

The rise (and decline) of American regional science: lessons for the new economic geography?

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Abstract

Regional science weaves in and out of the story of post-war economic geography. The vision of one man, the American economist Walter Isard, regional science represented the first systematic attempt to further joint work between geographers and economists. Within this context, the tasks of the paper are twofold. The first is to provide an interpretative history of the rise of regional science, and to a much lesser extent its decline. The interpretative framework derives from science studies, and in particular the work of Bruno Latour. The history is based on archival material and interviews. The second is to speculate briefly on the implications of both the interpretive framework used in the paper, and the history of regional science told, for the new economic geography that similarly attempts to convene discussions between economists and geographers.

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1. Introduction

The intellectual exchange between economics and economic geography that this journal seeks is not new (Arnott and Wrigley, 2001). As early as 1910, George Chisholm, possibly the first ever Anglo-American economic geographer, argued for the incorporation into economic geography of precepts drawn from the German location school of economics in order to ascertain ‘the geographical relation of the market to the seats of industry’ (Chisholm, 1910; see also Wise, 1975, p.2). Thirty years later Harold McCarty, an economic geographer at the University of Iowa, attempted to bring the two disciplines into conversation on the premise that ‘economic geography derives its concepts from the field of economics, and its method largely from the field of geography’ (McCarty, 1940, p.xiii). The most systematic attempt to foster interchange between economics and geography, though, and which not only anticipates but also enters into the new economic geography that dominates contemporary discussions in this journal, is American regional science (on the relation between regional science and the new economic geography, see Krugman, 1995; Isserman, 1996; Fujita, 1999). Founded by Walter Isard, an economist with a PhD in economics from Harvard, regional science was formally institutionalized in 1954, and marked by the use of formal neoclassical economic theory and rigorous statistical techniques in representing and explaining a space economy.

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During its heyday from the late 1950s to the mid-1970s, regional science was a robust, expanding, and vibrant field of inquiry. It drew money from non-profits like the Resources for the Future, Inc., and RAND Corp., as well as various branches of the US government including NASA, the Departments of Interior and Energy, and the Office of Naval Research. It proliferated internationally as well as any successful multinational corporation of the time, establishing branch plant associations in Europe in 1961, and in Asia in 1963. And most importantly for this paper from the beginning, and maybe even before the beginning, regional science enticed economic geographers, forging important linkages to the discipline. Geographers like Michael Dacey, Duane Marble, John Parr, Allen Scott, and Julian Wolpert taught regional science at Penn, while students like Michael Dear, Ken Denike, John Seley, Michael Teitz, and even Doreen Massey and Neil Smith studied there. Furthermore, in the history of regional science according to Isard (2001, p.421, fn.3), it was discussions at the early 1950s regional science meetings that Edward Ullman and William Garrison attended that inspired the analytical turn in human geography in the first place, producing among other things increasing numbers of economic geographers with the potential to connect intellectually with economists.

The purposes of this paper are twofold: first, to examine the rise, and to a much lesser extent the decline of regional science; and second, to provide a brief interpretation of that history for the project of 'foster[ing] communication between "economic geographers" and "geographical economists"' central to the *Journal of Economic Geography* (Arnott and Wrigley, 2001, p.2). Both the history and interpretation rely on the perspective of science studies, an interdisciplinary body of work seeking to understand the practices of science and social science in terms of the social context in which they are situated rather than a consequence of the operation of universals such as rationality, truth, and progress (Hess, 1997; Barnes, 2001a). Science studies is often controversial, but it is now well represented in histories of both geography and economics (see respectively, Livingstone, 1992; Mirowski, 2002), and even in economic geography (Barnes, 2001a, 2002).

The paper is divided into four unequal parts. First, by drawing upon Bruno Latour's work in science studies, I set out a general framework for understanding the relation between social context and the particular forms taken by science such as regional science. In particular, I describe Latour's model of 'five loops' involving the circulation of people, institutions, ideas and objects that come together to define specific scientific practices and modes of representation (Latour, 1999a, pp.99–100). Second, drawing upon interview and archival material I provide a brief account of the emergence of regional science up until 1960 when Isard and his students publish the foundational volume, *Methods of Regional Analysis* (Isard et al., 1960), that is secure enough to anchor the discipline for at least two decades. Third, in the longest section, I use my historical account and Latour's model of the five loops to understand the relation between social context and scientific practice for the development of regional science. Finally, I use Latour's five-loop model to understand regional science's recent decline, and which I then use to speculate more generally about the difficulties attendant on any engagement of geography and economics, and represented currently by the new economic geography and this journal.

2. Science studies

As an institutionalized field, science studies goes back at least to 1966 with the establishment of the Science Studies Unit at the University of Edinburgh, and as a set

of practices it goes back even farther to writers like Kuhn, Mannheim, Durkheim, and even Marx (Hess, 1997). It is also important to note that science studies is written in the plural, signifying a diversity of both viewpoints and approaches (and reflected in its sometimes-fierce internal debates, e.g. between Bloor, 1999a,b and Latour, 1999b).

If there is a common thread to science studies, it is the assertion that scientific knowledge is shaped by the contingent local circumstances in which scientists carry out their practices that go all the way down to the very truth claims of the scientists themselves. To use Haraway's (1991, p.191) phrase, there are no 'God tricks', the idea that it is possible to have 'vision from everywhere and nowhere'. As a result, the task of science studies is to understand how truth claims are made in a Godless world; that is, to understand both how contingent local circumstances make truth as it is, and why it sticks at certain times and places and not others. What was it about the context of 1950s America that produced the kinds of truths regional science were so good at generating, and why did they have resonance then, influencing inter alia the course of human geography, but now they seem to have lost much of their purchase? And even with the launch of this journal in 2001, interest in regional science has not returned. So, why did the truths of regional science stick early on, but not later?

Useful for answering this question are the writings of perhaps the best-known figure within contemporary science studies, Bruno Latour (1987, 1993, 1999a). His work provides a general explanatory scheme for understanding the relation between science and society, and a suggestive grid for prosecuting historical empirical investigations of particular cases such as regional science.

A central theme throughout Latour's writings is the close link between social context and scientific practice. In *Pandora's Hope* (1999a, p.80), he writes 'the notion of science isolated from the rest of society [is] as meaningless as the idea of a system of arteries disconnected from the system of veins . . . [It is] the rich vascularization that makes the scientific disciplines alive'.

Pandora's Hope presents two different models of the connection between science and society (Figure 1). The first, and which Latour rejects, envisages a hard core of scientific practice surrounded by a soft periphery of social context. In turn, this core-periphery model produced two competing accounts of the relation between science and society that dominated debate in the philosophy and sociology of science in the 1970s and 1980s. In the first, the soft periphery is conceived merely as the context of discovery, making no difference to scientific practices per se, and which are explained by an invariable internal rationalist logic. In the second, the soft social periphery is treated as the explananda, externally determining the interior hard core. The first type of explanation is known as internalism, and the second externalism.

While the relationship between science and society is conceived quite differently in these two explanations, for Latour the more important point is the common representation in both that society and science are pre-existing spheres, already formed. In particular, in neither explanation is science and society constituted through interaction. Or, to use Latour's earlier metaphor, missing is the 'rich vascularization' between the two.

But it is rich vascularization that Latour seeks, and possible to represent using science studies. He writes, 'the project of science studies . . . is not to state a priori that there exists "some connection" between science and society, because *the existence of this connection depends on what the actors have done or not done to establish it*. Science studies merely provides the means of tracing the connection *when it exists*' (Latour, 1999a, pp.86–87, original emphases). The task for Latour is to follow actors as they

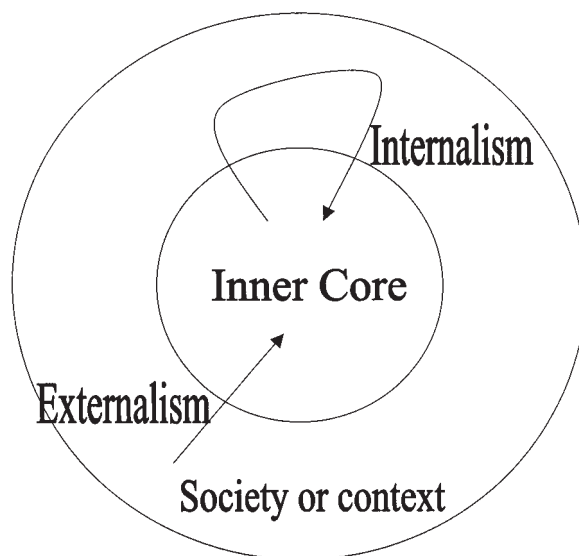


Figure 1. Redrawn from Latour (1999a, p.92). Reprinted with kind permission of Harvard University Press.

circulate, that is, course through veins and arteries, and as they connect (sometimes) science and society.

Latour's second model of the relation between science and society attempts to represent that circulation and coursing. He calls the making of connections through circulation 'translation' (Figure 2) that 'consists of combining two hitherto different interests . . . to form a single goal . . . Even if the balance is equal, neither of the parties . . . will be able to arrive at exactly [their] original goal. There is a drift, a slippage, a displacement which, depending on the case, may be tiny or infinitely large' (Latour 1999a, p.88). In Figure 2, the drift, slippage, and displacements of translations are represented by the no-entry symbols; they are the points where attempts to bring together different interests comes to a halt. But cessation might only temporary, with the blockage itself later re-translated, and the process moving on again. An example, one that I will elaborate in detail below, is regional science. Initially it has a difficult time in the mid-1960s in persuading British geographers that there was a need for the new discipline. Translation came to a stop. But new momentum was added when Isard sent Allen Scott from Penn to Britain in January 1967, and who became in effect regional science ambassador for the UK. In turn, Scott was able to re-cast regional science—to retranslate it within the British context—and with that a chain of successful translations began to unfurl once more.

There is one other point about Figure 2. Latour makes a distinction between the exoteric and esoteric. They are the end points of translation between the most hard-core science writings—for example, Isard's formalist rendering of general location theory such as equations and a map of population potential found in his 1956 book *Location and Space Economy* (Isard, 1956, pp.66–67; Figure 3)—and the softest piece of journalism such as a cartoon from *The New Yorker* published in the same year (Figure 4). Important for Latour are not these extreme points but the connections, the chain of translations in between. What connects a cartoon from *The New Yorker* with the equations and maps of regional science? What connects society with science? Looking

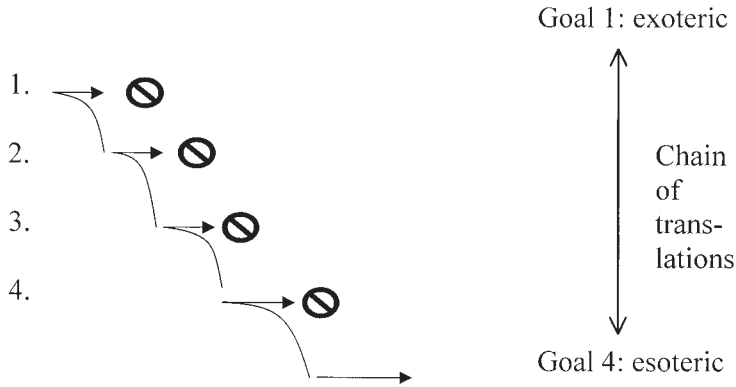


Figure 2. Redrawn from Latour (1999a, p.92). Reprinted with kind permission of Harvard University Press.

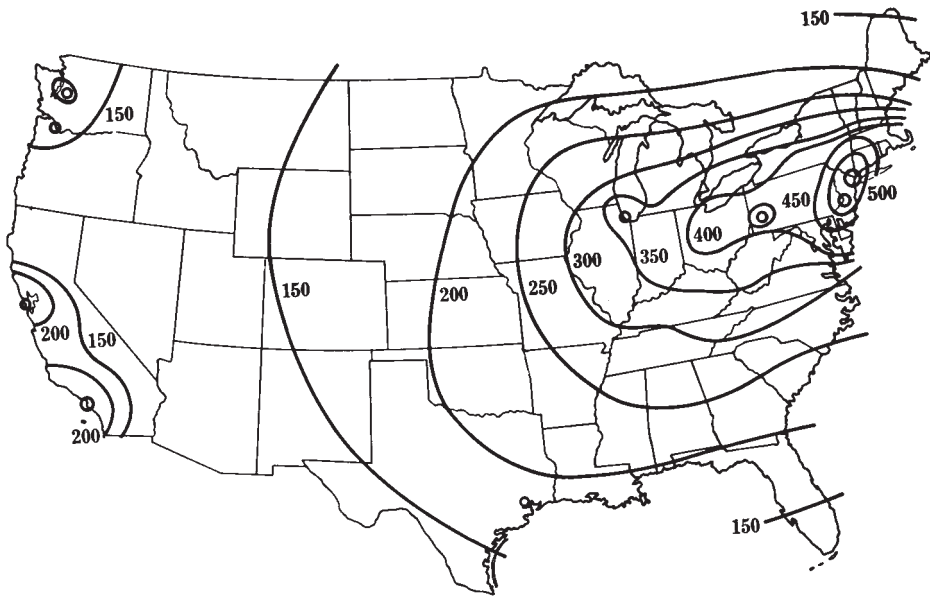


Figure 3. Contours of equal population potential for the United States, 1940. *Location and Space Economy* (Isard, 1956, pp.66–67). Reprinted with kind permission of Walter Isard.

from only the two ends themselves, from the two pieces of paper—one of equations and a map, the other of a cartoon—creates bewilderment and bafflement. But once one moves off the ends, and begins moving along chains of translation, however implausible or unexpected, one begins to get somewhere. One sees rich vascularization.

Scientific activity for Latour, then, is entirely interlaced with the social, and vice versa, with elements moving in and out, and circulating in-between. In providing his case study of the relation between the French nuclear physicist, Frédéric Juliot, and Raoul Dautry, the French Minister of Armaments at the time France was defeated by the Nazis in 1940, Latour offers a general map of such circulations and translations (Figure 5), and to replace

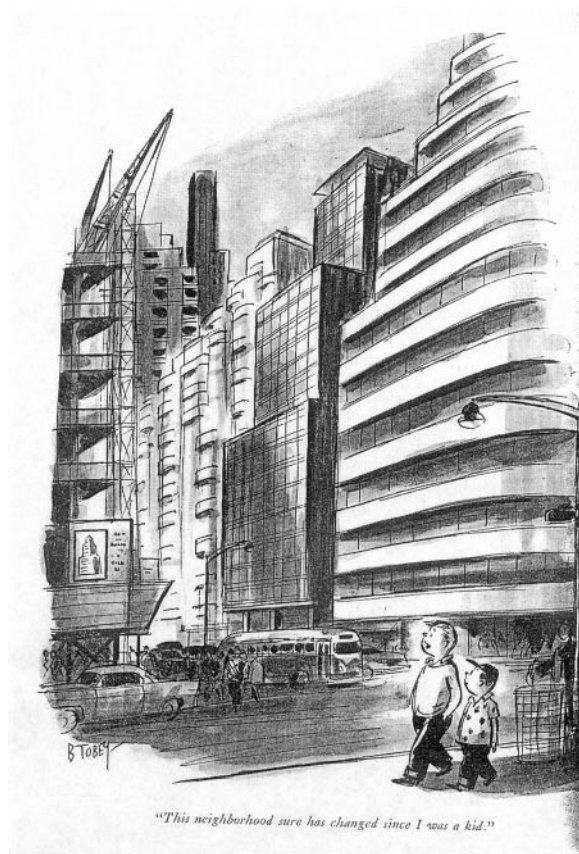


Figure 4. ‘This neighbourhood sure has changed since I was a kid’. Cartoon from *The New Yorker* (1956). Reprinted by kind permission of *The New Yorker*.

the core-periphery rendering of Figure 1. Four circuits—instruments, colleagues, allies, and the public—come together in a fifth as links or knots. Briefly, instruments is used very broadly to include the means by which the world is mobilized and engaged; colleagues or autonomization means how a discipline or profession becomes independent and forms its own criteria of evaluation and relevance; alliances and allies are the various people and groups that must be persuaded to join the project in order that it can develop at any scale and multiply; the public is the world outside the academy; and the links and knots are the points where the four circuits of objects, people, institutions and the wider world come together, and are temporarily held in place.

I intend to use Latour’s map for two purposes. First, and primarily, I will illustrate the rich vascularization between regional science and its social context especially around its origins, and subsequent early development. To do so I draw upon archival materials, specifically papers and correspondence of Walter Isard found at the Kroch Library at Cornell University (and where Isard is Professor Emeritus of Economics), as well as interviews I conducted with Allen Scott in March 1998, and with Walter Isard in June 2000. I also use archival materials of the Regional Studies Association housed at the London School of Economics. Second, I make use of the map’s normative claims about

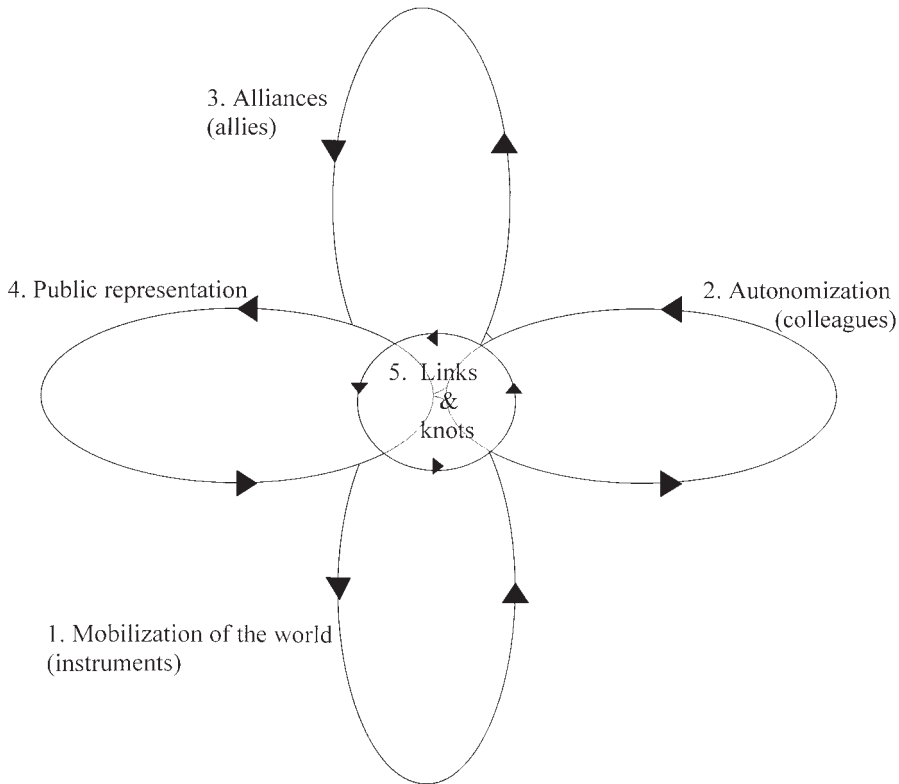


Figure 5. Redawn form Latour (1999a, p.100). Reprinted with kind permission of Harvard University Press.

sustainable scientific inquiry; that is, its suggestion that the continued reproduction of science is predicated on uninterrupted flows around the five circuits, and its obverse, that a blockage to such flows creates crisis. I will use this idea to try to understand both the subsequent decline of regional science, and more generally, the potential difficulties in launching another discipline, the new economic geography, that like regional science attempts to link systematically economics and economic geography.

3. Early regional science: a short history

It is tempting to see regional science as emerging fully formed out of the head, Zeus-like, of one man, Walter Isard. Certainly, the powerful impress of Isard's vision marks all parts of regional science's incarnation.

From Dutch Quaker Reform roots, Isard trained in mathematics and later economics at Temple and Harvard Universities, and for a brief period at the University of Chicago, in the late 1930s and early 1940s (Boyce, 2003). Influenced at Harvard by Alvin Hansen, the American popularizer of Keynes, and Abbot Usher, an economist with the unlikely interest in location theory, Isard completed his PhD in 1943 on the relationship between transportation development and cycles of urban construction. A conscientious objector during the war and assigned to the Civilian Public Service, Isard carried out alternative

duties as an orderly in a psychiatric hospital in Connecticut. It was there during graveyard shifts that he translated into English some of the great German location theorists—Weber, Predöhl, Lösch—who would be so important for his later regional science project. In fact, in 1948 Isard wrote to Edgar Hoover offering him those translations, saying ‘paradoxically enough, when I used to take care of anywhere from 50–200 inmates of an insane asylum on the night shift I kept sane by doing Lösch’ (Isard letter to Hoover, 12 January 1948, file 3939, Box 11).

Following a social science research council fellowship at Harvard, in 1949 Isard began working with Wassily Leontieff to develop regional input-output models. During this late 1940s period, he also made contact with Edward Ullman who taught regional planning at Harvard. Ullman was to become a critical entrée for Isard into geography by introducing him to among others Chauncy Harris, a fellow graduate student of Ullman’s at the University of Chicago, William Garrison, who was to be Ullman’s future colleague at the University of Washington, and Edward Ackerman, and with whom Ullman had worked at the Office of Strategic Services during the war. In turn, Ullman, Harris, Garrison, and Ackerman, were critical in launching geography’s own quantitative and quantitative revolution in the second half of the 1950s, and which drew energy and momentum in part through its association with regional science.¹

In 1953, Isard left Harvard to become an Associate Professor of Regional Economics at the Department of City and Regional Planning at MIT (Boyce, 2003). The Economics Department at MIT that included Paul Samuelson and Robert Solow was already ‘the brash self-confident face of the “new” mathematical economics’ (Mirowski, 2002, p.226). In particular, Samuelson’s 1947 *Foundations of Economic Analysis* pioneered and upheld the purity of a mathematical economic rationality (Samuelson, 1998). Isard’s emerging regional science project during this period clearly played off this celebration of mathematical economics, and more generally the inclination of American social science towards mathematical modelling, and which was propelled often by Cold War military concerns (see Mirowski, 2002, ch. 6, and Nasar 1998, chs 12 and 13 on the connection between RAND, the Cold War, and mathematical modelling). That said, there doesn’t appear to be a direct connection between Samuelson’s *Foundations* and Isard’s project; indeed, Isard (1956, p.25) criticizes it for assuming ‘all factors and producers, commodities and consumers, are . . . congregated at one point’. Moreover, Isard also recently said that he found his three years at MIT frustrating (Isard, 2001, p.417).

In terms of the institutional development of regional science, however, that period was catalytic because during it the first conference of the Regional Science Association was convened at the Sky Room, Hotel Tuller, Detroit, 27–29 December 1954. There were 45 separate participants including representatives from a wide range of social sciences,

1 Geography’s own quantitative and theoretical revolution was partly a response to the same broad post-war social factors that gave birth to regional science: a fascination with science and technology, a move within especially American social sciences to quantification and mathematization, and a concern with instrumental rationality to solve practical urban and regional problems brought about by economic growth (Barnes, 2001b). A number of geography’s initial ‘revolutionaries’ were also appalled by the sorry intellectual state of their discipline (also a concern of Isard, see below), and characterized by what they thought as shallow description, trivial pursuits, and poor scholarship (see Gould’s, 1979, retrospective indictment). The abstract explanatory rigour, incisive logic, and unimpeachable standards of science would be a scouring antidote. One should also note that while geography’s quantitative and theoretical revolution drew sustenance from regional science, it was always more empirically inclined and less concerned with abstract mathematical theoreticism, and in this sense was a parallel movement rather than integrated within it.

levels of the state, business, and non-profits.² While there was earlier scepticism expressed about the project, particularly by some geographers—the Harvard geographer, Derwent Whittlesey (Whittlesey letter to Isard 24 August 1954, File 3959, Box 27), wrote to Isard in 1954 saying ‘I cannot be at all enthusiastic about your suggestion that a new association be formed . . . There already exists a forum [geography,] for discussing regions in the broadest possible manner both orally and in print’—Isard was sufficiently buoyed by the Detroit meeting to set up ‘a skeleton type of organization’ (Isard, 1955, p.xv), and to make the conference an annual one. In addition, Isard required each participant to bring 200 mimeographed copies of their paper that he later collectively bound and sold for a dollar a copy, thereby also launching the first (and oldest running) regional science journal, *Papers and Proceedings of the Regional Science Association* (re-titled in 1991, *Papers in Regional Science*).

An interesting sidebar is the origin of the name regional science. This was not Isard’s first choice. He wanted ‘spatial science’ or ‘spatial analysis’. But as he writes, ‘given the tremendous impact of the path-breaking exploits in outer space [around the same time] (*Sputnik* in particular), we judged that there would be great difficulty in distinguishing our efforts from the outer space activities of physical scientists and engineers. We chose regional science as the next best term’ (Isard, 2001, pp.418–19). Certainly, traditional scientific attributes are apparent in Isard’s seminal volume, *Location and Space Economy*, published in 1956, just before he left MIT (the publication of which was not without its own struggles, see Isard, 2001, p.421, fn. 4). Couched in mathematics, it is a highly formalized theoretical synthesis drawing upon German location theory and neoclassical economics, and outlining an agenda of empirical investigation. It became the blueprint for the regional science project, and in particular, it underlay what became regional science’s most insistent claim: that a spatial plane be added to ‘the wonderland of no dimensions’ (Isard, 1956, pp.25 and 26) inhabited by economists.

At the same time, that book served to mark the boundary between regional science and economic geography. In Isard’s vision, economic geographers would be the hewers and drawers of data, and regional scientists would provide the analytical acumen through the kind of mathematic theorizing that Isard was so good at (Isard, 1990a, pp.304 and 5). For Isard this division partly reflected the inherent nature of the two disciplines—‘Geography was primarily fact-oriented’ (Isard, 1987, p. 157)—while regional science was a science defined by ‘the intention to apply concerns of rigorous techniques of investigation to analysis and to develop theoretical structures and concepts of general applicability’ (Isard and Reiner, 1966, p.1). And partly it reflected the dire nature of economic geography of early 1950s. In a retrospective review in 1979, Isard (1979, p.9) recalls, ‘Geography was not at a high level, academically speaking, being primarily concerned at that time with cartography, the gathering of data and their simplest processing, with little concern for analysis, except of the simplest type. . . . No attempt was made on the part of economic geographers to fuse . . . location[al] factors into a simple cost calculus. I remember the resistance to my attempt to do so in a paper presented at the meeting

2 In terms of disciplinary affiliation, the majority of the participants were economists, geographers, and planners, with a scattering of political scientists and sociologists. The non-profits included participants from RAND and Resources for the Future, Inc. There were no scientists present, but one physicist was associated with regional science. John Stewart, an astronomer at Princeton, was a collaborator with William Warntz, a Research Associate at the Regional Science Research Institute during the late 1950s, and practitioner of social physics (renamed by him ‘macrogeography’, Barnes, 2001b).

of the Association of American Geographers in Worcester, 1950. When I submitted the paper for publication in *Economic Geography*, the leading journal in that field, I had to fight vigorously to persuade the editors to permit me to retain the footnotes and citations essential to scholarship'. Or as he reflected at the time to the Michigan geographer, Robert Hall, 'It seems to me that if geographers were to spend more time and effort on theoretical analysis, they could derive much more meaning and significance from their descriptive and empirical material' (Isard letter to Hall, 12 July 1951, File H, Box 13). Of course, it was in providing that theoretical analysis to which *Location and Space Economy* aspired.

In 1956, Isard moved to the University of Pennsylvania as part of a larger recruitment drive by the chair of the Department of Economics, Irving Kravis. Isard's condition for appointment was the establishment of a PhD programme in Regional Science. Reflecting both Penn's anxiousness and burgeoning student enrolments, Isard's request was granted within two weeks of him making it. Two years later in 1958, the Department of Regional Science was officially founded, with Isard as chair, and during that same year, the *Journal of Regional Science*, co-edited initially by Isard and his student Ben Stevens, was inaugurated (Isard and Stevens were also the first two faculty members of the Department, although Stevens did not finish his thesis until 1959, and at MIT; Miller, 1998). Two years after that, the first PhD of the Department, William Alonso, graduated. And in that same year, 1960, Isard published the second foundational piece of regional science writing: the collective tome, *Methods in Regional Science* (1960). Isard et al. (1960, p.vii) had come to recognize that his earlier 'general theory and space economy is of little direct use in treating the concrete problems of reality', and so this second volume became a primer on all the operational techniques a fully-fledged regional scientist would ever need to know when confronting the 'real' world of 1960s America. Together with his earlier text, *Location and Space Economy*, the new volume provided the twin pillars of regional science's success during the next two decades; a combination of the synthesis of formal location theories 'into one general doctrine' (Isard et al., 1960, p.vii), and a set of sophisticated techniques for manipulating primarily geographical empirical data.

4. The five loops of regional science: a Latourian interpretation

That success, I want to suggest following my argument in the first section, was not a consequence of the inherent correctness of regional science's propositions and analysis—the internalist position—nor was it simply the result of a causally effective surrounding social corona—the externalist position. But it was a consequence of 'rich vascularization', and represented by the five loops identified by Latour (Figure 5).

4.1. Mobilization of the world (instruments)

The first of Latour's loops is the mobilization of the world by which he means through a series of translations bringing the non-human world into human discourse. As he writes, 'It is a matter of moving toward the world, bringing it to the site of the controversy, keeping it engaged, and making it available for argument' (Latour, 1999a, p.100). This is accomplished by carrying out a series of translations, where the objects of study are increasingly brought under control and domination: 'instead of moving around the objects, scientists make the objects move around them' (Latour, 1999a, p.101). In science, various forms of inscription such as drawing diagrams, writing equations, constructing tables, or composing technical prose achieve this.

In particular, the process of translation through inscription produces what Latour calls immutable mobiles, primarily books and papers. They are mobile in that they can be physically transported, and immutable in that once inscriptions are made they remain unless purposefully destroyed or accidentally lost. Further, as artefacts, books possess the virtue of reducing what they portray to the same form, a flat page. As Latour (1990, p.46) puts it, once inscribed on a page phenomena and events possess the ‘same optical consistency and semiotic homogeneity’. We see this in Figure 3. A set of diverse events around the distribution of population in the United States in 1940—immigration and emigration, births and deaths, marriages and divorces—are translated and enrolled to further the interests of Isard’s regional science project. Through the use of numbers from the US Census, equations of potential taken from physics, and machines like slide-rules and mechanical calculators, specific demographic events in far-flung places in the US are made to appear on an eight-and-a-half by eleven-inch piece of paper, and bound between the covers of a book called *Location and Space Economy*. In doing so, Isard mobilizes the world, allowing him to move around these objects, rather than the other way around. After mobilization, Isard is able to order and control, shuffle and manipulate, varied events and things, thereby imbuing him with an extraordinary degree of certainty about the phenomena that are inscribed.

In many ways, the *raison d’être* of regional science is this kind of mobilization. Certainly, this is what *Methods in Regional Science* was all about. As Robert Kuenne (1990, p.6), one of Isard’s former students, writes, *Methods* ‘constituted an important user’s manual . . . for an infant field . . . Of Walter’s works, it was probably the most influential in shaping the boundaries of regional science, orienting its empirical research analytically and defining new theoretical directions in the 1960s’. The various techniques that *Methods* laid out—the gravity model, linear programming, industrial impact analysis, input–output models—were practical, how-to-strategies to curb and tame the variegated unruliness of the world. Once put into a table, or an equation, or a diagram, the world was made to work for the regional scientist, allowing it to become the basis for an argument to further their interests.

In making the world work for them, regional scientists also engaged in a geographical move. They shifted from sites where the world is experienced in the raw—new factories in Puerto Rico, a new dam on the Colorado River, iron and steel plants in the New York–Trenton–Philadelphia region, nuclear power plants in California—to sites like Dietrich Hall, and later the McNeil Building, University of Pennsylvania, where Regional Science was housed, and where the task of ensuring ‘optical consistency and semiotic homogeneity’ was carried out.

4.2. Autonomization (colleagues)

The second loop goes to regional science’s greatest success: its institutionalization. Termed ‘autonomization’ by Latour (1999a, p.102), it ‘concerns the way in which a discipline, a profession, a clique, or an “invisible college” becomes independent and forms its own criteria of evaluation and independence’.

The autonomization of regional science is interesting because of both its recentness and transparency. One cannot argue that regional science was always there, somehow antedating itself. Rather, the autonomization of regional science derived from conscious effort and management, galvanized by the ferocious energy and keen ambition of Walter Isard. So, how did Isard shape its particular institutional form?

From the late 1940s, Isard worked tirelessly towards its formation. As early as July 1950 along with the UCLA business economist Philip Neff he wrote to Pendleton Herring, the director of the Social Science Research Council, requesting that a committee be formed to examine regional economic studies (Isard and Neff letter to Herring 28 July 1950, File 3539, Box 29). The request was turned down (Pendleton Herring letter to Isard, 13 February 1951, File 3539, Box 29) on the grounds that ‘the objectives and program . . . were not sufficiently or coherently specified’. But this didn’t deter Isard. By dint of unflagging correspondence and travel throughout the early 1950s, Isard organized sub-meeting after sub-meeting within national conferences of different academic organizations to discuss with interested members the possibility of regional science (see the detailed listing and chronology in Boyce, 2003). Those organizations included the American Economic Association, the American Sociological Association, the American Political Science Association, and the Association of American Geographers. Isard’s Newsletter of the Committee of Regional Economic Studies of 19 August 1952, for example, reports an informal meeting that he held earlier in the same month with geographers at the AAG conference in Washington, DC at the Hotel Statler, and which included Edward Ullman, Harold Mayer, Robert Platt, and Preston James. It was through this strategy of carefully preparing the ground across a range of social sciences that he was able to ensure the multidisciplinary participation found at least at the early Regional Science Association conferences once they began in 1954.

Of course, the 1954 conference was just the beginning. To achieve disciplinary status required much more. It needed a programme of education, and which began with the 1956 PhD programme at Penn, and later the formation of the Department in 1958. Education also proceeded through the conferences themselves, and at least in the early 1960s in North America through the Regional Science Summer Institutes held at Berkeley. In fact, it was the very first Berkeley institute that Peter Haggett (1965, p.vi) ‘gate crashed’ in July 1962 that proved so momentous for human geography, contributing to perhaps the best known volume emerging from geography’s quantitative and theoretical turn, Haggett’s (1965) *Locational Analysis*. It also required money, and larger sums than the dollar a copy charged for the *Papers and Proceedings*. Those larger sums were forthcoming partly through the granting bodies mentioned at the beginning of this essay, but mainly from the University of Pennsylvania that gave Isard (2001, p.418) a ‘budget unit’ that enabled him to recruit aggressively leading scholars in the field for faculty positions, and to provide fellowships for graduate students. As Teitz (2001, p.411) puts it, organizing regional science ‘took substantial resource. It was not a vast investment . . . but it was enough to make regional science happen’. Finally, it required a literature including venues for publication. Isard’s two early volumes were crucial immutable mobiles—*Location and Space Economy* sold more than 20 000 copies. Equally important were *Papers and Proceedings*, and later the *Journal of Regional Science*. Both serials—and the many more that subsequently emerged—took on a sociological role in institutionalization. Through the very performance found in their pages, regional science became real. One couldn’t deny its existence. It was no longer just an idea, an abstraction, but a physical reality. Pendleton Herring’s earlier dismissal of regional science could no longer cut it. Just look at the journals. They were the physical embodiment of the discipline’s objectives and programme. In addition, those journals performed another sociological role: they were outlets for publication for non-tenured regional science professors seeking tenure. In this sense, the circle is squared. Regional science at Penn is perpetuated in part by granting assistant professors of regional science

tenure at Penn on the basis of publishing in regional science journals published by Regional Science at Penn. This is not to derogate the quality of those publications, but only to point to the critically important sociological processes required to sustain regional science as an institution.

4.3. Alliances (allies)

The third loop revolves around alliances. As Latour (1999a, p.103) puts it, ‘groups that previously wouldn’t give one another the time of day must be enrolled in the scientist’s controversies’. Without garnering interest from others, a discipline would go nowhere. No one would read its works, students would not enrol in its classes, granting agencies wouldn’t fund its research, and University Presidents and Deans would not be interested in establishing department or hiring faculty. Enrolling allies is also clearly bound up with translation. To obtain an ally is to convince them that their goal, their interest, is best met by joining with you.

The process of garnering allies is always fraught, and there is no guarantee of success. Isard clearly initially failed to convince Pendleton Herring of the US Social Science Research Council to be an ally, and, as it turned out, Isard did not persuade sociologists or political scientists to stay the course of regional science in spite of the initial wooing.

Isard, however, forged an important and enduring alliance with geographers. But again, it wasn’t always a smooth relation. Early on, as noted, Whittlesey had strong reservations about the regional science project, arguing that geography’s regional tradition was a broad enough church to encompass regional science. Or another example, and which I will elaborate, is Isard’s attempt to establish a British section of regional science during the 1960s. There is initial resistance, but in the end, Isard mainly prevails, and an alliance is forged with at least some parts of British geography.

The background is the internationalization of regional science during the 1960s. In 1960, the constitution of the Regional Science Association is changed to allow for the possibility of national sections. This sets the stage for the first European conference that Jacque Thijsse organizes in The Hague in 1961 (Isard, 1990a, p.297). Within this context, Isard in the same year approaches the Cambridge economist, J. R. N. Stone, for help in forming a British section, but he is flatly turned down (Folder 3959, Box 7). Isard’s project goes into abeyance until early 1964, when there are stirrings of interest from G. P. Hirsch, who is at the Agricultural Economics Research Institute in the UK, and who in July of the year organizes a meeting at the London School of Economics. A committee is formed, but rather than becoming the British section of the RSA an entirely new organization is created, the Regional Studies Association.³ Furthermore, despite Isard’s consistent urging they refuse to substitute ‘Science’ for ‘Studies’ in their title. By late 1965, Isard

3 The emergence of the Regional Studies Association deserves its own separate paper. Unlike regional science, its origins were closely tied to central and local government regional planning in the UK, and initiated by the Labour government’s National Economic Plan (1965) administered by the Department of Economic Affairs (DEA). In fact, the sub-national organization of the Regional Studies Association mirrored the Regional Economic Planning Councils and Boards established by the DEA. Perhaps because of these very practical planning concerns, the Regional Studies Association was less interested in high theory—science—always plying a ‘more modest’ line as Peter Self, one of the founding members of Regional Studies said at one of the early meetings [RSA Research Meeting, no date, Folder Annual General Meeting and Conferences 1965, Box 15, Regional Studies Association] (see also Sharman, 1967; Wise, 1987).

gives up on further persuasion, and rejects their offer of formal affiliation (Folder 'British section', Box 7). For Isard this alliance will not work. Too much is given up.

In 1966, however, there is renewed interest in regional science especially by younger British geographers who are dissatisfied with a lack of scientific engagement by the Regional Studies Association. A fledgling British section is established that year, and in a strategic move by Isard, Allen Scott, a Regional Science faculty member at Penn, is sent to London for two years in January 1967, to solidify and bolster these efforts. Setting up a European regional office of the RSA based at the Bartlett School of Architecture and Planning, University College London, Scott becomes regional science's representative for Europe, organizing conferences, helping to set up new journal and publications, and spreading the word. In short, Scott's task is to create new allies within geography, and elsewhere.

That Scott came to occupy this position was partly serendipitous. Originally taking a degree at Oxford, Scott won a Fulbright travel fellowship to attend graduate school at Northwestern University in the US. After graduating, and supervised by Garrison, he took up his first academic position in 1965 in Regional Science at Penn. Two years into his career, however, he was informed that the earlier award of the Fulbright required him to go back to Britain, and he must leave the United States. At this point, Isard offered Scott the unique position I described. While paid by Penn, he would be based in London for two years, after which time he would be eligible to come back to Philadelphia to resume his original job.

Although the last part of that plan did not unfold as expected, the benefit for regional science of Scott's role in creating allies was quickly apparent. On 11 March 1967, Scott organized a 'bull session' at the Bartlett School on regional science for 40 participants, a combination of geographers, planners, and transport engineers. And in August of the same year, he organized a preliminary conference of the British section of the RSA that included papers by Alan Wilson, Richard Chorley, and Isard who flew in for the occasion. There is one other important alliance that Scott creates while in Britain, and that is with what are to become Pion publishers. As Scott (1998) tells it, in Spring 1968,

I was having lunch with David Harvey one day, and he said, 'I met this guy called John Ashby'. At the time, Pion was an electronics firm. They had an earlier engagement in the semiconductor industry. But they had sold out for a pile of money, and wondering what to do, and thought maybe they should get into publishing ... So, David Harvey gave me John Ashby's card. I called him and went around to find these two guys, one on one side of the desk, the other on the other side. And I persuaded them to publish [the papers from the Bartlett School Regional Science conference] as an experiment. They wanted to get into publishing and find some niches for themselves. I argued that Regional Science was a virgin niche, and their scientific ability would hold them in good stead.

The rest is history. Several volumes of what were called the London Papers in Regional Science were subsequently published, and in the following year, *Environment and Planning* was inaugurated under the editorship of Alan Wilson, and which at least in its formative years was, if not in name, a journal of regional science.⁴

4 One of the referees for this paper provides a slightly different account of Pion, and the origins of *Environment and Planning*. Their recollection is that Pion was never a high-tech company because although the two founders, John Ashby and Adam Gelbtuch (the 'two guys' on each side of the table), began Pion with the expectation of operating an R&D firm, none of the products they developed were commercially successful. Gelbtuch, though, who was Russian, had translated scientific papers for the Ministry of

4.4 Public representation

The fourth loop is the relation between regional science and the everyday public world in which it is carried out. For Latour this is maybe the most important of the loops, with the other three dependent upon it, and affecting ‘a lot of presuppositions of scientists themselves about their objects of study’ (Latour, 1999a, p.106). It is also the loop that is most difficult with which to deal in this paper given its enormous scope: 1950s and 1960s America, and all it implies about the Cold War, science and technology, Fordism, suburbanization, civil rights, the affluent society, and much more.

Let me make just two points. The first stems from Philip Mirowski’s (2002) *Machine Dreams* that critically reviews and analyses economic thought over the post-war period. Relying on archival documents, including letters, memos, and privately circulated mimeographs of scientists such as Norbert Wiener, John von Neumann, and Warren Weaver, and economists such as Kenneth Arrow, Leonid Hurwicz, and Tjalling Koopmans, Mirowski argues that key to the development of post-war economic theory is the formation of the US military–industrial complex, and the concomitant role of the computer. To simplify his argument massively: the increased role of the military necessitated that techniques and strategies be developed for military ends, including the allocation of resources within non-market settings. Such a task was given, among others to the economists who were expected to devise appropriate theories and techniques. It is also here that the computer becomes vital. It is not only an instrument that enabled complex calculations to facilitate particular strategic and allocation ends, but it became a metaphor for describing a new kind of economic theory based upon achieving a set of goals within a centralized command and control environment represented by the military, and its extension into government. For Mirowski, the culmination of that trajectory was Operations Research (OR), and including game theory, linear programming, general equilibrium theory, and systems analysis. Its most important site was RAND, inaugurated in 1948 in Santa Monica as the US Air Force’s think tank, and which commissioned large numbers of studies, both empirical and theoretical by the economists above. As Mirowski (1999, p.690) puts it, 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’.

My contention is that regional science is one of the precipitates that derive from that new locus within America of ‘economies’ and ‘the scientific approach to government, corporate management and society’.⁵ Certainly, regional science was concerned with developing within a spatial context, both urban and regional, the same kinds of

Defence when he was a graduate student at Imperial College, London. Given their lack of success as inventors, Ashby and Gelbtuch therefore decided to move into specialist publishing, translating foreign scientific papers for the English speaking academic world. The story is that it was such a profitable venture their accountant told them they needed to find new publishing outlets that would lose money. Also perhaps ‘apocryphal’ (as the referee puts it) is the story that John Ashby’s son came home one day saying that his geography schoolteacher said that there were exciting changes in geography relating to science and physics. Ashby went to see Peter Haggett, and the idea of the journal was born. It was originally to be called *Environment*, but there was already a journal of that name, so it became *Environment and Planning*. (For yet one more interpretation of the origins of *Environment and Planning*, see John Ashby’s obituary: www.pion.co.uk/ep/html/dra_obituary.)

5 There is the paradox, on the one hand, of Isard’s pacifism—he later founds Peace Science as a discipline—and, on the other, the argument made here that his work is also part of the military–industrial complex. There are

operational techniques found in economics. Ben Stevens' doctoral dissertation, for example, developed a spatial version of linear programming that was then used for metropolitan and regional planning. Central to that task was the computer, although even it could barely keep up with the sometimes inordinate demands put on it by Isard. For example, Isard's Philadelphia Region Input–Output Table contained 496 sectors, and created from 15 000 interviews. As Lahr and Miller (2001, p.xxxii) write: 'The model was so large that IBM took on the task of computing the Leontief inverse gratis (fortunately) as a computational exercise that could test the power of their machines'.

The second and related point goes to US post-war economic growth. Manus Chaterji (1999, p.342), a former student of Isard, writes that regional science was 'born in an auspicious time'. The scientific techniques and theories of regional science would have had little purchase had there not been issues of regional and urban expansion, and affiliated issues of transportation, energy and basic industry provision, and which were closely linked to an increasingly affluent and urban society. Isard's (1990a,b) papers in the 1950s are all about managing, and dealing with growth and expansion: in the airline provision, in nuclear energy, in iron and steel. While the military–industrial complex might form the backdrop to regional science's techniques and theories, the immediate applications were to burgeoning metropolitan regions, their infrastructure needs, and interconnections. Regional science touted itself as providing hands-on solutions to everyday American urban and regional problems (carried out especially by the Regional Science Research Institute founded in 1956).

4.5. Knots and links

The final loop is where it all comes together, or falls apart. As Latour makes clear, this loop is not at last the real thing, quintessential regional science. It may be at the centre, even forming its heart, but it cannot be sustained without the other four circuits. Cut off any of the loops, and they will all die.

For Latour, the interesting facet of this fifth loop is not philosophical, its ontological or epistemological status, but practical: how tight is it tethered to the other four loops? The tighter the knots, the stronger is the centre, and the sturdier is the science done, and the greater is its extension. This was certainly the case for regional science in the 1950s and 1960s. It was so impressive as a discipline because of the assuredness of the knots that were tied with its data, with its colleagues, with its allies, and the public world. As a result, it was able to move out into the world, and find a home on every continent, and in countries from Australia to Yugoslavia.

5. Unmaking and remaking disciplines

But such success was not to last. From the late 1970s, coursing and circulation became increasingly irregular and blocked. Angina and palpitations set in. In this final section,

two points. First, that while he may not have consciously wanted to support the military–industrial complex, the techniques he used such as game theory or linear programming emerged precisely from that intersection at institutes like RAND. Moreover, if Mirowski is right, the very possibility of their use in civilian life stems from the acceptance of a military centralized control and command mentality. Second, Isard contributed reports and presumably accepted money from some of the institutions that were an integral part of that complex. For example, he wrote a joint report for RAND in 1957 (Moore and Isard, 1957).

I want to use Latour's scheme to evaluate both the subsequent decline of regional science, as well as the later attempt by the new economic geography to take over, in effect, regional science's role in engaging economics and economic geography. In both cases, the evaluation is based on Latour's criteria of disciplinary success and failure, and implicit within his five-loop model. Success stems from the achievement of rich vascularization, unimpeded flows around the five circuits, while failure follows from clogged and severed vascularization, reduced, and incomplete flows.

5.1. The decline of regional science

On this basis, let me briefly describe the emerging impediments to regional science in each of the five circuits that then precipitate its decline. With respect to the first, mobilization of the world, difficulties surfaced ironically as Isard attempted greater and greater mobilization. In particular, Isard's ambition for large-scale modelling became so large they no longer fit on a eight-and-half by eleven inch piece of paper: 'optical consistency and semiotic homogeneity' were lost. In particular, Isard's penchant for worldwide systems analysis embracing multitude components of the social and physical world was so excessive that his project began to collapse under the weight of the density of relations he aspired to represent. There are early indications of potential problems as he tried in 1960 to construct synthetic models of regions that integrated 'Historical, sociological-political theory', 'Values (culture)', 'comparative cost', 'urban complexes', 'linear programming', and 'probability, gravity, [and] potential models' (Isard, 1990b, p.151). By the early 1980s added to the mix were peace and war, demographic events, ecology, and government policy (1990a, chs 13–15). There are limits to what can be represented even on a page, and Isard met them in his attempts at comprehensive, multi-regional modelling. A map of the population potential of the United States is one thing, 'An integrated multination model' quite another (Figure 6). The further consequence is that as the diagrams held an ever more tenuous relation to the world, rather than furthering the interest of regional science, they stymied it, paradoxically making the discipline appear disconnected from the reality it claimed it was so good at representing.

The second loop, autonomization, also became under stress. The first sign of difficulties was Isard leaving Penn for Cornell in 1979 over his desire to establish Peace Studies. This was followed in December 1993, by the Dean of Arts and Sciences, Rosemary Stevens, closing down the Department of Regional Science at Penn altogether. She did so, as she said, 'well aware that our Department of Regional Science ... has been the flagship department for the discipline' (quoted in Bailly and Coffey, 1994, p.38). This suggests that despite its history and international importance, Dean Stevens no longer believed that regional science was viable as an academic discipline. Like Geography at Harvard in 1948, it no longer cut it intellectually and had to go. But Geography as a discipline was well represented at other places in the US, and so there was a margin of safety for its survival. This was not the case for regional science that had failed to establish itself in many US universities—apart from Penn, programmes existed at Cornell, University of Illinois, and the University of Arizona. It is true that regional science journals remain going concerns, with *Papers in Regional Science* purchased by Springer in 1999, and the *Journal of Regional Science* bought by Blackwell in 1993. Earlier putative regional science journals like *Environment and Planning (A)*, however, now carry little regional science except perhaps as critique (Kramsch and Boekema, 2002).

it so has coursing and the circulation. Isard's post-war verities on which regional science was constructed have become increasingly marginal in a world of post-isms: post-industrialism, post-modernism, and post-Cold war. It is as if because Isard's American is not what it was, regional science cannot be what it was either.

The wider consequence of these circulatory problems is the unloosening within the fifth loop of knots and ties. As they have been undone, so has regional science. The sources of its strength—its mobilization of the world, its autonomization, its allies, and its public representation—have now become its weaknesses. Rather than making the discipline, they are progressively unmaking it.

5.2. The new economic geography

As regional science is being unmade, another discipline has begun to take over the intellectual terrain it once occupied, the new economic geography. Pioneered especially by the economist Paul Krugman (2000), the new economic geography, like regional science, upholds mathematical modelling including rigorous statistical testing, asserts the importance of including space within economic analysis, and as represented by this journal is keen to foster dialogue between economics and economic geography. In a sense, regional science is being remade. Indeed, some regional scientists, the best known example is Fujita, are now at the forefront of the new economic geography (Fujita et al., 1999). But can the new economic geography persuade economic geographers to become allies, and engage in exchange, as regional science once did? By applying Latour's evaluative model, it is not clear that it can.

With respect to mobilizing the world, the new economic geography faces two difficulties. First, for critics not enough of the world is mobilized. Instead, they complain of over theoreticism and a lack of engagement with empirics (Martin, 1999; Sheppard, 2000, 2001; Sunley, 2001). For example, while Krugman (1995, p.84) champions the Dixit–Stiglitz model as providing just the 'analytical trick' needed to present a rigorous, logically integrated causal model for the new economic geography, he also admits that it is 'completely unrealistic' (Krugman, 1995, p.60). Second, the venues for mobilizing that world—the immutable mobiles—are still scarce. There is this journal, and the Clark et al. (2000) volume has the potential to become an important immutable mobile (although so far its reception by geographers is disappointing, Agnew, 2002; Peet, 2002).

While individuals associated with the new economic geography such as Krugman are prestigious, offering kudos and credibility to the new discipline, there is no concomitant institution with prestige, or a programme of education. In contrast, these were key sources of regional science's strength. Isard may not have had the standing in economics of someone like Krugman, but he possessed unbounded energy to organize institutionally. Of course, this journal is part of the process of autonomization of the new economic geography, but it is only one part, and for the discipline to be sustainable, the other elements identified by Latour are necessary too. In particular, after more than 20 years of political economy, and the eschewing of modelling and quantification, what is required is a massive retro-education of economic geographers in formal theory and techniques if they are to participate in the new economic geography. Summer schools of the type regional science offered would be a beginning, but the only summer school for economic geography currently on offer in Anglo-America is entrenched in political economy (<http://www.wun.ac.uk/economicgeography/>).

Individual representatives of the new economic geography, such as Krugman, certainly travel around the globe, but they have not engaged in the process of building formal alliances. There are neither European nor Asian sections of the new economic geography, nor someone like Allen Scott who works to promote them. Furthermore, the only alliance seriously promoted is between economists and economic geographers. But at least on the economic geographers' side, there is deep suspicion about such an association by some of the discipline's leading exponents. Amin and Thrift (2000, p.8) talk about economic geography as the economist's 'prey', and which seems hardly a good attitudinal basis for disciplinary exchange. Certainly, there are always critics, but there is a stark contrast between what regional science achieved—with well-known economic geographers teaching and studying regional science at Penn—and the prevailing critical, even hostile, attitude towards the new economic geography of many economic geographers (see the responses to Amin and Thrift in *Antipode*, Volume 33, No. 2, 2001).

The issue of public representation is mixed. Krugman, in particular, through his columns in the *New York Times* is now a public intellectual, with the power not only to represent the world to non-academics but also to change it. It is unclear, however, to what extent his pieces are informed by the new economic geography. In this sense, there is a seeming disjuncture between his writings as a public commentator and his writings as a new economic geographer. One of the criticisms made by economic geographers about the new economic geography is that it fails to deal with the realities, problems, and issues of a turn-of-the-millennium spatial economy. Rather, critics complain of the theory being shop-worn, and out of date (Martin, 1999; Sheppard, 2001), that is, another case of being 'lost in time' (Bailly et al., 1996, p.157).

The upshot is that the knots and links of the new economic geography are at best slackly tied, if they are tied at all. Such criticism is perhaps churlish and premature. Isard, after all, spent many years preparing the ground before he formally inaugurated regional science. The wider point, though, and it is Latour's, is that intellectual movements, including engaging economists and geographers in communication, are not abstractions, residing in some empyrean realm, but to be successful must be prosecuted in the real world of material artefacts, social relations, institutions, political ambitions, and face-to-face contact. There must be coursing and circulating. It happened in regional science for a period. For it to happen in the new economic geography will depend on the same processes, implying that those loosely bound knots must be tethered much more securely if the project of the new economic geography, and its engagement of economics and economic geography, is to survive.

6. Conclusion

Until perhaps now regional science represented the most explicit and systematic attempt to engage economists and economic geographers in conversation. At least initially, those efforts were highly successful, and it was only later that waxing turned to waning. My argument, following Latour, is that the history of regional science is propelled by neither an internal basal logic, nor the social circumstances of its origin. Rather, it is shaped by the mutual relation between logic and context, and represented by flows of people, ideas, books, data, machines, mimeos and memos, telephone calls and telegrams. The task is to trace these agents as they course and disseminate, as they produce and reproduce, and which in a preliminary way I tried to do.

I also argued that Latour's scheme is not just a typology, a series of boxes to be filled, for fitting histories of scientific endeavours like regional science. His base metaphor of vascularization is also a judgment about the maintenance and sustainability of any scientific enterprise. Regional science is such a good case study because one sees the full arc implicit in Latour's scheme, from energetic beginnings to sputtering decline. Not that his model is teleological, with an inevitable trajectory of degeneration. For it is possible that regional science will rise again, its resurgence prompted possibly by a renewed intercourse between economic geography and economics sparked by the new economic geography. But as I also argued, so far that intercourse remains fragile, without many of the buttresses identified by Latour. All of this is to say that while the separate futures of economics and economic geography are assured (in both cases, coursing and circulation are vigorous), the future of the intermediate ground between them is not. This is odd. There are compelling intellectual reasons for interaction between economics and economic geography, and especially in an era of inter-disciplinarity. But perhaps this goes to Latour's central point: good ideas are never enough. They must be mobilized within a larger world that is material, social, political, and even economically geographical.

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