
Lives lived and lives told: biographies of geography's quantitative revolution

Trevor J Barnes

Department of Geography, University of British Columbia, Vancouver BC V6T 1Z2, Canada;
e-mail: tbarnes@geog.ubc.ca

Received 15 May 2000; in revised form 24 November 2000

Abstract. In this paper I draw upon both biographical and sociological approaches to examine one moment in the history of geography's quantitative revolution of the late 1950s and early 1960s: the publication of Brian Berry and William Garrison's paper, "The functional bases of the central place hierarchy", in *Economic Geography* in 1958. The origins of that paper are traced through the life stories—the 'lives told'—of the two authors. In particular, I try to connect the specific life trajectories of Berry and Garrison up until 1958 with the wider social and cultural contexts in which they lived. The theoretical impetus for the study are three literatures: the first is science studies, and especially the work of Bruno Latour and his ideas of 'black boxing' and 'translation'; the second is on the history and sociology of quantification; and the third is on biography, particularly scientific biography. The broader argument of the paper is that the seemingly disembodied numbers, calculations, and precisely drawn figures and graphs that increasingly inflect human geography from the late 1950s, and found in such papers as Berry and Garrison's, are socially embedded, a consequence not of a universal rationality but of specific lives and times that infuse the very substance of the works produced.

"Nothing happens when you live. The scenery changes, people come in and go out, that's all. But everything changes when you tell about life; it's a change no one notices: the proof is that people talk about true stories."

Jean Paul Sartre *Nausea* (1964, page 57)

Introduction

The break Sartre recognizes between what I will call 'lives lived' and 'lives told' is a defining feature of the nearly thirty oral histories I have collected over the last three years from 'pioneers' of geography's quantitative revolution of the late 1950s and early 1960s.⁽¹⁾ On the one hand, there is the public record of my interviewee's life, manifest as their writings in journals, books, and reports, and couched in such vocabulary as inverted matrices, correlation coefficients, and the axioms of topology. As Sartre suggests, in these lives lived the scenery sometimes changes: people take on new institutional affiliations, write with different coauthors, publish in new journals, or take up new topics, "that's all". On the other hand, "everything changes" when my interviewees talk about their lives: of difficult beginnings in Welsh villages or the Depression-era South, of new starts in far-off places, of struggles late at night as graduate students with machines and numbers, of a spirit of revolutionary zeal and evangelism, of battles with surly editors and nonbelievers, of important conferences

⁽¹⁾The interviews are part of a larger project to write a history of geography's quantitative revolution of which this paper is one component. I have interviewed the following geographers since October, 1997: John Adams, Brian Berry, Larry Brown, Bill Clark, Kevin Cox, Michael Dacey, Michael Dear, Roger Downs, Bill Garrison, Art Getis, Reg Golledge, Michael Goodchild, Peter Gould, Chauncy Harris, Geoff Hewings, John Hudson, Jim Lindberg, Fred Lukermann, Dick Morrill, Gunnar Olsson, Phil Porter, Allan Pred, Gerard Rushton, Allen Scott, Ned Taaffe, and Waldo Tobler. The interviews were taped and transcribed. The transcripts were then sent back to the interviewees for changes and amendments. Only the amended transcripts are used for quotation.

and key funding agencies, of spiteful practical jokes, of acts of graciousness and compassion, of mistakes and muddles, and of truth regained. These lives are not simply lived, marked publicly by numbers and equations on a page, but when told they take on a shape, a trajectory, a richness, and what Sartre calls a “truth”.

I want to use the stories that I have heard to disrupt the lives-lived version of the quantitative revolution by contrasting it to a lives-told version. Specifically, I intend to use the stories I have collected to present the quantitative revolution as a set of social and biographical processes (lives told) rather than a set of final accomplishments (lives lived). Bruno Latour makes a distinction between “ready-made science” and “science in the making” (1987, page 4). The purpose of this paper is to present geography’s quantitative revolution in the making, rather than as ready-made.

The paper is divided into two sections. First, I make a general argument in favour of a lives-told approach to understanding the history of science, and worked through in this paper as the history of geography’s quantitative revolution. I take that approach to involve sensitivity both to the trajectory of individual lives and to the wider social context in which those lives are lived out. I draw briefly upon three literatures: around science studies, the history and sociology of quantification, and scientific biography.

Second, to illustrate my argument I will use a specific case study that draws on my interview material and other biographical and historical sources. It is around an early and influential paper that emerged from one of the first group of quantifiers in postwar US geography, and clustered at the University of Washington, Seattle, during the second half of the 1950s. The paper is Brian Berry and William Garrison’s “The functional bases of the central place hierarchy” published in *Economic Geography* in April 1958. Although this paper is important, I am not suggesting that it single-handedly launched the quantitative revolution, or even introduced central place theory into geography.⁽²⁾ I choose it as a vehicle to illustrate my lives-told approach in part because unlike other equally important papers I am able to reconstruct its history, and in doing so link it to individual biographies and their wider social context. My contention is that, if such an argument can be made for this one paper, it can be extended to other papers. As a result, and in line with the general argument of this special issue, the seemingly disembodied numbers, calculations, and precisely drawn figures and graphs that were the mainstay of the quantitative revolution should be treated not as the product of a universal rationality, but of specific lives and times that infuse the very substance of the works produced.

Lives lived and lives told: science studies, quantification, and biography

Science studies

Let me begin by defining the two terms, lives lived and lives told. In both cases they involve a narration of lives, but the narratives are fundamentally different in conception. The narrative of lives lived is in terms of a scientist’s final accomplishments, and represented by their empirical findings or theories or equations or even laws. Such a

⁽²⁾ Berry and Garrison’s paper was clearly significant, but it was not *the* paper of the quantitative revolution. But no such paper exists. The quantitative revolution was the consequence of a whole series of events that while including the publication of academic papers, such as Berry and Garrison’s, also comprised conferences, lectures, new courses, workshops, and funded research proposals (and discussed in more detail below). The paper, then, was just one component of many. In addition, because the Social Sciences Citation Index does not begin until 1966, eight years after the publication of Berry and Garrison’s article, and after the quantitative revolution is well entrenched in human geography, it is not even possible to assess accurately the paper’s importance as measured by its citations. That said, the paper still makes it as a “Top Twenty Citation Classic” for the period 1966–1984 (Wrigley and Mathews, 1986). Given that it achieves this status without the benefit of citations from its first eight years after publication, and which are usually the most important in the life of an article, is surely at least an indication of the significance of the paper for the discipline.

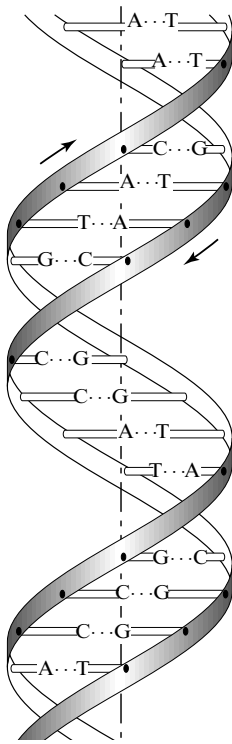


Figure 1. A schematic illustration of the double helix. Printed from *The Double Helix: A Personal Account of the Discovery of the Structure of DNA* by James D. Watson copyright 1981. With permission of the publisher W.W. Norton & Company, Inc.

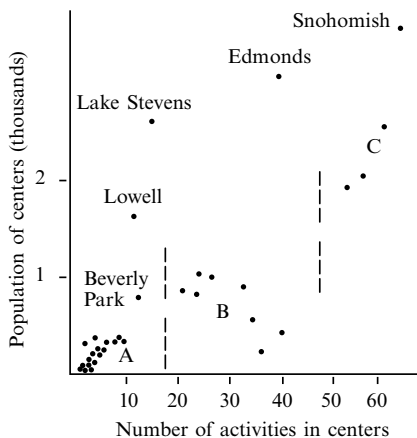


Figure 2. Classes of central places in Snohomish County, WA. Taken from Berry and Garrison (1958, page 152).

narrative strategy is the basis of the orthodox approach to understanding science, including geography's own quantitative revolution. That approach claims that science should be judged on only its final products, for example, the structural model of DNA (figure 1), or a diagram of classes of central places in Snohomish County, Washington (figure 2). In neither case is it important to know anything about the individual scientists involved, that is, Francis Crick and James Watson, or Berry and Garrison, respectively, nor the wider social, cultural, and political processes in which that work occurred. The scientist's role is to act only as a special kind of medium: to let the world speak through the voice of a universal scientific rationality. As Charles Darwin (1974, page 68) expressed it in his own autobiography: "my mind has become a kind of machine for grinding general laws out of a large collection of facts."

In this lives-lived narrative, science produces the scientist, and not the other way around. For this reason “science doesn’t sit well with biography. Science... is to do with excising the personal, with minimizing the individual, with eliminating the intimate” (Livingstone, 1999, page 21). In this light, Albert Einstein (1949) only did what came naturally to him as a scientist when he wrote his “Autobiographical notes”. Striving to break “free... from the chains of the ‘merely personal’ ” (pages 17), Einstein begins his account by saying, “Of what is significant in one’s existence one is hardly aware, and it should certainly not bother the other fellow” (page 5). There then follow ninety pages of equations.

In contrast, the narrative of lives told emphasizes the processes producing science, and not simply final results. The consequence is a very different conception of the history of science. Best found in the literature of science studies (for a useful review, see Hess, 1997), it is a history concerned as much with practices as it is with end products. This is an important distinction. When one focuses on end products, such as Crick and Watson’s DNA model, or Berry and Garrison’s diagram of orders of central place, there is a tendency to treat each as a “black box”, to use Latour’s (1987; 1999) term. Black boxing occurs when scientific work “is made invisible by its own success. When a machine runs efficiently, when a fact is settled, one need only focus on its inputs and outputs and not its internal complexity” (Latour, 1999, page 304). For Latour, though, and for others like him, black-box history is inadequate because it fails to connect the accomplishments of science to the social practices that made them possible, and that are heterogeneous, messy, often hesitant, and thoroughly social. In this science-studies view, knowledge never arrives in the abstract, pulled down from the firmament in some purified form. Rather, it is the outcome of grounded practice. Scientists are not faceless organs of scientific rationality, but real people with particular kinds of socially defined bodies, histories, skills, and interests. Furthermore, those characteristics made a difference to the kind of knowledge that they produce. Berry and Garrison’s diagram did not come out of the blue, nor was it the distillation of some pure form of rationality, measured drop by drop onto the page, but arose from the detailed social practices of those two men living at a particular time and place, late 1950s America. It was not inevitable that a central place hierarchy of Snohomish County would be drawn, but that it was drawn is a consequence in part of a confluence of forces that can be understood in terms of lives told.

The history and sociology of quantification

The counterclaim to this lives-told view is that the scientific enterprise really is a black box; its results the titrated products of a pure and unblemished rationality without reference to lives and times. The paradigm example, it is often argued, is mathematics. Such a position is strongly argued by G. H. Hardy (1941), the Cambridge mathematician, and espoused in his reluctant autobiography. It is reluctant because, following the lives-lived approach to science, Hardy (1941, page 1) believes that “the function of a mathematician is to do something, to prove new theorems, to add to mathematics, and not to talk about what he [sic] or mathematicians have done.” For this reason Hardy titles his book *A Mathematician’s Apology*, an apology for having allowed himself to be on the same page as numbers and equations. If Hardy’s argument is correct, however, it gives credence to a black-box history of the quantitative revolution because that movement defined itself precisely by its disembodied numbers and equations.

Specifically, Hardy believes that the truths of mathematics reside in a Platonic sphere of absolute certainty that lies outside the human realm. Hardy (pages 63–64) writes: “I believe that mathematical reality lies outside us, that our function is to discover or *observe* it, and then the theorems which we prove, and which we describe

grandiloquently as our 'creations' are simply our notes of our observations." Or again, "317 is a prime not because we think so, or because our minds are shaped in one way rather than another, but *because it is so*, because mathematical reality is built that way" (original emphasis, page 70). In Hardy's view, the lives told of mathematicians do not bear on mathematical truths because the latter preexist in a separate sphere uncontaminated by personality, context, or the merely contingent.

The problem with Hardy's position, though, as Ludwig Wittgenstein (1964, part 1, paragraph 3) pointed out early on, was that it rested on an unsupportable circular logic. Wittgenstein argued that, in order for Hardy to know whether the preexisting world of mathematical truth provided correct answers, he must already know what those correct answers were, but it was those correct answers that the preexisting world was supposed to reveal (see also Bloor, 1983). Admittedly, Wittgenstein's critique does not prove that mathematical truths follow from the lives and times of mathematicians—a lives-told approach—but at least it clears the ground for that possibility.

In this respect, useful here are a series of historical studies over the last fifteen years that argue that the very apparatus of mathematics and statistics, including its proofs, formal techniques, and logic, are inseparable from the people with whom, and the places in which, they originated (Crosby, 1997; Daston, 1988; Desrosières, 1998; Gigerenzer et al, 1989; Hacking, 1990; Porter, 1986; 1995; Restivo, 1992; Stigler, 1986). Note that the authors of these studies are *not* saying that a mathematical vocabulary is wrong, or is without utility, or has no bearing on the material world, or especially that anything goes. But they are claiming that mathematical practices are no different in kind from any other; that is, they are socially embedded and physically embodied, and are thereby amenable to an analysis from a lives-told approach. As Barnes et al (1996, page 199) write: "there is a call for a sociological explanation just as much in the cases of rigorous logic and sophisticated mathematics as when dealing with standards of behaviour or deportment of dress, or any other ... social and cultural phenomenon."

In this interpretation, mathematics and statistics are learnt forms of social practice like any other, and which can be immensely useful for undertaking all kinds of tasks from building bridges to building bombs. But because it is useful it does not mean that it derives from some eternal sphere of truth as Hardy suggests. Rather, as with all social practices, while guided by certain rules, mathematics accretes often idiosyncratically over time, always responding to the background social and biographical conditions of its practitioners. This is the significance of the historical studies cited above. They describe the social and biographical contexts in which the different elements of the mathematical enterprise arose and, in turn, entered into the mathematical lexicon itself. That context might be the state, the etymological basis of the very word 'statistics' (Desrosières, 1998; Hacking, 1990; Porter, 1995); or Western European imperialism, bound up with quantitative skills in navigation, trade, and accounting (Crosby, 1997); or the particular social and political interests of the designers of pieces of mathematics and statistics, which arise from specific biographical circumstances.

A useful example is Donald MacKenzie's (1979; 1981; 1999) work on two late-19th-century British statisticians, George Yule and Karl Pearson. The social and biographical circumstances of each were quite different. Yule was an aristocrat, from an "old-established élite famil[y]" (MacKenzie, 1981, page 181), whereas Pearson was a member of Britain's emerging professional middle class (chapter 4). Although Yule was a student of Pearson, the interesting point is that they provided very different interpretations and solutions of the same statistical problem and that, in turn, can be related back to their quite different social and biographical circumstances. That problem was the measurement of association between categorical variables or attributes. Pearson argued for his own regression coefficient, the r_T coefficient, based upon the

biometric work that he had already completed, and, in particular, on his correlation and regression studies of inheritance. Yule, in contrast, devised his own statistic, the Q-coefficient, that was a measurement based only on the categories themselves [for the statistical details see MacKenzie's (1981, chapter 7) lucid exposition].

The critical point is that both coefficients were logically impeccable—"logic and mathematical demonstration alone were insufficient to decide between them" (MacKenzie, 1981, page 167). That is, mathematical reasoning fails to provide a singular truth. Why? For MacKenzie it is because mathematics is also a social product, in this case a result of differences in Yule's and Pearson's respective social class positions and associated personal interests and ambitions. Pearson, as a member of the aspiring professional classes of late-Victorian England, saw eugenics as a means to advance his own class interests. For him the social hierarchy consists of professional classes at the top, who are there because of their innate mental abilities, and the poor, the unemployed, and the criminal at the bottom, who are there because of their lack of such abilities. In the eugenics view, Pearson's view, the professional middle classes should be encouraged to have more children, and thereby pass on their innate intelligence to their offspring, while the poor should be discouraged from procreating altogether. In this way, the intellectual stock of a country rises, and the social problems associated with the poor disappear. Given this social vision, and intimately connected to Pearson's own life and social position, he interpreted measurements of association of categorical data in terms of the same underlying and assumed continuous patterns of variation of the type found in the biometric contexts that justified eugenics. And for that reason Pearson was keen to promote the r_T coefficient, the counterpart to the correlation coefficient used to examine and justify the inheritance of attributes in his studies of eugenics. Yule, in contrast, as a member of the older ruling class, and consequently with a life and social interests if anything opposed to Pearson's, had no political motivation in upholding eugenics and its related statistical apparatus. As a result, he interpreted the measurement of association quite differently.

The broader point that emerges from the literature on the history of quantification is that mathematics is not innocent. It, too, is infused by lives lived, which enter into the very sinews of numbers, equations, and symbols. But if mathematics is not a black box, doubts are necessarily cast on similar claims made about geography's quantitative revolution.

Biography

The example of Yule and Pearson is important for another reason: it points to the importance of biography. For it was in part biographical knowledge about Yule and Pearson that allowed MacKenzie to prise open statistic's black box. In elaborating this point, I raise three issues about a biographical approach that then inform my case study below (see also Livingstone, 2000).

The first is that, although the focus of a biographical approach is on the lives of individuals, it is still quite possible to include discussion of the wider political, social, and cultural context. Indeed, some argue this is one of its advantages. Thomas Hankins (1979, page 5) writes, biography "... gives us a way to tie together the parallel currents of history at the level where the events and ideas occur." Or again, Robert Young (1988, page 123) in a brilliant review, argues that "biography is human nature on the hoof, embedded in lived contradictions, replete with the mediations and articulations of social, familial and historical life."

Cojoining the biographical and the social, however, involves bringing together not only two different scales of analysis—individual and societal—but also two different intellectual traditions. Both have strengths, and weaknesses. In particular, as

Young argues (1988), there is a tendency in traditional biography to make the subject autonomous and fully centred, while in social accounts there is the opposite tendency to reduce an individual's life to fundamental social variables such as class, or ethnicity, or gender. Clearly, there needs to be recognition of both individual agency and social structure. On the one hand, as Thomas Söderqvist (1996, page 76) puts it, "we must give attention to [the] abilities... of freely acting, ethically responsible, individual scientists... to break contexts." Crick and Watson, and Berry and Garrison were not just dupes of their context, but highly creative and original thinkers, and theoretical breathing space must be given to those possibilities. On the other hand, neither set of researchers was undertaking their work in a historical and social vacuum. This is the importance of MacKenzie's findings around Yule and Pearson. Their social context enters into the very fibre of their work. The trick is to allow for such context, but not to make individuals mere instants of it. The consequence is that biography must negotiate a knife-edge between social context (structure) and personal creativity (agency).

Second, any biography, as David Livingstone (1999, page 23) writes, is "necessarily implicated in the production of what might be called a controlled fiction." Biographies, even autobiographies, are not the real person; indeed, for some there is no coherent "Me, Myself, I" to begin with. The unity we discern in biographical lives is in large part a result of the craft of the biographer in reinventing someone's life as plausible, compelling, and singular, and made possible, in part, by various rhetorical devices such as use of the third person (Kermode, 1979, page 117). In this sense, "biography is fundamentally a narrative which has as its primary task the enactment of character and place through language—a goal similar to fiction" (Nadel, 1984, page 8). But at the same time, it is not pure fiction because there are always those facts—the facts of life—that constrain and contradict our theories and interpretations.

The upshot is that any biography, even the oral histories presented below, are a melange of fact and fiction—of events that happened, but also of rhetorical strategies, vested interests, and acts of interpretation, selective memory, and wishful thinking. This is not to imply that people's stories of themselves, or even the stories I tell about their stories, are dishonest and without merit. Richard White (1998), the American historian, recently compared the oral history of his mother's immigration to the United States from Ireland, and her subsequent marriage to a New York Jew, with the various written records that exist about the same events, and found glaring incongruities. It was neither that White's mother was lying, nor that her memory had failed her, but that memory is actively constructed, mobilized to satisfy all kinds of interests, and scripted from a panoply of narrative tropes available. So again there is another knife-edge to negotiate, this one between the facts of a life, and the telling of it.

Finally, and following, because biographies are constructed, they necessarily also say something about the intentions of those who write them. This point is well illustrated in Sharon Traweek's (1988; 1992) studies of the "beamtimes and lifetimes" of US and Japanese high-energy, experimental physicists. In her ethnography, that also functions as a collective biography of physicists as they move from undergraduate students to university professors, she is keen to stress her own location, and the narrative strategies she deploys to reveal it. Traweek (1992, page 435) writes:

"Many scholars in cultural studies, anthropology and feminist studies have argued for at least two decades that the role of the researcher in the production of knowledge has been erased in academics accounts for a specific set of reasons and by a specific set of narrative devices. They have also argued that the mythological abstract, absent, omniscient narrator must be replaced by other kinds of narrators and narratives, especially by stories about us finding sense in the mess of

everyday life, about situated knowledge. In my case, this means telling stories about not only how I found sense being made in the mess of everyday laboratory life, but also how I happened to be in such places.”

So in Traweek’s account we learn something about her childhood and upbringing in California, her former husband, an illness that requires hospital attention, and her various personal travails both with physicists and anthropologists (of which she is one).

Of course, there is a danger of self-indulgence, and exhibitionism (neither of which Traweek comes close to displaying). But once the idea of ‘telling it as it is’ is given up, which science studies and some forms of biography seem to have done, then it is necessary to say something about yourself, and how who you are affects the account that you have written. In turn, such reflexivity must enter into any biographical accounts of scientists. Here is a third knife-edge that a biographical approach must negotiate: between telling the story of the subject, and telling one’s own story.

Writing biography, then, is not simple. It requires a set of negotiations to be made between individuals and their context, between the facts of a life and its narrative form, and between the subject and their author. Writing ninety pages of equations, even of quantum mechanics, might well be an easier task.

Telling lives: two people and a paper

Ron Johnston (forthcoming) notes how the quantitative revolution is rapidly receding from human geography’s institutional memory. Accounts of it in recent textbooks are either emaciated or nonexistent (for example, Cloke et al, 1990; Peet, 1998). Johnston finds this regrettable primarily because it discourages the use of spatial analysis in the present. While this might be so, I think the historical diminishment of the quantitative revolution is also unfortunate for other reasons.

Partly it is my belief that the quantitative revolution was a pivotal moment for human geography, shaping it theoretically, methodologically, and sociologically for years afterwards. It placed human geography for the first time within the social sciences. Even when the quantitative revolution was criticized, that criticism was expressed in the tenets and vocabulary of social science, and which remained unquestioned. Partly it is personal. Effacing the quantitative revolution erases part of my own history within the discipline. As an undergraduate I was at the tail end of that movement, and my doctoral thesis was based almost entirely upon applying the logic and terminology of matrix algebra (summarized in Sheppard and Barnes, 1990). In this sense, I am sure that unconsciously motivating this project is a desire to recoup not only the pasts of my interviewees but my own past as well. And partly it is because the quantitative revolution, as I will try to demonstrate, provides an exemplary illustration of lives lived. By following the movement’s pioneers through society, in their case through an America of the Cold War, of increasing affluence, and of expanding state planning, we see the arc of individual lives intersecting with a wider social context shaping both the form and substance of intellectual inquiry. Paradoxically, perhaps, within geography’s recent history there may be no better illustration of lives lived than the quantitative revolution.

The quantitative revolution

For reasons of brevity it is not possible to provide a detailed review of that revolution here (see, though, the excellent accounts of Johnston, 1991, and Livingstone, 1992). The key point is that, although there is a long history of numeracy among geographers that dates back at least to when the American Geographical and Statistical Societies were formally twinned during the middle of the 19th century, quantification was neither systematically applied within geography, nor was it the basis of a disciplinary vision.

That fundamentally changed in 1950s America when for the first time two distinct centres of geographical calculation first emerged: one at the University of Iowa, Iowa City, around Harold McCarty, and a second at the University of Washington, Seattle, around Edward Ullman and William Garrison (Barnes, 1998).

Outside of these two centres there were other significant sites within the USA, especially Northwestern University in Illinois where Garrison received his PhD in 1950, and the University of Chicago where Ullman was a student. After finishing at Chicago, Ullman went to Harvard to teach briefly, and it was there that he met August Lösch in 1938 who was visiting on a Rockefeller fellowship and who suggested that he read Walter Christaller's thesis on central places (Berry, 1995, page 298). In turn, that reading of Christaller led Ullman (1941) to write "A theory of location for cities", which was one of the reasons a group of very bright graduate students gathered at the University of Washington, Seattle, in the mid-1950s. That said, neither Northwestern nor Chicago, at least in the late 1950s, possessed the same critical mass of quantitative-minded graduate students that was found at either Iowa or Washington (although that changed quickly).

Given this brief historical sketch, a conventional history of science (that is, the lives-lived version) would interpret the rise of the quantitative revolution as the triumph of scientific reasoning. After a century of prescientific practice, albeit with the odd exception, people like Garrison, McCarty, and Ullman finally see the beacon of science, which then illuminates not only their individual paths but also that of the discipline. Certainly, this was how some of the protagonists saw their work. Garrison (1956, page 428) very early on talked about "the universal language of mathematics", and William Bunge (1971, page 137), a student of Garrison's declared, "I believe in science; in the powers of rational thought" And reflecting on his graduate years at Northwestern University during the very early 1960s, Allen Scott (interview, March, 1998, Los Angeles) says, "I remember being in a frame or mind where I thought that anything useful to be said in academic, scientific terms was going to be said mathematically. ... [S]cientific work was the work of eventually mathematicizing every statement we could make about the earthly condition."

For the reasons I have given, though, this lives-lived approach to the quantitative revolution, that emphasizes only the purity of mathematical reasoning, is unsatisfactory. We need to loosen the black box by examining lives told. Certainly, other historians of geography's quantitative revolution have recognised the importance of this task, and brought to bear a variety of perspectives including: Robert Merton's institutionalism (Taylor, 1976); Kuhnian and post-Kuhnian philosophies of science (Johnston, 1991); recent histories and analyses of US social science (King, 1993); and first-hand reflective essays by the revolutionaries themselves (Billinge et al, 1984). I want to add to this existing corpus the lives-told approach that I have outlined. Necessarily, this must be focused on particular individuals, their practices, and their wider social context. To make my task manageable I focus on only the single paper and its two authors: "The functional bases of the central place hierarchy" by Berry and Garrison (1958).

This was not just any paper, however. The article was Berry's first journal publication, and based on his masters thesis completed in 1956. The paper was ranked a "top twenty citation classic" by Wrigley and Mathews (1986). It was reprinted in six different edited collections, including a version in Russian. It was one of the first papers to be published by a Washington "space cadet", as Garrison's students were called (coined by Joe Spencer following a Pacific Coast Association of American Geographers regional meeting in 1956), thereby consolidating Washington's early reputation as a centre of calculation. And it provided a methodological template for future work in the genre: first, begin with an abstract theoretical scheme—in

this case, Christaller's and Lösch's central place thesis—and then, assess its empirical validity by using inferential statistics—in this case, least-squares regression and χ^2 . It became the blueprint for literally thousands of papers that followed.

If I were going to present a black-box history of this paper I would begin with the various diagrams, tables of data, and statistical significance levels and coefficients that pepper the article [for example, figure 2]. I would use them to demonstrate that Berry and Garrison do what all good scientists do; that is, let the world (in this case, the settlement patterns of Snohomish County, WA) speak through the voice of scientific rationality. Further, I would argue that precisely because the article is such a celebrated one—with 69 citations between 1966 and 1984, and presumably many more before the first date (and not recorded because the Social Sciences Citation Index begins only in 1966), and a few after the last one (Wrigley and Mathews, 1986)—it indicates that the world really is as it is presented in the paper, and as such the black box does not need opening. Following a lives-told approach, however, I intend the reverse. For that reason I begin not with the numbers, but with a much more significant component of the paper: the title page that includes a brief statement about the two authors:

“Mr. Berry is a Teaching Associate and Dr. Garrison is an Associate Professor in the Department of Geography, University of Washington. The present paper is one of several related to patterns of routes, urban sizes, and land uses stemming from recent research at that university” (Berry and Garrison, 1958, page 145).

My lives-told questions are: how was it that Mr Berry, a Teaching Associate, and Dr Garrison, an Associate Professor, came together at Washington to write “the present paper”? And what was it about their lives and times up to that point that produced “the recent research at that university” of which their paper is a consequence? I contend that if we can answer some of these lives-told questions we will begin to ease open the black box.

William Garrison⁽³⁾

Entering Peabody College, Nashville, Tennessee, at age 16, Garrison had his education interrupted by the Second World War. Enlisting in the US Airforce in 1942 as an aviation cadet, Garrison attended a nine-month meteorology course taught by both Henry Wexler, a meteorology professor at MIT, and John Leighly, the Berkeley geographer. It was there that Garrison was exposed to higher level mathematics including “a tough course in dynamic meteorology involving thermodynamics and fluid mechanics” (Garrison, interview, March 1998, Berkeley). As Garrison says, “I was not properly equipped to do it given my background. But it gave me a great deal of confidence because once I figured out that once I put my mind to it I could do anything” (interview).

Garrison, of course, was not the only geographer affected by World War 2. Furthermore, and in line with the earlier argument, those wartime experiences necessarily entered into the constitution of geography itself as a discipline. One wartime influence on US geography that is discussed in the literature is the formation in 1942 of the Office of Strategic Services, created to assist military intelligence, which at its apex employed seventy-five geographers (Ackerman, 1945; Kirby 1994; Stone 1979). But another influence, less noted, is the mathematical training provided by the military during the war to a number of geographers who were later involved in the quantitative

⁽³⁾Much of the information for this section derives from an interview I carried out with Bill Garrison in March 1998, in his office at Berkeley. The other sources are: an interview Garrison gave to Wes Dow in April 1972, and found in the Geographers on Film Transcription series; and a personal reminiscence by Garrison in the 1979 “Seventy-five years of American Geography” special issue of the *Annals of the Association of American Geographers*.

revolution. Garrison is one, but so are Ned Taaffe, who also trained as a meteorologist, and Bill Warntz (Warntz 1984; Janelle 1997). Indeed, Warntz (1984) said that it was reading John Quinay Stewart's *Coasts, Waves and Weather* (1945) when he was stationed as a meteorologist in Gander, Newfoundland, that led him to physical analogies such as gravity and potential models and, in turn, to the very development of macrogeography and the problems of spatial interaction. Similarly, when asked about the origin of his ideas Garrison says, "I had a number of very fortunate opportunities. I had been a meteorologist in World War Two and I had the opportunity to reflect on the nature of some kinds of systems which gave me a kind of systems bent" (Dow, 1972, pages 2–3).

Following his wartime service Garrison returned to Peabody, and completed both his undergraduate and masters degrees. In 1947 he moved to Northwestern University to undertake a PhD under Malcolm Proudfoot, but he was dissatisfied with the resulting dissertation—"It was awful...I didn't know how to go about it, I didn't know how to use the data, and the most fortunate thing was that Northwestern University library lost it!" (interview). However, it was coteaching at Northwestern with Clarence Jones, an old-style regional economic geographer, that was to prove formative, albeit negatively. One of Jones's introductory economic geography texts begins: "Everyone likes to travel. Most of us wish to visit distant lands" (1935, page 3). The distant lands Garrison wanted to visit were not defined by Jones's (1935) eightfold regional economic classification scheme, but by theory. As Garrison says, Jones's lectures "led me to keep asking: 'What's the theory? What's the theory? What's the theory' " (interview). Specifically, "... a systematic approach was in order... ." (Garrison, 1979, page 119).

In the fall of 1950 Garrison moved to Seattle to become an instructor at the Department of Geography, University of Washington. The chair, Howard Martin, who founded the department, hired him. A year later in April 1951 Donald Hudson arrived to become head. While not a quantitative geographer himself, Hudson was keen to create a vigorous and lively intellectual culture, and provided support and sometimes protection to Garrison and to his students (Dow, 1971a, page 2; Garrison, 1979, page 119). The kind of support Hudson provided was to be critical in launching the quantitative revolution. First, there was the money, made possible by the growth in higher education in postwar North America—the department had five teaching assistantships that allowed Hudson to recruit graduate students aggressively, such as Brian Berry in 1955 and, two years before him, Duane Marble (Dow 1971a, page 2). In addition, as Garrison (1979, page 120) remembers, "a high level of extramural contacts was also supported by Hudson who was skilled at getting travel money for young faculty." Second, there were the machines—the large Friden desk calculators, the university IBM 604 computer set up in 1954 and requiring wires to be manually plugged into a circuit board, and, especially important, the duplicating machine. Free rein to the mimeograph allowed students such as Berry and Marble to prepare and circulate internal position papers and, from March 1958, to launch a formal discussion paper series that was sent to kindred souls around the USA and the world. Finally, there were the intellectual resources in the form of like-minded visitors who Hudson brought in to teach at least term-long courses in the department. They included Les Curry, Ross MacKay (who was especially influential on Waldo Tobler's and Brian Berry's research), and Torsten Hägerstrand, whose visit established a Washington–Lund connection.

While Hudson created a conducive internal departmental environment, Garrison also found allies outside the department, especially in engineering and in economics. From engineering he became "involved with Ed Horwood and Bob Hennes.... They

were trying to build some models to evaluate transportation systems” (Dow 1972, page 3). In particular, this was the period just before the interstate highway system, and the state legislature had asked the engineering faculty how they could build a highway infrastructure. In turn, Horwood and Hennes, as Garrison says, “reached out and got me and some of the students involved in the kinds of analysis they were doing” (interview). The end result was Garrison’s participation in a state-funded highway project to study the benefits of rural roads to rural property (which he also used to fund the work of his students including Marble, and later Berry). In particular, as Garrison reflected, this kind of applied work was more to his liking: “I never thought that statistics was my niche. Rather, it is a particular kind of analysis that I like to think about as it relates to transportation” (interview). That practical bent was also emphasized in Garrison’s interactions with regional economists and, in particular, with Arnold Zellner, a young, recently hired econometrician who, as Garrison says, seemed “underemployed in economics” (interview). Zellner was also to play an important pedagogical role for Garrison’s ‘space cadets’.

The interactions with engineers and economists, the involvement with the State of Washington’s transportation planning (culminating in the collective Garrison et al 1959 volume *Studies of Highway Development and Geographic Change*), and work with his first student, Duane Marble, led Garrison in 1955 to offer Geography 426, the first advanced statistical course in a US department of geography. As Garrison says:

“When I arrived [at the University of Washington in 1950] I went through the question of ‘what can I do?’ It was a pretty slow evolution. [From 1955 onwards] I started giving a graduate seminar that had some analysis in it. I think it was a mistake now, because what I did was organize that seminar around... statistics which used as its textbook a statistics book. I think that course was a mistake because while it worked for the students to get them into statistics, it would probably have been better if the course had not been about statistics but on analysis and economic geography, more substantive” (interview).

Despite the regrets, the course was a breakthrough for geography. But as should be clear, this was neither some kind of epiphanic moment for Garrison nor the discipline, but very much connected to Garrison’s life and times: his wartime training and experience; his frustrations in coteaching with Clarence Jones; a ‘can do’ pragmatic, masculinist culture celebrating science and technology and, in part, a result of “the notion coming out of WWII that science had won the war” which was then later applied to winning the Cold War (interview); a favourable departmental and university environment; and the enormous physical redevelopment of cities and their transportation systems that called for precisely the kind of practical and instrumental reasoning that matched Garrison’s disposition, and the capacities of the techniques in which he was interested.

Garrison, though, was not just a stooge of that context; merely the right person at the right time and place. Garrison, and his students, in effect were retheorizing the whole project of geography (Barnes, 2001). They moved from an old vocabulary typified by Jones’s textbook, and couched in terms of regions, element complexes, classification schemes, and detailed fieldwork, to an entirely new vocabulary phrased in terms of optimal locations, space economies, ‘if, then’ statements of computer programming, causal explanations, numerical data, correlation coefficients, and regression lines. Such retheorization required creativity, intelligence, individual perseverance, self-confidence in the face of criticism (“It’s not geography”), and an enormous amount of work and effort. In other words, the quantitative revolution required perspiration and inspiration, and both were physically embodied in Garrison.

Brian Berry ⁽⁴⁾

Someone taking Garrison's first course in advanced statistical methods was Brian Berry, who had arrived in Seattle in the autumn of that year. Berry was from England where he had completed a joint economics and geography degree at University College London (UCL). While there he took the economic geography course "taught by Brian Law and Bill Mead, and [which] was exciting, lively, and expos[ed] me to new things" (Berry, interview, April 2000, Pittsburgh). In particular, Law [who was an economics graduate from the London School of Economics (LSE), and who later left academia eventually becoming managing director of the British confectionery company Mars Ltd] "taught his economic geography in a fairly systematic way. He used a Weberian model as a way of structuring his industry-by-industry discussions of heavy industry... He'd also recommended I read Hoover's (1948) book [*Location of Economic Activity*]." In addition to this interest in location theory, Berry had been taught mathematics up to calculus, and sat in at the LSE on lectures by R G D Allen on mathematical economics.

In his third year at UCL, encouraged by H Clifford Darby who was Head of the Geography Department, Berry applied to graduate school in the United States and, by Christmas 1954, well ahead of the other departments to which he also applied, he received a telegram from Hudson offering him a teaching assistantship. Receiving also a Fulbright travel scholarship, Berry set off for Seattle on 1 September 1955, travelling first on the Queen Mary, and then later on the Empire Builder (the train between Chicago and Seattle). He packed in his suitcase Lösch's book, *The Economics of Location* (1954), only recently translated into English from German. As Berry says, "I'd already read Hoover, so it seemed to me [that *The Economics of Location*] would be a very exciting book. So I bought a copy and carried it with me—read it on the ship" (interview). It was a prescient item to include. For that book, both in its methodological style and empirical substance, was to mark Berry's early work at the University of Washington, and, indeed, the early quantitative revolution more generally.

That Berry had the opportunity to board the Queen Mary and to take the train to Seattle in the first place was in part a consequence of wider changes to England's education system, which had been brought in by the Butler Education Act of 1944 that abolished fees for secondary education. As he came from a working-class family, Berry's expectation had been one of leaving school early to take up a trade. That changed, however, with the new Act, which was reinforced by the election in 1945 of a Labour government that further stressed the importance of education in promoting social equality. As Berry says, "... I was really one of the first group of post-war English children going to University, and the universal system of free education under the Education Act. My parents, and their parents had all left school when they were 14, and worked through apprenticeships and became mechanical engineers, and I really broke the mould by being able to move through a scholarship system ..." (Dow 1971b, pages 1–2).

Fortunate to be born when he was, Berry was also fortunate to arrive in Seattle when he did. Among the cohort of entrants with him the year he arrived were John Nystuen, Richard Morrill, and Ronald Boyce. And within two years other

⁽⁴⁾ Much of the information contained in this section comes from chapters 1 and 2 of Brian Berry's (2000) as yet unpublished autobiography, *Modo Vincis, Modo Vinceris: An Autobiography*. I am very grateful to Berry for allowing me to read this work. The other sources, apart from Berry's (1993; 1995) own published writings on his life, is an interview he gave with John Fraser Hart as part of the Geographers on Film Transcription series (Dow, 1971b), and an interview I conducted with him in April 2000.

graduates included Bill Bunge, Michael Dacey, Arthur Getis, and Waldo Tobler. He was assigned to the 'Citadel', a large room on the 4th floor of Smith Hall where the Department of Geography was housed, and the culture of inquiry among these young, male students was ambitious, competitive, self-confident, incessant, sometimes exclusive, but also often supportive, involving teaching each other the techniques that they themselves had only just learned. Berry (1993, page 436) says, "We survived by developing a bootstrap help-each-other lifestyle, working hard, playing hard, and taking great pride in trying to seek out analytical approaches to geographical problems and teaching each other about them."

Out of that masculinist culture, and associated with such pursuits as computing, number crunching, and mathematics, went a sense that they had found the Truth. As Berry (1993, page 437) says, "We came to believe in a cause that was new and that we knew was right." Concomitantly, there was an evangelical fervour that infected the Citadel's believers. As Berry puts it, we were "groping for theory, but I think there was, you know, a sense of people who suddenly found something new, they found religion. They got a religion, and by gosh they had to run out and tell everyone else about this, I think this is true in any kind of situation in which there is ferment" (Dow, 1971b, page 3).

In the process some of the nonbelievers were unsettled, and likely offended by the brashness. Michael Dacey, one of the believers, says "we were very aggressive, very ambitious, and very appreciative. ... We were full of missionary fervour, and I imagine we were unlikable brats. ... In retrospect we must have been very disorientating to the establishment" (interview, November 1997, Evanston, IL). But that establishing, at least in the shape of Hudson, remained supportive of this vibrant intellectual culture.

Berry, then, like Garrison brought with him the baggage of his time and place, and that influenced the kind of work that he did, and how he did it. There is his working-class background and his desire to succeed; there is his undergraduate education in geography and economics, including some advanced mathematics; there is his fortuitous arrival at Seattle with an outstanding cohort of graduate students; and there are his teachers who include Garrison and Ullman. But, of course, this is not the whole Brian Berry: there is his remarkable individual energy and determined concentration (he was to complete *both* his MA and PhD in three years); and there is his ability to learn abstract material, both theoretical and statistical, at great speed, and to bring it to bear on the concrete problems at hand, which was the case for his MA thesis.

The paper

Berry's ability to absorb quickly large amounts of new, often complex literature and techniques is evident in his first year (1955–56) when he tried out a variety of theories and abstract schemes including: social physics (Stuart Carter Dodd, an old-time social physicist, was at the University of Washington Sociology Department); econometrics where he took Zellner's course; and urban location theory—he wrote a seventy-page term paper for Ullman on the topic. It was that last paper, in which central place theory was a central component, that suggested a potential masters thesis. Already familiar with Lösch's work, and by then with Christaller's too, and also well immersed in location theory initially through Law and Meade, and then at Washington through Garrison, Berry sought to test empirically theoretical elements of central place theory. Also making this a suitable topic were, first, that Garrison had already written a paper with Marble on another German location theorist, Johann Heinrich von Thünen (Garrison and Marble, 1957), and, second, that perhaps even more importantly a suitable dataset already existed, collected by Marble. Marble was employed by Garrison on the project to assess the benefits of rural roads on rural

property. One aspect of the study involved surveying the settlements of Snohomish County, "inventorying types of business and numbers of stores in each of the county's nucleations" (Berry, 1993, page 437). In the end, though, Garrison had not used that part of the data, and Marble donated it to Berry for his thesis.

The thesis turned on testing whether a "hierarchical class-system of [central place] centers" (Berry and Garrison, 1958, page 145) of the kind recognised by Christaller existed in Snohomish County. To do that Berry first determined the number of functions found at each central place, and statistically grouped those places in terms of a hierarchy of classes; and second, calculated the range of all the different functions offered in Snohomish County, and again statistically grouped them. That completed, Berry's task was then to determine using statistical techniques whether a correspondence existed between the different classes of central places, and the classes of functions as measured by their range. If such a correspondence existed, it would validate Christaller's "hierarchical class system of central place centers".

It took Berry (2000, page 79) only "three weeks of number crunching for the thesis to come together, and another couple of weeks to write a first draft..." By the end of those five weeks he demonstrated that a central place hierarchy existed, at least in Snohomish County. More significantly, his thesis pointed the way to a new set of geographical practices compared with the old kind, and represented by Jones's regional approach. Rather than being undertaken in the field, research was to be carried out primarily at a desk with calculators, graphs, numbers, and spread sheets, and rather than designing descriptive classificatory schemes to pigeonhole places, it was to make use of a deliberately abstract, theoretical lexicon, to emphasize general causal relationships. Berry, along with others, was beginning a new geographical tradition.

But for it to begin, the word about theory and numbers needed to be spread. Berry suggested to Garrison, his principal supervisor, that they try to publish the thesis jointly as a group of three papers. When they submitted all three to the *Geographical Review* in 1957, Wilma Fairchild, the editor, responded quickly saying that "they were 'too mathematical', 'definitely not geography', and not acceptable for publication" (Berry 2000, page 82). The papers as a set were then sent to the *Annals of the Association of American Geographers* where one was accepted, but the other two, including "The functional bases of the central place hierarchy" were rejected because they were "not geography". In turn, those two were sent to *Economic Geography* where Raymond Murphy finally accepted "The functional bases of the central place hierarchy" "as an experiment to see whether his readers would object... [to] mathematics, not geography" (quoted in Berry, 1993, page 438). And so it was that in April 1958 the paper was published.

Discussion and conclusion

Let me now try to connect the two halves of this paper, that is, to bring together explicitly the literature on science studies, quantification, and biography, with the details of the case study. Drawing upon the science-studies literature, and especially Latour's work, I argued that Berry and Garrison's 1958 paper was not the end product of a series of abstract, logical manoeuvres, which is the lives-lived view of science. Rather, it was one link in a continuing processal chain of social practices that both lay, as it were, behind the paper's publication, and later stretched out in front of it. Latour (1999, page 311) speaks about such chains as sets of "translations"; that is, processes by which interests of various actors, both human and nonhuman, are "captured", "modified", and "displaced" so that they work together as elements within a wider network, in this case, the creation and later maintenance of the quantitative revolution. For example, those agents included, as we saw, machines such as the

mimeograph, books such as Lösch's *The Economics of Location*, and individuals such as Donald Hudson. In each case, to continue with Latour's vocabulary, those agents were "persuaded" to become "allies" and to be "enrolled" in a larger network: mimeograph machines were "willing" to crank out discussion papers for graduate students, *The Economics of Location* "allowed" itself to be used in understanding settlement patterns in Snohomish County, and Hudson provided money and protection.

There is another point to be made about such translations, a geographical one: they are the means by which events happening in one particular place are connected to other places. Latour quotes Pasteur, "Give me a laboratory and I will build a world." Latour takes that phrase to mean that, even though one might begin with something very small and local, say, discolourations in a culture flask at Pasteur's rue D'Ulm laboratory in Paris, providing appropriate translations are made and allies garnered, the whole world can change as a consequence: new academic disciplines are born, such as microbiology, new agricultural practices are enacted to ensure sterile conditions around food production, and new health standards are enforced (Latour, 1988).

A similar argument, albeit with more modest consequences, can be made about what happened in Smith Hall on the University of Washington campus during the second half of the 1950s. The work carried out on the 4th floor in the Citadel helped to remake geography. Of course, this was not instantaneous. But over time a chain of translations gradually unfurled from Smith Hall transforming the wider discipline. Specifically, the new geographical practices involving drawing regression lines, calculating χ^2 , and drafting maps of central place hierarchies, moved from this one particular location to many other centres first within North America, and later elsewhere. Most immediately, for this to happen, Berry and Garrison's paper, which embodied these new practices, needed to be published, which required convincing an editor, Raymond Murphy, to print it in a journal; that is, to enrol him into the new network that was being formed. As we saw, this was not easy. Berry and Garrison were trying to redraw the boundaries of geographical practice, and that necessarily produced conflict with those with a vested interest in existing practices.

But even once the paper was published it needed to be read and understood, and so the process of persuasion continued. This was to occur in different forms. From 1961 onwards the first of a series of NSF-sponsored summer institutes occurred at Northwestern University, the mandate for which was to teach quantitative methods to faculty lacking such skills. Correspondingly, new textbooks and courses were written to teach and persuade the next generation of undergraduate and graduate students. Important here also is that many of the Washington graduates secured teaching positions in some of the most prestigious departments and universities within the USA—Berry went to Chicago, Marble and Dacey to the University of Pennsylvania and later Northwestern University, and Nystuen and Tobler to the University of Michigan. Such positions gave them cachet and influence. At the same time, funding was becoming available for precisely the kind of collaborative, large scale, and data-driven research projects that suited users of quantitative methods (for example, from the Office of Naval Research and NSF). This again persuaded potential wavering geographers, if not their university administrators, of the virtues of a quantitative approach. Later still, new journals were created (for example, *Geographical Analysis* and *Environment and Planning*) that also flew the banner for quantitative geography. Of course, all of these developments were frequently contested, and sometimes occurred hesitantly, but they were vital for persuading geographers to take up the new vocabulary of equations and figures. Just as a whole new institutional apparatus was necessarily created in late-19th-century France to accommodate Pasteur's new laboratory practices, the same holds for geography following the new practices plied at Smith Hall in the 1950s.

Let me turn more directly to those quantitative practices. As I argued, following the recent literature on the history and sociology of mathematics and statistics, quantitative methods do not inhabit some separate realm, unsullied by the lives and times of their originators and users. Rather, as I tried to show, the use of quantitative methods by Berry and Garrison was very much connected to their life experiences and the wider context of 1950s America.

Admittedly, I did not demonstrate, as MacKenzie's work does, how social interests structure the internal design of the statistical techniques themselves. This is because Berry and Garrison were not designing techniques per se, but only deploying ones already constructed by others (correlation and regression had been invented by Francis Galton in the 1880s, and χ^2 by Karl Pearson in 1890). As a result, I showed only how certain local and contingent conditions such as Garrison's wartime experiences, or Zellner's influence on Berry, or the Washington State highway-engineering project, bore on their decision to use statistical techniques. There is an interesting irony here as well. Because Berry and Garrison used 'off-the-peg' statistics—in particular χ^2 and correlation–regression techniques drawn from the Neyman–Pearson tradition—it had the effect of unintentionally leading geography, as Peter Gould (1970) later put it, on "a wild goose chase". As Gould, and others showed, a number of statistical techniques, including χ^2 and regression, are often inappropriate for use on geographical data because of the problem of spatial autocorrelation that produces statistically biased results. As a result, perhaps the more general point is made even more forcefully. Berry and Garrison's use of statistical techniques cannot be the consequence of an unadulterated rationality. If it were they would have used a different set of techniques. Instead, to understand why they used the techniques they did, we must recognize the set of external influences bearing on their lives.

It was in trying to understand those lives better that I turned to biography. Latour in his work on opening sciences' black boxes urges "following scientists through society". At least in his early work, he meant that literally. He sat side-by-side with them at lab benches as they undertook titrations, cultivated bacteria in petri dishes, and peered through microscopes [the context was Latour's work at the Salk Institute, described in Latour and Woolgar (1979)]. Clearly, that possibility is denied me given the historical nature of my topic, which is why I turned to the oral and written histories of the leading protagonists. While these source texts are informative and revealing about the historical and social record, they are not innocent. It is for that reason that the literature on biography is a useful addition to science studies. It emphasizes both the rhetorical and constructed nature of the stories that are told, as well as the role of the biographer in telling them. Let me address both points in turn.

Running through the extracts I quoted of both Berry's and Garrison's accounts of themselves are a variety of tropes that help solidify the identity of each, and put them in a certain relationship with the quantitative revolution. For example, a powerful trope that both men use is a heroic one of surmounting successive obstacles: Garrison prevails over difficult mathematics as an aviation cadet; Berry overcomes the constraints of his working-class origins; Garrison, with the help of Hudson, fends off criticism from his sceptical University of Washington colleagues; Berry by dint of hard work and concentrated effort 'bootstraps' his way through econometrics, urban location theory, and crude computer programming techniques; and both Berry and Garrison, although initially suffering the rebuff of high-handed editors, are finally vindicated by writing a "top twenty citation classic". Framed in such terms, the successive overcoming of such obstacles makes the end result—the quantitative revolution—both justifiable and redemptive. The stories are told in such a way that something good and worthwhile must come out of them. Note, I am not trying to deny

the ‘reality’ of the events described by Berry and Garrison, but only that they gain their importance, and are compelling, because of the wider narrative structure in which they are embedded that both frames and animates them.

The broader point is that my equivalent to Latour’s observations at the lab bench—in this case, the facts of Berry’s and Garrison’s lives—are from the outset interpretations because they are self-conscious stories by those individuals of how they would like themselves and others to see their lives. This is not a criticism of Berry and Garrison: it is something that everyone does. But if science studies are to use biographical accounts then their nature must be recognized; that is, they are of a different kind of information from field observations at a lab bench. Granted that both lab field notes and oral histories take the form of texts. But in the first case they are texts written by the observer (Latour in his Salk Institute research), and in the second case, they are written by the observed themselves (by Garrison and Berry in my research).

Likewise, my own story of Garrison and Berry’s stories is thoroughly interpretive. As a result, and following Traweek, I need to say from where my own interpretations stem. How did I come to be at William Garrison’s basement office in McLaughlin Hall on the Berkeley Campus one drizzly, March, late afternoon, or eating breakfast with Brian Berry one early April morning in the Westin William Penn Hotel in Pittsburgh? I was in such places in large part because of my own past. Coming into geography when I did in the mid-1970s I straddled between location theory and social theory, between revering Berry, Garrison, and their ilk and scorning them. That I later completed a doctoral dissertation combining quantitative methods with Marxist social theory indicates I never resolved the differences. It is this continuing ambivalence, I think, that drives my project, and which accounts for the unlikely combination of sometimes veneration and sometimes disdain that inflects my own writing of the quantitative revolution.

In many ways this brings me back to Sartre’s *Nausea*. Although that novel is a philosophical tract on existentialist philosophy, it is also about the difficulties of writing about a person’s life history (albeit in this case not a real person). Antoine Roquentin, *Nausea*’s antihero, struggles with, and eventually gives up on, writing the life of the French 18th-century figure, the Marquis de Rollebon. The problem for Roquentin is “that nothing can ever be proved. The[re] are honest hypotheses which take the facts into account: but I sense so definitely that they come from me, and they are simply a way of unifying my own knowledge. Slow, lazy, sulky, the facts adapt themselves to the rigour of the order I wish to give them; but remains outside of them” (Sartre, 1964, page 23). I certainly have similar doubts about what I have written here. But, whereas Roquentin’s doubts produce the dreaded nausea that episodically overtakes him, and are associated with various existential assaults by tree roots, tableware, and bits of his own body, my doubts produce only more interviews—more lives told—and consequently assault by more cassette tapes, transcripts, and aged, faint blue mimeographs. It is an assault I welcome, though, because I believe that there is a ‘truth’ to those lives told. It is not necessarily a single, factual truth, the absence of which tipped Roquentin over the edge, but, following Söderqvist (1996), it is an “edifying truth”. By this Söderqvist means that lives told offer the possibility of making us think about our own life and situation by comparing with someone else’s. It is the possibility of taking us out of our selves by confronting the unfamiliar life and times of others that is potentially edifying, “of aid[ing] us to becom[e] new beings” (Rorty, 1980, page 360; quoted by Söderqvist, 1996, page 47). Indeed, for Söderqvist (1996, page 75) such edification is itself part of any existential project. If only Msr Roquentin had studied the lives of Brian Berry and William Garrison, rather than that implacable Marquis de Rollebon, things might have turned out quite differently for Roquentin, French existentialist thought, and histories of the discipline of geography.

Acknowledgements. I would like to thank Brian Berry for his comments on this paper, and for allowing me to quote from his unpublished biography. Three referees also provided extremely useful comments that improved the paper. In addition, I would also like to thank all of the people to whom I spoke, and which made the writing of this paper possible. Some of this research was carried out when I held a Canada–US Fulbright Fellowship (1997–98). Finally, I would like to acknowledge the support of a UBC Hampton and a Canadian SSHRC grant.

References

- Ackerman E A, 1945, "Geographic training, wartime research, and immediate professional objectives" *Annals of the Association of American Geographers* **35** 121–143
- Barnes T J, 1998, "A history of regression: actors, networks, machines, and numbers" *Environment and Planning A* **30** 203–223
- Barnes T J, 2001, "Rethorizing economic geography: from locational analysis to the cultural turn" *Annals of the Association of American Geographers* **91** (in press)
- Barnes B, Bloor D, Henry J, 1996 *Scientific Knowledge: A Sociological Analysis* (University of Chicago Press, Chicago, IL)
- Berry B J L, 1993, "Geography's quantitative revolution: initial conditions, 1954–60: a personal memoir" *Urban Geography* **14** 434–441
- Berry B J L, 1995, "Whither regional science?" *International Regional Science Review* **17** 297–305
- Berry B J L, 2000 *Modo Vincis, Modo Vinceris: An Autobiography* unpublished manuscript
- Berry B J L, Garrison W L, 1958, "The functional bases of the central place hierarchy" *Economic Geography* **34** 145–154
- Billinge M, Gregory D, Martin R L (Eds) 1984 *Recollections of a Revolution: Geography as Spatial Science* (Macmillan, London)
- Bloor D, 1983 *Wittgenstein: A Social Theory of Knowledge* (Macmillan, London)
- Bunge W, 1971 *Fitzgerald: Geography of a Revolution* (Schenkman, Cambridge, MA)
- Cloke P, Philo C, Sadler D, 1990 *Approaching Human Geography: An Introduction to Contemporary Debates* (Arnold, London)
- Crosby A W, 1997 *The Measure of Reality: Quantification and Western Society, 1250–1600* (Cambridge University Press, Cambridge)
- Darwin C, 1974 *Charles Darwin and T. H. Huxley Autobiographies* Ed. G de Beer (Oxford University Press, Oxford)
- Daston L J, 1988 *Classical Probability and the Enlightenment* (Princeton University Press, Princeton, NJ)
- Desrosières A, 1998 *The Politics of Large Numbers: A History of Statistical Reasoning* originally published in French in 1993; translated by C Nash (Harvard University Press, Cambridge, MA)
- Dow: transcriptions of Geographers on Film Series, operated by M W Dow, Plymouth State College, Plymouth, NH
- 1971a, "G Donald Hudson interviewed by John Fraser Hart", 19 April
- 1971b, "Brian J L Berry interviewed by John Fraser Hart", 19 April
- 1972, "William Garrison interviewed by Maynard Weston Dow", 24 April
- Einstein A, 1949, "Autobiographical notes", in *Albert Einstein: Philosopher Scientist* Ed. P A Schipp (Library of Living Philosophers, Evanston, IL) pp 3–95
- Garrison W L, 1956, "Applicability of statistical inference to geographical research" *Geographical Review* **46** 427–429
- Garrison W L, 1979, "Playing with ideas" *Annals of the Association of American Geographers* **69** 118–120
- Garrison W L, Marble D F, 1957, "The spatial structure of agricultural activities" *Annals of the Association of American Geographers* **47** 137–147
- Garrison W L, Berry B J L, Marble D F, Nystuen J, Morrill R L, 1959 *Studies of Highway Development and Geographic Change* (University of Washington Press, Seattle, WA)
- Gigerenzer G, Swijtink Z, Porter T, Daston L, Beatty J, Krüger L, 1989 *The Empire of Chance: How Probability Changed Science and Everyday Life* (Cambridge University Press, Cambridge)
- Gold P, 1970, "Is *Statistix Inferens* the geographical name for a wild goose chase?" *Economic Geography* **46** 436–449
- Hacking I, 1990 *The Taming of Chance* (Cambridge University Press, Cambridge)

- Hankins T L, 1979, "In defence of biography: the use of biography in the history of science" *History of Science* **17** 1 – 16
- Hardy G H, 1941 *A Mathematician's Apology* (Cambridge University Press, Cambridge)
- Hess D J, 1997 *Science Studies: An Advanced Introduction* (New York University Press, New York)
- Hoover E M, 1948 *The Location of Economic Activity* (McGraw-Hill, New York)
- Janelle D G, 1997, "William Warntz, 1922 – 1988" *Annals of the Association of American Geographers* **87** 723 – 731
- Johnston R, 1991 *Geography and Geographers: Anglo-American Geography Since 1945* 4th edition (Arnold, London)
- Johnston R, forthcoming, "On disciplinary history and textbooks: or, where has spatial analysis gone?" *Australian Geographical Studies*
- Jones C F, 1935 *Economic Geography* (Henry Holt, New York)
- Kermode F, 1979 *The Genesis of Secrecy: On the Interpretation of Narrative* (Harvard University Press, Cambridge, MA)
- King L J, 1993, "Spatial science and the institutionalization of geography as a social science" *Urban Geography* **14** 538 – 551
- Kirby A, 1994, "What did you do in the war, daddy?" in *Geography and Empire* Eds A Godlewska, N Smith (Blackwell, Oxford) pp 300 – 315
- Latour B, 1987 *Science in Action: How to Follow Scientists and Engineers Through Society* (Harvard University Press, Cambridge, MA)
- Latour B, 1988 *The Pasteurization of France* (Harvard University Press, Cambridge, MA)
- Latour B, 1999 *Pandora's Hope: Essays on the Reality of Science Studies* (Harvard University Press, Cambridge, MA)
- Latour B, Woolgar S, 1979 *Laboratory Life: The Social Construction of Scientific Facts* (Sage, Beverly Hills, CA)
- Livingstone D N, 1992 *The Geographical Tradition: Episodes in the History of a Contested Enterprise* (Blackwell, Oxford)
- Livingstone D N, 1999, "No more disembodied minds" *Books and Culture* **5** (3) 21 – 23
- Livingstone D N, 2000, "Putting geography in its place" *Australian Geographical Studies* **38** 1 – 9
- Lösch A, 1954 *The Economics of Location* 2nd edition, translated from the German by W H Woglum with the assistance of W F Stolper (Yale University Press, New Haven, CT)
- MacKenzie D A, 1979, "Eugenics and the rise of mathematical statistics in Britain", in *Demystifying Social Statistics* Eds J Irvine, I Mills, J Evans (Pluto Press, London) pp 39 – 50
- MacKenzie D A, 1981 *Statistics in Britain 1865 – 1930: The Social Construction of Scientific Knowledge* (Edinburgh University Press, Edinburgh)
- MacKenzie D A, 1999, "Eugenics and the rise of mathematical statistics in Britain", in *Statistics in Society: The Arithmetic of Society* Eds D Dorling, S Simpson (Arnold, London) pp 55 – 61 (a revised version of MacKenzie, 1979)
- Nadel I B, 1984 *Biography: Fiction, Fact and Form* (Macmillan, London)
- Peet R, 1998 *Modern Geographical Thought* (Blackwell, Oxford)
- Pickering A, 1992, "From science as knowledge to science as practice", in *Science as Practice and Culture* Ed. A Pickering (University of Chicago Press, Chicago, IL) pp 1 – 26
- Porter T M, 1986 *The Rise of Statistical Thinking* (Princeton University Press, Princeton, NJ)
- Porter T M, 1995 *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton University Press, Princeton, NJ)
- Restivo S, 1992 *Mathematics in Society and History: Sociological Inquiries* (Kluwer, Dordrecht)
- Rorty R, 1980 *Philosophy and the Mirror of Nature* (Princeton University Press, Princeton, NJ)
- Sartre J-P, 1964 *Nausea* (first published in French in 1938; translated into English by L Alexander) (New Directions, New York)
- Sheppard E S, Barnes T J, 1990 *The Capitalist Space Economy: Geographical Analysis After Ricardo, Marx and Sraffa* (Unwin Hyman, London)
- Söderqvist T, 1996, "Existential projects and existential choice in science: science biography as an edifying genre", in *Telling Lives in Science: Essays in Scientific Biography* Eds M Shortland, R Yeo (Cambridge University Press, Cambridge) pp 45 – 84
- Stewart J Q, 1945 *Coasts, Waves and Weather, for Navigators* (Ginn and Company, Boston, MA)
- Stigler S M, 1986 *The History of Statistics: The Measurement of Uncertainty Before 1900* (Belknap, Cambridge, MA)
- Stone K H, 1979, "Geography's wartime service" *Annals of the Association of American Geographers* **69** 89 – 96

-
- Taylor P J, 1976, "An interpretation of the quantification debate in British Geography" *Transactions of the Institute of British Geographers, New Series* **1** 129–142
- Traweek S, 1988 *Beamtimes and Lifetimes: The World of High Energy Physicists* (Harvard University Press, Cambridge, MA)
- Traweek S, 1992, "Border crossings: narrative strategies in science studies and among high energy physicists at Tsukuba Science City", in *Science as Practice and Culture* Ed. A Pickering (University of Chicago Press, Chicago, IL) pp 429–465
- Ullman E L, 1941, "A theory of location for cities" *American Journal of Sociology* **46** 853–864
- Watson, J, 1981 *The Double Helix: A Personal Account of the Discovery of the Structure of DNA* (W W Norton, New York)
- Warntz W, 1984 "Trajectories and co-ordinates", in *Recollections of a Revolution: Geography as Spatial Science* Eds M Billinge, D Gregory, R L Martin (Macmillan, London) pp 134–150
- White R, 1998 *Remembering Ahanagan: Storytelling in a Family's Past* (Hill and Wang, New York)
- Wittgenstein L, 1964 *Remarks on the Foundations of Mathematics* Eds G von Wright, R Rhees, E G Anscombe (Blackwell, Oxford)
- Wrigley N, Mathews S, 1986, "Citation classics and citation levels in geography" *Area* **18** 185–194
- Young R M, 1988, "Biography: the basic discipline for the human sciences" *Free Associations* **9** 108–130

