The quantitative revolution and economic geography

Introduction

The quantitative revolution represented a profound transformation in Anglo-American economic geography from the mid-1950s onwards, defined by the systematic application of scientific forms of theorising and rigorous statistical techniques of analysis and description. In the process, the previous regional economic geography concerned with describing, cataloguing, and delineating the economies of unique places was pushed aside, replaced by the “new geography” directed towards explaining, scientifically proving, and abstractly theorising spatial economic phenomena and relations. Economic geography was no longer rote memorisation of leading ports, major waterways, and principal products, but a science, spatial science.

As a result, the days were over when the American geographer, Clarence Jones (1935, v.), could stand before his Northwestern University economic geography class in Evanston, Illinois, and say without his student audience either rolling their eyes or tittering: "Everyone likes to travel. Most of us wish to visit distant lands. Some want to be explorers and learn the ways of the Eskimo or the forest Indians who gather Brazil nuts in the Amazon. Many wish to hunt lions and tigers in the forests and savannas of Africa." Jones could no longer say those words, or at least not say them without being ridiculed, because the quantitative revolution fundamentally changed what counted as economic geography, and who could be an economic geographer. Under the new definition, Jones’ words were unrecognisable as economic geography, and he was unrecognisable as an economic geographer. The practices of the discipline were deeply altered, and those who failed to adapt necessarily fell by the way.

The chapter is divided into four short sections. First, I discuss the wider historical origins of the quantitative revolution, and which lay in the Second World War, and its aftermath, the

Cold War. Both produced an academic turn to formalism and rigour that affected the social sciences as well as even some of the humanities. Economic geography was just another one of the disciplines affected, and if anything, it was a Johnny-come-lately (the gendering is appropriate) compared to several of the others. Second, I tell the story of the quantitative revolution in economic geography as it emerged during the 1950s within a set of fragmented geographical centres, each associated with a small group of believers who were young, bright, very ambitious, and exclusively male. These were the revolutionaries. Third, I characterise the kind of economic geography that was produced. It was theoretical, mathematical, reliant on machines, underpinned by an explicit appeal to the philosophy of science, particularly to some form of positivism, and reflected the peculiar set of social characteristics of its practitioners. Finally, by way of a conclusion, I discuss how the quantitative revolution turned out in economic geography, and its intellectual legacy.

In all this, my focus is on the tradition of Anglo-American economic geography, and much more American than Anglo. Partly this is because it is the tradition I know best, partly it is for reasons of brevity, and partly it is because the most sustained and systematic impulse toward quantification derived from post-war American social science, and which first entered American economic geography before diffusing elsewhere.

Historical Origins of the Quantitative Revolution

World War II, and later the Cold War, created the conditions for geography's quantitative revolution. The sociologist of science, Andy Pickering (1995), applies the term "World War II regime" to understand this period. For Pickering, the regime is distinctive because of a close interrelationship among several institutions that previously were separate, but through their mutual interaction, created an entity that never existed before. For the purposes of this paper, especially important was the new relation forged between science, including the social sciences, and the military. It was not just that they co-operated – that had happened before – but their aims and structure increasingly bled into one another, creating a new combined entity that stressed above all else scientific and quantitative forms of knowledge. US President Eisenhower just as he was leaving office in 1961 famously spoke of the military-industrial complex, and in 1967 Senator Fulbright talked of the military-industrial-academic complex. In both cases, they point to this new entity that joined academics, the government, the military and private business.

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The purpose of that entity was to achieve definite geopolitical ends, from eliminating Fascism in Europe to fighting communism in the Third World. Such ends were too important to be left only to the military. It was also necessary to involve non-military scientific experts. For example, physicists, such as those employed at the World War II Manhattan Project at Los Alamos under Professor Robert Oppenheimer, and who created the atomic bomb that fundamentally changed the character of not only the Second World War, but the second half of the twentieth century. Or, social scientists, such as American economists at the Enemy Bombing Unit in Second World War London who invented cost-benefit analysis that determined the most rational targets for aerial bombardment. The critical point for this chapter is that the kind of knowledge deemed useful for achieving immediate military purposes and the associated larger geopolitical ends emphasised rigour, mathematics, formal vocabularies and models, "hard" data, cutting edge technology, and objectivity. This form of knowledge was instrumental. It got things done. Even physical sciences that already generated this form of knowledge changed to accommodate the new demands, moving towards a "big science" model. But the transformation was even more profound for the social sciences and humanities. Carl Schorske (1997, 295) characterised the development of the human sciences during the period 1940-60 as a "passage…from range to rigor, from loose engagement with a multifaceted reality historically perceived to the creation of sharp analytical tools that could promise certainty where description and speculative explication had prevailed before." This was new, but exactly what one should have expected once social sciences and humanities were set within the military-industrial-academic complex. Some social sciences like economics and psychology had a head start in their transformation, others like geography, especially economic geography of the kind Clarence Jones taught, had a much longer way to go.

Specifically, in the United States, the beginning of geography's revolution was its involvement in the Office of Strategic Services (OSS), later the Central Intelligence Agency. Established in July, 1941, the OSS provided the intellectual brawn for US military intelligence. It brought together a who's who of American academics in the social sciences and humanities as well as from outside. The key geographer during the wartime period was Richard Hartshorne (1939), a political and economic geographer. In 1939 he had written the most systematic and philosophically sophisticated monograph ever in English of the history and definition of the discipline, *The Nature of Geography*. For Hartshorne, and this was the import of his 1939 volume, the mandate of geographers at OSS was the assiduous, atheoretical geographical representation and description; in other words, the rote memorisation, Clarence Jones version of the discipline. Such an approach, however, became increasingly out of synch with the practices of other disciplines represented at OSS such as economics, psychology, and even political science and sociology. They were becoming more like physical sciences drawing on new methods, mathematical techniques, and in some cases even abstract theories, and contributing to the "World War II regime."

Against this back drop, against the impress of Hartshorne's view of geography, some younger geographers employed at OSS became increasingly frustrated. They could catalogue geographical information, insert it within classification schemes, and describe, but that was it; they lacked the vocabulary, and tools to use their knowledge for specific instrumental ends. Edward Ackerman (1945, 122), one of those younger geographers, wrote after he had been demobbed: "Wartime experience has highlighted a number of flaws in theoretical approach and in the past methods of training men [sic] for the profession. It is no exaggeration to say that geography's wartime achievements are based more on individual ingenuity than on thorough foresighted training." The training he had in mind was the kind of preparation that some of the other social scientists at OSS possessed. Economists could draw upon mathematical skills, psychologists' statistical ones, sociologists conceived their discipline from the beginning as a science of society defined by empirical and numerical methods, and even anthropologists possessed ethnographic and field techniques. Geographers had only the map. And even then they weren't always sure what to do with it.

Ackerman wasn't the only geographer at OSS to be frustrated (Barnes and Farish, 2006). Geographers need not have worried, though. Another war was just starting, this one under the shadow of the Bomb, with the geopolitical end of resisting communism, and asserting democratic forms of government and the virtues of the free market (although the assertion of democracy was at best selective). Such an end would offer plenty of opportunities for geographers to contribute because even more so than World War II it was to be realised in part by the explicit use of science and social science. Dwight Eisenhower, then Army Chief of Staff, said in 1946:

> The lessons of the last war are clear. The military effort required for victory threw upon the Army and unprecedented range of responsibilities, many of which were effectively discharged only through the invaluable assistance supplies by our cumulative resources in the natural and social sciences and the talents and experiences furnished by management and science …. This pattern of integration must be translated into a peacetime counterpart …. (quoted in Allison, 1985, 290).

And so it was, even though as President Eisenhower, he later had doubts about what he unleashed. In the process, Cold War American social science became ever more scientific, more mathematical, more rigorous, even incorporating some of the humanities.

Geographers like Ackerman, and others who had been with him at OSS such as Chauncy Harris and Edward Ullman (and all economic geographers to one degree or another), once they returned to academic geography began trying to shift disciplinary course in line with the new intellectual current. But it was hard work. The discipline was firmly anchored in the regional approach. But it could not stay put forever. The military-industrial-academic complex was becoming more powerful and stronger, drawing in fresh recruits, transforming them. As geographers like Ackermann returned to university teaching with their new contacts, new ideas, new institutions for research funding (like the Office of Naval Research), and new machines (like the computer), so geography, and especially economic geography, began to be pulled and tugged, slowly but inexorably into the main stream.

While there were some direct Cold War linkages between economic geographers and the military-industrial-academic complex, especially in the form of research funding and contract work (and discussed below, but also see, Barnes and Farish, 2006), equally important were the indirect ones turning on an innovative intellectual climate defined by scientific rigour and mathematical modelling. This was the future, certainly economic geography's immediate future.

Centres of Economic Geographical Calculation

But it took time to realise. It was not until the mid-1950s that the first real pangs of conversion from the old Clarence Jones version to a shining new spatial science were felt, even though the conditions for change were laid more than a decade earlier. At first, the change was highly localised, confined initially in the United States to two centres: the University of Washington, Seattle, and the University of Iowa, Iowa City.

Key at the University of Washington were two faculty members: Edward Ullman, an OSS alumni, and briefly at the Department of Geography at Harvard before it was shut down in 1951; and William Garrison, who served on US Air Force bombers in the Pacific Theatre before completing a Ph.D. in Geography at Northwestern University. It was at Northwestern that Garrison as a graduate student co-taught with Clarence Jones in his economic geography course. Their intellectual sensibilities could not have been more different, however. As Garrison says about Jones's lectures: "they led me to keep asking: "What's the theory? What's the theory? What's the theory" (Garrison, 1998). Specifically, "... a systematic approach was in order...." (Garrison, 1979, 119).

It was precisely a systematic approach to economic geography, one attuned to Cold War science that Ullman and Garrison pioneered at the University of Washington during the early 1950s, making it the premier economic geographical centre of economic geographical calculation. Both were thoroughly embedded in Cold War projects, institutions and money (for details see, Barnes and Farish, 2006). The money was especially important, particularly from the Office of Naval Research (ONR). Both men held substantial and continuing ONR grants during the mid-to-late 1950s. They enabled them to fund graduate students, and who were to lead the quantitative revolution's charge.

A*nnus mirablis* was 1955, when in the Fall of that year a remarkable group of graduate students serendipitously gathered at the Department of Geography, University of Washington, to study. Later labelled the "space cadets," they were to change fundamentally economic geography, fulfilling Garrison's goal of a "systematic approach." That first term they were exposed to the first advanced course in statistics ever given in a US geography department, Geography 426, Quantitative Methods in Geography and offered by Garrison. Richard Morrill (1998) who was in that first class says, "it wasn't just the introduction to beginning statistics but the whole gamut from beginning to all that was known in those days. So, it was a ferocious baptism." But it was not only numbers to which they were exposed, but also machines. There were the large, thudding Frieden calculators, but more important was the recently acquired, even larger, computer. In an early advertisement for the Washington department, the Head, Donald Hudson (1955), boasted about the departmental use of an IBM 604 digital computer, also a national first. The programming technique of so-called patch wiring involving plugging wires into a circuit board was crude and inefficient, but it helped define and consolidate the scientific vision of the discipline.

And then there was theory. It came from economics, and, perhaps more unlikely, from physics. Economic geography up until then was renowned for its antipathy towards theory. The British geographer George Chisholm, one of the late nineteenth century discipline's founders, had even "wish[ed] ... th[e] love of pure theory to the devil" (quoted in Wise, 1975: 2). But the University of Washington's programme was defined by the titrated drip of undiluted theory. Ullman's seminar on urban location theory provided the basics of Walter Christaller's and

August Lösch's central place theory. Garrison offered the "cadets" during their first year a seminar in economic theory, likely also a first in Anglo-American geography. He assigned as the text Walter's Isard's just published (1956) *Location and Space Economy*, a volume defined by differential equations, simplified abstract assumptions, and hard-headed neoclassical economic rigour and logic. There were no lions and tigers here, no forests and savannas of Africa. But Garrison and his students now knew "what's the theory."

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The other centre at the University of Iowa turned on the work of Harold McCarty. Direct Cold War connections were less apparent, although even here much of the research funding derived from the Office of Naval Research. In 1940, McCarty had written *The Geographic Basis of American Economic Life*, a book propelled by a stiff economic logic not found in comparable contemporary texts by economic geographers. "Economic geography derives its concepts largely from the field of economics" (McCarty, 1940: xiii) he wrote in the Preface. This was his mantra. All of his subsequent work took economic concepts, and tested them on the fodder of regional geography. Importantly for our purposes, that testing was formal, involving for the first time in the discipline inferential statistics, in this case, correlation and regression techniques. It was an approach solidified in the collective report, *The Measurement of Association in Industrial Geography* (McCarty et al., 1956). Consisting of a multitude of regression studies relating industrial location to various variables, economic geography provided the case studies, economics the theoretical muscle.

A cantankerous colleague of McCarty's, Fred Schaefer, was another contributor, in his case, by providing a philosophical rationale, positivism. A political refugee from Nazi Germany, Schaefer (1953) published in the flagship journal of American geography, a pointed philosophical critique of Hartshorne's justification of regional geography. Schaefer's alternative was positivism, a philosophy based on the canons of natural scientific practice emphasising explanation, logical deduction, stringent empirical testing, and the virtues of a formal, universal mathematical vocabulary. It was everything that Hartshorne's view of geography and Clarence Jones's view of economic geography, were not. Schaefer's positivism gave those at the University of Iowa, but also at other later centres of economic geographical calculation, an intellectual rationale grounded in a body of seemingly unassailable analytical philosophical strictures and developed by the philosophical heavy weights of the 1950s: Rudolph Carnap, Alfred Jules Ayer, Saul Kripke, and Willard Quine.

Washington and Iowa were central to the quantitative revolution in the United States. Outside of it, two other centres deserve mention. The first was the Department of Geography at Cambridge University. In autumn 1958, Richard Chorley and Peter Haggett (with David Harvey as teaching assistant) began lecturing first year undergraduate for the first time in the history of the Department on "statistical methods, matrices, set theory, trend surface analysis, and network analysis" (Chorley, 1995, 361). Then in 1960, Haggett (1965, v) began offering a lecture course in economic geography, the "much-thumbed and much-revised lecture notes" for which became *Locational Analysis in Human Geography*, still perhaps the most elegant statement of economic geography as spatial science. However, as Haggett (1965, vii) says, *Locational Analysis* was less an original contribution, than "a report from an active battlefront" occurring elsewhere, that is, in the United States. The second is Lund, Sweden, and associated with the iconoclast, Torsten Hägerstrand. Virtually on his own back, Hägerstrand (1967[1953]) developed and deployed during the early 1950s a set of statistical and theoretical techniques to understand innovation diffusion across the Swedish space economy. It was sophisticated and original, resulting in

Hägerstrand visiting the University of Washington in the late 1950s, influencing several of the "cadets."

By the late-1950s, the elements necessary for the quantitative revolution in economic geography were assembled: statistical and mathematical techniques, machines, theory, and an underlying philosophy. They needed to be put together, though, and diffused to a wider audience. Exactly this happened during the 1960s. Partly, the diffusion occurred through the bodies of the revolutionaries themselves, such as the "space cadets," as they left Seattle for new jobs. As they moved, they spread the word of revolution to new sites such as the University of Chicago, University of Michigan, Bristol University, and perhaps ironically given its past history, Northwestern University. Partly the diffusion occurred through seminars, special sessions at conferences, training camps in quantitative methods for the uninitiated (the first was held again ironically at Northwestern in 1961), and dedicated meetings such as the Madingley Hall lectures that Chorley and Haggett arranged at Cambridge for secondary school geography teachers in England and Wales (the first was held in July 1963, and subsequently published as a series entitled *Frontiers in Geographical Teaching*). And partly diffusion occurred through the circulation of purple mimeographed papers, various discussion paper series (the first originated at the University of Washington in March 1958), and later formal journals such as *Geographical Analysis* founded in 1969 (for details see, Barnes, 2004a).

Economic Geography After the Revolution

If this was how the quantitative revolution in economic geography happened, what was wrought by it? After the revolution, the discipline was utterly different. Disciplinary training, practices, forms of writing, places of research, venues of publication, the hierarchy of academic status, all changed. In particular, five characteristics defined economic geography after the revolution.

First, the discipline was characterised by the aspiration to a particular form of theory, the kind characterising physical sciences and mimicked especially in economics. For this reason the quantitative revolution was perhaps a misnomer. What excited revolutionaries was less numbers (although they turned the crank of some), but abstract theoretical explanations couched as hypotheses, models, axioms, and, the pinnacle, laws. This view of what counted as theory derived from the philosophy of science, and in particular positivism (and first systematically articulated for geography by David Harvey, 1969). Theories were conceived as formal statements, expressed in the vocabulary of mathematics, positing causal connections among classes of phenomena, and capable of empirical verification.

The problem was that given the intellectual history of the discipline, the influence, for example, of George Chisholm, or Richard Hartshorne, no indigenous economic geographical theory existed. It was necessary to beg, steal, and borrow it. A recouped tradition of German economic location theory that included Johann von Thunen's (1783-1850) formulations of agricultural land use and rent, Alfred Weber's (1869-1958) model of industrial location, and August Lösch's (1906-45) general theory of the space economy, provided an initial set of core concepts. Added to these from orthodox neoclassical economics and an allied movement, regional science (Barnes, 2004b), were rational choice theory as well as general and partial models of market equilibrium. Geometry provided network and graph theory as well as the mathematics of topological forms and used to explain spatial patterns such as transportation routes and trip patterns. And finally, physics offered gravity, potential and entropy models that, in turn, were used to explain geographies of spatial interaction, for example, the relation between the location of consumers and the location of retailers. These last models likely represented economic geography's finest theoretical hour. Thousands of papers were published on spatial interaction producing (at least for some) the ultimate: an economic geographical law. Waldo Tobler (1970, 236) formulated the First Law of Geography in August, 1969: "everything is related to everything else, but near things are more related than distant things."

More generally, economic geographers of the quantitative revolution believed that shaping the economy were a set of autonomous independent spatial forces that could be discerned, represented, and explained by drawing upon formal theory. Theory made the economic geographical world transparent and understandable. While initially theory would be taken from others, the ultimate aim was to construct home-grown versions. David Harvey exhorted geographers on the last page of *Explanation in Geography* "to pin up on our study walls … the slogan … 'By our theories you shall know us'" (Harvey, 1969, 486).

Second, there was the quantitative part of the quantitative revolution. Quantitative did not mean simply numbers. Chisholm's *Handbook of Commerical Geography*, published in 1889, and the first textbook in the discipline, was not short of figures. Rather, quantitative for the quantitative revolutionaries meant the use of formal statistical techniques both to represent numerical data, and to draw scientific inferences. The first forays were in descriptive statistics with use of the mean, and standard deviation. But inferential statistics, that is, drawing conclusions about the larger populations from samples, quickly followed. McCarty's (1956) use of bivariate and multivariate correlation and regression was an early illustration, and by 1959 the full range of inferential statistics was mobilised by Garrison (1959) and his students in their collective volume, *Studies in highway development and geographic change*. Geography 426 had clearly left its mark.

It was a slightly different story in Britain, where the quantitative revolution was slower in disseminating. In 1964 at the Institute of British Geographers' annual meeting Peter Haggett showed a multiple regression equation. The next day he was summoned by his Head of Department at Cambridge, Alfred Steers, who had been in the audience. "This kind of thing has got to stop," Steers told him, "You are bringing the subject into disrepute" (quoted in Thrift, 1995, 381-2).

Of course, it did not stop, nor could it stop. Haggett, in fact, was promoted on the basis of his economic geographical quantitative efforts, becoming Professor of Urban and Regional Geography at Bristol University in 1966, and helping to turn that Department into another centre of economic geographical calculation. Haggett along with others continued to develop and refine statistical methods, making them more appropriate for economic geographical use. Initially, though, there was a problem in applying non-geographical statistical techniques to spatial data. Traditional inferential statistical techniques assumed that sample data was independent; that is, the value of one data point was not influenced by the value of another data point. Spatial data of the kind used by economic geographers, however, generally violated this assumption: the value of a variable in one location was generally related to the value of the same variable in a nearby location. Spatial autocorrelation, as it is known, thus undermined the assumption of independent sampling, thereby invalidating traditional inferential statistical techniques. Since the early 1970s, however, new statistical techniques appropriate to economic geographical data were developed. Ironically given Steers' fulminations, the writings of Andy Cliff, formerly at Bristol, but subsequently Professor of Geography at Cambridge, was formative (Cliff and Ord, 1973).

TEXT BOX HAGGETT on statistics and hypothesis testing

Third, to cope with the numbers, to compute statistical formulae, required machines. Revolutionaries needed to appropriate the means of calculation. Initially, numerical work was carried out either on mechanical calculators like the Monroe, or electric calculators like the Frieden. Brian Berry (2000), one of Garrison's "space cadets," remembers "in his first semester [fall, 1955] learning statistics on these great big desk calculators that groaned." But as calculations became more complex and fraught as multivariate statistical techniques, and increasingly large data sets were deployed, even the trusty Monroe and Frieden balked. Something bigger and better was needed. Fortunately, the military-industrial-academic complex came to the rescue. The first computer in the US, The Electronic Numerical Integrator and Computer (ENIAC), was first used to make calculations for testing ordnance at the Aberdeen weapons site, Maryland. Cohen (1988, 135-6) estimates that over the ten years it was in service ENIAC carried out more "arithmetic than had been done by the whole human race prior to 1945." This was exactly the kind of machine that the quantitative revolution needed. In fact, it got something even better. By the mid-1950s when ENIAC was decommissioned, computer development was already transformed (in large part because of Cold War imperatives and money). In 1954, IBM, which was contracted by the military as its computer manufacturer, began selling computers commercially to universities, the first going to Columbia. Using the computer meant bootstrap learning, though. Waldo Tobler (1998), one of the Washington space cadets, remembers his early computing experiences in Seattle:

> We had to go up to the attic of the Chemistry building at 2 am so we could run the computer by ourselves. They didn't have any computer operators in those days, and that was before computer languages like FORTRAN. … To cover programming on the [IBM] 650 you had to pick up two bytes of information on one rotation of the drum. It

> had a 2K memory which rotated real fast. And if you were clever, you could pick up two pieces of information in one rotation.

By today's standards, the IBM 650 on which Tobler and the other space cadets worked during the early hours was a lumbering dinosaur, but even then it could perform calculations with a speed, consistency, and stamina that no human could match, and as such vital to the success of the quantitative revolution. Moreover, as computer hardware became more sophisticated, and as computer software was developed to make the machine user friendly, their importance only increased.

Fourth, the quantitative revolutionaries in economic geography appealed to a larger philosophical project, positivism, to justify and legitimate. This was new. Economic geographers in the past rarely sought philosophical rationales, and when they did, their statements were murky. Chisholm's were (see Barnes, 2000). And Hartshorne (1939) spent well over 400 pages trying to make his philosophy of geography clear. But he later said that only one person other than himself understood what he had written. In contrast, positivism was a philosophy that prided itself on a scrupulous and transparent clarity.

Positivism as a philosophy had existed since its first formulation by Auguste Comte in the first part of the nineteenth century. During the twentieth century it was taken up and reworked as logical positivism by a group of philosophers, mathematicians, and physical scientists in Vienna (the "Vienna Circle"). They argued there were only two kinds of true, meaningful (scientific) statements, each of which could be precisely defined and delineated. First, analytic statements were true by definition, and were the basis of the formal sciences, logic and mathematics. Second, synthetic statements were empirically verifiable, i.e., statements that could be unambiguously proven true or false by comparing them to real-world observations

(Ayer, 1959; Passmore, 1967). They were the basis of the substantive sciences. Both statements were true and meaningful because they could pass muster on the "verification principle;" that is, they could be logically or empirically confirmed. In contrast, moral, political, aesthetic, and philosophical judgments could never pass muster, and were senseless or worse, nonsense

Positivism as a philosophy was made for post-war American social science, becoming part of the World War II regime, emphasizing the importance of a mathematical vocabulary in which to express theory, and the importance of quantitative empirical verification. Spatial scientists were certainly attracted. Fred Schaefer's (1953) paper was the opening shot. A colleague of Schaefer's in the philosophy department at Iowa, Gustav Bergmann, and with whom he discussed his paper, was a former member of the Vienna Circle. Further, it was Bergmann who finalized Schaefer's paper after Schaefer died from a heart attack in an Iowa City cinema before it was published. It was also Bergman who taught a philosophy of science course that all incoming geography graduate students at Iowa were compelled to attend (Amedeo and Golledge's, 1975, textbook in economic geography was written as a result of them taking that seminar, and in this sense, it is the heir to Schaefer's 1953 paper). If Schaefer's paper was the opening shot, then David Harvey's (1969) *Explanation in Geography* was the closing one. Much more than Schaefer ever did, Harvey provided systematic accounts of both the analytic statements economic geographers could make in mathematics, logic, and especially geometry, as well as their synthetic statements, particularly the use of the verification principle embodied in particular statistical techniques.

Finally, there was a repositioning within the discipline's social hierarchy, albeit not without contestation. Young, male, very ambitious, very able graduate students and junior faculty primarily forged the quantitative revolution, and as it succeeded, they became the new top dogs. In some cases, their advance was rapid. Brian Berry completed both his MA and PhD at Washington in only three years, and by Fall, 1958, he was an Assistant Professor at the University of Chicago. Within seven years he was Full Professor (at the age of only 31). Or again, Haggett was appointed at Cambridge in 1957 as a Demonstrator (and below an Assistant Lecturer). Less than a decade later, he was Professor of Urban and Regional Geography at Bristol University.

Of course, there was some resentment of these “Young Turks.” Steers’ chastising of Haggett was one example. Or again, Joseph Spencer, an old time regional geographer, and editor of the *Annals, Association of American Geographers* from 1964, vented his frustration by trying to block publication in the journal of quantitative/theoretical papers, suggesting that at best they be confined to a ‘Research Notes’ section. It was partly Spencer’s attitude that led to the proliferation of alternative publishing outlets such as departmental discussion paper series, and culminating in the establishment of a journal dedicated to quantitative economic geography, *Geographical Analysis*.

Perhaps the most marked social characteristic of the revolutionaries was their masculinism. All of the early quantitative economic geographers were male. Moreover, they often acted typically male, tending to be competitive, pushy, playing practical jokes, and being sometimes boastful and evangelical in their beliefs. Michael Dacey, one of the believers at Washington, says "we were very aggressive, very ambitious, and very appreciative. ... We were full of missionary fervour, and I imagine we were unlikable brats. ... In retrospect we must have been very disorientating to the establishment" (Dacey, 1997). Some of these characteristics come out in their writing, which could be brash and combative. It also made it difficult for women to participate. Susan Hanson (2002) when she enrolled at Northwestern University in

1967 says, “We knew very well that we were entering male turf.” And ten years later, not much seemed to have changed. Pat Burnett who was a faculty member at Northwestern in the late 1970s sued the Department for its “climate of sex discrimination” (Burnett 2002).

The broader point is that the social matters, going all the way down. The quantitative revolution in economic geography did not happen at arm’s length, as if theory, numbers, machines, and a positivist philosophy, entered the discipline autonomously, on their own volition. But they were always connected to a set of social relations. It was not spatial science in the abstract, but spatial science incarnate.

The End of the Story?

1969 might have been the high-water mark of the quantitative revolution in economic geography: Harvey’s philosophical bible of the movement was published, *Geographical Analysis* was launched, and Tobler announced the First Law of Geography. But it wasn’t to last.

Harvey was a central figure in the subsequent unravelling. Even before he finished *Explanation*, he had doubts. In 1971 at the annual meeting of the Association of American Geographers in Boston, those doubts erupted. He announced there that:

> [Geography’s] quantitative revolution has run its course and diminishing marginal returns are apparently setting in as … [it] serve[s] to tell us less and less about anything of great relevance…. There is a clear disparity between the sophisticated theoretical and methodological framework which we are using and our ability to say anything really meaningful about events as they unfold around us. … In short, our paradigm is not coping well (Harvey, 1972, 6).

The rest of the 1970s was a decade in which various elements of the quantitative revolution within economic geography were in turn held up for scrutiny, and found wanting. Harvey (1972) began by attacking the usefulness of the theory and statistical techniques he celebrated only three years earlier, portraying them as at best irrelevant, and at worse, politically regressive and counter-revolutionary. Another beachhead was made on the internal logic of mathematics itself. Gunnar Olsson (1975), disgruntled with the spatial interaction models he had earlier triumphed, argued that their very formal reasoning, especially the invocation of the dreaded equals sign, undid any claim to empirical veracity. They were true only in relation to the language in which they were written, and not true, and could never be true, to the world. Doreen Massey's (1973) target was the use of orthodox economic theory, including the German location models, in understanding industrial location that she argued failed at every level: on logic, empirics, philosophy, and politics. And right at the end of the decade, the bastion of positivism itself, including its attendant mathematical and statistical apparatus of theory construction and verification was forensically dissected by Andrew Sayer (1979, 1982) who declared the body well and truly dead. Drawing on critical realism, Sayer argued that positivism failed to recognise the importance of causal mechanisms, and instead was content with the sop of mere association, and manifest in economic geography as an endless stream of correlation and regression studies that explained nothing.

Of course, for some, the announcement of the death of the quantitative revolution in economic geography was greatly exaggerated. They were not willing to shuffle off to the other side. At least, not yet. None of the pioneer quantitative revolutionaries changed positions, and only some of those in the second generation did like Harvey and Olsson. But the latter were important, with status, and commanding attention. And in a small discipline like economic geography they made a difference, drawing allies to them, and to the new positions they held. There was no single death blow, but by the end of the 1970s, the quantitative revolution view of economic geography was fading. Interestingly, exactly the same trajectory characterised the allied movement of regional science. Rising meteorically from the mid-1950s, often in concert with quantitative economic geography, regional science's lustre dulled also from the 1970s (Barnes, 2004b). So much so that in 1995 the founding Regional Science Department at the University of Pennsylvania closed its doors to students, its faculty re-assigned to other Departments.

In its stead arose various stripes of a much more healthy and vigorous political economy based upon the writings of Harvey, Massey, and Sayer. While all three resolutely rejected positivism, number crunching, and any form of neoclassical theorising, the quantitative revolution had nonetheless left its mark. All three, in fact, began as spatial scientists, and the kind of systematicity, logic, and abstraction that they brought to their new political economic theory was in many ways a carry over from their earlier lives.

More generally, the legacy of the quantitative revolution still remains potent For example, the contemporary interest in geographical economics, including the founding of the *Journal of Economic Geography* in 2001, is a clear continuation of the quantitative revolution's impulse toward rigour, a formal vocabulary, and modelling. Similarly, so is the continuing interest in GIS, and towards which some of the Washington space cadets later gravitated such as Duane Marble and Arthur Getis. The legacy also continues indirectly. Perhaps the most important has been the continued stress on theory. Even the most virulent 1970s critics of the quantitative revolution shared the belief that the discipline must be theoretical. This was the real watershed, separating economic geography of the last fifty years from economic geography of fifty years before that. When political economic critics attacked the quantitative revolution, and offered their alternatives, it was in a theoretical vocabulary made possible precisely by that revolution. The quantitative revolution enabled economic geography to become a mainstream social science, and to be taken as seriously as the others. For that reason, maybe there should be more praising than burying, and the realisation that we are more the product of some defunct spatial scientist than we might imagine.

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Further Reading

On the effects that the Second World War and Cold War had on making the American social sciences more quantitative and theoretical see, Philip Mirowski's (2002) *Machine dreams: Economics becomes a cyborg science*, Cambridge, Cambridge University Press. On the military development of computers see, Paul Edwards' (1996) *The closed world: Computers and the politics of discourse in cold war America*, Cambridge, MA, MIT Press. The role of geographers at OSS is discussed well by Andrew Kirby (1994) What did you do in the war, daddy? In A. Godlewska and N. Smith, editors, *Geography and empire*, Oxford, Blackwell, 300-315. There are numerous accounts of geography's quantitative revolution. A good autobiographical collection is *Geographical voices: fourteen autobiographical essays*, edited by Peter Gould, and Forest Pitts, Syracuse, NY: Syracuse University Press (2002. My own paper, Placing ideas: *Genius Loci*, heterotopia, and geography's quantitative revolution, *Progress in Human Geography*, 29, 565-95 (2004), draws upon a series of interviews I conducted with several of the revolutionaries. In addition, very good is: Allen Scott's overview of economic geography during the last fifty years in Economic geography: the great half century, in G. Clark, M. Feldman, and M. Gertler, M., editors, *The Oxford handbook of economic geography*, Oxford: Oxford University Press, 18-48 (2000); and Ron Johnston's essay focussing on the British scene, Order in space: geography a discipline in distance, in R. Johnston, and M. Williams, editors, *A century of British geography*, Oxford, Oxford University Press, 303-45 (2003).

Text box: Theory from David Harvey, *Explanation in Geography*, pp. 87 and 118-19

"The quest for an explanation," writes Zetterberg, "is a quest for theory." The development of theory is at the heart all explanation, and most writers doubt if observation or description can be theory-free…. Some writers, Berry and Blaut among them, regard the evolution of a distinctive theoretical structure for explaining certain sets of phenomena as being the main justification for regarding geography as a distinctive and independent discipline within the empirical sciences. If this be true, then a clarification of the "nature" of geography depends upon the prior clarification of the nature, form, and function of theory in geography.

Economic concepts have frequently been used as the foundation for geographic theory. Economics has been the most successful of the social sciences in developing formal theory …. In particular the whole of location theory, which has been "especially concerned with the development of the theoretical-deductive method in geography," can be related to economic postulates. … [I]it is interesting to examine just one case in detail.

Central place theory has been described as the "one relatively well developed branch of theoretical economic geography." The foundations were laid by Christaller in 1933 … In the first part of his book Christaller uses elementary demand analysis to define a fundamental spatial concept – *the range of a good* – and this in conjunction with other economic arguments, led Christaller to define an "optimal" spatial organisation of a hierarchy of settlements. Christaller did not resort to formal deductive procedures nor did he attempt to develop a formal theory of settlement location. Lösch treated the location of settlements as part of the general location problem and, ground his analysis firmly in Chamberlinian economic theory, gave a far more powerful theoretical foundation for the settlement theories of Christaller. … Later elaboratioins of the theory by Isard and many other economists and geographers have served to tighten up parts of the theoretical argument and to indicate the empirical status of the theory.

Central-place theory provides just one example out of many to demonstrate how geographical theory may be derived from the basic postulates of economics. The existence of such postulates was undoubtedly an important necessary condition for the emergence of a theoretical human geography.

Text box: World War II Regime from Andrew Pickering "Cyborg history and the World War II regime." pp. 8 and 18

Everyone thinks of the World War II intersection of science and the military in terms of the material contributions of the former to the latter, few people … think about the social transformations that accompanied these material flows. But, as I shall now attempt to explain, social and material transformations hung together and reinforced one another. As a baseline, we can note that before World War II, science and the military in the United States were more or less decoupled.

[During] World War II, [however, there emerged an] intense coupling of science and the military. In a series of open ended social, technical, material, and conceptual developments, the civilian/scientific and military communities redefined themselves around endpoints that were interactively stabilized against and reciprocally dependent upon one another. The inner technical practices of both science and the military were radically transformed in World War II, science (especially the physical sciences) turning into object-orientated multidisciplinary "big science," and the military moving from traditionally structured tactics and strategy to scientifically planned warfare deploying the new technoscientific objects. There transformations were tuned to and aligned with one another via the establishment of new institutions of surveillance and control, by the creation of new technoscientific artefacts (like radar) and their circulation into military practice, and by the development of Operations Research, a new conceptual apparatus that invaded military practice carried by a civilian vector (physicists and mathematicians). … What had been largely separate and autonomous institutions before World War II – science and the military – had become profoundly transformed and locked together as complex, social, material, and conceptual cyborg entity by the end of it.

Text Box: Statistical models of hypothesis testing from Haggett's Locational Analysis in Human Geography, p. 278

One of the simplest models for the testing of hypotheses in human geography has been evolved by McCarty. He argues for a sequential approach to geographical research in which (i) problems are defined, (ii) hypotheses are applied, (iii) their effectiveness is evaluated, and (iv) new hypotheses evolved to explain the discrepancies. In specific terms we may frame McCarty's approach as a series of regression cycles, as those shown in Figure 1. In the *first cycle* (C_1) the problem is defined, data are collected, statistically analysed, and plotted as an isopleth map; we may term this the problem distribution (Y). Analysis of the map of the Y-distribution leads to the formulation of a hypothesis (H_1) to account for the geographical irregularities in its form. This initiates the *second cycle* (C_2) with the definition of an explanatory variable, X_1, for which data are collected, are statistically analysed, and related to the problem variable through regression analysis. Using the regression equation ($Y = f\ X_1$), the derivations for the actual distribution of Y from the predicated distribution Y_c can be measured. The map of these deviations of *residuals* represents the end of the second cycle. Analysis of the map of residuals may lead to a second hypothesis to account for the "unexplained" distribution. This initiates the *third cycle* (C_3), beginning with the definition of the second explanatory variable, X_2. As Figure 1 shows, this third cycle parallels the second cycle and may lead to further cycles (C_4 to C_n) until a satisfactory level of explanation is reached.

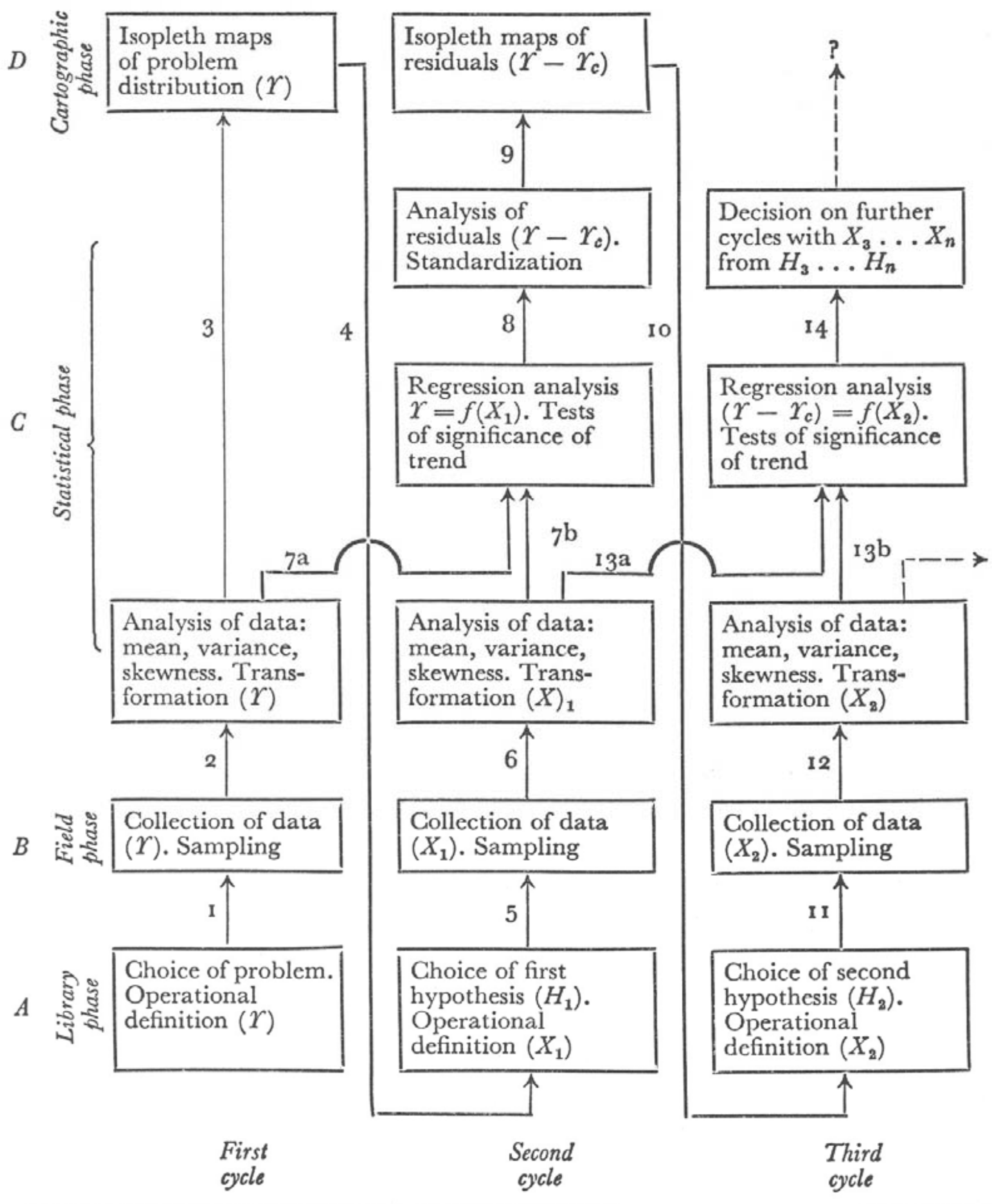

Figure 1 Model of regression cycles in geographic research, from P. Haggett, Locational Analysis in Human Geography, 1965, page 279.