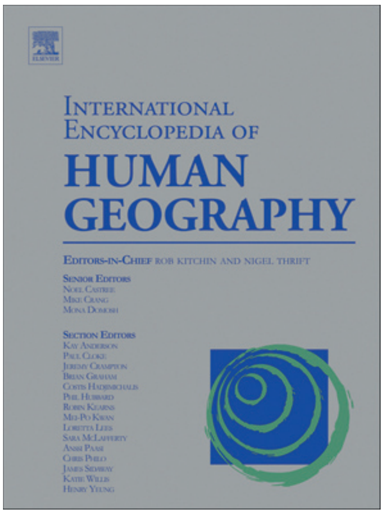

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Quantitative Revolution

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Glossary

Areal Differentiation It is a focus on spatial differences across human and physical phenomena on the Earth's surface, and especially as found among different regions.

Correlation and Regression It is a set of formal mathematical techniques developed in statistics to assess the existence and strength of a relationship among different variables.

Exceptionalism It is a term coined by Fred Schaefer to denote Richard Hartshorne's view that geography was different from other disciplines, methodologically unique.

Idiographic Concepts and approaches directed toward the elucidation of the unique and the particular.

Neoclassical Economics The orthodox school of economics defi ned by: the use of the rationality postulate, a belief in market equilibrium and optimality, and the deployment of formal theory and rigorous techniques of empirical testing.

Nomothetic It is the study and discovery of general traits within phenomena with the ultimate end of making universal law-like statements.

Scientifi c Laws These are statements of an invariant universal relationship holding between either different phenomena or different states of the same phenomena.

Spatial Interaction It defi nes the relationship of one place to another measured by the fl ows of phenomena (e.g., people, goods, and ideas) between them.

Theory It is defi ned as a set of abstract, logically connected statements for the purpose of explanation.

Introduction

The quantitative revolution represented a profound transformation in Anglo-American human geography from the mid-1950s and defined by the systematic application of scientific forms of theorizing and rigorous statistical techniques of analysis and description. In the process, the previous idiographic regional geography concerned with describing, cataloging, and delineating areal differentiation was pushed aside, replaced by a nomothetic 'new geography' directed toward explaining, scientifically proving, and abstractly theorizing spatial phenomena and relations. Geography was no longer rote memorization of place names, mountains chains, and population sizes, but was a science – spatial science.

Antecedents: Regional Geography, World War II, and the Cold War

Anglo-American geography before World War II was dominated by a regional approach, fixated on inventorying the peculiar characteristics of particular places, and carried out using one typological scheme or another. Put the St. Lawrence under rivers; the Rockies under mountains; Montreal and Toronto under cities; and barley, wheat, and corn under agricultural produce. By comparing the contents of the typological grid for different regions, the uniqueness of each place was manifest.

Such practices were given a splash of intellectual respectability by the American geographer Richard Hartshorne (1899–1992) when in 1939 he wrote the most systematic and philosophically sophisticated monograph yet published in English on the history and definition of the discipline, *The Nature of Geography*. Drawing especially on German writers, he argued that the building blocks of geographical knowledge were regions, defined as complex combinations of hard facts and specific causal relations, both of which were capable of objective disclosure. In turn, this combinatorial character ensured that each region was unique, not found anywhere else. Consequently, traditional scientific forms of explanation based on general laws did not apply. Natural sciences explain by asserting general (generic) relationships between homogeneous classes of phenomena: if class of phenomena A, then class of phenomena B. But Hartshorne postulated that geographical regions were never homogeneous; each region was unique. Traditional scientific explanation in which instances of broader classes of phenomena were related by a law-like statement had no purchase. Regions are not categorizable as homogeneous classes. They could be described, but neither scientifically explained nor predicted.

Just before *The Nature* was published, Europe was pitched into war, and 2 years later, America too was embroiled. Hartshorne was recruited as an administrator into what became the Office of Strategic Services (OSS), forerunner of the Central Intelligence Agency (CIA). The OSS, and especially its Research and Analysis Branch, was charged with collecting and analyzing information that bore on national security. To do so, a large number of American academics as well as foreign *émigré* scholars were recruited, and made to engage one another across their disciplinary affiliations. The result was a new kind of social scientific inquiry that emphasized science much more than the social. It was team based, mixing disciplines, instrumental, model and even machine driven, quantitative, and drawing upon abstract theory. Further, following science, it emphasized explanation rather than description, generality rather than uniqueness. Of course, this kind of pursuit was antithetical to Hartshorne's view of geography. Insofar as he could, he prevented geographers employed at OSS from undertaking this form of investigation. But the genie was out of the bottle.

The new approach fomented within OSS influenced a range of social sciences once World War II was over. Economics and psychology became paradigm examples and, to a lesser degree, sociology and political science, and even some humanities such as philosophy. Human geography became just another (albeit late) addition to the list.

Giving momentum to the change was also the Cold War. Believing that science and the kind of social science pioneered at OSS had won World War II, the US government through military organizations such as the Office of Naval Research (ONR) poured inordinate amounts of money into science and social science to win the Cold War. The result was yet further impetus to what Carl Schorske, a former OSS alumnus, called the 'new rigorism' within post-war US human sciences. As a force, it was difficult to resist. Certainly, a number of younger US geographers, some of whom had served at OSS, such as Edward Ackerman, Chauncey Harris, and Edward Ullman, could not resist. Once demobilized and back in the academy, they began hesitantly to practice a different kind of geography: a scientific kind. It was one in which regions were still important, but now conceived quite differently to Hartshorne, as explanatory, theoretical, and instrumental, a tool to achieve functional objectives. Geography's quantitative revolution was about to begin.

Centers of Geographical Calculation

It took almost a decade to realize, however. It was only in the mid-1950s that the first real pangs of conversion from a dull, older regional geography to a shining new spatial science were registered in the discipline as a whole. At first, the change was highly localized, confined initially in the United States to two centers: the University of Washington, Seattle, and the University of Iowa, Iowa City.

Key at the University of Washington were two faculty members: Edward Ullman, formerly in the OSS; and William Garrison, who, during the war, served on Army Air Force bombers in the Pacific Theater and after his demobilization, he completed a PhD in geography at Northwestern University. In the Air Force, Garrison was trained in statistics and mathematical methods. None of that knowledge was useful, though, once he entered Northwestern as a graduate student, where he was mired within the Hartshorne tradition. He later said in an autobiographical essay that his resulting regional-geography-based PhD dissertation involved "just a lot of walking around, and mere description." In contrast, he was interested in knowing "what's the theory, what's the theory, what's the theory" (personal communication).

He began to answer that question once he was hired at the University of Washington in 1950, assembling around him others interested in the same question. *Annus mirablis* was 1955, when in the autumn of that year a remarkable group of graduate students serendipitously arrived under the supervision of Ullman and Garrison. Later labeled the 'space cadets', they were the revolutionaries of geography's quantitative revolution. In the 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, 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" (personal communication). But it was not only numbers to which they were exposed, but also to machines. There were the large, thudding Frieden calculators, but even more important was the recently acquired, even larger, computer. In an early advertisement for the Washington department, the Head, Donald Hudson, boasted about the departmental use of an IBM 604 digital computer, a national first. The programming technique of the 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.

Then there was theory. Without any indigenous tradition, theories were frequently begged, borrowed, and stolen from other fields. Ullman's seminar on urban location theory, also given in 1955–56, provided the basics of Walter Christaller's and August Lösch's central place theory. Garrison offered the cadets a seminar in economic theory using the regional scientist Walter Isard's just published *Location and Space Economy*. Regional science was a kindred movement to the quantitative revolution, and early on at least there was a symbiotic relationship. Always more economic than economic geography, regional science was steeped in neoclassical economics. Isard's book defined by differential equations, simplified abstract assumptions, and hardheaded rigor and logic left Garrison's students with no doubts about "what's the theory."

At the other center, the University of Iowa, the work of Harold McCarty was important. As at the University of Washington, McCarty funded his students from grants provided by ONR. He used the money to carry out studies of industrial linkage and represented formally using inferential statistics. In 1953, McCarty was the first human geographer ever to use correlation and regression techniques.

A cantankerous colleague of McCarty's at Iowa, Fred Schaefer, was a second Iowa contributor. Schaefer provided the larger philosophical rationale for the revolution: positivism. A political refugee from Nazi Germany, Schaefer published, in 1953, in the flagship journal of American geography a pointed philosophical critique of Hartshorne's idiographic justification of regional geography that he labeled 'exceptionalism'. Schaefer's alternative was positivism – a philosophy based on the canons of natural scientific practice emphasizing explanation, logical deduction, stringent empirical testing, and the virtues of a formal, universal mathematical vocabulary. It was everything that Hartshorne's regional geography was not. Schaefer's positivism gave those not only at the University of Iowa, but also at other later centers of geographical calculation, an intellectual rationale grounded in a body of seemingly unassailable analytical philosophical writings.

Outside of this North American frame, two other centers require mention. The first was the department of geography at Cambridge University. In autumn 1958, Richard Chorley and Peter Haggett (with David Harvey as a teaching assistant) began lecturing first-year undergraduates – for the first time in the history of the department – on statistical methods, matrices, set theory, trend surface analysis, and network analysis. Chorley and Haggett were later dubbed 'the terrible twins' of British geography for their pioneering work that later changed fundamentally the practices of both economic geography (Haggett's specialty) and geomorphology (Chorley's field). Further, that Chorley and Haggett came from different halves of the discipline, but could speak the same language demonstrated the centrality of scientific reasoning for potentially uniting physical and human geography – a long-held dream, but hitherto never realized.

The second is Lund, Sweden, and associated with the iconoclast, Torsten Hägerstrand. Virtually on his own back, Hägerstrand 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 1960, all the elements necessary for the quantitative revolution in human 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 process unfolded during the ensuing decade. Partly, the diffusion occurred through the bodies of the revolutionaries themselves, as, for example, the space cadets left Seattle for new jobs throughout America. As they moved, they spread the word of revolution to new sites such as the University of Chicago, University of Michigan, Northwestern University, and later, Ohio State, SUNY Buffalo, Toronto, and Bristol. Partly, the diffusion occurred through seminars, special sessions at conferences, training camps in quantitative methods for the uninitiated (the first was held 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*). Finally, 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.

That said, not everyone was convinced, not every department of geography went quantitative, and not every subdiscipline turned to numbers. American cultural geography was a hold out, with Carl Sauer, its unofficial leader, writing in 1967, "I am saddened by model builders and system builders and piddlers with formulas for imaginary universals" (quoted in Sauer, 1987: xv). British geography was generally slower on the quantitative uptake than its US counterpart. Urban and social geography, and especially regional geography, did not experience change until the second half of the 1960s, if they experienced change at all. In some cases, resistance was by an old guard who was bent on holding on to the reins of power (which was the case of the University of California, Los Angeles (UCLA) regional geographer, Joe Spencer, who as the editor of the important publication *Annals of the Association American Geographers* had the means to retard the revolution), but in other cases, it was an intellectual and political antipathy against the movement. The University of Minnesota's Fred Lukermann, for example, was a critic as early as the late 1950s, and the University of Minnesota where he was chair and later dean never became a 'center of calculation'.

Geography after the Revolution

For this reason, Ian Burton's 1963 announcement of the completion of geography's quantitative revolution, precipitating within the discipline "a radical transformation in spirit and purpose" (Burton, 1963: 151–162), was premature and exaggerated. But something was changing, and throughout the 1960s, increasing numbers of geographers and geography departments were pulled in. Five characteristics of the revolution are particularly germane.

First, the discipline was characterized by the use of a particular form of theory, one found in the physical sciences, especially physics, and later mimicked by economics. For this reason, the quantitative revolution was perhaps a misnomer. What excited revolutionaries was less numbers *per se* (although enthused by some), but abstract theoretical explanations couched as scientific hypotheses, mathematical models, and, the pinnacle, laws. This form of theory derived from the philosophy of science, and in particular positivism. Here, 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 for quantitative revolutionaries, though, was the historical absence of theory of this type within geography, indeed, of any theory. Apart from a discredited dalliance with the theory of environmental determinism, human geography previously prided itself on its atheoreticism. Theoretical inspiration, therefore, necessarily needed to come from outside. There were several sources. A recouped tradition of German location theory that included Johann von Thünen'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 regional science, were rational choice theory as well as general and partial models of market equilibrium. Later from sociology came urban land-use models, urban factorial ecology, and the rank-size rule. 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. Finally, physics offered gravity, potential, and later entropy models that, in turn, were used to theorize spatial interaction. These last models likely represented human geography's finest theoretical hour. Thousands of papers were published on spatial interaction, producing (at least for some) the ultimate: a geographical law. Waldo Tobler (1969) formulated the 'first law of geography': "everything is related to everything else, but near things are more related than distant things."

While initially theory would be taken from others, the eventual aim was to construct home-grown versions. In what became the methodological and philosophical bible of the quantitative revolution, *Explanation in Geography*, David Harvey (1969) famously finished with the rallying cry, "By our theories you shall know us."

Second, there was the quantitative part of the quantitative revolution. Quantitative did not mean simply numbers. Geographers were numerate from the beginning. Between 1852 and 1871, the American Geographical (AGS) and Statistical societies were officially twinned, and even when they went their separate ways, the mandate of the AGS remained the collection, classification, and scientific arrangement of statistics and their results. 'Quantitative' for the quantitative revolutionaries, rather, meant formal statistical techniques and used to represent numerical data, and to draw scientific inferences. The first forays were in descriptive statistics, but inferential statistics, that is, drawing conclusions about larger populations from samples, quickly followed, at least in the United States.

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 was in the audience. "This kind of thing has got to stop," Steers told him, "You are bringing the subject into disrepute." Of course, it did not stop, nor could it stop. In fact, 2 years later, Haggett was promoted to professor of urban and regional geography at Bristol University, and in part because of his multiple regression equation. More generally, Haggett, along with other British geographers, throughout the second half of the 1960s and early 1970s developed and refined a new body of statistical methods designed to accommodate the peculiarities of geographical data, and which included novel spatial sampling methods, spatial autoregression techniques, and work on the areal modifiable unit problem.

Third, to cope with the numbers, and to calculate statistical formulae, required machines. Initially, 'computing rooms' meant places in which were found banks of hand calculators, either of the mechanical kind like the Monroe, or the electric kind like the Frieden. But as computations became more complex and fraught because of the use of multivariate statistical techniques, and increasingly large datasets, 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. Within 10 years, it carried out more calculations than had been completed during the whole of previous human history. 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 the 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. There was no online help and not even a language for programming. Instructions were sent by plugging and unplugging electrical circuits.

Waldo Tobler, 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. (personal communication)

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 vital to the success of the quantitative revolution.

Fourth, the quantitative revolutionaries appealed to a larger philosophical project – positivism – to justify and legitimate. Positivism as a philosophy existed since Auguste Comte's formulations 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 the basis of the formal sciences, logic, and mathematics. Second, synthetic statements were empirically verifiable, that is, statements that could be unambiguously proven true or false by comparing them to real-world observations. They were the basis of the substantive sciences.

Positivism as a philosophy was made for post-war American social science, 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 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 for publication after Schaefer died from a heart attack in an Iowa City cinema. It was also Bergmann who taught a philosophy of science course that all incoming geography graduate students at Iowa were compelled to attend (Douglas Amedeo and Reginald Golledge's 1975 textbook *An Introduction to Scientifi c Reasoning in 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 *Explanation in Geography* was the closing one. Much more than Schaefer ever did, Harvey provided systematic accounts of both the analytic statements geographers could make in mathematics, logic, and especially geometry, as well as their synthetic statements, particularly methods of empirical testing using 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 geography. In some cases, their advance was rapid. Brian Berry completed both his MA and PhD at Washington in only 3 years, and by fall, 1958, he was an assistant professor at the University of Chicago. Within 7 years, he was full professor (at the age of 31). Again, Haggett was appointed at Cambridge in 1957 as a demonstrator (i.e., below an assistant lecturer). Less than a decade later, he was professor of urban and regional geography at Bristol. Peter Taylor has argued that the quantitative revolution was propelled, in part by internal sociological reasons, to secure precisely a rapid rise in career status.

Perhaps the most marked social characteristic of the revolutionaries was their masculinism. All of the early quantitative geographers were male. Moreover, they often acted typically male, tending to be competitive, pushy, playing practical jokes, and being sometimes boastful and arrogant. Michael Dacey, one of the believers at Washington, says "we were very aggressive, very ambitious, and very appreciative. … We were full of missionary fervor, and I imagine we were unlikable brats. … In retrospect we must have been very disorientating to the establishment" (personal communication). Some of these characteristics come out in their writing, which could be occasionally brash and combative. It also made it difficult for women to participate. Susan Hanson, when she enrolled at Northwestern University in 1967, says, "We knew very well that we were entering male turf." Ten years later, not much seemed to have changed. Pat Burnett (personal communication) who was a faculty member at Northwestern in the late 1970s sued the department for its 'climate of sex discrimination'.

The broader point was that the quantitative revolution 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 connected to a set of social processes and bodily inscriptions. It was never spatial science in the abstract, but spatial science incarnate.

Revolting against the Revolution

The revolution could not last. It was out of synch with the discipline's own intellectual past that emphasized grounded context and geographical singularity; it was out of synch with its own historical and political moment of especially the late 1960s that began to react against the violence of abstraction, technology, formal ordering, and centralized authority; and it was even out of synch with scientific logic as assorted logical contradictions, inconsistencies, and *aporias* within the work of the quantitative revolutionaries revealed.

Harvey was a central figure in the subsequent unraveling. 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. (Published as Revolutionary and counter-revolutionary theory in geography and the problem of ghetto formation, *Antipode* 4, page 6, 1972.) (Harvey, 1972: 6)

The rest of the 1970s was a decade in which various elements of the quantitative revolution were in turn held up for scrutiny and found wanting. Harvey began by attacking the usefulness of the theory and statistical techniques, portraying them as at best irrelevant and worst politically regressive ('counter-revolutionary'). Gunnar Olsson, disgruntled with the spatial interaction models he earlier triumphed, argued that their very formal reasoning undid any claim to empirical veracity, and when they were applied they made bad worlds and not good ones, worsening the lot of humans and not improving it. In similar vein, Robert Sack argued that the very idea of a separate science of space was logically inconsistent with the scientific principles invoked. Finally, special disdain was directed toward positivism, thus becoming the hapless sparring partner for a series of postpositivist approaches, including Marxism, humanistic geography, critical realism, and critical theory.

In spite of this critique, there have remained some striking continuities. Two stand out. First, the quantitative revolution never really died, but morphed into new forms, especially in the practice and discussions around geographic information system (GIS) from the 1980s. Indeed, two of the original Seattle space cadets, Art Getis and Duane Marble, were pioneers in the later GIS revolution. Further, some of the debates in the quantitative revolution found echoes in discussions in GIS around visualization, datasets, values, and political relevance. The second is the persistence of a theoretical vocabulary and which was introduced by the quantitative revolution. Although rarely recognized, the upholding of a theoretical discourse represents a watershed, separating the last 50 years of the discipline's history from the 50 years before that. The meaning of theory has altered, but the constancy of a theoretical vocabulary has proven more important in subsequently shaping the discipline than any rupture. We are more the product of some defunct theoretical spatial scientist than perhaps we realize.

See also: Cold War; Masculinities; Quantitative Methodologies; Scientifi c Method; Spatial Science.

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