**What's wrong with American regional science? A view from science studies.**

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This paper overlaps with another paper of mine published in the Journal of Economic Geography, "The rise (and decline) of regional science: lessons for the new economic geography?" The latter is concerned with using the history of regional science to think through the prospects of the new economic geography, and represented especially by Paul Krugman's work. In contrast, this paper is concerned with making an argument about the decline of regional science based upon its universalist methodological position.

**Abstract**
Drawing upon archival material and primary texts, the paper argues that American regional science after early growth and success during the 1950s and 1960s waned because of an unwillingness to change its position in response to new circumstances. It was unwilling to entertain change because it believed it was a science with an unimpeachable method. In contrast, the paper argues that the early success of regional science had less to do with its scientific method than the local social and cultural context in which it was embedded. At first, regional science was able to forge tight connections to that local context, and consequently burgeoned. But as that context changed in the 1970s, American regional science failed to keep up, and instead continued to espouse what it thought were unchanging universal scientific nostrums. The result, though, was not growth and vibrancy, but a slow trajectory of decline and fall. The theoretical framework informing this critical history derives from science studies, and in particular the work of Bruno Latour.

**What's wrong with American regional science? A view from science studies.**

My first academic conference, and as a first year graduate student, was at the North American Regional Science association held at the Ambassador Hotel, Chicago, November 1978. It was exhilarating but disarming as well. For the first time I saw what professors did in their off hours. There seemed such a gap between the graceful and purified abstractions of the regional scientists who spoke, their vocabulary couched in mathematical equations or serried rows of numbers or tidily drawn flow charts presented as a series of overheads, and the behaviour of some of them once they left the lectern. There was drunkenness, a fistfight between a disgruntled author and recalcitrant editor, bets and bravado at a singles bar, imbibing of illegal substances, and cursing and sexual innuendo. I was shocked, but it didn't stop me from joining in.

Of course, I realise now from both becoming a professor and reading the science studies literature on which I will draw in this paper, that there is no disjuncture. Academic inquiry even of the most purified kind is thoroughly muddied by a wider social context, local cultural practices, and bodily acts and responses, and which can include even drunkenness, fistfights, and barroom bets and bravado. As Steven Shapin (1998: 23) puts it, "I have never seen a 'disembodied idea,' nor, I suspect, have those who say they study such things. What I and they have seen is embodied people portraying their disembodiment and that of the knowledge they produce." That's what those overheads I saw in the Ambassador Hotel of equations, numbers, and diagrams were all about. They were a performance of disembodiment, and in which at the time I believed. That was why I was so shocked. Off the podium, it was so clear that regional scientists were not spectral creatures of pure rationality, but only too human. What I realise now, of course, and it is the central theme of this paper, is that they were just as human on the podium as well. Their seemingly disembodied performances were precisely that, performances. The argument of this paper is that the problem with regional science is that it saw, and continues to see, such performances as always more than performances; that is, it believes in disembodied knowledge.

I want to contest this belief, the continued holding of which, I will argue, has led in part to the current precarious predicament of American regional science. For although emerging at a particular historical and geographical juncture, 1950s America, regional science believed it offered universal truths rather than particular ones (Polèse 1995: 314 says that "only out of America could a new field [, regional science,] arise (and succeed) with ambitions of universality"). Further, because of the certitude of its intellectual position, it was unable to transform itself as the original context changed. Instead, the same old "God Tricks," as Donna Haraway (1991:191) calls them, were performed repeatedly, that is, the view that it is possible to have "vision from everywhere and nowhere." But what was required was something different, and which was scepticism about the very idea of God tricks. This goes to the title of the paper. What's wrong with regional science is that because of its mistaken belief that it offered universal knowledge rather than local knowledge it was set in an intellectual aspic jelly of 1950s America, and subsequently found change very difficult. But change it must, if it is to survive.

A useful comparison here is with the Regional Studies Association of the UK that emerged about a decade after regional science. In fact, early on in the mid-1960s, Regional Studies toyed with the idea of joining regional science, but in the end shunned the latter's advances, and remained single. Interestingly, the justification put forward by the early founders of Regional Studies for staying separate was that Regional Science was unable to represent the peculiarities of the British and European situation. In the critical meeting at which it was

decided to form Regional Studies, Peter Self, one of the original members, said that regional studies would offer "a more distinctive approach [than regional science]," one that "would follow its own line," and would be "more modest," concerned with representing at best "Europe rather than the world" (RSA Research Meeting, no date, Folder: Annual General Meeting and Conferences 1965, Box 15, Regional Studies Association archive). While Regional Studies certainly experienced some difficult problems early on, it overcame them, and is now, I would argue, in a better state than regional science. It is better in the sense that it is more modest, follows its own line rather than striving for universality, and recognises that it cannot deal with the whole world but only distinct portions of it. As a result, it is better able to accommodate change, to adapt and be relevant to the present, and to be listened to by people with power and influence. That is, its strengths come from sensitivity to local knowledge.

To make the argument that regional science was stuck in a 1950s rut, from which it was not able to extricate itself, I begin with a general framework drawn from Bruno Latour's work in science studies that establishes a framework for understanding the relation between social context and scientific practice. In particular, I describe his model of "five loops" that represent "the mobilisation of the world," that is, the process by which circuits of people, institutions, ideas and things come together to form particular kinds of scientific practice and modes of representation (Latour 1999: 99-100). In the second section, I provide a brief history of early regional science arguing that from the beginning it aspired to God tricks. In the final section, I use Latour's five-loop model to understand regional science's rise and fall. In particular, by tracing the five circuits, I explain the burgeoning of regional science during the 1950s and 1960s, and why for a period it was such a force to be reckoned with. But from the 1970s the tight network of regional science that was so successful failed to change with new circumstances. Instead, and seen by examining the same circuits, regional science became increasingly out of joint with its time, experiencing decline and fall.

**Science Studies**

Let me start with my conceptual framework taken from science studies. Now a burgeoning and multidisciplinary body of literature, the principal assertion of science studies is that scientific knowledge is shaped by its local context that goes all the way down to the very truth claims of the scientists themselves. There is no universal knowledge, only local knowledge (Barnes 2003). Science is a set of contingent practices, intimately related to its wider social context. Sometime those practices become scientific truth claims, other times not. Some of those claims, for example, never make it out of the laboratory; in other cases they do, but are then ignored by the wider profession and might as well never have happened; in other cases still, they enjoy success for a period, but then decline and are forgotten; and yet in other cases, they become part of the scientific canon, such as Boyle's Law or Newton's Law of Gravity, and enjoy fame and durability (examples of each eventuality are found in Latour 1987). The task of science studies is to show that <u>both</u> scientific failures such as botched laboratory experiments and scientific successes such as making laws are <u>both</u> the consequence of a local context, rather than universals such as rationality or truth. That is, its task is to show how contingent local circumstances make truth as it is, and why it accrues at certain junctures, and not others.

Even within science studies, there are a number of different approaches to address the issue of local truth making (Hess 1997; Barnes 2001a). For brevity's sake, I focus on only one, the work of perhaps the best-known contemporary science studies writer, Bruno Latour (1987, 1988, 1999). That work is useful for two reasons, and illustrated in Latour's (1999) recent book, <u>Pandora's Hope</u>.

The first is that Latour offers a vocabulary and conceptual grid for understanding the specific linkages between science and society. Latour (1999: 80) 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." To show that vascularization, Latour traces the movement of various actors between the two spheres as they move in and out between them. Latour is concerned to follow items as varied as people, books, institutions, machines and even microscopic objects like a bacillus as they circulate, that is, course through veins and arteries, and in doing so join science with society. Latour calls the associations made through circulation "translations." Translation involves bringing together seemingly different entities, and persuading them that they have similar interests and should work together. Michel Callon (1980: 211) writes, "Translation involves creating convergences and homologies by relating things that were previously different." For example, Louis Pasteur relates a cloudy liquid in a flask in his Rue D'Ulm laboratory in Paris, to dead cows, and the spatial configuration of farms in rural France (Latour 1988). Or more germane to this paper, Walter Isard (1956: 66-67) relates Newtonian equations of potential, demographic events in the United States in 1940, and slide rules and calculating machines (Figure 1). In both cases, there is translation, bringing together seemingly different circulating objects, and showing that they relate, that is, they are translatable to a common purpose. It is at that moment of translation that a linkage is made between science and society. That flask of cloudy liquid is joined subsequently to sweeping social changes around public hygiene that not only transform France but the world, while Newton's equations are joined to a social project, regional science, that lasts at least 50 years and is represented on every continent.

But translation can be difficult, and doesn't always succeed the first time, and sometimes never. Pasteur spends long hours at his laboratory bench, arduously working with various circulating materials persuading them to become allies. They often resist, and don't co-operate. And even when they do, revealing the bacillus in a flask, further linking that cloudy liquid to yet more actors like cows, or farmers, or health officials makes the project of translation frequently perilous (Latour 1988). The same is true for regional science. Even if Isard can make Newton's equations, demographic data from the 1940 US Census, and slide rules and calculating machines work together to produce a map in a book, that's only the beginning. There is a lot more translation necessary to set up regional science. A good idea is never enough. For there will be all kinds of blockages and oppositions along the way (and discussed in detail below). For example, some geographers will say that regional science is nothing new, and not want any alliance. Others who are seemingly sympathetic will at the last moment go their own way and not join. And even machines like computers will resist, and not comply with the tasks set them. When these events happen, translation and circulation will come to a stop, with regional science halted. But stoppage is not necessarily permanent. New geographers may be convinced to join, those who initially opted out may later rejoin or new models of computers maybe persuaded to complete tasks at which old ones balked. Once these blockages are removed, regional science is recast, and a chain of successful translations will begin to unfurl once more, at least for a time.

In Pandora's Hope Latour provides a general map of the circulations and translations found in science (Figure 2). Four circuits –instruments, colleagues, allies, and the public – come together in a fifth as links or knots. I will define these terms in detail below, but the important point is that the map offers a vocabulary and conceptual grid for relating science and society. Specifically for my paper, it provides a means to understand the rich vascularization between regional science, and its social context. My first task is to present this rich vascularization by

fleshing out the skeletal outline of Latour's map, that is, to illustrate the circulations and translations between regional science and its local context.

The second reason that Latour's work is useful is that it provides an analysis of scientific success and failure. In particular, his map and the accompanying metaphor of vascularization suggests that scientific vitality is a consequence of continued circulation and coursing, repeatedly unfolding translations, found in the five loops. As the very term vascularization implies, provided flows of people, books, institutions, machines, and objects continue "scientific disciplines" remain "alive." This was certainly the case for regional science early on, and which possessed just such momentum, held together by the coursing of the five circuits that linked society and science. The reverse implication, though, is that when circulation and coursing slow as veins and arteries become blocked, scientific inquiry itself lags and is clogged. Rather than alive, the discipline becomes increasingly moribund. This is now the fate of regional science. Vascularization is increasingly impeded with a reduced flow between regional science and its local context. However, because of its belief in disembodied knowledge, in "God tricks," regional science has found it difficult to change. The result is that life-sustaining coursing and circulation continues to ebb, making the discipline at least in North America frail and ailing, and requiring urgent attention.

**A Brief Early History of Early Regional Science**

Before I chart the rise and decline of regional science using Latour's five loops, and thereby the tight connections with its local context, it is useful to provide a thumbnail sketch of its early history (discussed in more detail by Boyce 2003, and Isard 2003).

Central to its existence and maintenance is one man, Walter Isard. Born in Philadelphia in 1919, he studied economics and mathematics at Temple University, and in 1939 entered Harvard as an economics graduate student, completing his Ph.D. in 1943. A Quaker and a conscientious objector during the Second World War, he continued his interest in location theory by translating during graveyard shift some of the great German location theorists while carrying out alternative service at a Philadelphia psychiatric hospital. After the war, he worked for Wassily Leontief at Harvard, and it was there, beginning in 1950, that be began to organize the first of many sub-meetings within larger disciplinary national conferences (Economics, Sociology, Political Science, Geography), trying to persuade participants of the merits of regional economic studies (that is, he tried to "translate" their interests to his own). He also engaged in an energetic inter-disciplinary letter-writing campaign towards the same end. In 1953, Isard moved to MIT as an Associate Professor of Regional Economics at the Department of City and Regional Planning. And it is while he is at MIT that the earlier preparatory organising bore fruit when in December 1954 the first meeting of the Regional Science Association was convened at the Sky Room, Hotel Tuller, Detroit, attracting forty-five separate participants. While there was earlier scepticism expressed about the project particularly by geographers who thought that regional science was fully accommodated within their existing disciplinary rubric,[1] Isard was sufficiently buoyed by the Detroit meeting to set up "a skeleton type of organization" (Isard 1955: xv), and to make the conference an annual one. Isard required that each participant bring 200 mimeographed copies of their paper to that first meeting which he later collectively bound. Selling each copy for a dollar, he thereby launched the first regional science journal, Papers and Proceedings of the Regional Science Association.

Two years later in 1956 Isard published the seminal volume, Location and Space Economy. Academic movements require what Latour (1987) calls "immutable mobiles," books and journals that travel and spread the word (they are "mobile"), but are not corrupted by such

movements (they are "immutable"). One of their roles is to persuade others to join. In this sense, books and journals put on a performance to entice prospective allies to join (Barnes 2002).

Two features of Location made it persuasive. The first was that it was couched in the seemingly universal vocabulary of mathematics ("nature's own language" as Galileo put it, or for Isard, region's own language). In turn, Isard took that abstract and mathematical logic from two other sources: neo-classical economics and German location theory. Neoclassical theory from its inception in the late nineteenth century was a mathematical enterprise relying on theories taken from physics, and by 1950 it was even more so following the muscular mathematical efforts of especially Paul Samuelson (1947) who serendipitously was a colleague of Isard's at MIT (albeit in the Department of Economics; Mirowski 1989; 2002). Similarly, German location theory was relentless in its use of mathematical and numerical methods (Polèse 1995). In fact, in the case of Alfred Weber and his theory of industrial location, 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 (Barnes 2003). The broader point is that by drawing upon neoclassical and German location theorists, Isard's regional science project became a mathematical project, and in so far as mathematics aspired to universal foundations, it represented Haraway's "God trick." The second persuasive feature, especially for geographers, was Location's insistence that space mattered to economic life. In Isard's now famous phrase, it was necessary to move away from the "wonderland of no dimensions" (Isard 1956: 25-26) to a fully constituted spatial economy. Isard's conception of space, however, was a mathematically defined one, couched in terms of the numerical calculus of transportation cost differentials. As such, it was another "God trick," "the view from everywhere and nowhere" (taken up in Barnes 1998), and in this sense, as Polèse (1995: 315) suggests, the spaces studied by regional science "remained largely a-territorial and a geographic."

The year of Location and Space Economy's publication was significant for another reason: Isard moved to the University of Pennsylvania as part of a larger recruitment drive by the Department of Economics. The move also gave him leverage to create a university home for regional science, and critical to its institutionalisation. Specifically, Isard's condition for appointment at Penn was the establishment of a Regional Science Ph.D. programme, and which was granted within two weeks of the request. Two years later, a separate Department of Regional Science was established, with Isard as chair. And during that same year, 1958, another important immutable mobile, the Journal of Regional Science was established, and co-edited initially by Isard and his former student at MIT, Ben Stevens. Two years after that, the first Ph.D. of the Department, William Alonso, graduated. In that same year, 1960, Isard also published the second foundational piece of regional science writing: the collective tome, Methods in Regional Science (Isard et al., 1960). This was different from Location and Space Economy that had provided primarily a philosophical and theoretical rationale, and couched in terms of universal science. In contrast, Methods was a "how-to" manual, brimming with practical, operational techniques for coping with, and achieving specific planning ends, within 1960s America.

**The Rise and Fall of Regional Science**

The early success of regional science, I now want to suggest following my earlier argument, was not a consequence of the inherent scientific correctness of regional science's propositions and analysis – its God trick – but a result of "rich vascularization," and represented by the five loops identified by Latour (Figure 2). I intend to use those loops first, to demonstrate

the power of regional science after it is institutionalised at Penn, and second, to explain its decline from the end of the 1970s.

*The Rise*

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 objects of study into a stable network of inquiry. It is accomplished by carrying out a series of translations, where objects of inquiry are increasingly brought under control and domination: "instead of moving around the objects, scientists make the objects move around them" (Latour 1999: 101). In science, such a goal is achieved primarily through various forms of inscription, including prose, equations, numerical tables, diagrams, maps, and photos. Through such inscriptions, objects of inquiry are no longer out there, with a life of their own, but are down here, on the page, and consequently disciplined and regulated.

Such inscriptions when properly bound and set constitute immutable mobiles. Apart from their immutability and mobility, immutable mobiles also possess another critically important feature for mobilizing the world: they reduce what they portray to the same form, a flat page. As Latour (1990: 46) puts it, once inscribed on a page phenomena and events possess the "same optical consistency and semiotic homogeneity." Figure 1 taken from Isard's Location and Space Economy is a perfect example. 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 Isard's book. In doing so, Isard mobilizes world, allowing him to move around these objects, rather than the other way around. After mobilization, Isard is able to order and control, to shuffle and manipulate, varied events and things, thereby imbuing him with "an extraordinary degree of certainty" (Latour 1990: 46) about the phenomena that are inscribed.

In making the world work for them, regional scientists also engage in a geographical move, shifting from sites where the events they study take place to sites where representation (inscription) and presentation occur. For example, during the 1960s Isard worked on projects in Puerto Rico, Colorado, New York and New Jersey, and in his hometown of Philadelphia. But where the real action lay was not at these particular locales, but in Isard's office at the McNeil Building on the Penn campus. For it was there that data were compiled and analysed, figures drawn, equations derived, and prose written. The field sites, in other words, were less important than what Latour calls the "centre of calculation," in this case the Regional Science building at Penn that collects information, sorts and rearranges it, and then disseminates it in new forms. Further, once the new forms are produced, they travel as immutable mobiles to other geographical sites, often university libraries, but not always. They even end up in places like the Ambassador Hotel, Chicago, displayed on overhead projectors in front of awe-struck graduate students like me.

2. Autononization (colleagues)

The second loop goes perhaps to regional science's greatest success: its institutionalisation. Termed "autonimization" by Latour (1999: 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."

Initially key to regional science's institutionalisation was the ferocious energy of Isard. By both travelling to various national social science conferences, and by dint of an enormous amount of correspondence (the regional science archive lists eight 16 x 16 inch boxes of correspondence between just 1947-73), Isard forged the connections necessary to erect regional science as an institutional structure. The culmination was the first formal meeting in December 1954. But that was only the tip of the iceberg. There was the 90% not visible, and represented by at least five prior years of effort by Isard travelling on trains across the country, writing letters into the early hours, attending meetings, making presentations, and cajoling recalcitrant potential enrolees (Boyce 2003).

Even once the 1954 conference occurred, other elements of institutionalisation were then necessary to sustain the movement. Immutable mobiles such as Papers and Proceedings published in 1955, and Isard's Location appearing in 1956, were critical. Such volumes represented the material embodiment of regional science. In spite of some initial reluctance by publishers, Location, in particular, sold very well, achieving total sales of 20,000 (Isard 2001: 421, fn. 4). Consequently, the performance it put on was witnessed by a large audience, and across the world, and which, in turn, was vital in regional science achieving a global reach as an institution. Also crucial were education and training. It was not enough simply to put out immutable mobiles in the hope that they are read. There must be places where they are read, and where correct instruction of their reading is provided. Central was the establishment of the Ph.D. programme in regional science in 1956, and later the Department of Regional Science in 1958. It gave the institution of regional science both a place, and a means of reproducing itself. The conferences, and later the summer institutes that begin in Berkeley in 1962, functioned similarly, perpetuating through education regional science as an institution as well as extending its reach geographically. Finally, institutionalisation required flows and circuits of money, and more than the dollar-a-copy Isard charged for Papers and Proceedings. Money was required for maintaining experts in regional science, the professors, for novice trainees such as graduate students, for the travelling necessary to extend the institution, and for purchasing and maintaining the material goods and services necessary for realizing the regional science project, from slide-rules to Friden electronic calculators to mainframe computer time. Money came from research grants, contract work (the Regional Science Research Institute was established in 1956 as a non-profit to carry out applied project research), but mainly from Penn that provided what Isard (2001: 418) calls a "budget unit."

3. Alliances (allies)

The third loop revolves around alliances. By this is meant the necessity to entice new supporters to a project in order to strengthen it. Like translation, it involves convincing other actors that their interests are best served by joining with yours. Certainly, forging alliances was Isard's intent when he travelled so much, and wrote so many letters, in the early years of regional science. When he attended all those national US conferences of Economics, Political Science, Sociology, and Geography in the 1950s he was trying to persuade economists, political scientists, sociologists, and geographers, that they would be better off, that is, their personal and intellectual interests would be better served, if they formed an alliance with regional science. Geographers were especially receptive to such persuasion. Geography's quantitative and theoretical revolution had begun in the late 1950s, and early revolutionaries saw in regional science an allied movement that offered clout and prestige, intellectual nourishment, and a forum in which to present work (Barnes 2001b). From their perspective, regional science could not have emerged at a better time. Indeed, several of them taught at Penn in Regional Science (Michael Dacey,

Duane Marble, Julian Wolpert, and Allen Scott were all faculty members at one time), making a tie between geography and regional science that bound, at least for a time.

Garnering allies, though, is often fraught, with no guarantee of success. And even if success came, it could take much time, and require much work. One example is the relationship of regional science to British geography. In 1960, the constitution of the Regional Science Association was changed to allow for international sections. Such a change was vital for the expansion of regional science as an institution. But when Isard approached the Cambridge economist, J. R. N. Stone in the 1961 to set up a British section, he was rebuffed (Folder 3959, Box 7, Regional Science Association). By 1964, however, there was renewed interest, this time from an agricultural economist, Gunter Hirsch, who was part of a larger group of British economists, planners, and geographers interested in establishing a new association concerned with addressing regional questions (in part spurred by the recent election of a new Labour government, one mandate of which was to implement regional economic policy). In fact, in July 1964 Isard travelled to London to give a lecture at the London School of Economics to solidify support from Hirsch and the group he represented. The Guardian even sent a correspondent who finished their article by reporting, "The meeting ended with an interdisciplinary group being set up to study the possibility of the British Isles section of the Regional Science Association" (The Guardian, 13th July, 1964, File "Newspaper clippings," Box 22, Regional Studies Association). That group, though, as I already suggested in the introduction, and which included Gunter Hirsch, Peter Self, and the geographer, Peter Hall, in the end spurned Isard's plea for them to become allies, and instead established their own organization, the Regional Studies Association.

But Isard is not someone who easily accepts refusal. So, in January 1967 Isard finally sent one of his own faculty members at Penn, Allen Scott, to Britain to help organize a British section of the Regional Science Association. Scott was based at the Bartlett School, University College, London, and within weeks he organised a "bull session" with 40 participants to discuss the possibility of regional science, and shortly after that he and David Harvey became joint-secretaries of the British section of the RSA. Additionally, Scott facilitated the instigation of at least two important immutable mobiles for British regional science through a relationship he formed with John Ashby at Pion publishing. Pion agreed to publish both the papers from the British Regional Science meetings as the London Papers in Regional Science, and a new journal, Environment and Planning under the editorship of Alan Wilson, that if not in name was a British journal of regional science.

4. Public representation

The fourth loop is the relationship between regional science and the world that lies outside the academy, in this case, the wider social and political context of 1950s and 1960s America in which regional science emerges. It includes the Cold War as well as real wars like Korea and Vietnam, Galbraith's affluent society and the fight against poverty, Fordist workers and the "company man," "urban regeneration" and suburbanisation, the space programme and the military-industrial complex, and so on. It is impossible even to list all these elements in a short section such as this one, let alone provide a comprehensive analysis. I limit myself to two related points.

The first derives from the economist Philip Mirowski's Machine Dreams (2002). He 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. As Mirowski (1999: 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."

While regional scientists did not directly contribute new OR techniques or novel computer algorithms, and were usually outside the key geographical locations where such work took place (the most famous is at RAND in Santa Monica), regional science nonetheless was part of that wider locus of a "scientific approach to government, corporate management and society." Rather than creating original techniques and algorithms, it applied them to a spatial context, both urban and regional. Isard, for example, was initially interested in applying general equilibrium theory to a multi-regional economy. Or Isard's first Ph.D. student, Ben Stevens, developed a spatial version of linear programming that was then used for metropolitan and regional planning. Or yet again, the last chapter in Methods, "channels of regional synthesis," is an unbridled form of systems analysis. Similarly, while regional science did not develop computerisation along new lines, it made massive use of the ability of such machines to carry out numerical calculation and statistical analysis of large geographical data sets. But even mainframe computers could not always keep up with the tasks that Isard demanded of them. For example, no computer even in the early 1970s was able to invert a 500 x 500 input-output matrix for the Philadelphia economy that Isard had made to calculate the effects on the city of military expenditures in Viet Nam.

The second related point is that the OR techniques and computer runs carried out by regional science were perfectly suited to a post-war America characterised by regional and urban expansion, and affiliated issues of transportation, energy and basic industry provision that were closely linked to an increasingly affluent and urban society. Such techniques provided the state and corporations with the ability to manage that growth and expansion scientifically, instrumentally, as befitting the new mindset of centralised command and control. Mario Polèse (1995: 314), a student in Regional Science at Penn during its heyday of the 1960s, perfectly captures that political and cultural moment and social scientific aspiration.

> Yes, social problems could be solved, if only we mastered the right tools and had adequate data ... Human relationships could be modelled, economic conditions controlled. Existential angst was out. In many respects, early regional science was not far from Soviet thinking: if only we could get the coefficients right in our input-output tables, we could finally model (and plan) the economy … The emphasis was on methods, techniques, and modelling. One naturally came to America to learn methods and techniques. America is not a land of introspection, but of action and results.

5. Knots and links

The final loop is where the other four come together and are bound as knots and links. But this last loop is not the grand finale, at last the real thing: genuine regional science. It remains utterly dependent on the other four loops, and attendant circulations. Blockages or

impediments among any of the circuits can produce debilitating, and even terminal consequences.

For Latour the most important indicator of health at the centre is the strength of the circulations around it. The greater the strength of circulation, the stronger is the fifth loop, and the sturdier is the science done, and the greater the potential for geographical extension. Regional science was so impressive as a network in these early years because its circulation of immutable mobiles, professors and students, money, alliances, and interactions with the larger public world outside was so vibrant and powerful. No wonder it was able to find a home on every continent, and in countries from Australia to Yugoslavia.

*The Fall*

Sometime in the late 1970s because of changes that occurred both on the ground and intellectually, the previous strong circulation and coursing that regional science enjoyed became blocked and sluggish, creating the potential for a major coronary. Regional science, however, responded hesitantly. In the face of evidence to the contrary it continued to hold to the idea that it represented the view from nowhere, rather than a view from somewhere; that is, it overlooked the deep vascularization between science and society that had always made it a local rather than a universal project. The blockages that arose are found in all five circuits.

First, regional science seemed less able to engage and mobilize the world, that is, loop 1. I half recognised this problem even at the Regional Science conference in Chicago 25 years ago. Isard gave the plenary talk at that meeting, and showed Figure 3, an integrated multi-regional model that appeared to present the meaning of life, the universe, and everything else (as Douglas Adams might have said). Figure 3 might provide optical consistency and semiotic homogeneity, but its God's eye ambitions are so blatant and overblown, that its hubris and folly are also clear. Even Robert Kuenne (1990: 8), a former student of Isard's, half acknowledges such oversized aspirations when he writes: 'The term large scale modelling does not adequately convey the Isardian view of a seamless, monistic universe. About the only topic left in the category that Walter [Isard] is not known to have researched is the providential integration of the relations between Heaven and earth. And there are rumours even there!" The point is that there are limits to engaging and mobilizing the world, and Isard's diagram met them. While the population potential of the US in 1940 might be representable, a universal model of conflict, economy, population, and transportation and much else besides stretches credibility. The man simply models too much. No wonder that Andrew Isserman (1993) used as his title for his now well-known Regional Science Presidential talk, "Lost in space." In Figure 3 Isard appears lost in the spaces of the page, with consequently a weakened ability to dominate and control objects of inquiry. Rather than tightening his purchase on the world, Isard's inscriptions lose their grip on it, becoming a source of weakness not strength.

Second, autonomization, loop 2, is similarly under acute strain, with the institutional structure of regional science suffering a significant trauma in the US. Key was the closing of the Regional Science Department at Penn in 1993 by Dean Rosemary Stevens. She made that closure, as she wrote in a letter to those urging her to keep the department open, "well aware that our Department of Regional Science … has been the flagship department for the discipline" (quoted in Bailly and Coffey 1994: 38). It suggests that Dean Stevens believed that regional science as an institution no longer has a place in America's Ivy League universities. Interestingly, a similar fate befell the discipline of Geography that from the late 1940s was shut out of Ivy League colleges beginning with Harvard, then at Princeton, Yale, and even at Penn. But at least other Geography Departments were sufficiently thick on the ground, especially in the

American Mid-West, to enable geography as a discipline to survive in spite of the sometimes-grim prognostications made about its fate. But regional science has no such institutional safety net at least in the US. As goes Penn, so goes regional science. In particular, apart from the symbolic loss that the closure represents, there are the more tangible costs that effect regional science's institutional viability: graduate students are no longer trained in the tradition, there are no more jobs for regional science professors (or for geographers attracted to regional science), nor are there flows of money from the university, from research grants, and from contracts. The coursing and circulating that was critical for institutionalisation has slowed and turned stagnant, undermining the very institution itself.

The third loop, alliances, has also suffered since the 1970s from a series of defections. In particular, traditional allies in planning and geography in the US have now all but deserted it. As I suggested, geography was an early collaborator, and geographers were there at the beginning (William Garrison, Allan Rogers and Chauncey Harris all attended the first Regional Science meeting in 1954). That tradition continued through the 1960s and 1970s, and students who attended the Regional Science Department at Penn over that period included Doreen Massey, Neil Smith, and Michael Dear. But as that particular list of names and what they now represent suggests, the partnership of regional science and contemporary human geography has suffered a serious break. Even those geographers who were committed to regional science, possessing the analytical acumen to participate – for example, attending that 1978 Regional Science meeting in Chicago were Gordon Clark, Allen Scott, Eric Sheppard, and Michael Webber – none would now call themselves regional scientists. The point is that regional science's alliances have been severed, isolating it, and depriving it of much needed energy and support. That said, there has a been a glimmer of revival stemming from the development of Paul Krugman's new economic geography that attempts to make the study of the space economy respectable among economists. Some geographers are also involved, although none have provided much support to Krugman per se. It is unclear yet to what extent regional science might be pulled along on the coat tails of the new economic geography, and even whether the new economic geography itself will survive. Its narrowness, mathematical abstruseness, and the wandering attention of Krugman himself, are not propitious signs (Barnes 2003).

The fourth loop, public representation, has attracted critical attention from even regional scientists themselves (Isserman 1993: 24; Polèse 1995: 315; Bailly, Coffey and Gibson 1996: 157). Their argument, and similar to the position of this paper, is that the theories, models, and assumptions of regional science have not moved much beyond their origin of 1950s America, and its context of Fordist industrial growth, Keynesian interventionist policies, and large scale top-down metropolitan and regional planning models. But the world has changed. Fordist manufacturing such as those iron and steel complexes Isard studied in New York and New Jersey in the 1950s have given way to flexible mini-mills, and more fundamentally to a post-industrial service economy. Keynesian interventionism entered its death throes in the late 1970s replaced by a neo-liberal, hollowed-out, and non-interventionist state of various new-right governments (most famously represented by Thatcherism in Britain, and Reaganism in the US). And large-scale, top-down metropolitan and regional planning models, and the theoretical and technical apparatus they implied, were made redundant from the early 80s by a new ethos based upon bottom-up local development, resting upon market-based entrepreneurialism (Polèse 1999). There is also another change, an intellectual one that further makes regional science out of time. Over the last twenty-five years there has been an increasing public scepticism about the beneficence of science and technology, the belief in which regional science had traded on to

advance its interests in the 1950s, that is, to justify the God tricks it pursued. Such scepticism has been translated within the academy as a move away from rationalist, formal, and universal explanations, to ones that are relativistic, eclectic, and local (seen in the move in many social sciences and humanities to post-structuralism, feminism, post-Marxism, and post colonialism; Barnes 2001b). Needless-to-say, these later approaches have not yet made it into regional science, thereby pushing the movement yet farther away from the contemporary lively channels of social science discourse and debate, and into torpid backwaters.

The wider consequence of these four circulatory problems is unloosening within the fifth loop of knots and ties. "Things fall apart, the centre does not hold." But slouching towards the McNeil Building on the Penn campus was not Yeats' anti-Christ, but more prosaically the forces of the late 20th century. They have systematically taken away power from regional science by displacing its representations, undermining its institutional base, severing its allies, and making irrelevant its model of the world.

**Conclusion**

2004 is the 50th anniversary of the founding of the Regional Science association. Walter Isard (2003) has written his celebratory history, and other papers were commissioned to mark the event. David Boyce's (2003: 15), for example, "conclude[s] that the prospects for regional science are very bright … Building upon our rich heritage of more than 50 years I forecast only continuing success." I would like to believe Boyce, but the analysis I've given here makes me more pessimistic. Holding on to the upper case "S" science part of regional science, I argued, effectively arrested regional science's development, making its prospects dim, and resulting in a forecast of decline. I realise these are disrespectful and even boorish remarks to make about a Golden Anniversary, and in this particular forum. Michael Breheny (1984: 1; quoted in Jensen 1991: 99), though, was right when he said that regional science is "the least reflexive of disciplines." It is unreflective, and consequently inured to change, because of a commitment to a God's eye view. It is so convinced of its own rightness, of its Archimedean position, that it remained aloof and invariant, rather than being sensitive to its changing local context.

My argument is that regional science would have fared better if rather than emphasizing science, it emphasised the region, and which was the case in Regional Studies. Regions are where life is lived, where there is messiness and smudged boundaries, where there is difference and local truths (Polèse 1995: 314-15). Stressing regions would have given impetus to the view that knowledge cannot be brought down from the Heavens, Moses-like, but must be worked through modestly at ground level, in the brouhaha, particularity, and contradictions of real places. Here it is interesting to note that Isard (2001: 418-19) initially did not want his science to be called regional; it was to be "spatial science." But fearing that his new discipline would be confused with a branch of astrophysics, he changed its name. Given his scientific aspirations, however, spatial science would have been more apt.

In that light, I am very glad to publish this paper in the **Canadian** Journal of Regional Science. The regional designation is vital. In my view, it connotes not branch plant status, which was Isard's view of national sections, but the recognition that there are distinct scientific approaches to distinct regions. Regional Science is dead. But long live Canadian regional science.[2]

**Footnotes**

1. The geographer Derwent Whittlesey at Harvard wrote to Isard on August 24, 1954, (File 3959, Box 27, Regional Science Association), 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." Even in 1964 following a talk by Isard at LSE to rally support for a British section of the Regional Science Association, and reported in the Guardian newspaper, William Kirk, then secretary to the Institute of British Geographers, wrote to the editor: "I am at a loss to differentiate between what Professor Isard calls a regional scientist and what I would call a regional geographer" (Letter to the Guardian, 20th July, 1964, File "Newspaper clippings," Box 22, Regional Studies Association).

2. Mario Polèse was in the audience when I delivered this paper, and he told a telling story about his experience at Penn in the 1960s when he was a graduate student. He and some other graduate students were dissatisfied with Isard's proposed seminar use of his "big fat book" that was "the theory of everything" (personal communication). Isard would not change the text, and so Polèse and the others switched their doctoral degrees to Penn's City Planning Department (although he remained on good terms with Isard). Only after my presentation did I read Polèse's 1995 paper that makes a similar argument to my own that the universal aspirations of American regional science rendered difficult the understanding of particular regions like Canada. Indeed, when helping to found the Canadian Regional Science Association in the early 1980s, Polèse was keen to emphasize a plurality of approaches rather than the single universal one asserted by Isard. It is represented by the pluralization of science in the French title of the Association, "l'Association canadienne des sciences régionales."

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Archive of the Regional Studies Association, Archives branch, LSE Library, London School of Economics and Political Science, London.

the group of $N_1$ individuals produces at the point where the second group is located is given by $V_2 = GN_1/r$; and (4) that the *potential at any point* produced by the entire population of any given terrain is given by $V = \int_r^1 D\,dS$, where $D$ is the density of population over the infinitesimal element of area $dS$, the integration being extended to all areas of the plane where $D$ is not zero. The potential at any point, according to Stewart, may also be taken as an inverted measure of the proximity of the point to people in general.[17]

Stewart has computed population potentials for various areas of the world for different periods of time.[18] Since population is reported not for infinitesimal elements of area but rather for comparatively large units of area, only approximations to potentials can be achieved. In Fig. 3 are depicted equipotential contour lines for the United States in 1940.[19] It is extremely interesting to observe that east of the Sierras there is, in all directions, a continuous fall in potential with increase in distance from New York City, the major peak of the country, except that all other cities are local peaks on the general downhill slope.[20]

Working with approximate averages of potential for rural areas in a sequence of 28 states from Texas to Maine[21] and using double log

[17] "Demographic Gravitation . . .," *op. cit.*, pp. 32-36.

[18] See "Empirical Mathematical Rules . . .," *op. cit.*, pp. 476-79; "Potential of Population . . .," *op. cit.*, p. 22; and "The Development of Social Physics," *op. cit.*, p. 250.

[19] "Potential of Population . . .," *op. cit.*, p. 22. In this article Stewart does not indicate the number of areas in which he divided the United States. Obviously the larger the number of areas, and hence control points, the more precise the computed potentials, and the more likely that local peaks corresponding to cities will appear. See "Empirical Mathematical Rules . . .," *op. cit.*, pp. 473-82, for a discussion of some of the problems in computing potentials.

In the construction of Fig. 8, Stewart weighted population in the Deep South by a factor of 0.8; in the main sequence of 28 states from Maine to Texas by unity; and in the Far West by 2. For his reasoning on this step, see "Potential of Population . . .," *op. cit.*, pp. 29-30. However, it should be noted that if population is unweighted, a similar contour map results except that the potentials in the Far West are of considerably smaller value. See his "unweighted" map in "Concerning Social Physics," *Scientific American*, Vol. 178 (May 1948), p. 22.

[20] This statement, of course, might require qualification if a finer-grained map were constructed. Nonetheless, the resulting configuration of contours and its relation to the distance variable would still be impressive. And it is very likely that all of the contour lines in non-urban parts would close around New York City. According to Stewart, the major structure of United States' potentials has not altered much since 1840 when New York City was already the principal peak.

[21] Observations are confined to these 28 states because these states exhibit a con-

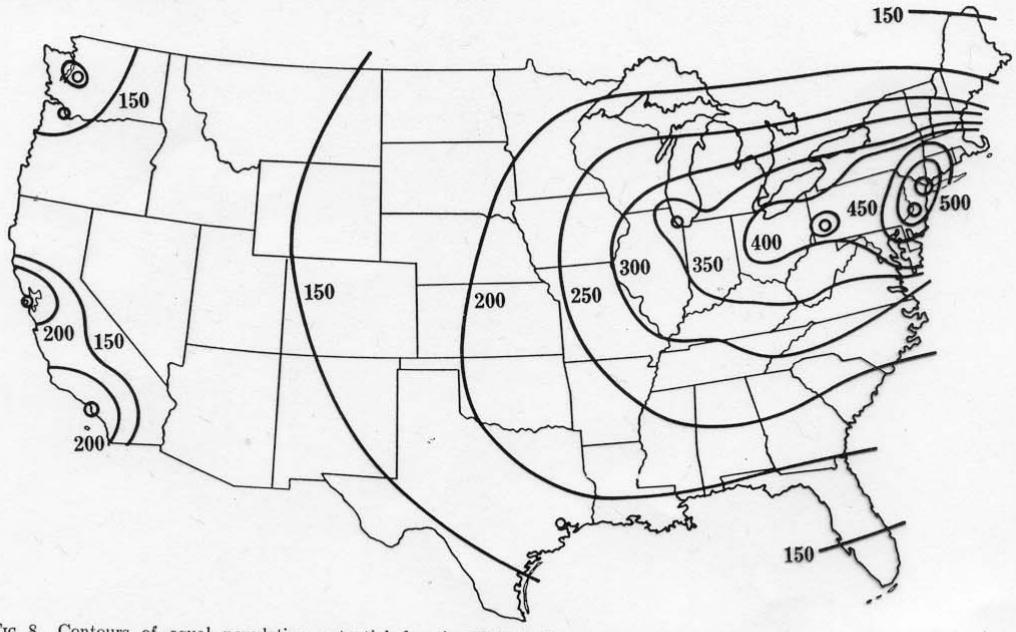


FIG. 8. Contours of equal population potential for the United States, 1940. (Weights of 1, 0.8, and 2 have been applied respectively to the populations of the main sequence of 28 states from Maine to Texas, to the 9 states of the Deep South, and to the 11 states of the Far West. Units are thousands of persons per mile.) (Source: Reaves Cox and Wroe Alderson, Editors, *Theory in Marketing*, Richard D. Irwin, Inc., Homewood, Ill., 1950.)

Figure 1: Isard's Location and Space Economy.

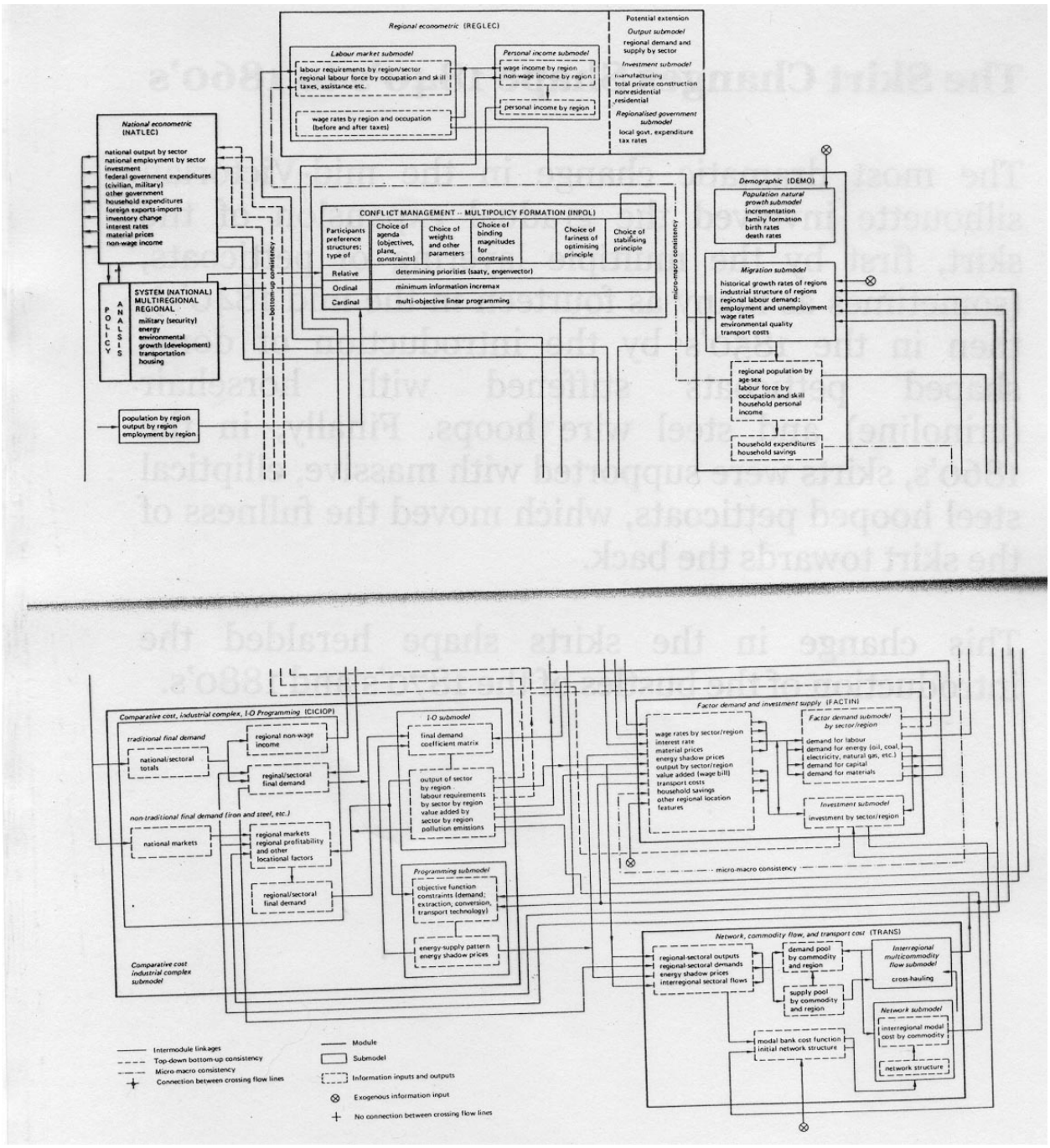


Figure 3 Isard's Conflict and policy analysis within an integrated multiregional model

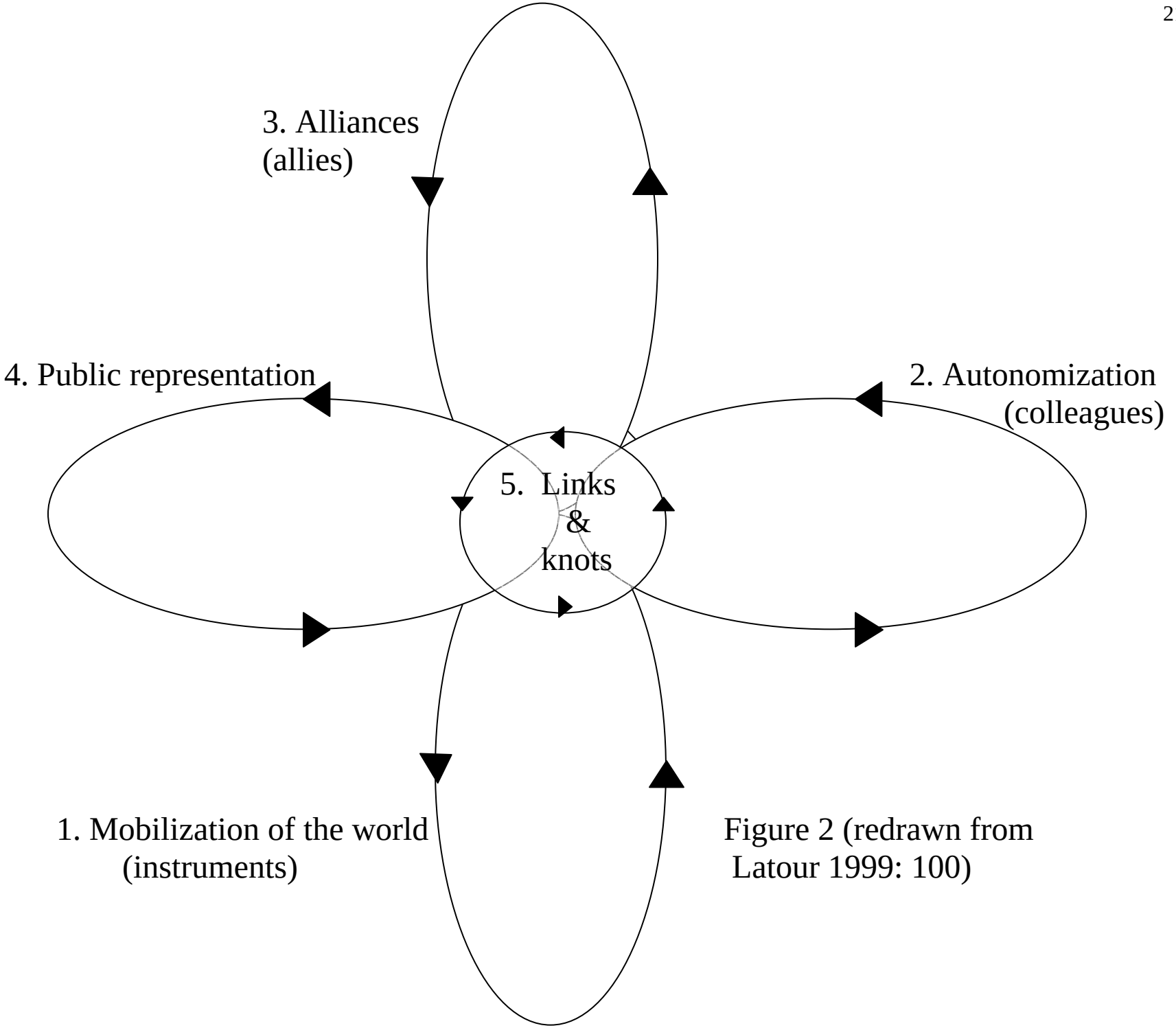


Figure 2 (redrawn from Latour 1999: 100)