

# The place of locational analysis: a selective and interpretive history

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**Abstract:** The paper has two purposes. The first is to provide a selective review of the history of locational analysis as it bears on economic geography. Three periods are examined: the German location school of von Thünen, Weber and Lösch that begins in the first part of the nineteenth century and ends in the middle of the twentieth century; American spatial science that starts in the mid-1950s and is in decline by the late 1970s; and the new economic geography associated with the economist Paul Krugman, and inaugurated by his 1991 book, *Geography and trade*. The second is to make a methodological argument. Locational analysis is most frequently justified in terms of the purity of its logical and mathematical reasoning, permitting some commentators to trace an unbroken line of progress over its 175-year history. I argue that such a claim is based upon acceptance of a broader philosophical position, rationalism – the belief that the foundation of knowledge is reason – which takes its most perfect form in logic and mathematics. Rationalism has been criticized in various ways ever since it first emerged in the Enlightenment, however, and the same critical sensibility informs this paper. Specifically, I criticize the rationalist interpretation of locational analysis by drawing upon a recent interdisciplinary body of literature arguing for the importance of local knowledge, the idea that knowledge, even abstract theoretical knowledge of the kind found in locational analysis, is shaped not by the universal but by the peculiar historical and geographical context of its production. This anti-rationalist argument in favour of local knowledge is exemplified by discussion of the three periods of locational analysis that form the paper's core.

**Key words:** history of economic geography, location theory, science studies.

... science is indelibly marked by the local and the spatial circumstances of its making ...

Steven Shapin (1998: 6)

## I Introduction

This paper has two purposes, reflected in the pun that forms the title. First, by using selected examples, I review the changing historical form and role of locational analysis as it bears on economic geography. The term locational analysis is best associated with

Peter Haggett's (1965) book of the same name, and refers to the logically and mathematically rigorous investigation of the 'spatial arrangement of phenomena . . . and related flow patterns' (Johnston, 2000: 464). Its heyday in economic geography was the 1960s and early 1970s. The roots of locational analysis, however, are much earlier, and associated with the beginning of the German location school inaugurated by von Thünen's 1826 treatise, *The isolated state* (von Thünen, 1966).<sup>1</sup> Recently, though, while economic geographers have become less interested in locational analysis, economists have become more interested. Writers like Paul Krugman (2000) now talk about 'the new economic geography', the defining feature of which is a mathematically recondite examination of spatial arrangements and flows, that is, locational analysis. The first task of this paper is to map the changing place of locational analysis over its 175-year history.

Second, I intend to make a methodological argument about the importance of context – place – to the very development of locational analysis. Through its Latin root *locus*, place already etymologically informs locational analysis. I want to expand and elaborate this connection. Most approaches to locational analysis treat it as if the geographical and historical contexts of its development have no bearing; that locational analysis as a body of axioms, postulates and mathematical theorems shines by its own light. Such a view is known in the philosophy of science literature as internalism, the idea that there is a deep-seated, autonomous and universal principle that guides theoretical development. It is universal in the sense that once grasped it reveals the essential properties of the phenomena investigated. Against such a view, there is the contrary position of externalism, the belief that knowledge, even theoretical knowledge, is intimately related to the local context in which it develops (for a discussion of both internalism and externalism, see Shapin, 1992; and in economic geography, Barnes, 2001a). In the externalist view, the local enters into the very sinews of knowledge produced (Shapin, 1998). In reviewing the history of locational analysis, I will adopt an externalist position and argue that its different components are local constructions, closely tethered to the circumstances of the place and period in which they were devised. It is in this second sense that I am also concerned to understand the place of locational analysis.

The paper is divided into four main sections. The first briefly lays out the history and meaning of the term locational analysis, and reviews its usual justification couched in terms of rationalism, and defined by the use of rigorous logic and mathematical forms of representation. This is the link to internalism. It is assumed that the deep-seated, autonomous and universal principle that internally guides valid theoretical development is rationality. Rationality in the form of logical and mathematical reasoning provides a set of invisible rails permitting progress and advancement. Over the last 25 years, however, rationalism, and the concomitant internalist position, has been criticized in a varied body of work within anthropology (Traweek, 1988; Clifford and Marcus, 1988), feminism (Harding, 1986; Haraway, 1991), human geography (Gregory, 1994; Barnes, 1996) and perhaps most consistently and systematically in the sociology of science (Bloor, 1976; Woolgar, 1988), that in contrast emphasizes the importance of local knowledge. Such work conceives knowledge, even abstract formal theoretical knowledge found in locational analysis, as locally constituted, the result of external factors, and not a single ineluctable internal logic. Indeed, for some, formal logic itself is ' . . . of an entirely local character' (Barnes and Bloor, 1982: 5). The rest of

the paper takes up the argument of the localness of knowledge, and contends that, in spite of its rationalist trappings, locational analysis is a local practice. Examples are drawn from three periods. The second section focuses on the formulations of the German location school of the nineteenth and twentieth centuries, and in particular on the contributions of von Thünen, Weber and Lösch. The third section examines American spatial science during the immediate postwar years. The fourth section reviews specifically the work of the economist Paul Krugman's version of the new economic geography. My argument is that in each of these three different periods the most insistent and persuasive feature is a contingent localness and not an ironclad rationality.

## II Locational analysis, rationalism and local knowledge

### I Locational analysis and rationalism

Although the term appears in his book title, Haggett (1965) does not explicitly define locational analysis; but, from his first chapter, 'Assumptions', it is clear what he has in mind. It is a mathematically and numerically based study and explanation of location and spatial distribution drawing upon the traditions of location theory within economics (Haggett, 1965: 13), and the 'neglected geometrical tradition' of geography (Haggett, 1965: 15–16). These different components come together in the idea of a model, defined as 'an ideal representation of reality in order to demonstrate certain of its properties' (Haggett, 1965: 19). While Haggett recognizes different kinds of models in geography, the substance of his book is concerned with only a single type: one that simplifies the complexities of location and spatial distribution using mathematical forms of representation, and which connects to reality's 'properties' by using analytical techniques.

While it is clear from reviews, including Haggett's own, that the models of locational analysis are historically long-standing, and associated with a range of disciplines and substantive interests, for the purpose of this paper their most interesting characteristic is a common reliance on a mathematical mode of representation and validation (see also Scott, 1976; 2000; Pooler, 1977; Blaug, 1979; Ponsard, 1983; Beckman, 1987; Gertler, 2000). My contention is that this mathematical mode is used to justify locational analysis by appealing to rationalism. Rationalism is a word carrying much baggage (for two broad reviews from very different perspectives, see Chisholm, 1966, and Rorty, 1979). I mean by it the epistemological position that asserts that the foundation of knowledge is reason, and which takes its most perfect form in 'the rigour and certainty associated with the deductive structure of mathematics' (Markie, 1998: 78). It follows that under rationalism '... as we move from one subject to another the subject matter changes, but so long as the nature of the knower's mind remains constant, the same method ... can be used in each case' (Markie, 1998: 78). This is exactly the case in the history of locational analysis. Practitioners move across different substantive material and disciplines, but the rationalist method rooted in mathematics is a constant ensuring certainty, progress and truth.

As a feature, the belief in rationalism defines the three schools of locational analysis on which this paper focuses. First, the German location school, which includes the works of Johann von Thünen in the first half of the nineteenth century and Alfred

Weber's and August Lösch's in the first part of the twentieth century, is relentless in its use of mathematical and numerical methods. Mathematics is used to derive abstract formulations about location, for example, von Thünen's rings of crop patterns, or Weber's location triangles, or Lösch's hexagons of market areas. In the case of Weber, the mathematical derivations were so complex that he enlisted help from the brilliant German mathematician Georg Pick, who later assisted Einstein in formulating his theory of relativity (Blaug, 1979: 28; Alonso, 1990: 26). In addition, all three deployed numerical evidence and methods to test their abstract formulations. Von Thünen kept meticulous numerical records on his own Tellow estate in Mecklenburg recording prices, yields, crop patterns and costs over a nine-year period, using them 'to compute optimal solutions to management problems' (Niehans, 1987: 637). In fact, von Thünen said that the numerical recording and calculations alone 'had proved so colossal that [they] precluded almost all other study' (von Thünen, 1966: xvii). Lösch (1954), on the basis of two Rockefeller travel fellowships, made excursions to the USA before the Second World War in order to gather empirical information about settlement patterns and markets, which he used in the fourth section ('Examples') of his book, *The economics of location*.

Similarly, American spatial science that begins around 1955, associated initially with the Universities of Washington and Iowa, and later Chicago, Northwestern, Michigan and Ohio, was defined by its astuteness and acuity in making use of mathematical formulations (Barnes, 2000). Those formulations were taken from established traditions like the German location school, but also from other sources such as physics and astronomy, where notions of gravity, potential and later entropy and their associated mathematical appurtenances became widely deployed in understanding the location of economic phenomena and their interactions (Pooler, 1977). In addition, this approach was marked by its use of an increasingly sophisticated set of statistical techniques, and the use of computers and big data sets to enable large-scale numerical testing and validation (Gould, 1969). Furthermore, over time economic geographers began formulating their own discipline-specific mathematical approaches to location without relying on others. Leslie Curry (1998) and Michael Dacey (1974) developed innovative and highly mathematical approaches to location analysis based upon stochastic variation, while others, such as Andrew Cliff and Keith Ord (1973), developed original formulations of geographical statistics such as tests for spatial autocorrelation.

More recently, from the early 1990s, Paul Krugman, a peripatetic American economist, has pioneered his own 'new economic geography'. Krugman's work draws upon the German location school, and also overlaps with 1950s and 1960s American spatial science that preceded it, and Isard's (1956) regional science movement that flourished at the same time (for discussions of Krugman's work by geographers, see Martin and Sunley, 1996; Martin, 1999; Sheppard, 2000; and, on the regional science connection, Isserman, 1996). Krugman's argument is that, while German location theorists, economic geographers and regional scientists had good intentions, none satisfactorily dealt with locational analysis because they neither recognized the central problem – to provide an integrated causal model of markets – nor possessed adequate mathematical tools. Fortunately, salvation is at hand. Krugman argues that the invention of the mathematically sophisticated Dixit-Stiglitz model during the 1970s provides just the 'analytical tricks' (Krugman, 1995a: 84) needed to present a rigorous, logically integrated causal model of locational analysis that makes the market central.

Admittedly, Dixit-Stiglitz is ‘completely unrealistic’, but for the economic geographical task at hand it is ‘fabulously useful’ (Krugman, 1995a: 60).

The important point is that, in each of these cases, the validity of location analysis is justified in terms of a rationalist logic. Von Thünen terms his deductive analytical method ‘*Form der Anschauung*’, and saw it as all-important (Binder-Johnson, 1962: 214). He wrote: ‘This method of analysis has illuminated – and solved – so many problems in my life, and appears to me to be capable of such widespread application, that I regard it as the most important matter in all my work’ (von Thünen, 1966: 4). William Garrison (1956: 428) at the University of Washington, who launched the careers of many of the American spatial science ‘space cadets’ in the late 1950s, said in one of the first debates in the discipline about mathematics in 1956 that its use is justified because it is a ‘universal language’. Finally, Krugman justifies his project on the grounds that it provides incisive models of the space economy using the micro-foundation of rational choice theory, and associated mathematical techniques. These analytical tricks and clever models then ensure, as he puts it, that there is a ‘happy ending’ (Krugman, 1995a: 88), which in his world means mathematical tractability. The suggestion, then, is that there is something about the internal logic of rigorous, precise mathematical methods that produces the right outcome and that leads to the right answer. This is the rationalist vision.

## 2 Local knowledge

Against this view of a set of procedures that when used ineluctably lead to the truth is another approach, the idea of local knowledge. A term coined by the English philosopher Gordon Ryle, and popularized by the American anthropologist Clifford Geertz (1983), local knowledge is the notion that first, all knowledge is geographically and historically bounded in terms of its generation, and, second, that the local conditions of its manufacture affect substantively the nature of the knowledge produced. In this context, local refers to the conditions in which knowledge is produced, and not the geographical domain to which knowledge applies. Einstein’s theory of the universe, for example, is a piece of local knowledge, even though the theory’s explanatory province is infinitely large.

Let me discuss in turn the definition’s two component parts. First, knowledge is historically constrained, and *produced* within particular material settings that include, for example, individual geographical sites, particular kinds of human bodies, and even specific building types, machines and equipment (Livingstone, 2002: 7–40). The key word is ‘produced’, which contrasts with the discovery view of knowledge (meaning literally to ‘uncover’). In the discovery approach, knowledge is presumed already to exist, requiring only the correct conditions to be revealed. To say that knowledge is produced, however, suggests something different: that there is an active process of creative construction ‘on site’ according to specific local rules and inputs. For example, Steven Shapin (1994) sets out the local geographical and historical context that intimately informed the work of the seventeenth-century English scientist Robert Boyle. It involves Boyle’s social background – he was son of the Earl of Cork – and concomitant ‘gentlemanly’ status, independent wealth that enabled him to establish his own laboratory and to remunerate various assistants, the availability of trained crafts-

people to construct necessary equipment such as the air pump, and available only in particular places like London or Oxford, and not Dorset where he had his country house, nor Ireland where his father held large amounts of land. Boyle's work, and the famous equation that it eventually produced, was not a bolt of lightning out of the blue, but emerged from a set of closely bounded geographical and historical conditions. It was a piece of local knowledge.

Second, material and historical settings, and corollary social interests, enter into the very lineaments of knowledge itself. Producing knowledge is a social activity. Knowledge does not come from heavenly inspiration, but from engaging in particular kinds of social practices that are historically and geographically variable. Donna Haraway (1991: 95) writes that knowledge does not 'come from above, from nowhere, from simplicity' but from ground level, from somewhere and from complexity (a point central to the wider literature of science studies; Hess, 1997; Barnes, 2001a). Knowledge is irreducibly social, never innocent, and always coloured by the context of its production. Examples within human geography would include: environmental determinism that expressed (and legitimated) the racist and imperialist impulses of late nineteenth-century Europe (Hudson, 1977); regional science that represented the instrumentalist, masculinist and economistic sentiments of a mid-twentieth-century United States engaged in cold war and focused on creating an affluent urban society (Barnes, 1996: 130–36; Mirowski, 1999); and, most recently, the 'cultural turn' that reflects a postmodern reflexive capitalism in which culture and economy are so intertwined that they cannot be parted (Thrift, 1997; Barnett, 1998). My argument in this paper is that the same general argument holds true for locational analysis; it too is fundamentally moulded by the context of its local production, and is not the precipitate of timeless logical truths.

All knowledge, then, is local knowledge, born from specific contexts. The important consequence is that universal claims to truth suggested by rationalism are insupportable. Knowledge claims are made inside the circle of local context, with no means of moving outside for 'a God's eye view', as Haraway (1991: 193) puts it. For this reason, one needs to be sceptical of the usual justification of locational analysis couched precisely in terms of a God's eye view.

One should recognize, though, that admitting the locally constituted nature of knowledge does not confine knowledge as hermetically sealed within its original local context. Knowledge travels. It travels in the form of books, letters and more recently e-mails and attachments, and in the form of the movement of people (on travelling theory, see the classic essay by Edward Said, 1983; for a sociological interpretation, Bruno Latour, 1987: Chapter 6; for a geographical one, Barnes, 2002). Yet allowing for knowledge to travel does not mean that it is universal. As Joseph Rouse (1987: 72) argues, what we think of as universal knowledge is the result of scientists moving 'from one local knowledge to another rather than from universal theories to their particular instantiations'. As I will now argue, this is the case for locational analysis. As a piece of local knowledge, it proceeds hesitantly and provisionally from one particular site and type of practice to another, but it is not based on, nor does it achieve, universal rationality.<sup>2</sup>

### III The German school of location

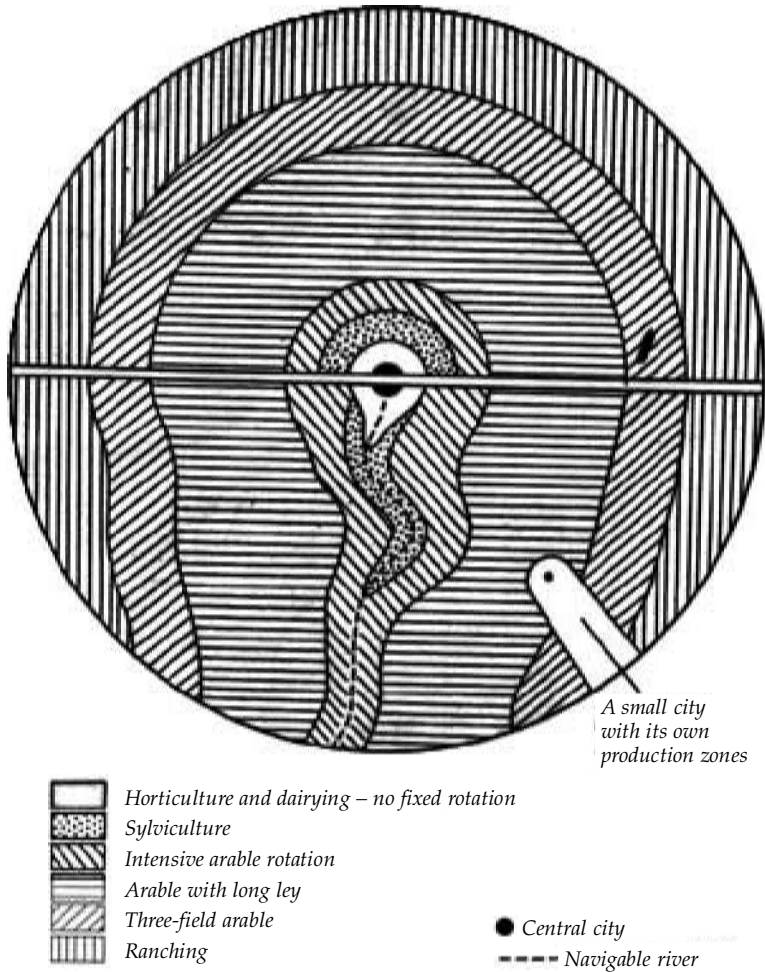
The German school of location theory is both internally variegated and historically elongated (there is more than a 100-year gap between the principal publications of von Thünen and Lösch). Arguably, such a lag undermines the very designation 'school'. That it is seen as a school at all is in part because different contributors are assumed to be pursuing the same rationalist logic. So, the historian of economic thought, Mark Blaug (1979: 26), believes that connecting the different members of the school is a common scientific method that he calls hypothetico-deductive. The regional scientist Claude Ponsard (1983: 127) in his history of locational theory avers that binding the German location school is the tie of 'formalized analysis'. The geographer Brian Berry (1978: vii) says: 'To a new generation of young geographers, progress in geography means theory building, theory-testing, and theory-refining in an accumulative and unidirectional sequence, and theory was location theory – von Thünen, Weber, . . . and Lösch.' In contrast, I will argue that differences among members of the school are more significant than any methodological commonalities, casting doubt on the very designation 'school'. Rather than generating universal knowledge, each member produced a piece of local knowledge, a reflection of their peculiarly local circumstances and personal convictions, and not the distillation of some superordinate rationality.

#### 1 Von Thünen, 1783–1850

Von Thünen is credited as the progenitor of the school. A member of the landed class (but not part of the aristocracy), von Thünen attended university and agricultural college before purchasing the Tellow estate in Mecklenburg in 1810. Although there is evidence that von Thünen had worked out his main ideas as early as spring 1803 (Dempsey, 1960: 44; Binder-Johnson, 1962: 213), it was only after he purchased Tellow, and as a result of his agricultural experiments and meticulous statistical recording, that he wrote the book in 1826 that would later make his reputation in locational analysis, *The isolated state* (von Thünen, 1966).

On the surface, von Thünen lives up to the reputation of a dispassionate rational scientist equally at home both as an observer – fastidiously recording the numerical results of his trials at Tellow – and as a theorist – using geometry, algebra and calculus to anticipate neoclassical marginal economics by 50 years (Blaug, 1990: 121–43; Niehans, 1987), and regional science by over 100 years (Isard, 1956: 27). In the standard interpretation (Dempsey, 1960: 47; Blaug, 1979; 1990: 121–43; Niehans, 1987), his success is attributed to the application of a rationalist method – *Form der Anschauung* (Binder-Johnson, 1962: 212) – involving the postulation of abstract assumptions that are progressively relaxed, and the mathematical deductions from which are made increasingly complex and realistic. As Schneider (quoted in von Thünen, 1966: xxiii) puts it, in von Thünen's work 'the importance of model construction for the understanding of reality is made evident with unsurpassable lucidity and forcefulness'. The culmination, perhaps, of that lucidity and forcefulness is von Thünen's concentric model of agricultural land use (Figure 1), and which was later to connect directly to geography's 'neglected geometrical tradition' of locational analysis (Haggett, 1965: 15).

How indubitable, though, are the methods used to derive such a diagram? Following



**Figure 1** Von Thünen’s concentric model of land use, from *The isolated state* (von Thünen, 1966: 216)

my earlier argument, I will suggest that von Thünen’s work is intimately related to its local circumstances, and consequently undermining the idea of the unfolding of a single abstract method that inexorably produced the concentric figure (albeit drawn not by von Thünen but by ‘one of my friends’ who is never named; Binder-Johnson, 1962: 215).

Critical are von Thünen’s philosophical and political beliefs. Early on he abandoned his ‘inherited views . . . of the owning classes’ (von Thünen in Dempsey, 1960: 223), and following Hegel he constructed not just an isolated state but an ideal one (Barnbrock, 1974; Harvey, 1981) – a state that included harmony among the classes, and in particular between workers and bourgeoisie. Interestingly, von Thünen intended to entitle his book *The ideal state*, but his half-brother, Christian Dietrich Von Buttell, convinced him to change it (Dempsey, 1960: 49-50; Binder-Johnson, 1962: 214). It would have been a

better title because von Thünen's model was less a description of the world than a vision of how it should be. He thought society itself could be organized in the best interests of everyone.

Central to his political vision is von Thünen's idea of a natural or just wage that in turn is bound up with the marginal land, and his concentric model of agricultural land use. In an 'ideal state', von Thünen argued that all workers would receive the 'frontier wage' that would simultaneously maximize total output, capital accumulation and the wage rate. Literally carved on his Belitz cemetery gravestone, von Thünen argued that wages should be equal to the geometric mean of the product of the subsistence needs of workers and their net product:

$$W = \sqrt{A.P}, \quad (1)$$

where  $W$  is the frontier wage,  $A$  is the subsistence needs of workers and  $P$  is the net product per worker.

When wages are set at this level, von Thünen believed social harmony would prevail. It would be Dr Pangloss' 'best of all possible worlds'. The marginal land is vital to von Thünen's presentation because it sets the level of the frontier wage. It represents the one space in the isolated state where the disturbing influence of the land-owning class is erased, that is, where workers are on their own. As a result, von Thünen could calculate for that one plot the level of the 'just' or 'natural' wage. There has been a sustained debate in the literature around the analytical derivation of von Thünen's claim (see the various essays collected in Blaug, 1992), but in some sense it misses the point. As Dickinson (1969: 898) rightly comments, the frontier wage 'is not a descriptive proposition, asserting how wages are actually determined. Rather, it is a normative proposition, asserting how wages would be determined in a just and rational society.'

For von Thünen as a politically progressive liberal in a period of 'incipient struggle' and 'ferment' (von Thünen in Dempsey, 1960: 218 and 219), the frontier wage offered the potential of a final state that would ensure that 'all of those evil conditions which sicken the social situation of Europe [will] disappear' (von Thünen in Dempsey, 1960: 327). Von Thünen's rings were not a description of how the world is, but how it should be once social harmony was realized. This is not to claim that von Thünen's approach, one that included a strong dose of Hegelian philosophical idealism and attendant teleology, should be uncritically accepted, but it needs to be set against a rationalist interpretation that represents his work as the manifestation of only an abstract mathematical rationality. Von Thünen was always a partisan, someone who had a vision of the world that was partial, contestable and irrevocably marked by his own situation and beliefs. The model he constructed was not the precipitate of an undiluted logic, but intimately bound to his local situation.

## 2 Alfred Weber, 1869–1958

Mark Blaug (1979: 28) calls Alfred Weber 'the true heir of von Thünen'. While true, it is not in the sense that Blaug means. My argument is that what he shares with von Thünen is the practice of local modelling.

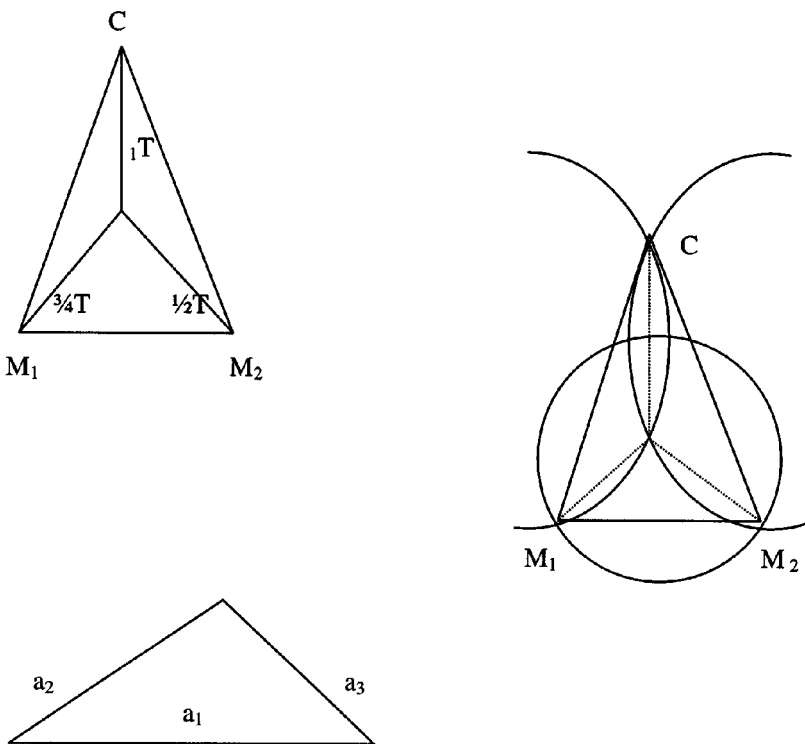
As a student at the University of Berlin, Weber worked under the economist Gustav Schmoller, completing his doctoral dissertation on the sweating system in the clothing

industry in 1897. That dissertation, in turn, became the basis of his book first published in 1909, which made him an economic geographical icon, *Über den Standort der Industrien* (translated as *Alfred Weber's theory of the location of industries*; Weber, 1929). Concerned with identifying the factors that determine the location of industry, Weber uses much mathematics and geometry to derive a solution best represented by his famous location triangles (Figure 2).

Ponsard (1983: 23) interprets Weber's mathematical formulations, including the location triangles, as a 'reaction against the prevailing historical school of thought. . . . [For] Weberian theory is a projection of pure economics into the spatial domain, elaborating laws which are abstract, mechanical, independent of any economic system, and applicable to the modern world.' Certainly, Weber's methodological statement at the beginning of his book lends support to Ponsard's claim. Weber (1929: 4) writes:

Methodologically we shall always proceed by isolation, not only the first part dealing with pure theory, but in the second part as well. . . . For in the task of stating the pure rules of location it will be possible to use deduction exclusively. We shall be able to start from certain very simple premises and to deduce therefrom the entire system of 'pure' rules of location.

As with von Thünen, however, there is a subtext buried in Weber's text. The book that made Weber famous was part of a local project, the aims of which in many ways were antithetical to a rationalist interpretation. According to Derek Gregory (1981), Weber



**Figure 2** Weber's location triangles, redrawn from *Alfred Weber's theory of the location of industries* (Weber, 1929: 55), with permission of the University of Chicago Press

was intent to follow and to deepen Schmoller's contextual historical perspective. Schmoller and Werner Sombart were critical figures in the turn-of-the-century German historical school, a central tenet of which was its opposition to rationalism. It is therefore difficult to understand why, if Weber was the formalist Ponsard claims, he would be working with Schmoller.

Rather than seeing Weber's location triangles and associated theory as the precipitate of a disembodied set of logico-mathematical procedures and diagrams, it is better to situate them within Weber's specific historical and geographical setting, and the tradition of German romantic intellectual thought.

Gregory (1981) argues that the central material context of Weber's work was the rapid industrialization and urbanization of late nineteenth-century Germany, and its concomitant political and social transformation. As Weber (1929: 6) says, in the introduction, his task is 'to clarify the ways in which the location of industries form the "substance" (I do not say cause) of the large agglomerations of people today'.

This physical setting was not the only influence, however. Equally important was the intellectual context. Weber's analysis and substantive focus were shaped, as were von Thünen's, by a deep critical sensibility toward the prevailing social and economic conditions. In particular, following a long line of German romantic and conservative writers, he was aghast at those 'large agglomerations of people', and the processes of industrialization that were responsible for them. They represented for him science and civilization run amok, creating a general cultural crisis not only in Germany but in the west as a whole (Aron, 1964: 51). For Weber, it was the cultural crisis that was critical, not the location of industry as such. He says as much in his introduction: 'We shall see that the kind of industrial location which we have today is not entirely explained by the "pure" rules of location, and therefore is not "purely" economic. It results to a large extent from very definite cultural aspects of modern capitalism...' (Weber, 1929: 12-13).

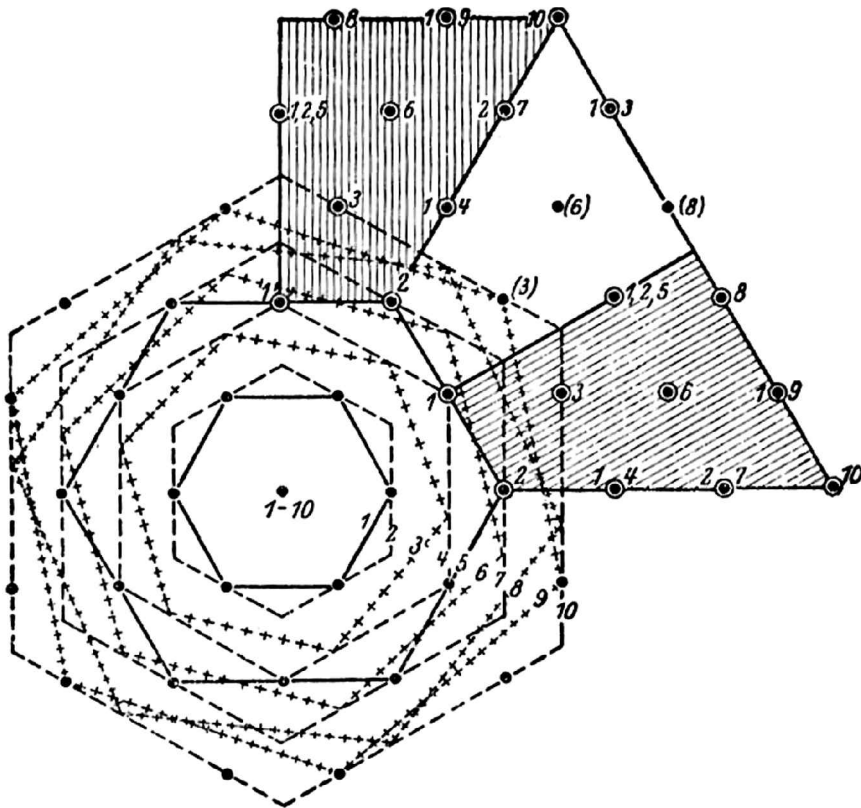
His later works were to focus exclusively on those cultural aspects, rather than locational analysis (for a bibliography, see Aron, 1964: 138). In this reading the location of industry for Weber was always only a surface symptom of something much more fundamental. Indeed, in reflecting on his earlier work, Weber talked about his location triangles as having 'hung in the air' (quoted in Gregory, 1981: 186). The implication is that they required grounding, which in some sense Weber does in his later studies in 'cultural sociology' (Weber, 1939).

Those studies throw a pall on particularly science and civilization, found especially in the large industrial agglomerations of Weber's time. They represented an 'unexampled barbarism . . . in which society and civilization reign unchallenged, in which our egotistical impulses develop unchecked, and in which no spiritual aspiration disciplines or animates human beings' (Aron, 1964: 113). Not surprisingly, Weber never returned to locational analysis. In so far as his earlier work represented the application of scientific principles, and associated beliefs around progress, rationality and universality, it presaged exactly the 'unexampled barbarism' that he was later to rail so vehemently against. For my purposes, whether one agrees with his cultural sociology is moot. Rather, as with von Thünen, what is significant is the halting task of trying to re-embodiment Weber's work; of not taking his industrial location theory and associated triangles as objects in themselves, but as products of particular local practices and beliefs.

## 3 August Lösch, 1906–1945

Finally, there is August Lösch whose brilliance as a locational analyst was in providing 'the first modern and systematic integration of various earlier [location] theories into a unified analytical structure' (Ponsard, 1983: 87). In particular, Lösch devised an integrative general equilibrium theory of the space economy, culminating in his mathematically derived generalized landscape of location of industry, and couched in terms of hexagon nets imposed on the landscape, and presented in his book *The economics of location*, published in 1940 (Figure 3).

Born and raised in Swabia – he dedicates *The economics of location* 'to the land of my birth, the land that I love' (p. xvi) – he was a student in the Economics Department at the University of Bonn. There he was part of a circle of students who worked with Joseph Schumpeter, the Austrian economist, who later left to take up a position at



The 10 smallest economic areas. The sectors containing many towns are hatched. Alternative regional centers are in parentheses. Simple points represent original settlements. Those enclosed in circles are centers of market areas of sizes indicated by the figures.

**Figure 3** Lösch's nested central place hierarchy, from *The economics of location* (Lösch, 1954: 118)

Harvard (Stolper, 1986). Lösch, however, was later to reunite with Schumpeter in 1934, and then again in 1938, following grants from the Rockefeller Foundation that allowed him to travel to the USA and Canada where he collected the data that formed the basis of the last section of *The economics of location*.

As with von Thünen's and Weber's books, the dominant interpretation of Lösch's *The economics of location* is rationalist (Ponsard, 1983). I will argue it is more complicated. In particular, there is a tension in Lösch's book between two very different metaphors that he uses to characterize the economic geographical landscape. One derives from physics, and stresses law-like relations and a mathematically defined order (Gregory, 1994: 56–58). This view is captured by Peter Haggett's (1991: 70) characterization of Lösch as like a 'solid state physicist', who conceives 'each settlement like an atom bonded together in a matrix of molecular interconnections'. The other is an organic metaphor averring holism, with properties of emergence and unpredictable evolution. Here places are conceived as possessing an organic solidity made up of complex social and cultural relations worked out over generations. In turn, these different metaphors reflect two very different intellectual traditions, and, in some sense, two different geographies bearing on Lösch. The first comes from his studies at Bonn as an economist steeped in rationalism, and the second comes from his upbringing in Swabia and his attachments to German romanticism.

Other commentators also recognize that there is a tension running throughout Lösch's book, although not in the exact form that I describe. David Harvey (1996: Chapter 2) interprets Lösch as a consummate dialectician moving between the two opposed poles of what is and what should be, while Peter Gould (1986) portrays the strain in terms of Lösch's inclination towards the concrete and an equally strong counter-inclination towards the abstract. However it is defined, I would argue that the consequence is a set of fissures running the length of Lösch's book that are never closed, and which reflect different local knowledges.

In deploying the first metaphor, Lösch, like the physicist, constructs a rational, ordered world derived from the application of an abstract analytical logic. In turn, the rational landscape he constructs, which functions as a gold standard against which to compare all else, is necessarily defined in mathematical terms. 'There are mathematical calculations in this book because it is reprehensible not to trust reason and rest content with vague words and hazy sentiments' (Lösch, 1954: xv).

More fundamentally, Lösch upholds rationality against what he thinks is the alternative: mayhem and tumult. As Derek Gregory (1994: 58) writes: 'Lösch wanted to disclose, to make visible, the systematic order or a rational economic landscape – what he called "the rational and therefore natural order" – because he was convinced that such a demonstration held out the prospect of domesticating the "illogical, irregular, lawless" forces that ravage [a] chaotic reality.' It is for this reason that he famously says: 'the real duty of the economist is not to explain our sorry reality, but to improve it. The question of the best location is far more dignified than determination of the actual one' (Lösch, 1954: 4). Dignity is conferred by an abstract, purified rationality, not a concrete, soiled reality.

Cheek-by-jowl with Lösch's rationalist sensibilities, though, are a set of romantic ones that emphasize concrete experience over abstract rationality, holism over analysis, and messy real places – 'our sorry reality' – over a logically constructed landscape.

In the preface to the first edition, Lösch (1954: xv) says that 'my youthful experience

in a little Swabian town constitutes the real background of this book ... [and] my original experience there confirm my final theories'. So, it is not rationality that is the litmus test, but the 'reality' of Lössch's experience. Gould (1986: 15) also senses this same ambiguity:

[Lössch's] landscape is not just geometry, but is inhabited by people joined by a complexity of social relations, not the least of which may be a deep sense of rootedness, of *Bodenständigkeit*, in the region itself. Few had a deeper sense of such rootedness than Lössch himself, as he constantly uses his Swabian homeland as an intellectual touchstone to test the truth of a humane assertion.

Likewise, having earlier praised mathematical logic, he later argues a different position, saying that he could never be 'satisfied with a cold mathematical consideration. This would resemble a mere brick shell that is not ready for occupation; a skeleton without flesh or blood' (Lössch, 1954: xv).

Finally, there is a tension between Lössch's commitments to a highly regulated mathematical order used to express a rational space economy and his romantic commitment to particularity of specific places. He says, again contradicting his remarks about the subordinate nature of reality to rational ideals: 'Actual knowledge of a real area is the mainspring of research. It is the best guarantee that investigation will be directed towards actual possibilities in stead of losing itself in a mere play of intellect' (Lössch, 1954: 349).

In sum, like the work of both von Thünen and Weber, Lössch's was not the distillation of pure scientific logic, titrated drop by drop onto the page. Lössch might well have been a genius, but he was also someone influenced by the time and place in which he lived; that is, his knowledge is locally situated. Specifically, he was a man caught between two very different conceptions of the world – a rationalist one and a romantic one – which he mobilized as different guiding metaphors in constructing knowledge. Such ambiguity was not confined to Lössch. As Herf (1984) argues, it characterized the 'reactionary modernism' of early twentieth-century Germany. In this sense, context entered into the very lineaments of Lössch's work, making it resolutely local.

#### IV American spatial science

While Lössch's book was a piece of local knowledge, it travelled, both figuratively and literally, to other places that were also local. Peter Gould (1986: 7) remembers taking it 'to that first class of Northwestern's winter term [with the regional economist Charlie Tiebout in January, 1957], glad to get out of the raw wind that swept flurries of snow across Lake Michigan'. Eighteen months earlier, Brian Berry (1993) also remembers packing it in his luggage and reading it during his transatlantic journey to Seattle, Washington, where he was about to begin his first year as a graduate student at the University of Washington. Furthermore, such travels made a difference (Shapin, 1998; Barnes, 2002). Lössch's book helped produce, along with other travelling books, people and numbers, a very different view of economic geography in Anglo-America, one defined by law-seeking (nomothetic) locational analysis rather than the mere description of unique regional economies (idiographic) that hitherto dominated the discipline. Economic geography's quantitative and theoretical revolution was about to begin.

That revolution began as a set of local encounters centred on one or two key individuals at specific geographical sites – in this case, the University of Iowa, Iowa City, and the University of Washington, Seattle.

Fred Schaefer at the University of Iowa is often credited with beginning the revolution, and thus allowing locational analysis into Anglo-American economic geography. His 1953 *Annals* article arguing for morphological laws – if event A in location alpha, then event B in location beta – was a forceful rebuttal of regionalism (Schaefer, 1953). While the paper became a *cause célèbre* for some (Bunge, 1968), Schaefer had few friends in the Geography Department at Iowa, and certainly no base of graduate students either to work with him or to spread his word. Moreover, Schaefer's premature death at 48 in an Iowa City movie theatre in June 1953 brought to a close any further direct influence. If Schaefer's paper has a bearing on locational analysis it was in retrospect, as later spatial scientists looked for precedents, slogans and, in some cases, martyrs (see also King's, 1979: 128 comments).

Someone much more influential than Schaefer at the University of Iowa for implementing an agenda of locational analysis was Harold McCarty. Before McCarty founded the Iowa Geography Department in 1946, he published *The geographic basis of American economic life* (1940), on the surface a conventional regional account of US economic geography. McCarty's argument for making regions the unit of analysis was not the traditional one couched in terms of uniqueness, but rested on the conviction that market forces made economic geographical reality that way. As McCarty (1940: 19) writes, 'motivated by a desire for profit, producers in various parts of the nation have endeavored to engage in those types of economic activity which bring the greatest return. Largely because of differences in natural and human resources, economic life in the various sections of the country has taken different forms, . . . tend[ing] to become regionalized or concentrated in particular areas.' There is a hard economic logic driving McCarty's locational analysis that was new to Anglo-American economic geography (although clearly not new to the German school). In McCarty's conception, economics provided the theory, geography the empirical fodder. As he puts it, 'economic geography derives its concepts largely from the field of economics and its method largely from the field of geography' (McCarty, 1940: xiii).

McCarty's use of that word method is very broad, though, because the specific technical methods he later employed came not from geography but from statistics. In particular, he pioneered the mathematical use of regression and correlation analysis in economic geography, conceiving the very discipline in their image (Barnes, 1998). The justification for using correlation and regression is found in his 1954 *Economic Geography* paper where he argued that because of the complexity of economic geographical phenomena one could not identify causes simply by inspecting the data (McCarty, 1954). The best one can hope for are geographically limited associations that could then be compared to a body of location theory, of which the best worked out and most germane was found in economics. Economic geographers should classify, delineate, measure and collect data, and even look for statistical associations, but to explain they needed the economists. It was a view solidified in the collective report *The measurement of association in industrial geography* (McCarty et al., 1956). Consisting of a multitude of regression studies linking the location of industry to a number of different variables, explanation was provided by drawing upon theories of economists.

At the other centre of calculation, the University of Washington at Seattle, a

remarkable group of graduate students, including Brian Berry, Bill Bunge, Michael Dacey, Art Getis, Duane Marble, Richard Morrill, John Nystuen and Waldo Tobler, had been gathering from the mid-1950s, and by the end of that decade they collectively launched a decisive assault on traditional economic geography, emphasizing above all else locational analysis. Key was a young faculty member, William Garrison, who acted as mentor, protector, critic, muse and paymaster for many of the group. Garrison had joined the faculty in 1950, following a Ph.D. at Northwestern University, and before that wartime service in the US Air Force where he gained expertise in statistics and mathematics as part of his training as meteorologist (Barnes, 2001b).

Statistics and mathematics were to define the program at Washington. Garrison gave the first advanced course in statistics in a US geography department in 1955. Also important to this new conception of economic geography defined by equations, graphs and numbers were machines. There were the large, cumbersome electrical mechanized Friden calculators, but more important was the even larger, more cumbersome, computer. In an early advertisement for the department, the Head, Donald Hudson (1955), boasted about the departmental use of an IBM 604 digital computer, also a national first. The programming technique of so-called patch wiring involving plugging wires into a circuit board was crude and inefficient, but it helped define and consolidate the new vision of the discipline based on science and technology.

Empirical investigation within early American spatial science, then, was firmly wedded to a numerical and statistical form of inquiry. Yet the revolution was not only quantitative but also theoretical. Initially, theoretical sources of locational analysis were the familiar ones – von Thünen, Weber and Lösch – but they multiplied rapidly. From physics emerged gravity and later entropy-maximizing models; from sociology and land economics came models of urban land use, social physics, including the rank-size rule, and urban factorial ecology; and from geometry came network and graph theory and the analysis of topological forms that were incorporated into transportation studies (Pooler, 1977). Marking all these different theoretical ventures was the emphasis on conceptual precision, deductive logic and analytical rigour, that is, rationalism.

However, as with the German location school, rationalism is insufficient to explain the rise and form of American spatial science, and associated locational analysis. For it was very much a product of a set of local, contingent factors found in 1950s America that were simultaneously historical, institutional, financial and social.

Historically, there was the influence of the second world war, and later the cold war. Both were bound to science. For many, science had won the war, and would be the basis of winning the cold war. The resulting enhanced status this gave science was picked up by American social science that traded on the science part of its title. Economic geography simply followed suit. Another influence, which will be discussed in more detail in the next section, was the importance of Operations Research that brought together scientists and social scientists, especially economists, to engage in strategic planning in the war using mathematical models and methods of verification (Mirowski, 1999). After the war, those same techniques, for example, linear programming, in combination with the first forays into computer use, began to be picked up by engineers and planners in dealing with postwar American urban expansion, design and infrastructure provision. In fact, the main source of funding for Garrison's graduate students at the University of Washington during the mid-1950s was a contract he had secured with the State of Washington, in conjunction with the University of Washington

engineers Ed Horwood and Bob Hennes, to assess the effects of building a highway system on Seattle and its surrounding counties (Garrison *et al.*, 1959). The methods Garrison taught his graduate students in his advanced statistics course, and later in his economic geography seminar based upon Isard's (1956) work in regional science, were perfectly suited to answer those strategic planning questions about highway development (Barnes, 2001b).

Another significant wartime influence is the mathematical statistical training provided by the military to a number of geographers who later went on to take up locational analysis. Garrison as noted was one, but so was Edward Taaffe, who also trained as a meteorologist, as well as William Warntz (Warntz, 1984; Janelle, 1997). Warntz (1984) said that it was reading J.Q. Stewart's (1945) *Coasts waves and weather* when he was stationed as a meteorologist in Gander, Newfoundland, that led him to physical analogies such as gravity and potential models, and, in turn, to the very development of macrogeography, and the problems of spatial interaction. Similarly, Garrison (Dow, 1972: 2–3) said: 'I had a number of very fortunate opportunities. I had been a meteorologist in World War Two and I had the opportunity to reflect on the nature of some kinds of systems which gave me a kind of systems bent.'

Second, the institutional context involved achieving a particular configuration of personnel, resources and politics. Initially important to the success of locational analysis at the University of Washington, for example, was a strong, powerful and open-minded head, Donald Hudson, who after his appointment in April 1951 was keen to promote a vigorous and lively intellectual culture. Having no prior training in locational analysis, he nevertheless recognized its potential, and provided vigorous support allowing it to flourish. He did so partly through his management of graduate recruitment – the Department had five teaching assistantships that allowed Hudson to hire aggressively graduate students like Brian Berry in 1955, and two years before him Duane Marble (Dow, 1971: 2). Partly, it was through encouraging internal debate by, for example, by allowing graduate students free access to the mimeograph, and an unlimited supply of paper. This permitted students such as Berry and Marble to prepare and circulate internal position papers, and from March 1958 to launch a formal discussion paper series that was sent to like-minded individuals around the USA and the world, and which in turn helped to launch the revolution in other places. Finally, it was partly through providing intellectual resources in the form of catalytic visitors whom Hudson brought in to teach at least term-long courses in the Department. They included Leslie Curry, Ross MacKay (who was especially influential on Waldo Tobler's and Brian Berry's research), and Torsten Hägerstrand who introduced both new techniques, such as Monte Carlo simulation, and a temporal perspective that hitherto had been lacking in the static location analysis of the German school.

Third, that such visitors could be brought in, and that people like Hudson could employ graduate students and allow them to be exuberant with the duplicating machine, was possible in part because of available financial resources. Those increased resources stemmed partly from the burgeoning of higher education in North America, and partly also from wider changes in economy and society turning on the rise of Fordism and a Keynesian-welfare state that helped realize Galbraith's 'affluent society' of 1950s America (Scott, 2000); but there were also much more specific sources of financial munificence. In the USA, one of the most important was the Office of Naval Research (ONR). Initiated in 1948, it revolutionized research funding. Evelyn Pruitt

(1979: 103), an administrator of the program from its inception, argues that, before ONR, research grants were for individuals, mostly for summertime activities involving modest projects, and typically worth only a few hundred dollars. After the establishment of ONR, research tended to be more collaborative, year-round large projects, and funded in tens of thousands of dollars. ONR financially underwrote cultural geographical projects and regional geography during the 1950s, but the style of research funding it favoured especially suited the kind of large-scale collaborative projects pursued by the new economic geographers. For example, Berry's central-place research (1959–61) was funded by ONR. Such projects became even more suitable once 'the Geography Program [at ONR] began to encourage more research on the use of computers . . . seeking ways efficiently to measure, store, describe, and display data concerned with geographic surfaces, conditions, and processes' (Pruitt, 1979: 107).

A final significant local context was a social one. The social composition of the early quantifiers and theoreticians was, almost without exception, young, white and male. The resulting culture of inquiry tended to be competitive, ambitious and self-confident (see Barnes, 2001b, for more detail). This is neither to say that there weren't good intellectual reasons for transforming economic geography nor to cast aspersion on the work carried out, but the social characteristics of the new economic geographers left its mark. Taylor (1976), for example, interprets the quantitative revolution in terms of the career moves of the participants. Peter Gould (1979), whether he meant to or not, signals the heroic nature of spatial science when he refers to the period as 'Augean'. It was after all, Hercules, that most masculine of heroes, who cleaned the Augean stables in a single day after 20 years of neglect.

In sum, the locational analysis associated with American spatial science did not originate in blinding revelation, with people suddenly seeing its truthfulness after being exposed to the light of scientific rationality. It was a much more hesitant, complex and local process. In making this argument, I am not belittling the achievements of the movement. The protagonists involved were exceedingly able, creative and imaginative scholars. In many ways, their work still sets the theoretical agenda for economic geography. For this paper, however, I am less concerned about the merits of what they did in locational analysis and more concerned with the justification of what they did. To put it all down to the slavish following a universal rationalism is to underestimate their historical and geographical place, that is, their local knowledge.

## V The new economic geography

The most recent proposal for locational analysis comes from economists, and is dubbed the new economic geography. Its most enthusiastic supporter is the American economist Paul Krugman. A prolific writer and now public figure – his web page claims that he has written in total 'eighteen books (I think) and several hundred articles' (Krugman, <http://web.mit.edu/krugman/www/>), and spends 'an hour or two each day . . . on the phone with reporters' (Krugman, 1995b: 37) – Krugman says he had an 'utterly conventional background', growing up on Long Island in New York, followed by an undergraduate degree at Yale and a PhD at MIT (Krugman, 1995b: 30). It was at MIT as a graduate student that he had an epiphany, 'a vision on the road to Damascus' (Krugman, 1995b: 33), when working on trade theory. He realized that the

conventional model resting on constant returns to scale and perfect competition could be transformed by the Dixit-Stiglitz model devised in the late 1970s, which allowed increasing returns and imperfect competition. As a result, as he puts it, 'the lovely little model of Dixit and Stiglitz . . . would form the core of my professional life' (Krugman, 1993). In particular, that lovely little model permitted Krugman to argue that increasing returns are a motive for specialization and trade even in the absence of comparative advantage. The 'new trade theory' was thus born, which catapulted Krugman into the forefront of his profession. He won the John Bates medal for the best economist under 40 in 1991 (Dixit, 1993), and popular writing about him describes him as 'Nobel-bound' (Hirsh, 1996).

In the late 1980s, he discovered economic geography after reading Michael Porter's book, *The competitive advantage of nations* (Krugman, 1995b: 40). This was his second road-to-Damascus experience. He finally 'realized that [he had] . . . spent [his] whole professional life . . . thinking and writing about economic geography, without being aware of it' (Krugman, 1991: 1). As a result, as he puts it, 'I have engaged in a systematic process of proselytising on its behalf: my intention is to establish economic geography as a branch of economics that is taken as seriously as international trade, and I believe that I will succeed in that plan' (Krugman, 1995b: 40).

At the basis of his new vision of economic geography is increasing returns – as he says, they are 'the main excuse I have for my existence' (Krugman, 1995b: 41). 'Economic geography, like the new trade theory, is largely about increasing returns and multiple equilibria. The technical tricks needed to make models tractable are often the same' (Krugman, 1995b: 41). For this reason, he begins his economic geographical modelling with Dixit-Stiglitz, to which he adds transportation costs using Samuelson's 'iceberg model'. In conjunction with his laptop that allows him to simulate a variety of potential outcomes (multiple equilibria), he works out his locational analysis at the regional scale. For any given region, there are either agglomerative or dispersal effects that depend on the relative strengths of transportation costs versus labour (im)mobility: 'the lower transport costs are, the more the forces of spatial agglomeration will prevail over those of dispersion; the more immobile labour is, the more dispersion will prevail over agglomeration' (Martin, 1999: 68). Krugman is keen to stress that such a process is always open to historical accident, creating the possibility of multiple equilibria. Once, however, particular regions attain an advantage, increasing returns will gain purchase, resulting in path dependency and lock-in. While the eventual outcome is equilibrium, a priori one knows neither the precise solution nor if it will be optimal. The same increasing returns model is also then used by Krugman (1996) to represent the growth and spatial distribution of city systems, allowing him under certain mathematical conditions to derive, for example, what he calls a central place system (for a different interpretation, see Sheppard, 2000).

The word model is critical for Krugman; by it he means a highly stylized and formal representation of the world possessing the ability both to simplify and clarify (echoing Haggett's earlier definition). Unlike Haggett (1965: Chapter 1), though, who recognizes multiple strategies of valid geographical inquiry, Krugman identifies only one. 'To be taken seriously an idea has to be *something you can model*' (Krugman, 1995a: 5; original emphasis). Not just any old model will work, however: it must be formalized as 'Greek-letter writing' (Krugman, 1990: ix). A model must be formal, because if it was not we would not know if it was logically consistent, and logical consistency is a must. As

Gary Dymski (1996: 442) writes, for Krugman 'whether an explanation is useful depends on whether it is tractable and whether it is logically consistent: and the only way to determine the latter is to evaluate its mathematical consistency when it is formalized'.

This was the problem with both traditional regional economic geography that preceded spatial science and current postspatial science incarnations. While traditional economic geography 'was a field full of empirical insights, good stories and obvious practical importance', it was 'neglected [that is, neglected by economists] . . . because nobody had seen a good way to formalize it' (Krugman, 1995a: 41). The postspatial science approach in economic geography that now dominates the discipline is even worse. Represented by an interest in 'post-Fordism . . . [initiated by] the Derrida-influenced regulationist school', postspatial science economic geography produces only 'deconstructionist geography!' (Krugman, 1995a: 85). While it is doubtful that Derrida really has much interest or expertise in regimes of accumulation and modes of regulation (see Eagleton's, 1995, send-up of Derrida's economic acumen), the important point for Krugman (1993) is that both the old and the new kinds of economic geography eschew formal models, and in doing so shun the 'scientific-intellectual outlook that is arguably the true glory of our civilization'.

Krugman, though, is even critical of mathematical modellers within economic geography, such as American spatial scientists and regional scientists. The problem is that they are not as good mathematicians as economists, and in any case never possessed the right analytical tricks to deal with the problems at hand. Krugman (1995a: 87) writes: 'one cannot fault the geographers for their failure to develop full-maximization-and-equilibrium models – although one can perhaps complain about their failure to understand how far short of that ideal they were falling. And one can understand the reluctance of the mainstream economists to muddy the clarity of that mainstream with the somewhat murky modelling efforts of the geographers.'

Krugman's project, then, is to take works of economic geographers and to make them intellectually viable by expressing them in the formal vocabulary of economic models, and, in particular, that of Dixit-Stiglitz, the mathematical pedigree of which is unimpeachable. As he writes, 'we will integrate spatial issues into economics through clever models . . . that make sense of the insights of the geographers in a way that meets the formal standards of the economists' (Krugman, 1995a: 88).<sup>3</sup>

Krugman (1995a: 37–55) talks here about the lost 'five traditions in economic geography': central place theory, statistical regularities such as Zipf's rank-size rule, cumulative causation, local external economies, and land-rent models. They were lost (that is, became 'peripheral to the economics profession'; Krugman, 1991: 3) because they were never expressed formally in terms of the two principles that lie at heart of all economic activity: 'Obvious opportunities for gain are rarely left unexplained, and things add up. (Or as I sometimes put it, \$20 bills don't lie in plain view for very long and every sale is also a purchase)' (Krugman, 1995a: 74–75). These two principles – 'maximization (of something) and equilibrium (in some sense)' (Krugman, 1995a: 75) – must always be present in any formal mathematical economic geographical model, and once they are, which Krugman achieves through his 'analytical tricks' and 'clever models', economic geography can be 'resurrected . . . as a major field *within* economics' (Krugman, 1991: 7; my emphasis).

Gary Dymski (1996) argues that, by holding to the twin principles of maximization

and equilibrium, Krugman remains solidly within mainstream, rational-choice equilibrium (RCE) economics. As Dymski (1996: 445) also notes, however, RCE economics is not all alike, and since the second world war there have been various versions, like Krugman's, that relax some of the classic assumptions of the late nineteenth-century Ur-version first put forward by Léon Walras. Yet, while specific assumptions are different, the beliefs in maximization and equilibrium (even multiple equilibrium), and the appropriateness of mathematics to represent them, stay unquestioned. In this sense, Krugman's economics remains shackled to orthodoxy (see also Martin, 1999).

While Krugman's position might be construed as condescending, or even imperialistic, the issue for this paper is whether it can also be interpreted as local knowledge. I think it can. A recent paper by Nigel Thrift provides a clue. Writing against any takeover of economic geography by economists such as Krugman, Thrift (2000: 701) raises the issue of the culture of economics itself. As he laconically puts it, 'A cultural geography of economics and economists? Now that's a thought.' Here Thrift raises the issue of the kind of intellectual culture that infuses economics. Although he does not get very far in addressing it, others have. Perhaps the best expositor is the American historian of economic thought, Philip Mirowski.<sup>4</sup> Concerned precisely with finding the local origins of the infatuation of economics with mathematical formalism, and in particular with the ideas of maximization and the derivation of equilibrium – hallmarks of Krugman's work and RCE models more generally – he locates them initially in late nineteenth-century European physics, and later after the second world war in Operations Research (OR) (Mirowski, 1989; 1998; 1999).

For brevity's sake, it is not possible to provide the details. The gist is that, using historical and biographical evidence plus analytical and mathematical arguments, Mirowski (1989: Chapter 5) tracks the initial use of the maximization thesis, and associated equilibrium postulate, to a late nineteenth-century branch of physics called energetics that was then metaphorically incorporated into economic theory by early neoclassical economists such as Walras. Within such a physical metaphor, early neoclassical economists substituted utility for energy in the constrained maximization equation of the physicists, and made the rationality postulate the equivalent to the physicist's principle of least effort. Just as the technique of constrained maximization reveals the path of least effort of any particle's movement, so the same technique is used to discover the most efficient (rational) actions of producers and consumers, and the resulting equilibrium point of rest. Mirowski's broader point is that it was out of the peculiar cultural and intellectual context of the European late nineteenth century that neoclassical economics arose, reflecting not universal truths but a peculiar local conjunction of circumstances.

More recently, Mirowski (1998; 1999) argues that the extreme penchant of American economics in the postwar period to uphold the virtues of mathematical reasoning and abstract theorizing, exemplified by the kind statements and models put forward by Krugman, was given further momentum by a particular set of historical events spurred by the second world war and later the cold war. They were critical elements in a local context that produced a highly formalized postwar neoclassicism. To summarize Mirowski's complex argument crudely: in order to assess and to achieve military strategic ends both in the second world war and more importantly within the cold war scientists and economists were brought together under the rubric of OR, perhaps the

most famous site for which was RAND, inaugurated in 1948 in Santa Monica as the US Air Force's think tank. For Mirowski (1999: 690), OR was the 'workshop where the post-war relationship between natural scientists and the state was reconfigured, and the locus where economics was integrated into the scientific approach to government, corporate management and society'. By tracing the work and life-trajectories of particular economists, as well as the influence of key institutions and sources of funding that were often military, and which also intersected with geographers such as ONR, Mirowski argues that OR, and in particular 'RAND was the inspiration for much of the advanced mathematical formalization of neoclassical orthodoxy in the immediate post-war period. But beyond that, . . . RAND itself was consciously constructed to break down disciplinary barriers between the natural and social sciences, and to spread the gospel of complexity. . . . The tool identified for attacking this novel class of problems [was] . . . the computer' (Mirowski, 1999: 704). Modern economics begins to become what Mirowski (1998: 14) calls 'a cyborg science', requiring the performance of both humans and machines, and which again was characteristic of early attempts at locational analysis by American spatial scientists. The broader point of Mirowski's postwar story, as well as his earlier one, is to disrupt internalist histories of economics that portray it as the relentless progressive march of a universal rationality. In so far as Krugman, with his talk of the impeccability of mathematics, his deployment of abstract theory and his reliance on his laptop, is heir to the events that Mirowski describes, it also disrupts any internalist history of recent locational analysis.

In sum, the critical argument I have made about Krugman's new economic geography is different from those recently made by other economic geographers. Critics such as Ron Martin (1999), Peter Sunley (Martin and Sunley, 1996) and Eric Sheppard (2000) argue that the approach Krugman advocates was carried out by economic geographers and found wanting at least 20 years ago, and so now produces only a 'dull sense of *déjà vu*' (Martin, 1999: 70). Rather than behind the curve, economic geographers are in front of it: they have been there and done that. While their arguments are persuasive, my criticism is different. It is that Krugman offers only a more extreme version of what has always characterized locational analysis – that is, a rationalist justification. Yet such a justification is not adequate. Rather, Krugman is giving us back only his own local culture, the culture of mainstream North American economics learnt at Yale and MIT, and which was based initially on a now out-of-date nineteenth-century physics that emphasizes maximization and equilibrium, and later in the postwar period on the integration of economics within the US military-industrial complex.

## VI Conclusion

In conventional histories of locational analysis (Blaug, 1979; Ponsard, 1983), the trajectory is relentlessly progressive. If Krugman sees farther than Garrison, it is because he stands on his shoulders, just as Garrison stands on the shoulders of von Thünen. That this is possible, suggest such histories, is because of the inexorable refinement in, and expansions of, mathematical technique, which doubles for rationality itself. So, while von Thünen is still dealing with geometric means, Garrison is wrestling with eigenvectors and eigenvalues, and Krugman is working through

Fourier transforms. Likewise, the very technology used to undertake such mathematical manipulations is itself progressively altering, permitting advances that could not be imagined in previous generations. Von Thünen handwrites and hand-calculates in his voluminous journals, Garrison has his students patch-wiring early computers, and Krugman goes everywhere with his laptop, 'a technology that lets me produce a paper – equations, simulations and all – in a hotel room over a weekend . . .' (Krugman, 1995b: 37).

The purpose of this paper is to disrupt such a progressive history. It is too good to be true, whereas the history of locational analysis is too true to be good. Instead, my history of locational analysis emphasized the halting, local, contingent factors that shaped it, rather than universal ones such as the logic of mathematics. This is not how most of the protagonists themselves thought, and think, of their work. They believed, and believe, that they had found in mathematics an ineluctable principle that would guarantee the truth. My contention, however, is that the principles regulating their work, rather than reflecting suprahistorical truths, are local in conception, reflecting the particular circumstances of their origin. Furthermore, the duration of those principles, that is, how long people were willing to continue using and elaborating them, to pass them on and to defend them, is also a social (and geographical) process, and has as much to do with local context as any inherent quality of the principles themselves.

In making this argument, I am denigrating neither the intellectual worthiness of locational analysis nor those who practise it. All of the people I discussed are enormously bright, imaginative, intellectually vibrant and creative. At issue, however, is the precise nature of this creativity. In the rationalist view, creativity involves adhering to a pre-established mathematical logic, and following it through to its inescapable conclusion. In contrast, my argument is that there is more to creativity than cranking through equations. That is a necessary skill, too, but there is always more going on, which I tried to illustrate in this paper.

Peter Haggett (1965: 310) concludes his book on locational analysis by writing 'Although we may intuitively shy from a system which threatens to reduce "Ophelia loves Hamlet" to  $H \leftarrow O$ , the rigour of the axiomatic method has a powerful attraction'. The power, and, more particularly, the critical role and practical importance that the axiomatic method in the form of locational analysis has played in the history of economic geography are undeniable. At the same time, I have argued in this paper that we must shy away from reducing that history to the axiomatic method itself. Just as *Hamlet* is so much more than  $H \leftarrow O$ , so the history of locational analysis is much more than numbers and equations. Hamlet says: 'There are more things in heaven and earth, Horatio, than are dreamt of in your philosophy.' The same holds true of the dreams of a rationalist philosophical account of locational analysis.

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and argument. The paper was first given as the Humboldt lecture, Catholic University of Nijmegen, April 2001.

## Notes

1. Allen Scott (1976: 106) pushes the date back farther, arguing that von Thünen's work 'is merely derivative' of the earlier writings of Sir James Steuart, an eighteenth-century Scottish political economist.

2. Germane to this paper, the same point has been made for mathematical reasoning more generally. It, too, is locally constructed. David Bloor's (1976; 1983) arguments on this point are lucid and compelling. I have also tried to make a similar argument within the context of human geography's quantitative revolution (Barnes, 1996: Chapter 6; 2001b).

3. Krugman (1995a: Chapter 3) does recognize the metaphorical nature of modelling (as also did Haggett; Haggett and Chorley, 1967). Yet he fails to realize, as again did Haggett, that accepting the metaphorical nature of models leads not to rationalism but to the local knowledge view of scientific practice (seen in Hesse's, 1963, and later Bloor's, 1976, work, and discussed in economic geography by Barnes, 1996: Chapters 4 and 5). By definition, metaphors are never equivalent to the truth – Rorty (1991: 13) says that they are a 'voice from outside logical space' – and they gain purchase by resonating with the concerns of a particular local context. Moreover, it is hard to know in what sense Krugman is using the word metaphor, given that any metaphor he employs, such as the 'iceberg model', always ends up as a series of mathematical equations. Of course, one could argue that mathematics itself is a metaphor, but this is not his claim. Revealingly, he says that while 'ideas that come from people who do not write formal models may have rich insights. . . . Strangely, though, I can't think of any' (Krugman, 1995a: 88).

4. Mirowski is perhaps now the best known of a growing group of economists who are concerned to provide theoretically sophisticated histories of their discipline, often drawing inspiration from science studies (as is the case in this paper). Examples can be found in the journal *History of Political Economy*, and in various collections such as Backhouse *et al.*, 1998; Davis, 1998; Morgan and Rutherford, 1998.

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