regions in Western Europe and North America. And finally, in terms of its method, economic geography was about accumulating hard facts, often in numerical form, displaying them in tables and maps, and since the post-world war II period undertaking statistical analysis including occasionally formal mathematical modelling.

The form of the pre-1980 version of the discipline can be traced back to the very origins of the university-based economic geography that started in the late nineteenth century in Western Europe and the United States. The earliest Anglo-American professors of economic geography were white males like George Goudie Chisholm (1850–1930) at Edinburgh University (MacLean 1988), Lionel W. Lyde (1863–1947) at University College London (UCL) (Clout 2003), or J. Russell Smith (1874–1966) at the University of Pennsylvania (Rowley 1964). They wrote about

where and how commodities were produced, and about transportation simply as a means to move goods from where they were produced to where they were consumed. Their concern was purely the commodity, and as a corollary the brute facts of nature, the labor process (but rarely labor), physical infrastructure, and new technology. Theirs was a "God's eye view," to use Haraway's term, providing knowledge of the whole world, with detailed empirical inventories of every continent to prove it. Those inventories took the form of maps, black and white photos, appendices stuffed with tables, and on almost every line of text a statistic. George Chisholm (1889: iv) says in the preface to his volume, *Handbook of Commercial Geography*, that his intention was *not* to "encumber the book with a multitude of minute facts." But those minute facts litter every page.

While seemingly the "gaze from nowhere" (Haraway 1991: 188), early economic geography was of course the view from somewhere and from some*body*. It was the view from the center. That is, from those countries that were industrialized, wealthy, anxious to pursue trade, and that most wanted to maintain or expand the colonies they controlled. The beginning of the "Scramble for Africa" coincided almost exactly with the date of the first formal rendering of economic geography as an academic discipline in 1882, defined by the German geographer Wilhelm Götz (Sapper 1931).

The whiteness of the bodies that did the gazing came out, at least in some of those bodies, as racist theories of development couched as environmental determinism. The early twentieth-century Yale-based geographer, Ellsworth Huntington, was perhaps the most notorious. His 1915 book, *Climate and Civilization,* included a chapter on "Work and Weather" (Huntington 1915). Huntington calculated that mental work efficiency was maximized if mean seasonal temperatures do not fall below 38°F, and physical work efficiency was maximized if mean seasonal temperatures do not exceed 65°F (Huntington 1915: 129). *Ipso facto*, mental and physical work occurred most efficiently in the world's temperate regions, and, as it just happened, occupied by white populations (that is, if one forgets the original aboriginal inhabitants who usually had been decimated by European colonization). People in North or Central Africa or South or East Asia, or Central or South America had not "made it," nor could they have ever made it, because the climate regime under which they labored made them labor so badly. The early American economic geographer J. Russell Smith fully approved, writing to Huntington to say that the latter's "chart showing the relation of human output to temperature" was "real geography" (quoted in Livingstone 1994: 143). The British economic geographer Lionel Lyde was not much better. As reported in a 1911 *New York Times* article, "How Men Changed their Color," Lyde suggested low amounts of light and significant rain made humans light skinned, and high amounts of light, and significant aridity made humans dark skinned.[1]

[1] How men changed their color, *The New York Times*, August 6, 1911. Available on-line at: http://query.nytimes.com/mem/aLawrchive-free/pdf?res=F60F15FE3D5813738DDDAF0894D0405B818DF1D3

Economic geography continued as a man's discipline following its incarnation as regionalism during the inter-war period (regions as natural economic geographical units), and later as post-war spatial science that attempted to make economic geography a natural science (Barnes 2011a). The latter involved the systematic application of hard science forms of theorising and abstract modelling, and the use of rigorous statistical techniques of analysis and description (Barnes 2011b). Later critics interpreted the hyper rationalism of spatial science's epistemological form with its assertion of universality and certainty as classic tell-tale signs of phallocentricism, that is, a gendered, masculinist conception of knowledge (Doel 1999). Certainly, women rarely participated within spatial science. At an iconic conference where spatial science debuted on the world stage, the 1960 International Geographical Union meeting at Lund, Sweden, not one of the 63 paper presenters or discussants was a woman. The conference photographs show a sea of men's faces except for the tea room, where two well-dressed, smiling women fill a table full of cups from two large teapots.[2] Or another example: Patricia Burnett and Susan Hanson both faced considerable obstacles in participating in U.S. spatial science. Burnett in the end sued Northwestern University's Geography Department, where she was a professor, for its "climate of sex discrimination" (P. Burnett, Interview with the author, Cambridge, MA, May, 2002). While Hanson (Interview with the author, Worcester, MA, May, 2002), a graduate student at Northwestern during the 1960s, remembers that when the chair of the department, Ed Espenschade, "spoke, he only asked how the family were, but never asked about scholarly work. Realistically ... one did not expect anything different! We knew very well that we were entering male turf."

In that light, Doreen Massey's (1984) book *Spatial Divisions of Labour* was a watershed volume for economic geography in two senses. First, the book offered a very different agenda for economic geography. Production and the factory were still prominent, but it was not necessarily production by men. Moreover, to understand what went on inside the factory, Massey argued that it was necessary to refer to what went on outside, particularly in the home (the site of "domestic reproduction"). As a result, distinguishing different kinds of bodies and what they did at different sites was critical. The intellectual sensibility was different too emphasizing the cultural as much as the economic, interpretation as much as analysis, and deft prose as much as deft arithmetic. The second watershed, of course, was that the book's author was a woman. There had been women economic geographers before, but not many given the struggles they endured as the experiences of Burnett and Hanson illustrate. But there had never been a woman economic geographer who had built into her conceptualization of the disciplinary problematic the absence of bodies, and female bodies in particular, providing a larger theoretical framework to set and resolve that lack.

[2] Photographs of the conference were taken by the University of Chicago geographer Chauncy Harris, and can be found in his Papers lodged at the University of Chicago (Papers of Chauncy Harris, University of Chicago, Box 26, Folder 3). The conference organisers prepared a post-meeting souvenir brochure of the event using photographs by Harris, and containing pictures of the all-male line up, as well as the two tea ladies.

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That was the genius of Massey's book, which divides the history of economic geography into before Massey and after.

After Massey came a different kind of economic geography that was less straight-laced and buttoned up, less about numbers, less about the economy, less about white men. Massey's work didn't so much determine how economic geography was conducted, but provided the disciplinary room for experimentation with other forms of inquiry and methods. More women came into the discipline, often bringing concerns about the body, and using those concerns to re-theorize the subject (both human and academic). J. K. Gibson-Graham (1996) and their book *The End of Capitalism (as We Knew it)* represented all of these things. They were two women (Kathy Gibson and Julie Graham), who drew on feminist theory, advocated and undertook novel methods (action research), and radically re-drew what counted as disciplinary knowledge and practice. They showed what was possible from a different cultural sensibility.

More recently, the "relational turn" that entered the discipline about 10 years ago can be interpreted as yet another expression of the desire to widen further the range of bodies that potentially contribute to economic geography's knowledge (Bathelt and Glückler 2003; Yeung 2005). The "relational turn" understands the relationship among different economic agents in terms of a network of close interconnections even when those connections in physical distance are not close at all. Actors across the world are tethered to one another by complex sets of relations. Everyone within the network is joined. This is a message, though, also for the discipline of economic geography. All economic geographers, even those physically distant from Anglo-American economic geography, are connected. All are part of a global disciplinary network. Everyone is connected, all bodies count. In the case of the "relational turn," it has been especially Asian economic geographers, as well as Continental Europeans, who want to count, to be part of what they conceive as a world-wide network linking the discipline, and formalized now in three "Global Conferences in Economic Geography," the first significantly in Singapore in December, 2000, followed by meetings in Beijing and Seoul.

The larger point is that the minds that come up with and develop academic disciplines cannot be divorced from the bodies that they inhabit. Disciplinary knowledge is embodied knowledge. Consequently, one needs to know something about the changing bodies practicing economic geography to understand changing disciplinary knowledge.

2.2.2 Books

Although books are material objects – even in virtual form, they require *hard*ware to be read – this doesn't lessen their cultural import. As artefacts they serve many cultural purposes. They introduce and acculturate novices into the field (the aim of the textbook); they are repositories of what Kuhn (1970) called "exemplars," classic theories, concepts, and case-studies on which disciplinary practice, craft, and judgment rest; they are deposits of disciplinary memory; they are sparks

for creative new projects; they are objects to give to outsiders to show them what we do, and to persuade them of its worth; they are a means to forge allies both inside the discipline, as well as outside including funding bodies, government bureaucracies, even Royal Commissions. In short, the materiality of a book does not make it inert and dead, but is the very means of its cultural liveliness and engagement.

Books are able to achieve these cultural ends because they travel (they are "mobile"), while their inscriptions remain the same (they are "immutable"). That said, interpretation and judgment of those inscriptions vary depending upon the culture of the place in which they are made. For example, David Livingstone (2003, ch. 4) discusses the remarkable range of different interpretation given to Charles Darwin's *The Origin of Species* as it variously travelled during the late nineteenth century to Edinburgh, Belfast, Dublin, Princeton, NJ, and Charleston, SC. Both reading and writing are culturally infused.

The first English language textbook in economic geography was George G. Chisholm's (1889) *Handbook of Commercial Geography*. It was fundamental to the creation of the field, and its expansion. With that book one could show that economic geography was no longer just an idea, merely an intellectual distinction. It literally possessed substance. The discipline was materialized: set out in ink and paper, folded between two sturdy covers, and weighing in around a kilogram. One could now literally hold the new discipline in one's hands, and pass that discipline around to interested others. Certainly, it reached the hands of fresh-eyed university and extension students. Chisholm used his *Handbook* as a text for a full-year course of lectures he gave at Birkbeck College, University of London, beginning in 1896. But it was also passed on to education boards, and in the UK even to a Royal Commission that determined school and university curricula (MacLean 1988: 23). It also made its way to other academics, showing that there was a new discipline on the block. And it went to those, sometimes far afield, who wanted to practice the new discipline of economic geography themselves. For example, Emory R. Johnson (1906), a transportation economist at the Wharton School, University of Pennsylvania, cited Chisholm's book in 1906. That same year a former doctoral student of Johnson's, J. Russell Smith, used the *Handbook* to argue for the importance of establishing a separate Department of Geography and Industry at the Wharton School, which was created in 1906 with Smith as founding Head. In 1913, Smith (1913) used the *Handbook* as a template for his own textbook, *Industrial and Commercial Geography*, which was widely picked up as a text for the new discipline among US colleges offering courses in economic geography (Fellmann 1986).

Of course, it is not only Chisholm's book that provided a foundation for the discipline. J. Russell Smith's was later important, and at the turn of the nineteenth century so were those of Lionel Lyde (1863–1947), Professor of Economic Geography at University College London from 1903, and author of such stirring titles as *Man and his Markets* (1898) and *A Commercial Geography of the British Empire* (1903). By the end of the First World War, economic geography was up and running in Anglo-American universities. But already it was shifting perspective, tilting inward, increasingly leaning toward a regional perspective. The textbooks, though, kept on coming. But now written not about imperial global production and

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the world-wide movement of commodities, but about the peculiarities of unique regions, which were represented using stock typologies into which the economic geographical facts of a region were meticulously sorted. Being an economic geographer now meant learning the typology, and all the facts under each of its categories. Ray Whitbeck and Vernor Finch (1924) at the University of Wisconsin, Madison, for example, deployed a parsimonious four-fold classification typology in their textbook, *Economic Geography,* while the more expansive Clarence Jones (1935) at Clark University provided an eight-fold classification. In the scheme of things, it turned out to be wildly popular, becoming "the standard introductory text on the subject for thousands of college students" (Hudson 1993: 167).

But Clarence Jones's (1935) *Economic Geography,* which began with the immortal line "Most of us wish to visit distant lands ... to hunt lions and tigers in the forests and savannas of Africa," could not cut it as a text for what came next from the late 1950s, i.e., spatial science. Something completely different was required: a primer on geographical theory, modelling, and statistical techniques. Peter Haggett's (1965) book *Locational Analysis in Human Geography* brilliantly filled that need. It did so in part because it succeeded so well as a cultural artefact.

Locational Analysis did several things, including: (1) introduced novices to the field, persuading them of the virtues of practicing the new approach. The book's origins were "much thumbed and much-revised lecture notes" that Haggett (1965) used in his third-year class on "Locational Analysis" taught on Saturday mornings at Cambridge University. Those students would not have been exposed to a theoretical or mathematical geography before Haggett's class, and so were the perfect audience for trying out and sharpening the presentation. (2) The book travelled extensively, with its first edition selling 40,000 copies (Thrift 1995: 383), and accumulating 378 citations between 1966 and 1984 (the third most cited book in all of geography for that period; Wrigley and Mathews 1986). (3) It became an "obligatory passage point" in Latour's terms, with anyone practicing spatial science required to read it. In this, Haggett also prepared the ground from the mid-1960s by organising lectures for British sixth-form geography school teachers precisely around the themes of *Locational Analysis* (the Maddingly Hall lectures). Consequently, when sixth-form students went on to university to read geography they expected to read Haggett. (4) *Locational Analysis* was a flag bearer for the discipline, showing non-geographers what geography could do. The urban planner, Peter Hall, writing in *New Society*, for example, described it as "the most original and important book from a British geographer for many years" (quoted in Thrift 1995: 383). (5) It was stuffed with Kuhnian exemplars, and most frequently represented by the miniature diagrammatic spaces that littered its pages (there were 162 numbered illustrations in 310 pages of text). Each figure showed with great economy and clarity the exemplary virtue of some spatial theory, model or technique.

More examples could be given. The point is that books are not like tablets handed down from the mountain top carrying divine force. To be successful, readers must be persuaded that they should be read, be convinced of what they read, and then to pass on their reading. Books gain their power on the ground through cultural, not celestial, means.

2.2.3 *Machines*

Compared to other disciplines, especially those in the natural sciences, economic geography has not been a big user of machines to produce knowledge. Of course, early economic geographers like Chisholm and Smith were deeply concerned with machines, interested not only in individual pieces of equipment, but in the larger genus that by the late nineteenth century had created the object of their new study, economic geography. Writing in the late nineteenth and early twentieth centuries, Chisholm and Smith thought latent Promethean impulses had been at last unshackled, breaking loose old geographical constraints on the production and movement of goods. Innovations in transportation and communications – the railways, the steamship, the internal combustion engine, the telegraph, the telephone – allowed for a hitherto unachieved global integration of commodity production, circulation and exchange, finally circumscribing nature's historical niggardliness.

Germane here, in representing machines and their geographical effects, the early economic geographers used machines. Chisholm's book has no photographs, but Smith's is full of them. Many were gleaned from U.S. government sources, but a significant number were taken by Smith himself with his own camera as he travelled the sites of industrial and commercial geography. Chisholm's book is notable for its pull-out, two-tone, multi-patterned regional transportation and commercial maps individually glued between folio pages. Those maps were not drawn by Chisholm, but a colleague, "F. S. Walker, FRGS" [Fellow of the Royal Geographical Society]. Accordingly, it was Walker who deployed the machinery of map-making that during especially the second half of the nineteenth century was radically improved as a result of technological changes (Pearson 1983 provides a detailed inventory). It was now much easier to print different color tones, incorporate varied map markings, and change fonts and sizes, all features found in the *Handbook's* maps.

Machines came into their own, especially within the post-war spatial science version of economic geography. The hallmarks of that movement were numbers and calculation. At least initially, the numbers were limited, with calculations relatively simple. A slide rule was sufficient. Better still, though, was a mechanical calculator. There were manual ones like the Monroe, although increasingly they were supplanted during the 1950s by electrical versions like the Marchand or the Frieden. The situation wasn't static, though, with the amount of numerical data and the complexity of calculations increasing exponentially. By the late 1950s, even the Marchand and Frieden couldn't cut it anymore. More machine help was necessary. Fortunately, precisely at that moment help became available. The first commercially sold computer in North America was the IBM 650 launched in 1954, bought by Columbia University. Other universities quickly followed, including at the University of Washington and the University of Iowa, which both had geography departments beginning to travel down the road of spatial science. There was no formal training for those early spatial scientists in using the computer, though. It was a "bootstrap operation," as Brian Berry (Interview with the author, Pittsburgh, PA, April, 2000), a student at the University of Washington, called it. Nor initially were there any formal programming languages. Michael Dacey (Interview with the author,

Evanston, IL, November, 1997), who was a student with Berry at Washington, remembers one had to "programme with patch panels, actually plugging in wires." And Waldo Tobler (Interview with the author, Santa Barbara, CA, March, 1998), also at Washington, remembers:

> We had to go up to the attic of the Chemistry building at 2 a.m. 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 2K memory which rotated real fast. And if you were clever, you could pick up two pieces of information in one rotation.

In comparison to present computing capacity, these early machines and procedures were like lumbering dinosaurs. But in terms of the culture of the time, they represented rocket science, or at least, spatial science. Moreover, machines and operating procedures transformed rapidly, very quickly becoming *de rigueur* within the new culture of economic geography, spatial science. Haggett's (1965: 248) *Locational Analysis*, for example, extolled the virtues of "high-speed computers," using them to derive specific maps and numerical results in his own text. Those maps and results would never have been produced without these machines. They were not just lifeless instruments (tangles of wires joining switches, buttons and flashing lights), but separate and independently contributing actors. How a computer was designed, what it could do, made a difference to the knowledge that spatial scientists generated, thus affecting the very character of economic geography as a discipline.

The computer was perhaps the most important of the machines for spatial science, but there were others, admittedly less sophisticated but enjoying at least walk-on roles. The lowly slide-rule was an early performer, and later the line-printer, and the Xerox machine. Key at the University of Washington was another machine, the duplicator. The chair of the department in the mid-1950s, Donald Hudson, gave the graduate students unlimited access to paper, ink, and use of the machine. It was used initially for mimeographing internal discussion papers, but later as the means to reproduce a Departmental discussion paper series. It was sent around the world, and catalytic in forging spatial science in other places. Haggett (1965), for example, acknowledges its influence on his own work. Like the computer, the duplicating machine made a profound difference to how spatial science in economic geography was shaped. As a result, if we want to understand the discipline, we need to understand its machines. It is necessary write a history of economic geography from its machines outward.

2.2.4 Places

While the role of place of economic geography seems like a topic that should have gripped the attention of geographers, in reality it has been barely studied. This lack of response goes to the powerful hold of rationalism. Rationalism contends that place does not matter in the production of ideas. Rationalism is universal, found

everywhere, rendering irrelevant the peculiarity of the place in which inquiry occurs. It is the "view from nowhere," as Thomas Nagel (1986) labels it. Against this rationalist view, however, has emerged quite a different one over the last 40 years that holds that the character of place, its culture, is utterly critical to the formation of ideas that occurs there (or doesn't). It is a view put forward primarily by sociologists rather than geographers, however.

The first, Bruno Latour, conceives place as an imperial "centre of calculation," with circuits of travel radiating outward to collect and bring back materials of all kinds. At the center, the material is sorted, analysed, interpreted and displayed, used as the basis for decisions sent back to the periphery to effect action at a distance. Latour's geographical model it is at least half right as a depiction of the geography of the discipline during its early years. European national capitals were in particular the first sites at which the discipline initially emerged. Universities in Berlin and Paris were the first to employ economic geographers from the 1870s, and then slightly later, the University of London. As mentioned, Chisholm began as a sessional lecturer at Birkbeck in 1896, and in 1903 Lyde secured his Chair at UCL. In 1908 Chisholm went to another national capital, Edinburgh, where he was the founding appointment in Geography at that city's university, first, as Lecturer, and from 1920, as Reader. In the United States, the first courses in economic geography were taught in 1893 at Cornell and the University of Pennsylvania (Fellmann 1986). In 1906 at the University of Pennsylvania, Russell Smith became the inaugural chair at the Department of Industry and Geography. Not all these sites were imperial "centers of calculation," however. Ithaca, New York, where Cornell is located, was hardly that. And there were other places where economic geography was practiced that do not fit Latour's scheme such as the University of Chicago, home to the very first department of geography in America (1903), and a well-known early economic geographer, Walter S. Tower (Harris 1979). Or again, the geography department at Clark University in Worcester, MA, founded in 1921, began publishing four years later the flagship journal of the sub-discipline, *Economic Geography* (1925). But again Worcester was hardly an imperial center of calculation. As for Latour's thesis about the importance of circulations around centers of calculation, Chisholm relied heavily on the availability of statistics taken from the British colonies and accumulated in London. Those statistics were laced throughout the *Handbook*, especially amassed in a 60-page statistical appendix. But being in London also gave Chisholm access to all kinds of reports and journals, as well as foreign language books, on which he says in his preface he relied (Chisholm 1889:iii-vii). Or again, according to Clout (2003), Lyde relied both for his publications and his lectures on an enormous collection of meticulously kept clippings taken from newspapers and periodicals acquired in Central London. Whether either the *Handbook* or *Man and his Markets* were the basis of action at a distance is less clear. The *Handbook* went to 20 editions (the last in 1982), and was used as a text for many years for British professional exams in banking and commerce, so one would expect some effects. Brian Hudson (1977), at least, argues for a strong link between the texts of late nineteenth and early twentieth century British geographers and action at a distance in the British colonies they geographically represented.

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The works of the other two sociologists discussed, Hetherington (1997) on heterotopias and Gieryn (2002) on truth spots, are relevant to understanding the geography of spatial science and its immediate aftermath. At the basis of Hetherington's argument is that for fundamental social and intellectual change to occur there must be geographical sites, places, sufficiently open and porous to allow the new way, whatever that might be, breathing space and elbow room to develop. Hetherington's example is the Palais Royale in Paris that opened up a heterotopic space against the *ancien regime,* allowing it to "become the focus for other interests and hopes for social change" in revolutionary France (Hetherington 1997: 51).

The key heterotopic spaces for the emergence of spatial science in economic geography during the second half of the 1950s were the departments of geography at, respectively, the Universities of Washington (Seattle) and Iowa (Iowa City). At both places there was a reaction against the *ancien régime* of regional geography and a hope that it would be replaced by a revolution based on numbers, calculation, rigorous theory and the IBM 650. But what was it about the departments at Washington and Iowa as geographical sites that fostered the necessary elbow room and breathing space for change? Both departments until the mid-1950s were second-tier intellectually, on the margins of American geography. The department of Washington was older than Iowa's, and had been run by the Earls, a husband and wife team, both committed regionalists, but with little academic kudos. The department at Iowa was created only in 1946 when the Dean assigned a professor from the Business Faculty, an economist by training, Harold McCarty, as chair. In contrast, the powerhouse American geography departments after the War were staffed primarily by well-known regionalists. They had a vested interest in maintaining the status quo, such as at Wisconsin (Richard Hartshorne), Syracuse (Preston James), Harvard (Derwent Whittlesey), Chicago (Wellington Jones), Michigan (Robert Hall), and Berkeley (Carl Sauer). Given the challenge to regionalism that spatial science represented, it would have likely met with considerable resistance at these top-tier departments (especially given also the forceful personalities of some of the academic staff like Hartshorne, James and Sauer). Resistance to change would be less in weaker departments like Washington's or departments that had yet to establish a tradition like Iowa's. For brevity's sake it is not possible to provide a detailed unfolding of the "revolution" at those two places (details are found in Barnes 2001a, b, 2003). My argument, following Hetherington, is that they occurred because of the peculiar local cultural conditions found at each place, making them temporarily at least heterotopic spaces. It was not inevitable that change would occur. Spatial science did not emerge from any iron law of rationality asserting itself. Rather, it was conjunctional, the consequence of several factors converging, including the cultural character of the place where those conjunctions met.

In his 1977 textbook on quantitative methods for geographers, Peter Taylor presented, half-tongue-in-cheek, a flight map for "Quantgeog Airline Flight Plan" (Fig. 2.1; Taylor 1977: 15). It is a remarkable map because it is one of the few in geography that is not a cartography of things, but a cartography of an idea, spatial science. In Gieryn's terms, the sites on Taylor's map represent "truth spots." They

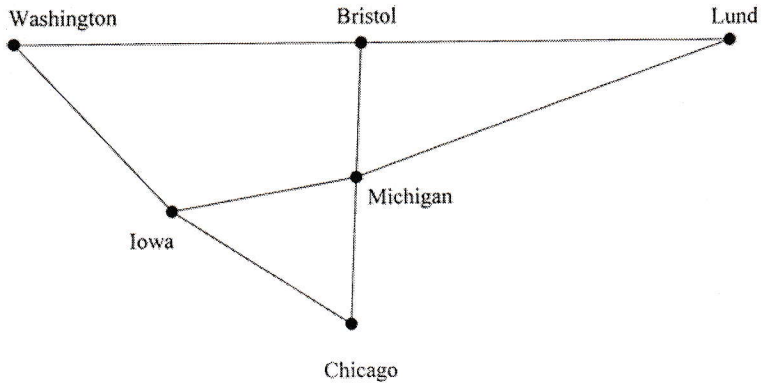

Fig. 2.1 Quantgeog airlines flight plan (Source: Redrawn from Taylor 1977: 15)

are places where the work of spatial science is done, and because the work is done at those sites, one believes that the truths generated hold not only in those places but everywhere else too. It becomes "true all over" (Gieryn 2002: 118). The term "truth spot" is a reminder that place occupies a persuasive force in knowledge claims. But the term can also be used to understand why some places suffer once that persuasive force diminishes. To use Taylor's map again, in 1977 two of the truth spots of spatial science he identifies were the University of Chicago and the University of Michigan. They became truth spots because students who were at the Universities of Washington and Iowa in the late 1950s gained jobs at Chicago and Michigan, and recreated at these latter sites the conditions that held at the former. But all this changed again when in the mid-1980s both the University of Chicago and the University of Michigan closed their departments of geography. These closures were partly a result of a suspicion held from the late 1940s among administrators of top-tier American universities, such as Chicago and Michigan, that geography as an academic discipline was not up to the task of making any of its knowledge claims "true all over." This step occurred directly from the closure of geography at Harvard in 1948 following the belief of its President, James Conant, that geography was "not a university subject" (quoted in Smith 1987: 159). But partly it was also a result of key faculty who practiced spatial science leaving Chicago and Michigan for elsewhere. University administrators thought if anyone was capable of making claims true all over it was the spatial scientists. But if they went, so did the status of the truth spot.

2.3 Conclusion

In putting forward his pragmatist philosophy in 1910, the American philosopher William James made the comparison with rationalism. Rationalist philosophers, he wrote, "have always aimed at cleaning up the litter with which the world is

apparently filled. They have substituted economical and orderly conceptions for ... tangle." In comparison, pragmatism "offers but a sorry appearance. It is a turbid, muddled, Gothic sort of affair without a sweeping outline and little pictorial nobility" (James [1910] 1977: 26).

The same is true in comparing a rationalist account of the history of economic geography with a cultural history. The rationalist account sees ordered Progress, and increasing convergence to the Truth. History's litter would be cleaned up. But that is clearly not my cultural history of economic geography. It is a messy, "turbid affair," with bits of the past littering the present, with no "sweeping" narrative holding all the parts together, giving them direction. Instead, there are only a series of historical awkward corners, disjunctive with what comes next. But that is culture for you. It often behaves badly. That is what my four historical vignettes written around bodies, books, machines and places show. They do the darndest things. Economic geography may consequently lack "pictorial nobility," but such images are usually fantasies, projections of idealism. Instead of writing a history of economic geography of what we would like it to be, my intent by incorporating culture has been to write it as it is.

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