

'In the beginning was economic geography' – a science studies approach to disciplinary history¹

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Abstract: Science studies are an increasingly prominent interdisciplinary body of work. Now a diverse literature, one of its most consistent and common themes is a reluctance to accept the standard model of scientific explanation ('internalism') that conceives scientific knowledge, and the disciplines with which it is associated, as the product of a rationality that is progressively realized over time. Instead, science studies emphasize the importance of local circumstances in shaping knowledge, which, in turn, makes such knowledge messy and context-dependent. The purposes of this paper are twofold. The first is to provide a selective review of science studies. In particular, the paper recognizes three subtraditions within the larger genre: Mertonian institutionalism, the sociology of scientific knowledge, and cultural studies of science. The second purpose is to begin developing a case study in order to apply such literature, that of the institutional origins of economic geography during the late nineteenth and early twentieth centuries, and linked to a series of wider social processes around commercial trade and imperialism. To make the case study manageable, I concentrate on only two authors and their respective key books: the Scottish geographer George Chisholm, who wrote the first English-language economic geography textbook, *A handbook of commercial geography* (1889); and the American geographer J. Russell Smith, author of the first US college text in economic geography, *Industrial and commercial geography* (1913).

Key words: economic geography, science studies, colonialism, trade.

Now, I realize that accounts differ. Your species has its much repeated version, which still charms even sceptics; while the animals have a compendium of sentimental myths. But they are not going to rock the boat, are they? Not when they've been treated as heroes, not when it's become a matter of pride that each and everyone of them can proudly trace its family tree straight back to the Ark. They were chosen, they endured, they survived: it's normal for them to gloss over the awkward episodes, to have convenient lapses of memory.

Julian Barnes, *A history of the world in 10 ½ chapters* (1989: 4)

I Introduction

In recounting *A history of the world in 10 ½ chapters*, Julian Barnes begins at the beginning with the Book of Genesis. Things are more complicated in writing about the emergence of economic geography, the primary focus of this paper. The difficulty is in knowing from which start to start. Do we begin in 1925 when the journal *Economic Geography* was first published at Clark University (Murphy, 1979)? Or in 1893, when economic geographical courses first appeared in US university calendars at Cornell and the University of Pennsylvania (Fellmann, 1986: 317)? Or in 1882, when the German geographer Götz distinguished between commercial and economic geography (Sapper, 1931)? Or in 1826, when what has become a classic treatise in economic geography, von Thünen's *The isolated state*, was privately published (von Thünen, 1966)? Or do we go back to the Book of Genesis itself, and see economic geography beginning with the first economic geographical act, Eve picking fruit in the Garden of Eden?

Of course, there is no definitive answer: it all depends upon one's perspective. This is the point that Julian Barnes is also keen to make, and clear from the epigraph. Barnes's first chapter in his history, 'The stowaway', retells the Book of Genesis story of Noah and the Ark from the perspective of a woodworm which, because it was not on Noah's list of invited animals, was forced to make its way aboard by stealth. Because the woodworm has no vested interest in maintaining an officially sanctioned story – it was not supposed even to be there – what emerges is a counter-punctual reading of life on the Ark. Noah is seasick, family fights between Ham and the brothers break out, and many (now extinct) animals are eaten. None of this emerges in the Book of Genesis, of course. Awkward episodes are glossed over, and indiscretions conveniently forgotten. Noah is presented as the paragon of the good, kind, and wise old man, and everything else follows from that attribution.

Barnes's point, though, is that history can be told in different ways, making it multiple and messy. Often, however, only one story is sanctioned (in this case Noah's), and once sanctioned it is maintained by powerful institutional forces and everyday beliefs and practices (such as the Christian church). In the process, other stories are silenced, messiness is tidied up, and the approved version naturalized, and made to appear inevitable.

The purpose of this paper is in a very preliminary way to do for economic geography what Julian Barnes does for the book of Genesis. It is easy to think that as a discipline economic geography's origins were both natural and inevitable, and contained an inner logic that propelled it forward. By drawing upon the burgeoning literature of science studies, I will argue that such a belief is wrong. Science studies does for conventional histories of science what Julian Barnes does in his book; that is, to contest and to disrupt single, neat, and seemingly inevitable narratives of progress and accomplishment. A

conventional history of science stresses final achievements, and sees them as the foregone consequence of the progressive unfolding of some fundamental principle that makes them possible (usually rationality). This kind of history, to use Bruno Latour's (1987: Chapter 1) term, presents science as a 'black box', as a set of tidy and inexorable theoretical and experimental triumphs. As Latour (1999a: 304) puts it, blackboxing occurs when scientific work 'is made invisible by its own success. When a machine runs efficiently, when a fact is settled, one need only focus on its inputs and outputs and not its internal complexity.' For Latour, though, and for others within the tradition of science studies, black box history is inadequate because it fails to connect the accomplishments of science to the heterogeneous practices, contending arguments, rhetorical flourishes, power plays, social imperatives, and institutional clout that make them possible. Rather than smooth and continuous, the history of science is a ragbag set of competing and fractured local practices and interests. This is not to impugn the creativity of individual scientists, only to contend, as Latour and Woolgar (1979: 31) put it, 'that the precise nature of this creativity is misunderstood'.

Such a perspective motivates the substance of the paper. By drawing upon the science studies literature, my purpose is to begin nudging open the black box of economic geography's early history. Emphasizing the sprawling and unkempt processes that made the discipline, the paper aspires to the account of the woodworm, and not to the Book of Genesis.

The purposes of the paper are therefore twofold. The first is to provide a selective review of science studies. This itself is an increasingly sprawling and unkempt literature. While I identify three main intellectual lineaments within the science studies literature – Mertonian institutionalism, the sociology of scientific knowledge, and cultural studies of science – the review is necessarily partial. The second is to take the ideas reviewed, and to use them to understand the beginnings of the formal institutionalization of economic geography during the late nineteenth and early twentieth centuries. Critical here, as I will argue, is the wider social and economic context of commercial trade and imperialism, and which informs all aspects of the discipline's institutionalization. For the sake of brevity, I concentrate on only two authors and their respective books: the Scottish geographer, George Chisholm, who wrote the first English-language economic geography textbook, *A handbook of commercial geography* (1889); and the American geographer J. Russell Smith, author of the first US college text in economic geography, *Industrial and commercial geography* (1913). While I provide interpretations of these two original texts, the biographical information about the two men is derived from secondary literature. I recognize that archival sources, such as personal letters, diaries, and notebooks, would have been helpful to my task; but the paper is not a study of biography as such, but a case study of the disciplinary origins of economic geography from the perspective of science studies, and which in other contexts has proven useful (for examples, see Hess, 1997).

II Science studies and disciplinary history

Over the last 40 years, ever since the publication of Kuhn's *The structure of scientific revolutions* (1962), there has been much debate and rancour around writing the history and conceptualization of scientific practice, and which has now spilt over into the social

sciences including human geography; see the Driver *et al.* debate (1995) in the *Transactions, Institute of British Geographers* around David Livingstone's book, *The geographical tradition* (1992); and Keith Bassett's (1999) review that takes my own work to task (Barnes, 1996). Key to such discussions are various writings in science studies, a body of literature in which there has been as much internal discord as external (on the former, see Pickering, 1992, Chapters 7–13; and, on the latter, see Sokal and Bricmont, 1999). Following Hess (1997), one can recognize at least three different subtraditions within science studies, all of which to different degrees I will later deploy in this paper: Mertonian institutionalism, the sociology of scientific knowledge (SSK), and an emerging cultural studies of science.² Before I review such works, however, it is necessary to sketch out the model of the traditional history of science against which all three variously react.

1 The traditional model

That model is known variously as 'presentism', or 'Whig history', or, the term that I will use, 'internalism'. It is the idea that disciplinary change is guided by the progressive-working out of some deep-seated, internal principle or idea that becomes fully realized at precisely that point in time at which the author of the history is writing. As a result, disciplinary change takes on two characteristics. First, it is autonomous, that is, there is no need to refer to external factors. Only the postulated internal principle is important, the logic of which once grasped necessarily compels. Those scientists who recognize the principle are deemed rational, and those who don't are not, and their irrationality requires further explanation. Second, it is cumulative. Disciplinary knowledge progressively improves as the guiding principle is realized. Consequently from the vantage point of the present, we can always reconstruct the past to show how its different elements cumulatively come together to produce current success.

For example, in a traditional account of the history of physics, one might interpret disciplinary change in terms of the progressive improvement in mathematical technique (the guiding principle): Newton's cosmic formulations were better than Kepler's because the former knew differential calculus, and Einstein's better than Newton's because in addition to calculus he knew non-Euclidean geometry. Similarly, Livingstone (1992: 4–10) provides illustrations of internalist histories within geography. Hartshorne's (1939) *The nature of geography*, for example, makes chorology the unfolding principle which is finally realized in 1930s American geography. In contrast, events and approaches that are outside of such central principles are either dropped from the story altogether, or treated as irrational errors or diversions. Newton's interest in alchemy is either not discussed in the history of physics or represented as a regrettable mental lapse, while Hartshorne crams geographical work that is not chorological into a single chapter, 'Deviations from the course of historical development' (Hartshorne, 1939: Chapter 3).

2 Mertonian institutionalism

Science studies that emerged in the late 1960s and early 1970s offered a very different view of disciplinary history (although there were precedents, in particular, Kuhn, 1962;

Mannheim, 1952; and even Marx's work – see Hess, 1997: Chapter 2). The first of the three variants, Merton's (1973) institutional approach, is closest to the traditional model. Merton was not concerned with challenging the impartiality or progressive nature of scientific knowledge. Rather, his objection to the standard account was that it made no mention of social values and norms that he thought pervaded the scientific community. Scientists were assumed to be asocial automatons in white coats. However, for scientists to get up each day to do the kinds of things that are necessary to keep the project of science going requires, thought Merton, their institutional socialization into a wider system of values. In particular, that value system determines among other things the academic division of labour, whether work is funded or even read, the nature of academic stratification and associated rewards, and the origin of disciplinary change.

Let me note three broad themes within this Mertonian research programme that will be useful below. First, in Merton's (1973: Chapter 13) original formulation, similar norms hold across any scientific community, and would include, for example, a belief in universalism, disinterestedness, and scepticism. In turn, for Merton such norms act as a form of social regulation, solidifying the scientific community and providing it with a guide for action. Later writers – for example, a former student of Merton's, Thomas Gieryn (1983) – suggested that such norms were not so much a description of scientists' general beliefs but rather a prescriptive sociological means to mark the boundaries of science, and to provide scientists with a means to defend their autonomy. Moreover, for Gieryn these prescriptive norms are not necessarily in the interests of all scientists, but typically only that subset that has the power to assign (and benefit) from them.

From this perspective, establishing the norms of a new discipline, such as economic geography, is all about boundary-marking. It is about both inclusion and exclusion. On the one hand, instigators of new disciplines seek to lay down a set of norms about how a discipline is defined, which methods and tools to use, and what kind of subject matter to examine. Those subscribing to these norms will be included as part of the new discipline (and in so doing recognize the foresight and initiative of the instigators). On the other hand, those same norms represent the delineation of a new piece of academic turf, and which necessarily is exclusive. For example, when Götz distinguished economic geography in 1882, he was saying that those continuing to hold to the norms of commercial geography would be excluded from the new discipline of economic geography. Or when Chisholm (1889) upheld the importance of using maps and statistics in economic geographical inquiry he was also setting out criteria for judging who was practicing economic geography, and who was not. In this interpretation, the role of disciplinary norms is less a description of what people actually do, and more a sociological rallying point serving to justify membership, and marking off members from non-members.

Second, during the initial phase of establishing disciplinary norms, there will likely be disagreement, hesitancy, and ambivalence; but, once some kind of consensus emerges, forged, for example, by textbooks like Chisholm's and Smith's, 'cumulative advantage' begins, such that those whose definitions were adopted garner further power and prestige in terms of awards, academic appointments, citations, and membership of important institutions. Specifically, researchers within the Mertonian tradition argue that academic stratification is the consequence of: (i) 'cumulative effects', that is, early academic recognition often leads to the snowballing of an

individual's reputation; (ii) 'halo effects', that is, an individual's reputation is directly related to their institutional affiliation; and (iii) 'Matilda effects', that is, women regardless of institutional affiliation or early success have less status than men. Certainly, with respect to this last point, economic geography until the last two decades has been an almost exclusive male preserve. The upshot is a tendency towards conservatism. Those (men) who already have status accrue further status. That conservatism is buttressed by another factor, 'invisible colleges', which are institutionally sponsored circuits of travel and meetings that allow the 'in group' to come together frequently, and that then helps them to hold on to such status (Crane, 1972).

Finally, there is the flip side of the first theme, which is around the sociological forces of institutional change. Research here stresses that when change occurs it typically originates from outside heartland institutions and relations. Gieryn and Hirsch (1983) use the term 'marginality hypothesis' to suggest that novel work is often generated in peripheral centres precisely because scientists there have less stake in existing paradigms. Subsequently, there has also been research on the kinds of strategies – rhetorical, institutional, sociological – that researchers in peripheral centres deploy in order to mount their 'revolution'. (Taylor's well-known 1976 paper on the quantitative revolution in geography, for example, comes directly out of this Mertonian tradition concerned with the power of rhetoric.)

3 Sociology of scientific knowledge

The second variant of science studies is the sociology of scientific knowledge.³ Its focus is less the sociology of institutions than the very nature of scientific knowledge itself. Such knowledge, it is argued, is socially constructed, a product or artifact of contingent social forces. In this view the social goes all the way down to the very truth claims made by scientists. As Pickering (1992: 1) puts it, 'science [i]s inherently and constitutionally social all the way into its technical core: scientific knowledge ha[s] to be understood as a social product.' Here, unlike in Merton's work, both the impartiality of scientific knowledge and its progressive form is challenged. Let me flesh out these arguments by reviewing two subtraditions of the sociology of scientific knowledge: the oldest, which is the Edinburgh school (or 'strong programme') founded in the early 1970s, and, currently perhaps the best known to geographers, actor-network theory (ANT).

a The Edinburgh school: The Edinburgh school claims that local social interests of scientists enter into the very lineaments of the scientific knowledge produced. Such an argument is prosecuted by means of detailed, historical case studies. For example, Shapin and Schaffer (1985) document the influence of Robert Boyle's royalist English civil war sympathies on his corpuscular theory of matter; McKenzie (1983) relates Karl Pearson's professional, middle-class attachments to the derivation of his r_T regression statistic; and Harding (1986) examines the masculinist sentiments of medical researchers in theorizing the process of egg fertilization by sperm. In each case, social interests do not just bias the research process, they constitute it. Note that this position is not a form of idealism, that reality can be anything we believe it to be. The Edinburgh school believes in reality – how could it not? – but thinks it is theoretically *underdetermined* in scientific theories; that is, the facts of nature by themselves never bring full

theoretical closure. For example, while there are myriad empirical studies that test the gravity model within geography, none prove the thesis of distance-decay in any absolute terms, nor could they. As a number of writers, both within the Edinburgh school and outside of it have shown (most famously in the Duhem-Quine thesis; Harding, 1976), there is always more than one theory that can account for the same set of empirical observations, making any given theory, such as the gravity model, underdetermined (Barnes, 1996: 114). It is then because of such underdetermination that contingent social interests become critical in determining theory choice. As Barnes *et al.* (1996: 28) write, 'the preferred theory is selected because it is salient in the local culture'.

Admitting the presence of local social interests, though, undermines the idea of cumulative progress. If theories cannot be completely determined by the facts, then theories are never fully commensurable because they are always relative to a set of local social interests that cannot be judged on purely rational grounds. As a result, one never knows whether one theory is better – more rational – than another. To use Kuhn's (1962: Chapter 10) well-known analogy, disciplinary history is more like a series of 'Gestalt shifts', with change marked by abrupt disjunctions rather than inevitable progress. For example, in economic geography one might see the discipline as concerned with optimal industrial locations using a theory based upon physical analogies (Warntz, 1957), or concerned with industrial decline using a theory based on a geological one (Massey, 1984); but one cannot see both at the same time because as theories they are incommensurable (Barnes, 1996: 116). Of course, there might be social or political reasons for appealing to one theory or the other, but looking at the economic geographical facts themselves is insufficient in determining which theory is best.

More generally, and following from this example, the Edinburgh school argues that theoretical change proceeds through a process of what they call finitism (Barnes *et al.*, 1996: Chapter 3). This is the idea that new theories typically stem from extending existing concepts in one field to new circumstances; for example, extending gravity theory to spatial economic flows, or the theory of sedimentary rock formation to regional investment patterns. The point is that there is nothing within a theory that limits its use in a different field. Furthermore, whether a theory is extended 'must be understood in terms of the contingent judgments of the scientists involved, and not as a consequence of what the theory "really implies"' (Barnes *et al.*, 1996: 94). It was not inevitable that economic geographers apply, say, theories of gravity to their empirical work. It was a locally contingent event. The consequence is a disciplinary history pitted by local piecemeal endeavours as scientists and economic geographers contingently move from one theoretical extension to another.

b Actor-network theory (ANT): The origins of ANT are primarily associated with a group of French writers: Michel Serres, Michel Callon, and especially Bruno Latour. Initially, Latour's work was influenced by the Edinburgh school (Latour and Woolgar, 1979), but increasingly the two diverged such that David Bloor (1999b: 132), perhaps the leading proponent of the 'strong programme', now calls ANT 'incoherent and indefensible' (see the debate between Bloor, 1999a and 1999b, and Latour, 1999b, in which no quarter is given). Undoubtedly, such animosity is due in part to the extraordinary success of ANT in garnering proponents ('allies') across the social sciences, including in human geography, and which has made it a minor cottage industry (see the actor-network resource site run out of the Sociology Department of Lancaster University and

associated with John Law; www.comp.lancs.ac.uk/sociology/antres.html).

ANT begins with the semiotic insight 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 signs, but in ANT they can be literally anything – human and non-human, tangible and non-tangible, ideas and machines. As such, entities possess no essential meaning. Meaning is always an effect or an outcome of the wider system of relations of which an entity is part. That wider set of relations is called a ‘network’, and taken from the French word *réseau* that was used by Diderot to define entities that blur the Cartesian divide of mind and body (Latour, 1997). Similarly, the use of network is to blur the conventional distinction between society and nature. For ANT is an attempt always to have both, or, more precisely, to argue that society and nature are themselves effects, the products of particular kinds of networks.

To treat an academic discipline as a network, then, is to treat it as the effect of a whole set of relationships among many different kinds of entities, both material and non-material, ‘social’ and ‘natural’. It is not just an abstract body of knowledge but, say, in the case of economic geography, a product drawn together from heterogeneous sources.

The entities that take on meaning within a network are actors or actants. In the vocabulary of ANT, they are ‘enrolled’ into a network through a process called ‘translation’. Translation involves bringing together seemingly quite different entities, say, Newton’s seventeenth-century theory of gravity and empirical measurements of incomes and prices in the mid-twentieth-century USA (Warntz, 1965), and showing that they are able to work together, that they have similar interests. As Callon (1980: 211) writes: ‘Translation involves creating convergences and homologies by relating things that were previously different.’

The ideas of network and translation are critical to ANT, but they are only the beginning points of what now has become a complex, variegated, and self-critical body of literature (for good reviews, see Law, 1997; 1999; Latour, 1999c). Let me note here six points that will be useful in the case study below. (i) Networks are always in the process of becoming, their existence precarious, their stability temporary. (ii) There is nothing outside of networks, including nature and society. Rather, as I suggested above, nature and society are the effects of such networks, and the results of a process Latour (1993) calls ‘purification’. (iii) There is a geography to networks, although typically it is not that of proximity (‘distance decay’). Instead, to use Serres’s term, it is one of ‘crumpled space’, that is, where relations (translations) are forged between entities separated sometimes by massive distances. For example, Latour (1997) says: ‘I can be one metre away from someone in the next telephone booth, and be nevertheless more closely connected to my mother 6000 miles away.’ That is, the strongest ties in a network are not necessarily geographically the closest. (iv) Forging those connections and extending the network require particular forms of inscription that will ‘travel’ of which perhaps the best example is the book, an ‘immutable mobile’ in Latour’s (1987) lexicon. That is, passing the message in order to make more allies – the name of the game in ANT – is facilitated by books because they are easily transportable (‘mobile’), and in the process of being transported their message is not physically corrupted (‘immutable’). (v) The ‘truths’ emerging from such networks are not universal but local to the network itself. As Latour (1997) writes: ‘Instead of starting from universal laws – social or natural – and to take local contingencies as so many queer particularities that should be either eliminated or protected, [ANT] starts from irreducible, incommensurable, unconnected

localities, which then, at a great price, sometimes end in provisionally commensurable connections.' Finally, (vi) holding a network together is power. 'Power is what makes [networks] what they are, and what eventually is responsible for their collapse' (Brown and Capdevila, 1999: 38). As in Foucault's work, it is not sovereign power, of some overarching body ruling and directing, but a decentered power, relational, capillary-like, and diffuse, affecting objects as much as humans.

4 Cultural studies of science

The last variant of science studies, and most recent, is a 'heterogeneous body of scholarship in history, philosophy, sociology, anthropology, feminist theory, and literary criticism' (Rouse, 1996: 238), and known as the cultural studies of science. As a movement it is difficult to summarize, but it is typically less interested in the particular practices of individual scientists as in the wider cultural and especially political meanings upon which they draw as well as perpetuate. How, for example, do race, class, gender, and colonialism enter into the work of scientists? In this sense, there is a critical edge to cultural studies of science not found in the other two subtypes (although in 'second-generation' ANT that critical sensibility becomes more apparent; Law, 1997).

There are now a number of writers within this subgenre, many of whom provide detailed historical accounts of the relationship between culture and science (such as Poovey, 1998, on numbers and statistics, or Gouk, 1999, on music and science). Perhaps the best known of the group, although not a historian, is Donna Haraway (1989; 1991; 1997), and it is on her work that I will focus, because it emphasizes the importance of the visual both as a metaphor in acquiring knowledge and literally as figures on a page, such as maps and diagrams, and which have been a mainstay of economic geography. Trained in zoology, Haraway's initial work showed the ways in which traditional theories of primatology were deeply racist and sexist, a situation that only began to change when women started entering the field bringing women's perspectives, feminist theory, and gender issues. As a consequence of that transformation, knowledge within primatology itself changed. In making that connection between social change and epistemological change, Haraway makes use of the term 'situated knowledge'. To understand what she means here, it is useful to contrast it to the traditional account of acquiring scientific knowledge. For her, standard scientific accounts are based upon a visual metaphor: scientists *see* the world and write down its truths. However, and this is the rub, in so doing they write themselves out of their own stories. Their role is solely a 'modest witness' (Haraway, 1997: Chapter 1). That presumption of modesty, Haraway argues, is a direct consequence of the starting point of visualization. It creates the illusory possibility of a disembodied observer. She calls this illusion a 'God trick', the idea that it is possible to have 'vision from everywhere and nowhere' (Haraway, 1991: 191). Moreover, it is just such a trick that is the basis of one of science's most cherished ideas, objectivity, the belief in the possibility of a single, final, detached, and unblemished depiction of the world. For Haraway, however, the 'gaze from nowhere' (Haraway, 1991: 188), as she calls objectivity, is really a kind of front that hides and protects the interests of those who propose and most benefit from it. As Haraway (1997: 23) writes, 'modesty pays off . . . in the coin of epistemological and social power.' It is in this sense that being a modest witness turns out not to be very modest at all.

More recently, and this bears significantly on the history of economic geography, she extends this argument about visualization, and attendant notions of objectivity, by arguing that it is also manifest on the page itself as diagrams, figures, representational maps, flow charts, graphs, and so on (Barnes, 1998). Such figures appear to be the objective disclosure of things as they are; but following her argument this cannot be so. Rather, they represent, to use her language, 'fetishization'. Fetishization occurs when the social processes that actually produce such figures are hidden; they are made to appear as a thing, as the object itself. As she writes, fetishism in the form of maps and figures are 'interesting mistakes – really denials – where a fixed thing substitutes for the doings of power-differentiated lively beings on which and on whom, in my view, everything actually depends' (Haraway, 1997: 135).

In contrast, beginning with situated knowledge is to recognize from the outset the role of 'power-differentiated lively beings' in the construction of knowledge. For Haraway, knowledge is not the view from nowhere, but always the view from somewhere: that is, it is situated. Moreover, it is situated in different ways: certainly in wider social and political contexts, but also in particular kinds of bodies, in particular kinds of places – laboratories, university offices, field sites – and in particular kinds of equipment and machines.

In sum, the field of science studies is now a substantial, varied, and interdisciplinary body of literature. Economic geographers are already making use especially of ANT (Leyshon and Thrift, 1997: Chapters 9 and 10; Murdoch, 1995; 1997; Henry and Pinch, 1999), and there is also some return traffic with some science studies writers now discussing the economy (Callon, 1998; Law, 2000; McKenzie, 2000). For my purposes, science studies is ideal for addressing the kinds of questions posed in this paper around the history of economic geography. It insists that academic disciplines are neither more nor less than forms of contingent social practice, and should be assessed accordingly. The contrast is with traditional accounts of knowledge that operate with notions of universalism and universal knowledge. Here local conditions are unimportant because 'truth' necessarily transcends them; as accounts, they offer 'a God's eye view' (Haraway, 1991: 193). Science studies, however, insists that disciplinary knowledge is not the result of divine vision, but of mundane situated practice (Shapin, 1994: Chapter 1; 1998: 5). In Julian Barnes's terms, it is knowledge of the woodworm.

III The institutionalization of economic geography, imperialism, and world commerce

Much has been made, and rightly so, of the links during the late nineteenth century between the institutional emergence of geography within the university and European imperialism. As Hudson (1977: 12) says, geography was 'vigorously promoted [from 1870 onwards] . . . largely, if not mainly, to serve the interests of imperialism in its various aspects including territorial acquisition, economic exploitation, militarism and the practice of class and race domination.'

In particular, recent histories of geography, including Hudson's own, have stressed the relationship between imperialism and environmental determinism that was espoused by a number of geographers during the late nineteenth and early twentieth centuries (Hudson, 1977; Peet, 1985; Smith, 1989; Livingstone, 1994). In the USA, envi-

ronmental determinism was best associated with Ellsworth Huntington and Ellen Churchill Semple (who was also the first American geographer to use the term 'economic geography' in print; Semple, 1900), and in Britain with a school of 'climatic moral economy' (Livingstone, 1994). In brief, environmental determinism, which was often combined with social Darwinism (Livingstone, 1992: Chapters 6 and 7), justified imperialism on the grounds that if, because of the prevailing environmental and climatic regime, an indigenous population could not exploit its economic geographical resources then it was 'the white man's burden' to do it for them.

While this is an important link, I will argue that environmental determinism never played a significant role in shaping early economic geography. Proponents tended towards technological determinism rather than environmental determinism. Furthermore, in so far as there was a relationship with environmental determinism it was not the functional relation suggested by Hudson in which economic geography arose as a consequence of the needs of the colonial project. In the remainder of the paper, I will concentrate on only economic geography, move away from Hudson's functionalism, and draw instead on some of the methodological strategies of science studies described above. The problem is that functionalism tends to dehistoricize and generalize, making the narrative overly neat and tidy. Following science studies, however, what is required is a historicization that emphasizes 'situated messiness' (Livingstone, 1992: 27).

That messiness begins with the very term 'economic geography', coined by the German geographer Götz, a former student of Ratzel, who in 1882 distinguished between economic and commercial geography (Sapper, 1931: 626–27; Warntz, 1959; Buchanan, 1973). For Götz, commercial geography 'chiefly served practical ends' (Götz quoted in Sapper, 1931: 627), that is, it was concerned with 'the accumulation of facts for utilitarian purposes' (Buchanan, 1973: 25). In contrast, the purpose of economic geography was 'the scientific task of dealing with the nature of world areas in their direct influence upon the production of commodities and the movement of goods' (Götz, quoted in Sapper, 1931: 627). Economic geography was to be an academic enquiry.

Yet launching a new academic discipline requires much hard work. It is not enough simply to have the idea as Götz did, but it must be put into practice by mobilizing institutional support, and a diverse array of resources, both human and non-human. This does not occur everywhere in a single blinding moment, but, as science studies suggests, in particular places, occurring hesitantly and untidily over time. Following Latour (1997), the entity that becomes economic geography begins in 'irreducible, incommensurable, unconnected localities' such as in Götz's Leipzig, or Chisholm's Edinburgh or J. Russell Smith's Philadelphia, which then, through the dint of hard labour, good rhetoric, propitious social and academic contexts, and useful machines and devices, produce 'provisionally commensurable connections'. Tracing out such connections is the purpose of the remaining part of this paper.

1 George G. Chisholm

That early economic geographers were not necessarily environmental determinists is clear in the work of George G. Chisholm (1850–1930), author of the first English

language economic geography textbook, *A handbook of commercial geography*, published in 1889. In fact, when Ellsworth Huntington contacted Chisholm to ask him to rank various peoples on the basis of their level of civilization for his now infamous cartographic renderings of environmental determinism (Huntington, 1924), Chisholm refused on the grounds that he had 'a peculiar incapacity for forming [such] judgments' (quoted in Livingstone, 1994: 143).

Of course, Chisholm was still implicated in imperialism – science studies would say it was unavoidable – but his relationship to it was messy and contingent. Born in Scotland and attending Edinburgh University, he later moved to London, becoming a member of the Royal Geographical Society (RGS) in 1884, and the Royal Statistical Society in 1886. Primarily making a living from writing and editing geographical textbooks, gazetteers, and atlases – he previously worked for the Edinburgh publisher W.G. Blaikie and Son on such projects as the *Imperial dictionary* before moving to London – he later supplemented his income by lecturing on commercial geography from 1896 onwards at the University of London's Birkbeck College (Wise, 1975; MacLean, 1988). The *Handbook*, which went to 10 editions under Chisholm, and from 1928 was edited by Sir Dudley Stamp, contains everything anyone might want to know about world commodity production and the geographical conditions for trade. Divided into four sections, the first sets out general factors influencing production, distribution, and exchange; the second provides detailed case studies of particular commodities (mainly primary goods); the third is a regional economic geography of countries organized by continent; and the fourth is a long statistical appendix.

Before commenting on the substantive content of Chisholm's book, one should recognize the importance of the volume as an 'immutable mobile' for the embryonic beginnings of the discipline. To anyone who doubted the new discipline, one could show its substance, literally, by pointing to Chisholm's book (it is over 500 pages long). Economic geography was not merely an idea, but in the form of Chisholm's book it was something tangible that could be held in the hand. Once published and in circulation, it was a means to gather new allies, whether they be students, university academic boards, or politicians. It became a point of passage, offering the possibility of translations, of drawing together entities that formerly seemed irreconcilable.

As for the book itself, it defined economic geography above all as an empirical science. It is true that Chisholm (1889: iii–iv) claims in the Preface 'to import an intellectual interest [to his inquiry] . . . and not to encumber the book with a multitude of minute facts', but that interest is difficult to find, whereas minute facts litter every page. The *Handbook* contains neither an explicit theoretical statement nor an organizational justification, but does display a 'marvelously meticulous mastery of detail' (quoted in MacLean, 1988: 25). Indeed, in one hyperbolic moment, Chisholm was tempted 'to wish . . . th[e] love of pure theory to the devil' (quoted in Wise, 1975: 2; the context was Chisholm's reading in German of Alfred Weber's 1909 *Über den Standort der Industrien*.) In another, albeit less hyperbolic moment, he said, 'if . . . there is some drudgery in the learning of geography, I see no harm in it' (quoted in MacLean, 1988: 25).

In 1908, Chisholm left London to become a lecturer in geography at Edinburgh (the first such position in Scotland), and it was there that he wrote several programmatic statements for the new discipline of economic geography (Chisholm, 1908a; 1908b). Handicapped by a sometimes circuitous writing style, Chisholm (1908a: 568–69) wrote in one long, unpunctuated sentence:

I would say then that it is the function of geography with respect to any class of phenomena that have a local distribution to explain that distribution in so far as it can be explained by variations connected with place in the operation of causes whose operation varies according to locality or according to the relation of one locality to another.

In spite of its obliqueness, Chisholm's statement unquestioningly points to the importance of local, place-based factors and their interrelationship in determining the geography of economic activities. It is neither the environmental determinism of Huntington and Semple, nor climatic moral economy. None the less, important connections between Chisholm's work and imperialism remain. That is, Chisholm's work is necessarily 'situated', reflecting his 'local culture'.

First, and most directly, the *Handbook* was 'situated' at the conjunction of a set of important economic and political conditions: the heyday of imperial British commerce, the technology that made it possible, and a global trading system with Britain at its centre (see Hirst and Thompson, 1997: Chapter 2). The late nineteenth century, when Chisholm wrote the *Handbook*, was a period in which Britain was the workshop of the world, 'heavily dependent upon international trade and at the hub of a world-wide empire' (MacLean, 1988: 21). In part this was made possible by 'improvements in the means of transport and communication' – 'the wants of any district can be communicated at once to the ends of the earth' (Chisholm, 1889: 47) – but also in part because of free trade, and a *Pax Britannica* that ruled the waves. Those local conditions, as suggested by the 'strong programme', enter into the very lineaments of the book, framing the kinds of questions posed around the geography of production and exchange, the kinds of answers given in terms of place-specific factors and technology, the kinds of statistical tables compiled of import and export figures, and the kinds of maps displayed of trade routes and regional commodity specialization. Chisholm's historical and geographical location provided the impetus and organization for his work; it was not just local colour.

Yet Chisholm was not only responding to the economic and political conditions of his time, he was also engaged in the sociological task of carving out a new disciplinary space for economic geography. His argument was that economic geography is so important because trade, which underlay the greatness of Great Britain, is above all a geographical phenomenon. In terms of ANT, he was attempting a new translation: he was making the case that commerce and trade that previously was the exclusive domain of economists like Adam Smith, or David Ricardo, or J.S. Mill, could be usefully framed ('translated') into a geographical perspective. Chapter 1 begins, 'The great geographical fact on which commerce depends is that different parts of the world yield different products, or furnish products under unequally favourable conditions' (Chisholm, 1889: 1). Important here is the resonance between Chisholm's statement and especially the Ricardian doctrine of comparative advantage, used to justify free trade (and at the time vigorously promulgated in Britain by the Manchester School of economists). In Ricardian theory trade only occurs if different places are differentially efficient in producing different goods: if not, there is no economic incentive to exchange. Chisholm's point is that the very basis of that differential efficiency is geography. It is 'the great geographical fact'. To celebrate trade, therefore, is simultaneously to celebrate economic geography; that is, to argue for its inclusion in a body of work, economics, that in the past ignored it.

A second connection is that Chisholm's book by its thorough use of maps, figures,

and tables of numbers, made visible, and therefore knowable, the complex filaments of the imperial project. For example, his map of South Asia (Figure 1), consisting of a series of commodity types written onto particular locations of the map, both fetishizes space in Haraway's definition, because it presents the region as only a cornucopia of commodity production without any sense of social process, and presents the region as fully surveyed and known (Asad, 1994). A similar point is made by Raymond Williams (1982: 170): 'It is very striking that the classic technique devised in response to the impossibility of understanding contemporary society from experience, the statistical model of analysis, had its precise origins within the [nineteenth century]. For without [that model] . . . the society that was emerging . . . was literally unknowable'. Williams was thinking only of knowing British industrial society, but the problem becomes even more difficult when one tries to know the whole world. Chisholm's solution is to extend that statistical approach to the higher geographical scale. This also speaks to the mundane quality of scientific research emphasized by science studies. One can hardly imagine a more mundane task than Chisholm's of assembling massive amounts of data in tabular form, searching down every last fact, and constructing detailed maps (drawn not by him, though, but by F.S. Weller, a Fellow of the RGS). Yet these mundane acts helped contribute to the reproduction of imperialism, establishing its meaning and justifying its end. They give the impression of Chisholm as a 'modest witness', as someone dispassionately recording the changing world around him; but, as Haraway argues, this is an inadequate depiction. Chisholm's work, whether he meant it or not, was one of the constituent elements of imperialism. Rather than a 'God's eye view', his maps and tables helped produce and legitimate the very system he was describing. This is not to say that they were wrong, only that they possessed effectivity, and were not simply inert, mirror reflections.

Such effectivity is also clear in the final connection, which is around education. John Willinsky (1998) argues that in all kinds of different ways the colonial project is patterned on an educational model: bringing back objects and information from 'field trips', writing up results and making displays, and teaching the 'uneducated'. Chisholm begins his Preface by writing, 'This book is designed to meet a want recognized by all who are interested in adapting our education to the needs of the time' (Chisholm, 1889: iii). In particular, he wants to teach 'those entering on commercial life' (p. iii), to make them better prepared and competitive in their business activities by furnishing them with the right knowledge.

Two contextual factors were especially important to this project. First, there was the commercial challenge Britain faced from Germany and its empire. Chisholm believed that one reason for the increasing economic success of Germany was the geographical educational prowess of its business class. Earlier commercial geographical books by Karl Andree (*Geographie des Welthandels*, 1867) and Karl Scherzer (*Wirtschaftliches Leben der Völker*, 1885) were especially important, he thought, in giving the Germans the edge. In fact, he acknowledges the influence of both seminal volumes on his own text, and which can be seen as his attempt to swing the educational pendulum back in Britain's favour (Chisholm, 1889: vi–vii).

Second, a Royal Commission headed by Scott Keltie, and spurred in part by the RGS, reported in 1885 on the poor state of geographical education in Britain (which in part arose as an issue because of the 1870 Education Act that established locally elected school boards that could compel school attendance of children up to age 13). As an

Revised p. 322 and 323.

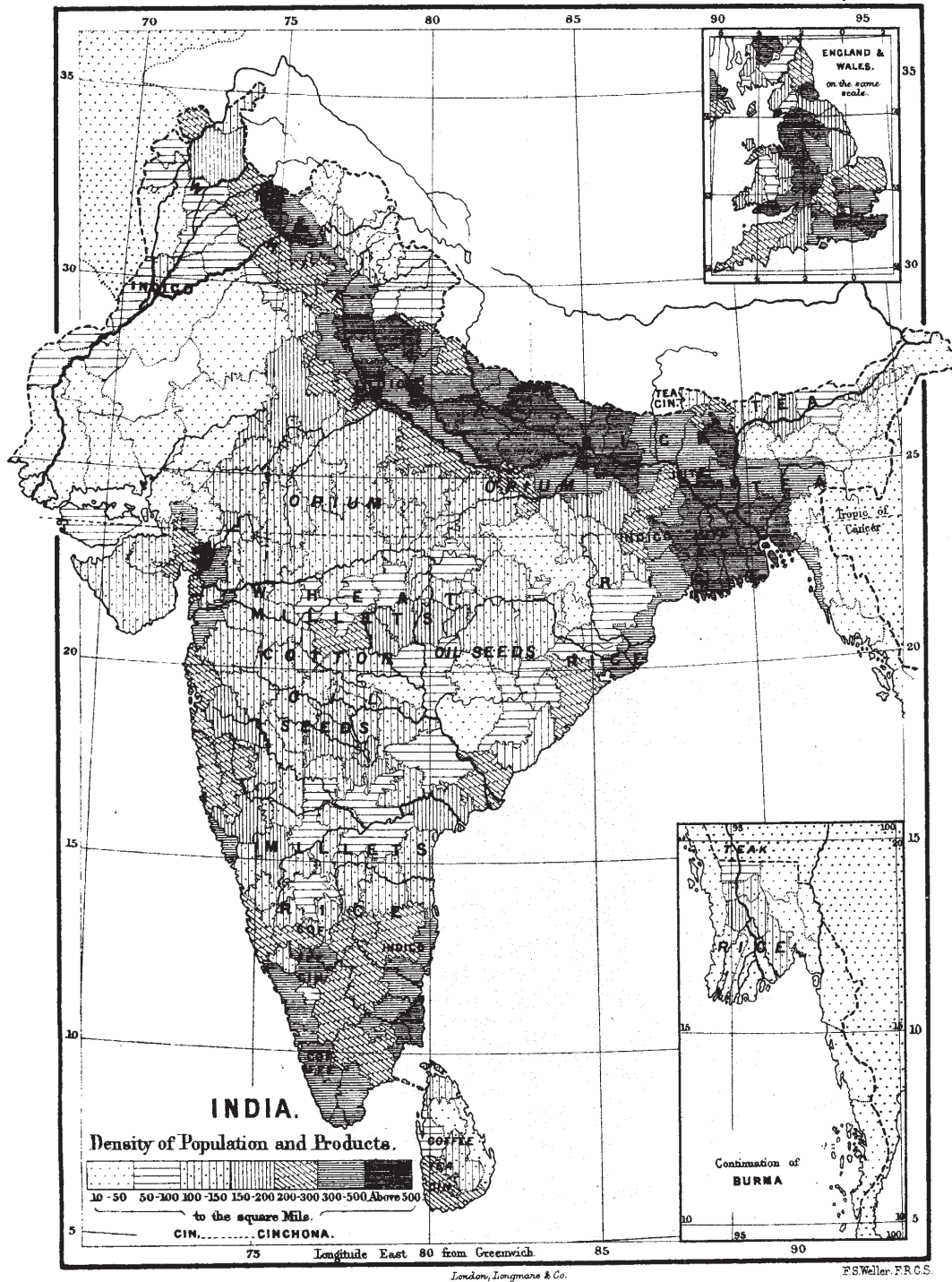


Figure 1 Map of South Asia taken from Chisholm (1889: between pages 322 and 323)

educational field, however, geography lacked qualified teachers, suitable textbooks, and even a curriculum (MacLean, 1988: 23). In this sense, Chisholm's book was exactly the right one for the right time and place, providing a curricular template, a source of detailed information, and something to be displayed both to students and to school governors and inspectors. Chisholm was speaking not only to schoolchildren in his texts, atlases, and gazetteers, however, but also to an emerging community of academic geographers. His work became part of the broader process of institutionalizing geography as a discipline especially after his appointment at Edinburgh University. That is, both Chisholm and his book were vital to the sociological process of creating a new discipline and new practices, as well as maintaining them.

It is precisely these kinds of issues that are central to Merton's institutionalism. Chisholm was a tireless organizer and promoter of geography at all education levels. Apart from his publications and his lectures to teachers, he was active in the RGS writing part of the *RGS Syllabus of Instruction* (1903), and corresponding with Keltie. Indeed, he received the Gill Memorial Prize from the RGS in 1902 for his work in the promotion of geographical education (MacLean, 1988: 25). Also in accordance with his institutional role was his participation in two of the first international 'invisible colleges' held within geography. There were field trips organized by the Harvard geographer William M. Davis first, in Europe in 1911, and the following year a Transcontinental Excursion across the United States to mark the 60th anniversary of the founding of the American Geographical Society. The point is that economic geography, as well as the wider field, did not somehow emerge fully formed from the ether, but required institutional instantiation, associated with the kinds of social norms, rewards, and forms of regulation that Merton recognized, and which Chisholm in his academic career embodied.

In sum, the *Handbook* was not an automatic response to the Siren calls of imperialism, but following science studies reflected a peculiar set of local and contingent circumstances: Chisholm's early work on maps and gazetteers at Blaikie's, his work as an extension lecturer and later hiring at Edinburgh, the zenith of British empire and commerce, the passing of the Education Act and the subsequent market for textbooks, the beginning of the institutionalization of geography as a discipline, the Victorian predilection for numbers, and so on. The colonial project was certainly helped on its way by Chisholm's work, but it was not there working behind his back ensuring that he wrote only the right words or drew only the right maps. It is also interesting that Chisholm (1921: 186) later in his life made various pleas against the exploitation of colonial labour. One biographer, MacLean (1988: 29), even claims that Chisholm was one of 'the pioneers of Third World Studies'. While this seems an exaggeration, it indicates another of science studies' emphases, the messiness and sometimes contradictory character of scientific practice. Imperialism could not simply call forth actors such as Chisholm to provide instant legitimization. It was always more complicated.

2 J. Russell Smith

Similar complications are found on the other side of the Atlantic. Here, as Fellmann (1986) argues, the inspiration for economic geography originated not in geography but in economics. Furthermore, the speed of introduction was rapid. Between 1895 and

1905, 16 out of a sample of 27 US universities introduced economic or commercial geography courses into their curriculum (Fellmann, 1986: 317). Such was its momentum that by the first world war, according to Fellmann (1986: 320), ‘... economic geography had clearly ousted Davisian physiography ... as the orientating viewpoint of the discipline.’

For Fellmann (1986), US economic geography developed not as a justification for imperialism but because of dissatisfaction by American economists with the abstractness of English classical and neoclassical economics. As a reaction, many turned to the German historical school of Gustav Schmoller and Werner Sombart that emphasized context and concrete detail (from a 1908 survey of 116 American economists, 59 had studied in Germany; Fellmann, 1986: 316). As the economist Edward Van Dyke Robinson explained (1909: 249; and quoted in Fellmann, 1986: 316): ‘After the Historical School of economists had introduced the idea of relativity as to time and place, the necessity was apparent not only for a historical but also for a regional treatment for economic phenomena – in other words for economic history and economic geography.’

While Fellmann’s emphasis on the German historical school is important, there were also more material factors at work in promoting economic geography. In particular, Neil Smith (unpublished) recognizes a ‘scale shift in the expression of US national interests and power – from the national to the international scale – ... in the years between 1898–1914’ (which were precisely the ones in which US economic geography emerged). Couched in terms of the ‘imperatives of expansion’, the USA increasingly looked outward as it actively ‘spread the American dream’ (Rosenberg, 1982) over the globe through trade and foreign investment. For example, between 1898 and 1914, US exports increased threefold to \$2.3 billion.

The consequences were many, but certainly one of them was the teaching of economic geography, which, as we shall see in J. Russell Smith’s book, perfectly captured America’s expansionist impulse. Further, given the German historical intellectual predilections of economists, and the physical-based predilections of geographers, such as Davis at Harvard, economic geography courses were initially taught in economics departments and business schools. An early site was the Wharton School at the University of Pennsylvania, and in particular the work of the transportation economist Emory R. Johnson. Offering a range of geographical courses at the School from 1894 onwards, Johnson was seconded to the Isthmian Panama Canal Commission by the US Congress in 1899, and given the responsibility of predicting the Canal’s traffic in 1915 (the year following its completion). He hired as his assistant J. Russell Smith (1874–1966), who later undertook a PhD under him (‘The organization of ocean commerce’, 1903). Spending a year (1901–1902) in Leipzig ostensibly studying with Ratzel and Sapper, but in reality more interested in studying European trade and shipping (Rowley, 1964: 24), Smith was hired by Johnson in 1903 to replace J. Paul Goode who left to teach at the newly created Department of Geography, University of Chicago. Founding the Department of Geography and Industry at the Wharton School in 1906, Smith quickly cemented his own reputation as well as that of the School’s as a center for US economic geography by publishing *Industrial and commercial geography* in 1913. It was both a new and improved as well as an American version of Chisholm’s *Handbook* (Starkey, 1967: 200)

In many ways, the same points I made about Chisholm’s book that I took from science studies apply to Smith’s. It was an important ‘immutable mobile’; it was related

to the wider social context of commercial expansion (the 'American Empire' as Brooks Adams called it; Smith, 2000: 202); it made heavy use of statistics, tables, maps, and, not found in Chisholm's volume, photographs; and it was intimately connected to a wider institutional educational project (and which may have been Smith's greatest contribution; see Rowley's detailed history, 1964).

Divided into two sections that correspond to the terms of its title, the first (and longest) part of *Industrial and commercial geography* discusses the production of particular resources and manufactured goods, and the second part examines world trade. Throughout there is a greater sense of dynamism and movement than in Chisholm's work. Smith is in awe of the changes ushered in by the steamboat, the railway, the telegraph, and the telephone. They release people from the tyranny of nature, and create a 'world market' (Smith, 1913: 16).

For Smith the most interesting aspects of the new 'world market' are the two separate but related geographies of 'management' and 'staple commodities' (Smith, 1913: 867). Management is located in one 'mere corner of the world' (Smith, 1913: 874), northwestern Europe and the northeastern seaboard of the USA. As regions, they have 'capital to spare' (p. 874), and also perform a 'transactional' (p. 869) role in being 'place[s] where bargains are made for goods that are [produced] elsewhere and which may never be brought to the center' (Smith, 1913: 867). Key to success is technological innovation: 'the telephone, the telegraph and the fast mail have helped to keep up the transaction center by putting the selling agent in easy communication with the factories and local centers of the producing and consuming districts in all parts of the world' (Smith, 1913: 869).

With the geographical core so defined, the geography of staples production follows. 'The most important class of commodities is tropic and subtropic foods . . . In exchange for these, the northern lands are sending machinery, clothing, and all kinds of manufactures and some foods. This is a natural trade' (Smith, 1913: 681).

Apart from the similarities to Chisholm's book, both methodological and substantive, there were differences, or at least different strengths and weakness. One of the strengths is Smith's rhetoric. By rhetoric, I mean the use of language to persuade, which is an important theme that runs through the different variants of science studies. It is found in Merton's arguments about why social norms are compelling, in ANT's thesis about how allies are won over, and in Haraway's contention about the persuasiveness of the 'gaze from nowhere'. In each of these different versions of science studies, there is the supposition that no foolproof, cast-iron proof is available to make the case, and so rhetoric becomes critical. Chisholm's rhetoric is based upon convincing the reader of the importance of commercial trade by overwhelming them with the facts. Smith's rhetoric is subtler. There are certainly the facts, but they are held together, and made convincing, by a strong storyline derived from two older mythic narratives: Adam Smith's fable of a barter economy, and the classical tale of Prometheus. Adam Smith's story is important because it naturalizes exchange. Whenever one place has a surplus of one good, and another place a surplus of a different good, trade will naturally unfold between them. In the past technological limitations have geographically confined the potential for trade, thereby never excising the possibility of famine or dearth. Yet this has changed. *Industrial and commercial geography* consistently makes the argument that, in the twentieth century, Promethean impulses are at last unshackled, breaking loose the old geographical constraints on trade. Innovations in transportation and communi-

cation now allow for global as opposed to local exchange, and, as a result, nature's niggardliness is circumscribed.

The combination of Smith's mythic narratives, his stress on the global geography of production and trade, his maps and photographs, his emphasis on transportation and communication innovations, all produced a very different conception of geography and geographical practices than any of the others on offer in the US academy. It was radically different from William Davis's 'physiography' carried out at Harvard (Livingstone, 1992: 202–15). In fact, for 20 years members of the two schools barely spoke (James and Martin, 1978: 20). For Smith, humans came first, not the physical environment: 'the primary interest is humanity rather than parts of the earth's surface' (Smith, 1913: v). Neither was it equivalent to the environmental determinism of Huntington and Semple. Admittedly, there is a section in the first chapter of the first edition of his book, 'Influence of climate in the origin of civilization' (Smith, 1913: 5–11), that is as extreme in its environmental determinism as anything Huntington wrote on his worst days ('environment makes the race', Smith bluntly wrote, 1913: 4). In fact, when Huntington wrote to Smith for help in ranking world civilizations, as he had done to Chisholm, Smith gleefully agreed, 'taking a half day off to sit in judgment upon the world' (quoted in Livingstone, 1994: 143). Beyond this introductory subsection, however, the environment plays little causal role: the pulse of 'civilization' is technology not climate. Finally, as indicated, the book is not as encyclopedic as Chisholm's tome. A colleague of Smith's at Wharton, W.S. Tower, had complained in a veiled reference to Chisholm's book that 'in economic geography, an enormous body of facts is easily available for discussion . . . but [there] is little in the way of principles which have been deduced from those facts' (Roorbach, 1914: 810). Smith's volume was a first-cut attempt at deducing such principles.

As with Chisholm's *Handbook*, the writing and success of Smith's *Industrial and commercial geography* cannot be explained by a simple functionalist argument about the needs of colonialism, or even global capitalism. Rather, its origins and achievements are multiple, reflecting its 'situated messiness', and hence requiring an approach such as science studies that balks at any simple reductionism, and remains sensitive to local context. There is the importance of the historical school of economists and their appreciation of context that made discussions of economic geography possible. This was significant not only intellectually but also sociologically within the university. In Merton's terms, it gave normative validation to economic and commercial geography courses that never existed before. Moreover, this development occurred at exactly the same time that geography was being institutionalized within US universities. As a consequence, when by the 1910s the influence of the historical school began to wane as economics waxed analytical, and as a result economic geography courses were dropped by departments of economics, they were picked up by newly formed geography departments, including Smith's own (see Table 2 in Fellmann, 1986: 319). Because of the status that they possessed as courses in economics departments, they had some cache and substance when transferred over to geography. Furthermore, given this timing, economic geography was there from the very beginning of American geography; it contributed to its very institutionalization as a discipline.

That institutionalization was further propelled by Smith's pivotal role as an educator, and more generally a proselytizer. For Smith's influence was not confined to the students he taught while at Wharton, and later at Columbia from 1919. He was, like

Chisholm, a dry lecturer, and exacerbated by 'a slight lisp and a low, monotonous way of speaking' (Rowley, 1964: 87). Yet he shone as a writer. His book was enormously successful, garnering compliments from the likes of Charles Colby and Isaiah Bowman even into the 1930s (Rowley, 1964: 50–51). More broadly, as Starkey (1967: 198) writes, his 'textbooks . . . led to the introduction of geography courses in American colleges and universities.' Like Chisholm's *Handbook*, then, Smith's immutable mobile was more than just a student text to be read to pass final exams. It became an active component in the very invention and maintenance of US economic geography as a discipline (and again like Chisholm, recognized by the award of medals and other honours; Rowley, 1964: 187).

IV Conclusion

Julian Barnes (1989) concludes his history of the world with a story set in heaven. At first, the story's protagonist cannot believe his good fortune in being there, but eventually he is worn down by heaven's perfection. Shooting 18-stroke rounds of golf every day, watching his favourite team Leicester City win the FA Cup every year, and having sturgeon and chips washed down by a magnum of champagne for dinner every night, leaves the protagonist tired of eternity, and fed up with flawlessness.

The science studies literature on which I draw in this paper is similarly suspicious of perfectibility, that marks internalist histories of science. Internalist histories suggest that driving forward any discipline is some core principle of rationality that eventually becomes fully realized, at which point there are no more methodological disputes, and no more problems. In contrast, the three variants of science studies that I reviewed contend that science is embedded within complex sets of social relations that variously shape its institutional form, rationale, practice, and knowledge. Because such social relations are always specific to a local context, there is no single, transcendent methodological principle that progressively unfurls to reveal the truth. Instead, science studies argues that science should be conceived as a fractured, contested, and untidy enterprise, and always relative to the geographical and historical context in which it takes place.

The purpose of my paper was to try out the concepts and ideas of science studies by providing a preliminary account of the institutional origins of economic geography. To make the task manageable, I focused on only Chisholm and Smith, and their respective primary texts. Clearly, even within this limited setting, much more could have been made about the contributions of both men, and their relation to economic geography's institutionalization. That said, I hope enough was provided to confirm the broad claims of science studies. The beginnings of economic geography were not associated with some supra-historical, -cultural or -geographical truth, but were local in conception, reflecting the tangled, situated social circumstances of its origin.

Specifically, by focusing on Chisholm and Smith, I delineated some of the elements that constituted the newly institutionalized discipline of economic geography and its world. The discipline was concerned with empirical detail, global geographical categorization based upon commodity specialization, and the spatial patterns and conditions of commercial trade. Economic geography did not have to turn out in the way it did. For example, it could have taken on an abstract theoretical bent (which was the

trajectory of economics), or even taken up socialism (Hepple, 1999, provides an example of one early economic geographer who did). That economic geography did not do so goes to the importance of the situatedness of early economic geographers such as Chisholm and Smith. The context in which they worked was not mere background atmospherics but entered into the very body of the discipline's knowledge. The context of commercial expansionism and imperial control made a difference. As I have also stressed, however, Chisholm and Smith were not mere dupes of that context: there was individual inspiration, along with a lot of perspiration. Starting a new discipline is hard work. Economic geography did not somehow antedate itself, having existed in some shadow form. It required real people to work energetically and with concentrated effort within a specific institutional frame to justify, and then maintain, the new subject. It is out of human practices that disciplines emerge, but always the practices of a specific time and place.

Of course, it would probably be nice to think that there was more than this going on, that economic geography could achieve a God's eye view at least once in a while; but the science studies literature of the last 25 years, on which I have relied, suggests otherwise. This is Barnes's point, too. We are much more like woodworms than Gods and Goddesses, and we need to write our histories accordingly.

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Notes

1. This paper is a first-cut attempt at beginning to write the history of economic geography from the perspective of science studies. For brevity's sake, the paper is concerned with only the beginnings of the discipline. Other papers, however, deal with more recent episodes in the discipline's history, including the influence of the German location school, the role of Harshornian regionalism, and especially the quantitative revolution (Barnes, 2000a; 2000b; 2001a; 2001b).

2. I recognize that there are sometimes sharp disagreements among the proponents of these three subtraditions, but, following Hess (1997), there is also the potential to bring their strengths together, not in the form of a grand synthesis, but as a set of individual arguments that taken together overwhelmingly make the case for the social origins of knowledge.

3. The sociology of scientific knowledge is the variant that is the most diverse of the three. It includes the Edinburgh school and actor-network theory that are discussed in this section, but also the Bath school, discourse analysis, social constructionism, laboratory studies, and a radical reflexive approach that applies the tenets of the sociology of scientific knowledge to itself (for references, see Pickering, 1992). There are overlaps among these different approaches in both substance and proponents, but, as will be clear, also sharp disagreements. Hess (1997: 82) argues that connecting these subvarieties is the common position of 'social constructionism' that he defines very broadly as '... the ways in which social interests, values, history, actions, institutions, networks and so on shape, influence, inform, characterize or co-constitute the content of science and technology.' Even this definition, however, is not broad enough for some. Latour (1993), for example, would deny the 'social' part of social constructionism, seeing society itself as an effect, or product, of other entities that he calls hybrids.

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