Geographical Analysis ISSN 0016-7363

The Local Shape of Revolution: Reflections on Quantitative Geography at Cambridge in the 1950s and 1960s

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The ''quantitative revolution'' in human geography which swept across so many universities in the 1950s and 1960s had its main diffusion centers in a few locations which were to have global significance. Two critical early centers were the University of Washington in the Pacific Northwest and Lund University in southern Sweden. But the experience of change was different in different locations as the general forces of perturbation sweeping around academia were translated into local eddies with local repercussions. Here, small and somewhat random quirks at the outset, led eventually to fundamental divergences between adoption and rejection. The theme is illustrated by reference to changes which occurred at Cambridge, one of England's two oldest universities, as seen from the perspective of someone who—as undergraduate, graduate student, and later, faculty member—was caught up in these changes and took some small part in propagating them. Special attention is given to the role of two environmental scientists, Vaughan Lewis and Richard Chorley, in introducing changes and the way in which later developments in human geography drew on preceding experiences in physical geography. The reasons behind the ''Cambridge variant'' and the questions of how intellectual DNA is passed across the generations are discussed.

Introduction

The Oxford English Dictionary shows the word ''legend'' to be of some antiquity. In the medieval period, it simply meant ''the story of the life of a saint''—a term that even their best friends would hesitate to apply to the group of aging quantitative geographers gathering in Brisbane in July 2006. But from 1613, the meaning changes to ''an unauthentic story handed down by tradition and popularly regarded as historical.'' This is much closer to the mark, and the term can be used to encompass some of the traditions which have grown up around the so-called ''quan-

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Submitted: May 22, 2007. Revised version accepted: September 28, 2007.

titative revolution'' in human geography which began to affect much of human geography a half-century or so ago. Those traditions were described in an early survey entitled *Recollections of a Revolution* (Billinge, Gregory, and Martin 1984). But as the half-century of its origins is celebrated, so the story is being looked at again more critically and has recently attracted some fine scholarly studies most notably by Trevor Barnes (2001, 2004). Such studies are particularly welcome as some of the key early players such as Torsten Hâgerstrand (Sweden), Richard Chorley (England), and Peter Gould and Michael Dacey (USA) have recently died (see obituaries by Haggett 2003, 2004, 2006).

Here, I want to contribute to the creation of such legends by looking at one small eddy within the vortex of change in quantitative geography. This occurred in *time* in two middle decades of the last century (the 1950s and the 1960s) and in *place* in Cambridge, England, and its Department of Geography. It was here that I spent 15 of those years (as undergraduate, graduate student, and faculty member) and it was that period—which for good or ill-shaped the remaining half-century of my work.

The Cambridge experience

My connections with Cambridge began in December 1950 when as a 17-year-old from a small West Country grammar school I took the entrance scholarship exams, survived the viva voce interviews, and (just) scraped over the necessary threshold grade in Latin. The following autumn, I went up to read for the Geographical Tripos. My college was St. Catharine's which, since J. A. Steers' election to a fellowship in 1925, had been steadily building up into one of the leading men's colleges for geographers.[1] Here, I was part of a small competitive group of geographers that included future chair-holders including M. D. I. Chisholm, Peter (later Sir Peter) Hall, and Gerald Manners, all three of whom were later to make significant contributions to economic and urban geography. Looking back at the lectures listed each term for geography in the *Cambridge University Reporter* for the years 1951–1954 I am struck by the huge range of topics from ''Tropical Landforms'' through to the ''History of Cartography.'' Quantitative geography was not represented in any self-styled form, nonetheless, some mathematical skills were required in various parts of the Tripos, notably in field survey, photogrammetry, and map projections. There were also parts of meteorology and glaciology in which familiarity with differential calculus was an advantage.

Cambridge, with the coastal geomorphologist Alfred Steers holding the Chair of Geography, had a special strength in physical geography, a field reinforced by a Downing Site building shared with a small but outstanding geophysics group led by Sir Edward Bullard. A leading figure among the physical geographers was W. V. Lewis (''Vaughan'' Lewis) (see Fig. 1a) who was working on the motion of cirque glaciers and estimating their erosive power (King 1980). Lewis was unusual in that he had read mathematics at Trinity in the 1920s before turning to geography (Oth-

Figure 1. Two central figures in the early stages of the ''quantitative revolution'' in geography at Cambridge University, England. Both were physical geographers. (Left) William Vaughan Lewis, 1907–1961. (right) Richard John (''Dick'') Chorley, 1927–2002.

ers who were later to follow a similar path from mathematics into geography with distinction were Alan Wilson, Michael Kirkby, and Mike Goodchild). One of the features of Cambridge was the small-group tutorial system in which a member of the faculty met weekly with undergraduates, usually in pairs, sometimes in groups of three. As I was specializing in physical geography, I was fortunate enough to have Lewis as one of my supervisors. Lewis had college rooms in Whewell's Court which was located immediately above those of the philosopher Ludwig Wittgenstein (whose death occurred in my first year). It was here that Lewis genially but relentlessly tore my weekly essays to shreds.

Unlike most of his colleagues, Lewis encouraged his students to think mathematically about geographical problems. He attracted physicists (such as John Nye) and molecular biologists (such as Max Perutz, who later won the Nobel for work on the structure of hemoglobin) over from the Cavendish Laboratory to work with him on the structure of ice crystals. My own tutor at St. Catharine's, R. C. Evans, was also an X-ray crystallographer. Cambridge at that time was a world center for using X-rays to probe molecular structures and it was there in 1953 that the young Crick and Watson published their 900-word paper in *Nature* on the double-helix structure of DNA (Watson 1968) which was also to win them a Nobel prize. Despite ration books and postwar austerity, the early 1950s was an exciting time to be at Cambridge.[2]

Lewis was important in that, up to his untimely death in a car crash in 1961, he provided an influential if critical protector within the Department for quantitative ideas right across the discipline. So that when, for example, the translation of August Lösch's great work appeared in 1954 as *The Economics of Location* (Lösch 1954), it was Lewis—familiar as he was with solid-state arrays in physics—rather than the economic geographers who saw its potential for illuminating settlement studies.

By 1955, after I'd completed the Tripos and a year of graduate work on industrial location models under the redoubtable Augustus Caesar, I went to a junior lectureship (at £600 per annum) to University College London. There I spent two happy years learning my trade in (Sir) Clifford Darby's department, a department whose remarkable record has recently been chronicled by Clout (2003). My closest link was with the distinguished Scandinavian scholar, W. R. Mead, and we shared together a course on economic geography in which Bill encouraged me to introduce industrial location models and some of Isard's thinking. As at Cambridge, quantitative geography per se did not figure in the curriculum and the more mathematical topics, photogrammetry, map projections and statistics (for the BSc students), were taught by staff from outside the geography department.

At UCL, I just missed Brian Berry who that same autumn had enrolled as a graduate student at the University of Washington at Seattle but whose formidable reputation remained as a legend. Brian's arrival in Seattle coincided with three *annus mirabilis* intakes of brilliant graduate students, one of those remarkable coincidences that was to make Seattle the epicenter of the quantitative revolution in human geography. The full story of Garrison's ''space cadets'' (Berry, Boyce, Bunge, Dacey, Getis, Kolars, Marble, Mayfield, Morrill, Nystuen, and Tobler) has been well told by Dick Morrill (1984) and there will be those at Brisbane who know the tale at first hand so I must not shoot their foxes. I did once try and show the movement as a diffusion wave that, for all its errors and gaps (the French and German influences are not included) might be worth repeating (Fig. 2).

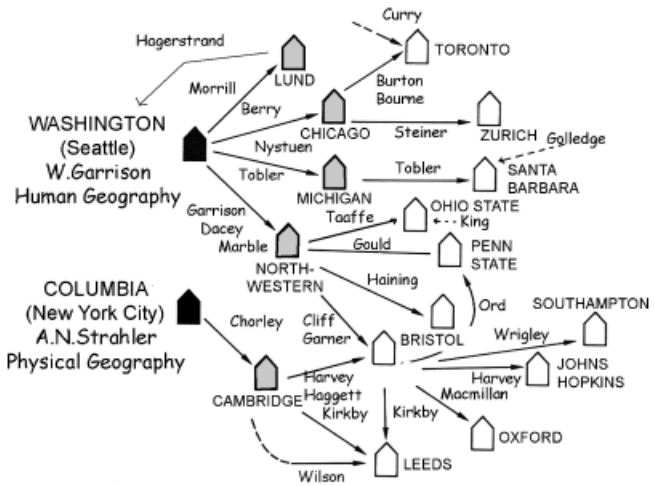

Figure 2. The quantitative revolution in geography as a diffusion process. An incomplete, simplified, and Anglo–Saxon view of the movement of quantitative methods between Departments of Geography in the third quarter of the 20th century. As an interesting variant on the spatial pattern, note that a number of the key players (Curry, Golledge, Johnston, King) had come directly or indirectly through New Zealand departments. Adapted from Haggett (1990, Fig. 7.6, p. 154).

If I had to pick out at second epicenter on this side of the Atlantic it would clearly have to be Lund. That gentle and remarkable scholar, Torsten Hägerstrand formed the essential link between Seattle and Europe. The IGU Symposium on Urban Geography in August 1960 (Norborg 1962) brought many of you in this room together and the list of those present in Lund reads like a roll-call of those associated with quantitative geography.[3] It speaks for Cambridge's relative isolation at that time that it was *not* represented at this meeting and it was left to Howard Bracey, an agricultural economist who was present (and whose writing on Wiltshire settlements was to interest the young Ron Johnston), to bring in the Hägerstrand ideas via Bristol. The channel into Cambridge probably came via David Harvey who, after research in historical geography at Cambridge, spent some time studying under Gerd Eneqvist at Uppsala and returned enthused by what was happening in Sweden.

When I returned to a faculty position at Cambridge from UCL in 1957 some things had changed on the quantitative front. The appointment of E. A. Wrigley had transformed and deepened the work on demographic analysis. His massive work of English parish records which was later to be incorporated into the Cambridge Population Studies Center (CAMPOP) was starting to demand computer organization to cope with the mass of parish records. Overseas visitors were also starting to bring in new ideas and I recall attending lectures by geologist Bill Krumbein from Northwestern who showed what strides were now being taken in the mathematical modeling of sedimentological processes.[4] Bill remained a powerful supporter of quantitative geography on both sides of the Atlantic.

The Madingley summer schools

But the key transformation in quantitative studies at Cambridge was the appointment in the following year (1958) of the remarkable Dick Chorley (Fig. 1b). I've written at length about Chorley elsewhere (e.g. Haggett 2006) so only a summary note is appropriate here. An Oxford geography graduate, Dick had done his graduate work under Arthur Strahler at Columbia before going on to teach at Brown.[5] Strahler was transforming geomorphology away from its historical roots into a dynamic, process-based study and Dick brought to Cambridge all the statistical and systems modeling he had been fired with at Columbia.

By the accident of good fortune, Dick and I had grown up in the same part of England, had deep family roots in the Somerset countryside, and both supported the same ever-losing cricket team. We became close friends. In that first summer, we plotted together how we could introduce some of Strahler's ideas into the laboratory classes at Cambridge. By then, the impact of Bill Garrison and his Washington group was also beginning to flood into England, carried by personal contacts, transplant of graduate students, unofficial journals (such as *MICMOG* and the Harvard Papers), and so on.

Given our joint interests, Dick and I wrote a small number of papers together exploring trend surface analysis (Chorley and Haggett 1965a) and hierarchic variance designs (Chorley et al. 1966) but I doubt if they ever had more than footnote interest. He was absorbed in the first of his four great volumes on the history of geomorphology while I was working on Brazilian historical geography and, as a side show, trying to make sense of what was happening across the water in a short course of eight lectures first given in the unpopular Saturday morning slot from 1959. These lectures were entitled ''Introduction to locational analysis'' and the notes eventually converted into a small book, *Locational Analysis in Human Geography* (Haggett 1965).

It was at that time that something happened which brought the parallel Chorley–Haggett tracks into some convergence. In 1948, the University has bought a rambling Elizabethan mansion (with its surrounding gardens and parkland) located about 3 miles west of Cambridge. Built in 1543 and briefly used as a royal residence, Madingley Hall (Fig. 3) then provided accommodation for the University's expanding graduate-student population before becoming the headquarters of the University Extra-Mural Board. Like Oxford, Cambridge has a long history of courses given in the surrounding area (Welch 1973), staff going out to lecture in distant towns and villages and bringing in outsiders on in-house residential courses. But as residential courses for servicemen (especially United States Air Force members)[6] diminished in the 1950s, the Board turned to the idea of using Madingley to provide Summer School courses for school teachers. School mathematics in England at that time was going through major changes with the introduction of linear algebra and statistical models and teachers were asking for more guidance. Through R. E. Pahl, an extra-mural staff tutor (later to be professor of sociology at Kent), the Board learnt that geography was one of a number of subjects also experiencing change. It was in 1962 that an unexpected invitation came to both

Figure 3. Madingley Hall, Cambridgeshire. Located three miles west of Cambridge, the Hall was the home of a series of summer symposia on ''Models in Geography'' for five years from July 1963.

Chorley and I to consider putting on a week-long residential course for geography teachers on ''new developments in the discipline''. Its structure was to parallel the course already started on the ''new mathematics'' and which had proved a success. More senior people had been approached but turned it down and no doubt they were getting desperate.

When the invitation came, both of us were in the United States: Chorley working with Stan Schumm (another Strahler student) with the U.S. Geological Survey at Denver and myself teaching summer school at Berkeley. It says something for the non-quantitative nature of my California teaching that the two courses were ''Introduction to geomorphology'' and ''Latin America'', although gate-crashing the Regional Science Association at Brian Berry's behest gave a flavor of what was happening in that very exciting field. At the end of my courses and together with another Cambridge colleague, Roger Barnett, Dick, and I set off on a Labor Day weekend in September 1962 to Yosemite, on across the Sierra Nevada, and down the Owens Valley, before heading west across the Tehachapi Mountains to the Pacific coast and north to the Bay Area. Late morning on the Monday brought us to the northern part of the Owens Valley and the abandoned mining town of Bodie (Fig. 4). It was a scorching day and it was sitting on the shaded steps of the closed saloon that the invitation engineered by Pahl to set up the course next summer was discussed. It would mean each of us putting existing projects on the back burner, but in the end we finally accepted (Haggett and Chorley 1989).

The following summer (July 1963) the first group of teachers paid their 10 guineas (the combined cost of a week's lectures plus full board, and tennis and croquet opportunities) and met in the elegant salon of Madingley Hall.[7] Because

Figure 4. The Bodie decision point. Haggett (29) and Chorley (34) in September 1962 at the abandoned California mining town of Bodie at which the Madingley symposia, the *Models in Geography* volume and the *Progress in Geography* set of journals had their origins.

residential accommodation was limited, only 28 places were available (the others were for the mathematicians) and happily the course was oversubscribed. It was here that over the next five summers assembled a remarkable combination of 135 young and enthusiastic school teachers who were to go on to revolutionize the subject at school level. The title of the course meandererd from year to year from ''Modern geography'' (1963) to ''Theory and techniques in modern geography'' (1967). Typically, the course began with lectures covering developments in the each of the main areas of geography with an emphasis on spatial modeling. There were then practical classes covering a few main techniques (e.g. morphometric analysis, graph theory, linear programming, multivariate modeling, remote sensing applications, spectral analysis). Mid-week provided an opportunity for field work (with an emphasis on sampling designs) and visits to the Cambridge geography department. The end of the week had a specifically schools focus wrestling with practical classroom implementation problems.

We needed around 14 lecturers to man each week. Chorley and I organized and taught in each course with the help of Ray Pahl and a local high school teacher (Peter Bryan) who provided the essential school link. We then drew heavily on the local help of Tony Wrigley and Chris Board. Of the 37 lecturers who participated over the 5 years, half came from three universities (Cambridge, 8; London; 7; and Bristol, 4) with the rest widely scattered. Lectures were paid at the modest rate of five guineas per session and persuading colleagues to serve at a prime time for both family holidays and overseas fieldwork took all Dick's powers of persuasion. We also pressed visiting staff into service and Les King, John Rayner, and Kevin Cox (all from Ohio State) gave classes. For the participants and for the tutors, the country-house setting was idyllic, the food and company good, the pace civilized and the sun (mostly) shone. So it was on the croquet lawns of Madingley that a very gentle and minor revolutionary ripple was set moving.[8]

Given the efforts which contributors had put into the course it seemed worthwhile to have a permanent record of these summer schools. Thanks to Chorley's good links with the publishing house of Methuen, they agreed to take the first two volumes which emerged. The first Madingley volume, *Frontiers in Geographical Teaching* (Chorley and Haggett 1965a, b), was based on the first summer's classes and several chapters placed their emphasis squarely on teaching. Although subtitled simply as ''the second Madingley lectures'' the next volume was more ambitious in scope and addressed to a wider professional audience. *Models in Geography* (Chorley and Haggett 1967) has recently been re-assessed in a perceptive essay by Reg Golledge (2006). The book consisted of eighteen chapters divided into five sections: ''Role of models'' (Chorley, Haggett, and George); ''Models of physical systems'' (Chorley, Barry, and More); ''Models of socio-economic systems'' (Wrigley, Pahl, Keeble, Garner, Hamilton and Henshall); ''Models of mixed systems'' (Grigg, Stoddart, Harvey, and Haggett'; and ''Information models'' (Board, Morgan, and Harries). Although not overtly quantitative, it was marked throughout by a stress on models within a broadly positivist framework. David Harvey at that

time was working on his influential *Explanation in Geography* (Harvey 1969) and, with Barry Garner, played a key role in Bristol's quantitative program.

Despite its considerable size and two thousand references, the origins of the volume were anything but solemn. Contributors first met at Cambridge in the Department of Geography's handsome Clark Collection Room, its walls lined with classical geographical and exploration volumes from earlier centuries. It was not clear that their authors would have approved the ''robustly anti-idiographic'' aim of the proposed volume, or of the distinctly junior status of their authors.[9] We drew mainly on Madingley course lecturers but added one or two others (notably Barry Garner and Janet Henshall) who were sympathetic to the project.

A third book to emerge directly from Madingley came from our class notes on the geographical analysis of linear structures and was published by Arnold as *Network Analysis in Geography* (Haggett and Chorley 1969). It was planned as the first of an ambitious three-volume trilogy on spatial analysis but the other two (on the analysis of surfaces and of point patterns) never got written by us but were eventually to be much more professionally handled by others (e.g. Getis and Boots 1978).

The aftermath of Madingley

By the mid-1960s it was clear that the Madingley courses would have to come to an end. Chris Board who had played a key role in the cartography and field work parts of the summer schools had already left for the London School of Economics. Tony Wrigley, who had handled the demographic and historical geography components, was increasingly caught up in his ever-growing historical demography center. I was going off to a newly established chair at Bristol. Dick Chorley himself was wanting to devote more time to the next volume (on Davisian geomorphology) in his four-volume work on the history of geomorphology. The group was breaking up.

But it seemed a pity to let the momentum built up over the first half of the 1960s to wane. So we cast around for some ways to keep it going. An annual hardback series reviewing developments across geography was one way forward. David Stoddart recalls that ''The PIG idea really jelled during an afternoon perambulation of the Madingley gardens during the July 1965 meeting''[10] Arnold had published *Locational Analyis* in 1965 and it had sold unexpectedly well and gone into several translations. On the basis of this, their new geography editor, John Davey, agreed to experiment with an annual hardback series eventually entitled *Progress in Geography*. We gave less thought to the title than perhaps we should and, as David Livingstone (2006) has shown, ''progress'' is philosophically a heavily loaded term. But these were confident days and the shelves of the University Library were bursting with new review series ranging from Astrophysics to Zoology. In the end, the decision really lay between ''Progress in.'' and ''Advances in . . . ''. Again a letter from David Stoddart relates:

> Richard showed me your letter about *Advances in Geography*. I must say I prefer *PIG* as previously announced. The vistas for a letterhead then become enormous. How about a large sow with lots of little piglets about their basic functions ? We had a brief board meeting around the *Fountain* [a local Cambridge pub near the Department] last Tuesday and I will put some names on paper for the editorial board for you soon.[11]

The ''board'' was twofold. A local editorial group made up of the gang of four (Board, Chorley, Haggett, and Stoddart) who had all been laboratory demonstrators together at Cambridge. The advisory board was much more distinguished: its membership of Brian Berry (Chicago), George Dury (Sydney), Torsten Hâgerstrand (Lund), Kenneth Hare (Toronto), Les King (Ohio State), Jim Parsons (Berkeley), Gottfried Pfeifer (Heidelberg), and Stan Schumm (Colorado State) reflected the catholic aims of the new series but also contained a strong modeling emphasis.

The first volume was published in 1969 and drew heavily on American and Australian contributors. The volume was lead off by a 50-page review by the irrepressible Peter Gould on methodological changes since the 1950s**.** Peter acted as our missionary for the series in North America. By then the American scene was swinging as a typically enthusiastic letter from Gould at Penn State at this period attests:

> In the beginning of February we are holding with Ohio State (Ned Taaffe, Howard Gauthier, Les King), Pittsburgh and Penn (Alan Scott and Julian Wolpert) an informal get-together. Four sessions: one on graph theory, one on theory of search, one on spectral analysis, and one on learning and behavioural models. Do come over and join us. It should be fun.[12]

The year 1969 was a significant one in terms of the quantitative revolution. It saw the publication of Les King's influential *Statistical Analysis in Geography* (King 1969) and the first issue of the journal *Geographical Analysis*, both coming from Ohio State. In England, Alan Wilson took on the editorship of a new journal *Environment and Planning* and was to drive it and his own research group at Leeds.[13] At Bristol, geographer Andy Cliff and econometrician Keith Ord (Cliff and Ord 1969) were beginning their productive cooperation on spatial autocorrelation. The somewhat amateur and light-hearted phase of quantitative geography in the 1950s and 1960s was clearly ending and a new and more professional period was getting under way.

Nine volumes of PIG were published between 1969 and 1976. In the early years manuscripts were hard to find but as its reputation grew, so the pressure of new material led to delays in publication and a decision was taken to replace the hardback series with a quarterly journal. While we had hoped and pressed for a single journal, a survey of readers and subscribers suggested that two separate journals would be more acceptable. The first issues of *Progress in Physical Geography* and *Progress in Human Geography* were published in the year after the last hardback volume. Each journal carried a similar subtitle indicating that it aimed to be an ''international review of geographical work'' in the ''natural and environ-

mental sciences'' and the ''social sciences and humanities'' respectively. This was deliberately added to emphasize our wish to carry papers by nongeographers who were doing important geographical work. While both journals were catholic in content, the inclusion of a built-in ''progress report'' section allowed the editors to ensure that quantitative methods in both physical and human geography were amongst the fields regularly reviewed and updated. The original ''gang of four'' was first supplemented by two further editors, Bruce Atkinson of Queen Mary College London and David Lowenthal of University College London and later by Ron Johnston of Sheffield University and Andrew Goudie of Oxford. Thirty years later both journals continue and flourish as a long-term legacy of Madingley.[14]

Retrospect

As Stoddart's (1986) entertaining essay has shown, the study of geography within the University of Cambridge has had a complex history. For the two decades considered here, that complexity continued and the quantitative issues described above were never more than a small part of a larger canvas in which important advances were being made within the Department in areas as diverse as coastal geomorphology and medieval historical geography. But if we concentrate on the quantitative scene alone, then it is clear that the local shape of revolution at Cambridge showed some differences from the more traumatic changes occurring across the Atlantic. Five differences can be identified.

First, there is a time lag. If we tie the start of the quantitative revolution in human geography to the critical Washington years, that gives a nominal start line of 1954. To judge by the content of American geographical journals there was mathematical work before that date but it tended to be at the interface with cartography and meteorology rather than human geography. Using the 1954 marker, there is then a 4- to 5-year time lag before statistical modeling begins to appear in the Cambridge curriculum.

Second, a difference in the hearth from which the diffusion wave came. For Cambridge the most relevant epicenter was Columbia (rather than Seattle) and thus the key figure was Strahler (rather than Garrison). Strahler (1992) has given a very valuable retrospective of the period from his viewpoint.

Third, a disciplinary bias. In the United States, the main flow of ideas into human geography came mainly from the social sciences whereas at Cambridge it was applications in the environmental sciences that provided the template. There the move into human geography comes later and sometimes models are directly derived as when Horton numbers (originally applied to stream systems) are applied to transport networks (see examples in Haggett and Chorley [1969]).

Fourth, the medium through which wider change was propagated was different. By the 1950s, graduate schools were well established in many major U.S. universities whereas in England, graduate schools were an exception. It is instructive that Madingley mainly worked because of the high standards and enthusiasm of

geography teachers in the United Kingdom. Despite the hard work that went into the High School Geography Project, I suspect that the U.S. system was too large and qualified geography teachers too thin on the ground for a parallel wave there to have propagated there. It is instructive that spread in some Canadian provincial system (e.g. Ontario) was faster because they had a school infrastructure in geography much closer to the U.K. pattern.

Fifth, there is a difference in resource levels. The U.K. lacked any equivalent to the Geography Branch of the Office of Naval Research and NSF-sponsored institutes such as those held at Northwestern and Michigan were not feasible at that time. Resources had to be scraped together from many sources, not least the willingness of school teachers to pay fees out of their own pockets. There was also a contrast in computing resources. Although Cambridge pioneered computing through EDSAC, access was largely limited to ''heavy science'' users. We looked across with envy at the spread of computers such as the IBM 7090 series to major American centers. As late as 1966, I found a stark contrast between the resources available at Northwestern University with its huge Vogelback Computer Center and Bristol where punched cards had still to be sent each day by van to another university (Southampton) for processing. One good effect of the ''sealing wax and string'' tradition on our side of the Atlantic there was great interest in efficient experimental designs where the greatest return was having to be squeezed from the smallest number of observations.

For every generalization one thinks of counter examples and it is important to emphasize that my experience was based on a single university. While the Madingley movement had set off a diffusion wave through British universities that could be recognized and plotted with a ''Cambridge-Bristol axis'' (Whitehand 1970), it was not the only generator. For the U.K. as a whole, Stan Gregory (1983) has shown that Cambridge was something of an anomaly. Important advances in statistical modeling were made at Manchester and Sheffield where meteorological research within geography was important and at Leicester and Nottingham. Gregory's role was particularly critical in producing the first statistical text for geographers (Gregory 1963) and initiating the IBG Quantitative Methods group.

Some movements have only a local interest and after a time the waters close over without leaving a trace. It would be presumptuous of me to comment on quantitative geography at Cambridge from the 1970s on as I was only then a distant observer. But it is worth noting that of the five current professors of human geography at Cambridge, all have some links with earlier mid-century movements. Bob Bennett worked with Richard Chorley on environmental systems while both Andy Cliff and Bob Haining took higher degrees at Northwestern at the height of its quantitative period when Garrison, Dacey and Marble (all ex-Washington) were all on the faculty. Ron Martin's work in economic geography has a strong quantitative and theoretical bent while Richard Smith worked with Tony Wrigley in the Cambridge Population studies group.[15] The flow of intellectual DNA from one generation to another continues to work.

Looking back, quite why the ''quantitative revolution'' as a whole appeared when and where it did we must leave to the historians of science. The history of a discipline is, after all, only a ''fable agreed upon'' and I give here reflections which are inevitably one-sided and personal. I'm torn between the two competing views that being an active participant in a past battle gives one either (a) unique insights or (b), debars one completely from taking a balanced view. I've some sympathy with the paleontologist Stephen Jay Gould's[16] weary conclusion after studying fossils from the Burgess Shale documenting the early-Cambrian revolution in life forms:

> Little quirks at the outset, occurring for no particular reason, unleash cascades of consequences that make a particular future seem inevitable in retrospect. But the slightest early nudge contacts a different groove, and history veers into another plausible channel, diverging continually from its original pathway. The end results are so different, the initial perturbation so apparently trivial. (Gould 1989, pp. 320–21).

If family reasons had not brought Chorley back from Brown University to England, if Lewis had not been tragically killed in a car crash, if our Bodie meeting hadn't happened, would the Cambridge experience have been fundamentally different or would other actors have played essentially similar roles? May the cosmic tape player holds infinite counterfactual scenarios, each one of which may be as plausible as the others? So, given the seniority of the participants, my expectation for the Brisbane 2006 meeting is less for a renewed revolution than for a small critical nudge which will set some young mind running down a new track, determined to do things better than my generation did. That's where hope for the future evolution of quantitative geography really lies. Looking back on the early Cambridge experience, and Madingley and *Models* in particular, I continue to hope that—despite its somewhat amateur and exploratory character—the short period played some small part in opening up geography to a wider range of scholarship so that it stands today deeply suspicious of too much orthodoxy, not least our own.

Acknowledgements

It was an honor to be asked to take part in this historic session and I thank the IGU and the Australian Research Council through ARCRNSISS for this unique opportunity to join with old friends—in spirit, if not in the flesh. Because surgery prevented my travel to Brisbane, one of my old Bristol students from the 1960s, Prof. Barry Boots of Wilfrid Laurier University, Canada, was kind enough to present this article on my behalf. In its original form, the article was accompanied by 60 slides. I'm grateful to the Archivist at the Royal Geographical Society, Susan Strong, for help in accessing Richard Chorley's papers and for Rosemary Chorley for her permission to use them. Two anonymous referees suggested valuable additions and helped correct my increasing tendency to tunnel vision.

Notes

1 The impact of Steers on Cambridge at both college and university levels is difficult to overemphasize; see the review of his life by Stoddart (1988). Steers himself wrote two typically short and self-effacing reviews of geography in the college in the *St. Catharine's College Magazine* for 1982 and 1983.

2 I've recently described the atmosphere of these years in ''Fifty years on: Cambridge geographers celebrate golden anniversary at Girton'' (*Girton College Newsletter*, Spring 2006, 18–19).

3 The list of participants includes many names from the United States associated with the quantitative revolution: B. J. L. Berry (Chicago), L. Curry (Maryland), M. F. Dacey (Pennsylvania), W. L. Garrison (Northwestern), W. Isard (Pennsylvania), D. F. Marble (Pennsylvania), H. H. McCarty (Iowa), R. L. Morrill (Washington), H. Porter (Washington), E. N. Thomas (Iowa), and E. L. Ullman (Washington). The photographs of the meeting taken by the late Chauncy Harris still give a powerful evocation of that critical Lund meeting.

4 Krumbein was later to consolidate his ideas into an influential textbook (Krumbein and Graybill, 1965).

5 Going through his unpublished papers after his death, I found a long note by Chorley of his first meeting with the ''tall, gangling, 34-year old Midwesterner who reminded me of James Stewart'', Arthur Strahler, at his office on the third floor of Schernmerhorn Hall at Columbia. Although the meeting was a brief one, it was to have an immediate impact, viz. ''With the comment: ''Well, I guess you might like to look at this!'', Strahler handed me a copy of his erosional slope development paper [Strahler (1950)] and ushered me out. In a state of excitement I went to the Lions' Den, ordered myself a large chocolate malted with two scoops of ice cream, and settled down to read the paper. The milk shake rose about half the length of the straw—and remained there! I recall that the juke box was playing Glen Miller's ''String of Pearls'' at the time''.

6 William Warntz, who did much to introduce gravity models and the social physics concepts of Zipf and Stewart into human geography was based at Mildenhall near Cambridge in the mid-1940s as navigator on a USAAF bomber crew. Much of his work on geodesic fields was based on wartime experience of plotting optimum routes for aircraft across barometric surfaces. For a useful review of his work see Isard (1960, pp. 501–565).

7 Apart from the writer's own recollections, correspondence, and diaries, details of the Madingley Summer Schools are given in two sources. The Cambridge University Archives contain the Annual Reports of the Board of Extra-Mural Studies for 1963–1967 which give summaries of each course. Detailed papers including course outlines are retained and can be found through E. Welch, *A typescript catalogue of archives of the Board of Extra-Mural Studies, University of Cambridge* (typescript, 86 pp, 2004). Other Madingley papers are included amongst Richard Chorley's voluminous academic papers including notes, correspondence and photographs lodged as 11 boxes (two of them are large tin trunks!) in the archives of the Royal Geographical Society, London.

8 After a gap of 5 years, the Annual Madingley Symposia were resumed in July 1973 and continued until August 1978. Only three of the original team, Chorley, Chris Board, David Keeble, and Rosemary More (by now Mrs. Chorley) took part and they were augmented by

the then Cambridge staff (including Malcolm Anderson, Robert Bennett, Michael Chisholm, Andy Cliff, Derek Gregory, and Ron Martin) all of whom were to go on to make crucial contributions to geographical modeling.

9 As chairman, Chorley produced a fistful of Western Union telegrams of support from around the world but their provenance (e.g. Lambarene, Congo) and signatories (e.g. S. Albert) suggest they were light-hearted forgeries.

10 Letter from David Stoddart to Peter Haggett, September 27, 2000.

11 Letter from David Stoddart to Peter Haggett, March 11, 1967.

12 Letter from Peter Gould to Peter Haggett, November 30, 1965.

13 I recall very clearly my meeting with Alan Wilson and John Ashby (Pion) in the lounge of a Park Lane hotel at which the series was launched and Alan agreed to take on the editorship. Both Wilson and Ashby were physicists and saw the value of a *Physics Letters* type of journal. It became a publishing phenomenon, the original quarterly journal *Environment and Planning* founded in 1969 moving to a monthly status and spawning three sister journals *Environment and Planning B: Planning and Design* (1974–), *Environment and Planning C: Government and Policy* (1983–) and *Environment and Planning D: Society and Space* (1983–). Through the immense hard work of Alan and his Leeds staff, the journals grew to provide the leading outlet for spatial analysis research across the social sciences.

14 Bruce Atkinson continues today as senior editor of PPG while Ron Johnston has recently been succeeded by Roger Lee for PHG.

15 I have not detailed individual publications here but the staff pages of the Departments excellent web site gives full information. See www/geog.cam.ac.uk/

16 As a Columbia geologist, Stephen Jay Gould had (like Richard Chorley) taken Arthur Strahler's course in quantitative geomorphology.

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