Quantitative Methods in Human Geography I: The World We Have Lost – or Where We Started From

Ron Johnston, Richard Harris, Kelvyn Jones, David Manley, Wenfei Winnie Wang, and Levi Wolf
University of Bristol, UK

Abstract
Although pioneering studies using statistical methods in geographical data analysis were published in the 1930s it was only in the 1960s that their increasing use in human geography led to a claim that a 'quantitative revolution' had taken place. The widespread use of quantitative methods from then on was associated with changes in both disciplinary philosophy and substantive focus. The first decades of the 'revolution' saw quantitative analyses focused on the search for spatial order of a geometric form within an, often implicit, logical positivist framework. In the first of three reviews of the use of quantitative methods in human geography this progress report uncovers their origin with regard to the underlying philosophy, the focus on spatial order, and the nature of the methods deployed. Subsequent reports will outline the changes in all three that occurred in later decades and will chart the contemporary situation.
Keywords
Quantitative revolution, philosophy, spatial order, paradigm shift

Some fifty-five years ago, Ian Burton (1963) claimed that not only had geography '[i]n the past decade undergone a radical transformation of spirit and purpose', which he thought was 'best described as the "quantitative revolution"' but also that 'it reached its culmination in the period from 1957 to 1960, and is now over'. That conclusion was presumptuous; indeed – in the strict sense of the term 'revolution' – it was wrong in the long- as well as the short-term. Geographers in North America may have all been aware by 1960 of the debates regarding quantification and theory raging there, but most in the rest of the world were certainly not.[1] Furthermore, there was never a revolution, if by that term we understand total change following overthrow of the status quo. The 'revolutionaries' goal, exemplified in retrospect by Gould's (1979) scathing essay, was to end the dominance of regional geography and in this they were partly successful, especially in the UK by the 1970s. Gould (1979, 139) saw the period involving 'breaking out of the banal, factual boxes erected by the old men', who 'misread the signs and pinned on the wrong labels. It was not the numbers that were important but a whole new way of looking at things geographic' – p. 141 – seeking, in Whitehead's words, to identify 'what is general in what is particular and what is permanent in what is transitory'. Gould's generation was 'sick of the bumbling amateurism and antiquarianism that had spent nearly half a century of opportunity in the university piling up a tipheap of unstructured factual accounts'; 'Geography had stagnated for decades, without tools, without methodological

[1] One of us (RJ) was an undergraduate at the University of Manchester between 1959 and 1962 and heard not a whisper about it; only in the following two years, as a postgraduate reading more widely than specified earlier, did he encounter the relevant literature and debates about theory and statistics.

insight and development, without principles and constructs that gave coherence and order to observations' (p. 143). The use of statistics offered a useful methodology and although some of the early applications were – as Spate (1960) observed – 'naïve and inappropriate' the enlarged paradigm within which they were applied was set in a philosophical framework whereby geography now shared 'the questions of the sciences, something no field defined by inquiry into the particular and the unique could tolerate' (Gould, 1979, 143).

Quantitative-theoretical work never achieved disciplinary dominance, however, even in areas such as urban geography (as illustrated by Wheeler, 2002), partly because of resistance – not only from some traditional 'regional geographers' but also in historical and cultural geography too, plus some resistant economic and social geographers. The quantifiers had established a strong position in most North American geography departments, as exemplified by compulsory courses in statistics there; LaValle et al. (1967) reported that courses in quantitative methods were offered in 78 per cent of graduate schools in 1965, compared to only 3 per cent a decade earlier. But their putative revolution had failed; no sooner had they established this strong bridgehead than other revolutions were being fomented, offering alternative approaches to the discipline with very different epistemological and ontological foundations, let alone methodological protocols.

The period from the end of the 1950s to the early 1980s was characterised by strident debate between protagonists for the alternatives vying for precedence within the discipline, but there was no outright winner. Since then, the debates over quantification have become much more muted – although the developments in what became Geographical Information Science were subject to considerable attack from some quarters (e.g., Pickles, 1995). Instead, a mutual accommodation has been adopted (Cresswell, 2013), with adherents to the various positions content that each continues to be practised within the discipline's portfolio – although there have been continued contests for resources (teaching positions and graduate studentships within departments, for example) and occasional attempts to enhance one group's position by 'writing out' one or more of its competitors (as in textbooks: Johnston, 2006). Such widespread mutual tolerance has in many cases been associated with mutual misunderstanding and avoidance, however, associated in some by misrepresentations.

Quantitative human geography, broadly defined, has been the subject of many such misrepresentations, not least because of the extent to which it has changed very substantially since the 1960s-1970s, changes that have gone unrecognised in a number of recent human geography textbooks (Johnston et al., 2014a, 2014b). This series of three reports has thus been designed to document the extent of those changes, encapsulating the history of quantitative human geography over nearly six decades. Progress is rarely linear and cumulative (Albury and Schwartz, 1982); every generation fights its own battles and too much is forgotten, too easily but better understanding of the present requires an appreciation of the past battles, what was done and then rejected – and why. This first report thus establishes the foundations of quantitative human geography, focusing on three major components of the initial 'revolution' during its first two decades: philosophy, space, and methods. The next will describe the major changes that occurred in all three over subsequent decades, as a prelude to the final paper which will evaluate where 'quantitative geography' is currently at and going to.

Three revolutionary movements

There is no comprehensive, single-volume treatment of the nature of the original 'quantitative revolution'. Wayne Davies (1972) provided an early overview, and Trevor Barnes (2003, 2004) has written a several influential essays on the North American experience, alongside which one of the main activists has penned an autobiography (Berry, 2006); Peter Haggett (2005; see also Cliff et al.,

1995) and Stan Gregory (1976, 1983) have reflected on events in the UK, and their roles therein. This paper complements them by identifying the three main, inter-linked, themes in the original 'revolution' through the main contributions/ors.

Philosophy

Much of the original stimulus to the 'revolution' in North America was a growing realisation by some geographers that their discipline – human geography in particular; physical geography was then a relatively weak disciplinary component there – was intellectually inferior to the main social science disciplines, notably economics, sociology and political science. To a greater or lesser extent, they based their arguments on parallels drawn with the natural sciences, especially physics, whose law-seeking orientation involved mathematical rigour (Bunge, 1966, 203ff, reviews much of this literature). Geography was increasingly seen as offering little more than 'mere description' based on vague causal linkages between environmental conditions and human behaviour. Such bland empiricism should be replaced, it was argued, by adoption of the positivist approach to science. Observed patterns should not be portrayed as singular occurrences but rather as exemplars, possibly unique depending on contingent circumstances, of the operation of one or more interacting general laws. As set out in a seminal paper arguing this position (Schaefer, 1953), since geography is a discipline concerned with space then geography's laws should be spatial laws.

This position was to some extent only implicit in the debates that raged in those decades, but it underpinned many empirical papers published then. It occupied a central place in two of the pioneering textbooks, however. The first part of Abler, Adams and Gould's (1971) book on *Spatial Organization*, with the telling subtitle *The Geographer's View of the World*, began with a discussion of science and scientific explanation, before proceeding to outline geography's subject matter: understanding orientation took precedence over appreciating content. Haggett's (1965) earlier volume, *Locational Analysis in Human Geography*, took the same stance, although its 'philosophical' section was shorter; it was soon complemented, however, by Harvey's (1969) entire volume on *Explanation in Geography* which set out a positivist (to some, logical positivist) manifesto in considerable detail, although the word does not appear once in that volume. (It was also a philosophy that Harvey appeared to have rejected by the time of his next book – *Social Justice and the City*, 1973.)

As made clear by the title of another revolutionary textbook, *An Introduction to Scientific Reasoning in Geography*, the goal was to introduce geographers to 'scientific reasoning' (Amedeo and Golledge, 1975). To them – and to other advocates such as Bunge (1966) – geographers had traditionally undertaken work that was descriptive and taxonomic but involved little or no theory construction or hypothesis testing – characteristics they associated with scientific practice. Change was occurring, however; geography, which meant human geography since it was increasingly seen as a separate sub-discipline, was adopting scientific reasoning, as illustrated by Harvey's (1969) conclusion – 'by our theories you shall know us'. There were some attempts to identify common ground with physical geographers – notably around methods as illustrated by two volumes emanating from a joint summer school in 1966 (Garrison and Marble, 1967a, 1967b) and in the United States' National Research Council report (1965) on *The Science of Geography* – but they had little success; indeed a 1979 issue of the *Annals of the Association of American Geographers* celebrating the Association's 75th anniversary contained no essays on or by physical geographers (Marcus, 1979). Chorley and Haggett's (1965, 1967) two scene-setting edited volumes on changes to the discipline incorporated both physical and human geography, as did the serial *Progress in Geography* that they launched in 1969. This was split into the two separate journals – *Progress in Human Geography* and *Progress in Physical Geography* – in 1977 after a survey conducted by the publisher, John Davey, a split that

Peter Haggett certainly regretted; his introductory textbook – *Geography: a Modern Synthesis* (Haggett, 1972) – sought to maintain the discipline's wider unity.

But only one form of scientific reasoning was promoted at the time – that adopted by logical positivists who argued that theory was built, and laws eventually derived, through the empirical testing of hypotheses derived from theoretical models, frequently rooted in (Newtonian) laws of physics. Tests were devised to verify the theory and not, as Popper had argued, to falsify it. Few authors adopted that alternative approach to science, and it was not fully adumbrated until the appearance of Bird's (1975, 1993) critique: Petch and Haines-Young (1975; Haines-Young and Petch, 1980) had argued its relevance to physical geography.

Focus: spatial order

What should form the subject matter of that scientific reasoning? Science is the search for order so geography, it was argued, should be the search for spatial order (Haggett, 1990), and its language should be geometry (Bunge, 1966) – a spatial order that, as Michel (2016) argues, can be visualised.

But what order? Although they were all marginal to their home disciplines, several economists and sociologists had been exploring spatial order. One of their books – more specifically a particular footnote therein (Lösch, 1954) – stimulated Peter Haggett's decision to explore that literature and his 1965 book, based on several years teaching the material at Cambridge, brought it together in a coherent framework. The core was spatial interaction – contacts and movements between places. (According to Cox, 1976, the revolution involved a major switch from a focus on vertical relationships – between people and their local environments – to one on the horizontal – on relationships between places.) In Haggett's schema those interactions are confined into channels, whose networks connect places – nodes that are ordered into hierarchies. The intervening areas comprise surfaces with patterns of, for example, land use changing with distances from the nodes and the channels. He added a further element to that schema in the book's second edition (Haggett et al., 1977) – diffusion, patterns of change that emanate down the hierarchies along the channels as well as across the surfaces. Spatial order was thus presented as the outcome of social processes, which themselves were organized and influenced by the spatial configuration and dispersion of their participants.

Abler, Adams and Gould adopted a similar approach, though without an overarching schema. Morrill (1970), in a parallel pioneering text that said little about either philosophy or methods, focused on spatial order as the core element of 'the science of geography'. As in the other books, spatial order was largely seen as reflecting the influence of distance on human behaviour: geography was being presented, in the words of a Scottish-Canadian geographer not associated with the 'revolution', as a 'discipline in distance' (Watson, 1955; Johnston, 2003b) – or, as a historian expressed it at the time, the tyranny of distance (Blainey, 1966). The models to be tested proposed patterns reflecting the costs of crossing distance, and few – a major exception was Nystuen (1963) – attempted to produce more abstract, or higher-level, formulations of what concepts underpinned theories of spatial order.

Methods: quantification

Although the philosophical foundations and emphasis on spatial order underpinned the changes that the revolutionaries sought, the proposed methodological changes attracted most attention – both positive and negative – and led to their nomination as a 'quantitative revolution'. Its importance to the proposed changes was stressed in Abler et al.'s book that moved from its introductory chapters on the nature of science to one on 'Measurement' which they argued (1971, 111):

> … is rarely undertaken for its own sake. Rather it forms a crucial link in the chain leading from initial hunches and questions to intellectually satisfying explanations. On a map, for example, it might appear that tuberculosis decreases with distance from a city. But to move beyond such a geographic speculation we need accurate values of tuberculosis rates at carefully measured distances from the center. Only then can we investigate the relationship between the two variables, and perhaps uncover other factors, such as poverty, that explain the spatial variation of the disease.

In other words, as Cole (1969) expressed it more forcibly in his critique of a leading text on the regional geography of the British Isles, precision in establishing relationships (putative laws) requires exact measurement and irrefutable – because of the way in which they are presented – statements of relationships. Haggett made a similar argument. The validity of theoretical models of spatial order can only be established using empirical data. But those data are to be gathered and presented (1965, 185)

> … not simply to add to the present jumble of regional literature that we already have to hand, but in order that our existing concepts may be critically examined. It is all too easy to collect information in human geography, all too hard to collect information which is significant and relevant to specific locational questions … [hence the remainder of the book] follows the well-established paths of experimental design [although most of the work was observational rather than experimental], moving from the collecting of evidence through to the testing of hypotheses.

In most cases that testing involves statistical analyses (p.286):

> Few innovation waves have swept through geographical study more rapidly or more decisively than the vogue for statistical analysis. In 1955 the use of statistical methods in most geographical research papers was a curiosity …; by 1965 many American geographical journals … carried many papers with a high statistical content and even the more conservative English journals were showing weak trends in this direction.[2]

Although methods were marginal to Harvey's philosophical concerns in *Explanation in Geography*, nevertheless he too stressed the importance of making rigorous statements using mathematical languages (p.378): '… mathematical methods allow us to make objective statements about pattern. … [replacing] intuitive descriptions of, say, settlement patterns (using such vague terms as "dispersed". "nucleated", and the like) by an objective measure'.

This emphasis on the necessity for quantification was taken by many practitioners as fundamental to the changes in orientation being proposed, hence the widespread adoption of the term 'quantitative revolution', even by those not part of the vanguard (e.g. Balchin, 1993). The clear implication was that in order to practice that 'new geography' a facility in statistical methods to extract order (or pattern) from noise (stochastic variation) was a sine qua non, which meant that geography students required an introduction to the methods – and preferably had a mathematical background in their high school education. Textbooks were needed.

The first such book, explicitly written by a geographer for geographers, promoted neither changes in disciplinary philosophy nor a new focus on spatial order: its sole focus was on the rigorous use of statistical methods to analyse geographical data. Stanley Gregory was a climatologist whose undergraduate degree included courses from another climatologist – Percy Crowe – who was the first geographer to promote the study of variance and probability in handling statistical data (Crowe,

[2] Haggett was one of the pioneers who published one of those early papers in a conservative English journal (Haggett, 1964), having previously delivered it at a meeting of the Royal Geographical Society. It was presented in the discussion then, by Sir Dudley Stamp, as possibly a steam hammer cracking a nut and Chorley (1995, 360) relates how Haggett was later reprimanded by his head of department for bringing 'the subject of geography into disrepute by applying such mathematical methods'.

1933).[3] Gregory adopted, adapted and extended that approach in his pioneering research on British rainfall when teaching at the University of Liverpool in the 1950s. He not only determined that students of climatology needed an appreciation of statistical data handling but was encouraged by his colleagues to make his innovative undergraduate practical courses – the first was given in 1957 – relevant to all geographers, human as well as physical, albeit meeting with some considerable resistance (Johnston, 2018). The material was honed and formed the basis for *Statistical Methods and the Geographer* (Gregory, 1963). It contained nothing about philosophy, about theories, laws and models, nor about spatial order; rather, because 'it is increasingly apparent that the raw material with which the geographer is presented is becoming progressively more of a quantitative nature' (p. xiii) then 'a necessary corollary follows, that these numerical data should be analysed by sound statistical methods so that maximum value is obtained from them' (p. xiv). His book was designed to introduce standard statistical techniques 'particularly applicable to geographical problems'. It was a primer, from which many British academic geographers of the 1960s-1970s cohorts gained their basic appreciation of statistical procedures (the book went through four editions, the last appearing in 1978). Only descriptive procedures were discussed; there was no testing of hypotheses, merely means of making precise, unambiguous statements about frequency distributions, sampling, the differences between distributions, correlation, and bivariate regression.

Whereas for Gregory learning about statistical methods was largely a means of ensuring that quantitative data were used properly, to others their adoption was part of a wider manifesto for changing geographical practices. Haggett's (1965) book was split into two parts – 'Models of locational structure' and 'Methods in locational analysis' – with the latter necessary for the evaluation of the models described in the former. The range of methods presented was much wider than Gregory's, and firmly set in a hypothesis-testing framework, but the chapters were not presented as a primer; to find out how to apply the methods students would have to go elsewhere. The same was the case with Abler et al.'s volume: methods were introduced and illustrated, but not how they could be applied.

Other textbooks followed, including two from staff at the University of Nottingham (Cole and King, 1968; Ebdon, 1978) where pioneering quantitative work was linked with developments in computer and remote sensing applications (Mather, 1976; Unwin and Dawson, 1985); an early North American text was King (1969). And in 1975 the Quantitative Methods Study [now Research] Group of the Institute of British Geographers inaugurated its series of *CATMOG* (Concepts and Techniques in Modern Geography) booklets, short texts designed 'to fill a teaching need in the field of quantitative methods in undergraduate geography courses'.[4] (The Study Group was established by Barry Garner and Stan Gregory in 1964 to promote usage of quantitative methods across geography, although it did not formally affiliate with the IBG until 1968 because Gregory – then a member of the Institute's Council – thought that to do so would be the 'kiss of death' because of opposition from many senior geographers: Gregory, 1976, 1983; Johnston, 2018. A parallel Mathematical Modeling and Quantitative Methods Speciality Group of the Association of American Geographers – now named the Spatial Analysis and Modeling Speciality Group of the American Association of Geographers – was established in 1979; its name was changed in 2000.[5])

[3] Unwin (1999, 2) records that those who think that 'UK quantitative geography … was all to do with Haggett and Chorley on a supposed "Cambridge (Chorley) to Bristol (Haggett) axis" should note that there was also another axis from Manchester to London with two unsung and largely forgotten influences from climatologists Crowe and Gregory'.

[4] The first eight were on Markov Chain Analysis; Distance-Decay Models; Canonical Correlation Analysis; Spatial Interaction Shopping Models; Trend Surface Analysis; Classification; Factor Analysis; and Principal Components Analysis. By 1983, thirty-nine of these booklets had been produced. A total of fifty-nine booklets was eventually produced, and they are now available online - http://www.qmrg.org.uk/catmog/index.html.

[5] Its website is http://sam-aag.org/index.html.

Moving on

Despite Burton's claim, the quantitative revolution was certainly not over by 1960, but within a decade or so of that date much of what had been proposed had been generally accepted as part of geography's portfolio and was covered in university courses – such as the three-year honours degree in the discipline that was provided by almost all English universities at the time (Whitehand, 1970).

Such changes were seen by Berry and Marble (1968, 4) as all three elements of the 'revolution' – philosophy, the search for spatial order, and quantification – having entered the disciplinary mainstream:

> A new paradigm has been implanted in geography, in which the implicit definition of legitimate problems and research methods is significantly different from that of a generation ago.

The methods were quantitative, but 'quantitative analysis cannot function in the absence of a sound theoretical base' (p. 6). It was not just a 'quantitative revolution', therefore; it was a 'conceptual revolution' which placed 'technical ability in its true perspective; it is but one weapon in the whole methodological and procedural armoury' (Davies, 1972, 9). To many of those involved it was also a part of moves to make geography more 'relevant' or 'applicable', and therefore acceptable more widely within the academic division of labour. (Johnston, 2003a, and Smith, 1987, illustrate the difficulties geographers had winning acceptance in many universities.) The dominant early American centre of work illustrating all three components of the revolution was the Department of Geography at the University of Washington, Seattle, where a group of graduate students – widely termed the 'space cadets' (Dunbar, 2002) – worked with William Garrison on a range of projects associated with planning new spatial orders (as in their pioneering book: Garrison et al., 1959 – see also Garrison, 2002). In the United Kingdom, Alan Wilson – trained as a mathematician whose introduction to spatial analysis came when he chaired Oxford City Council's Transport Committee, which led to his pioneering role at the Centre for Environmental Studies in London and then an appointment as a professor in the Department of Geography at the University of Leeds in 1970 – introduced mathematical modelling to the discipline as the basis for applicable tools (e.g. Wilson, 1974). He, too, sought to maintain links between physical and human geography (Wilson, 1981) and also co-authored a text on mathematics for geographers (Wilson and Kirkby, 1975).

The two decades leading up to the mid-1970s saw those three components of the revolution – philosophy, spatial order, quantification – firmly established within geography, but they had not taken the discipline over. As standard histories, notably of human geography (Johnston and Sidaway, 2016), show, no sooner had they achieved what appeared to be strong permanent positions within the disciplinary portfolio than they came under attack from approaches based on very different philosophies and methodologies, and which sought a very different order – or none at all. Regardless, locational analysis – or spatial analysis, or spatial science – remained firmly in place as one of the discipline's significant components although the amount and level of statistical teaching has since diminished. In the UK, geography is included as part of wider concerns about the poor levels of numeracy amongst social science graduates (though they are generally better in geography than in several other disciplines there) and in national initiatives within both schools and Universities that have sought to address this problem (Harris, 2018; Harris et al., 2016, 46, refer to a 'quantitative de-skilling' within British human geography).

By the mid-1970s spatial science was also being changed from within: the philosophy was substantially modified; the order sought was different; and the methodological armoury moved in new directions. Having established the foundations, charting those changes is the focus of the next paper in this series.

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