Computing and the science of Geography: the postmodern turn and the geocomputational twist

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Introduction

In the wake of Kuhn's (1962) attack on established notions of scientific progress, Haggett and Chorley (1967) announced that Geography was undergoing a quantitative revolution. In fact, it was more of a battle cry than an announcement. It stirred up rebellion within the discipline and sent marauders off into neighbouring domains to bring back intellectual booty. Like the quantitative revolution, geocomputation is an enterprise stretching well beyond the borders of academic geography. The two movements have many other characteristics in common but they also have import differences, the most significant of which is the most obvious - a radical difference in accessible computing power.

In the period between the heyday of the quantitative revolution and the coining of the term geocomputation, much philosophical water has flowed under the geographical bridge. There have been major surges from humanism, Marxism and, latterly, postmodernism and there have been many minor currents. But throughout this period, the geocomputational tide has been rising, little noticed by the philosophers of geography. Much of their concern, as proponents or opponents, has come to focus on the 'postmodern turn'. Until recently, they have largely ignored the geocomputational twist in the tail of quantitative geography - or in what they had taken to be its tail.

This paper seeks to place the 'geocomputational twist' in its philosophical and historical setting, stimulated in part by a series of email exchanges between the organisers of GeoComputation '97 on possible definitions of the neologism. It presents an illustrated argument in support of two propositions: that the quantitative revolution and the burgeoning of computational geography belong to the same, long-standing, intellectual tradition; and that that tradition is flourishing. One could argue, as hinted above, that geocomputation is a continuation or addendum to the quantitative revolution but one can equally well view the latter as a rehearsal for the former. If one takes this position - standing, as it were, at the present looking back - then it is clear that the rehearsals were under way well before the 1960s. It is equally clear that geocomputation, when looked at in these terms, has a long way to go before it fulfils its promise.

The paper sketches out a few ideas on the foundations of scientific geography, where the latter term is taken to embrace rather more than the academic discipline. It looks briefly at measurement, calculation and computing technology prior to the electronic age, using Harrison's chronometers, the Varignon Frame, and the notion of market equilibrium as examples. It presents a thumb nail sketch of the standard picture of science in the quantitative revolution and of the social context within which scientific geography was promoted. And it considers certain counter-revolutionary criticisms of the notion that society may be studied scientifically.

An important part of the argument is a consideration of the changes that have occurred in science since the standard, physics-based, picture was painted. That picture was always a caricature. The expansion of the biosciences, the explosion of interest in nonlinear systems in general and chaos in particular, the associated discovery of the fundamental unpredictability of certain physical and biological systems, and the recognition that objectivity in science is a direction rather than a terminus have all contributed to the blurring of the supposed science/social science distinction. And at the centre of much of this change has been computing. It was, after all, in the humming of a Royal McBee that Edward Lorenz first detected chaotic behaviour.

Such behaviour might be thought to be a recent characteristic of the discipline itself, or, perhaps, of its philosophically self-conscious branches. But under the postmodern froth there is a strong geocomputational brew. Emblematically, whilst the revisionary metaphysicians have been exercised about the notion of truth, the spatial scientists have been harnessing fuzzy logic.

As for the social context, it has, of course, changed radically since the '60s. And those changes, as any good materialist should admit, have all but put paid to the Marxist project. Such force as there was in Harvey's (1989) accusation that modellers have produced 'little more than the proverbial hill of beans' has been eclipsed by the collapse of the house of cards that represented the Marxist project in practice. Of at least equal significance, arguably, has been the extraordinary advancement in computing power, the emancipatory effect of its widespread availability, and the wiring of society.

Drawing these threads together, the paper attempts both to justify the claims made about the methodological significance of the geocomputational twist and to highlight the shortcomings in the contemporary portfolio of geocomputational activities.

The Analytic Tradition

One of the difficulties inherent in understanding the debate about the nature of the quantitative revolution – and, by extension, the nature of geocomputation - is a persistent and often wilful misuse of terms. The words 'quantitative' and 'revolution' both require scrutiny, as does the term 'positivism'. As Taylor and Johnston (1995 p.52) have argued, the quantitative revolutionaries adopted markedly different approaches and had different views on an appropriate name for their movement:

> three early popular labels were "conceptual", "model-based", and "statistical" – before the label "quantitative" was generally adopted

This heterogeneity has been played down by critical historians who have found it convenient to use a single label and to ascribe a particular view of science to those it has been attached to (*ibid.* p.52):

> The quantifiers were criticised from a range of contrary positions for their excessively narrow interpretation of what constitutes science. In this process the quantitative revolution was reconstructed as a unitary monolith (*sic*) and any diversity associated with its theoreticians tended to be written out of the story.

Taylor and Johnston go on to argue that there were tensions within the movement (*ibid.* p.52)

> between deductive and inductive "science" and … between "pure" and "applied" geography

And they say that in the early stages (up to the 1970s) it was pure geography that dominated. Thus, at least in the first flush of the quantitative revolution, geography had some resemblance to the standard model of a science, with rationalist and empiricist wings and what Taylor and Johnston refer to as a 'mainstream concern for models and theory'. Arguably, then, 'scientific' would be a better label than 'quantitative'.

The term 'revolution' is not particularly illuminating either. As the introduction to this paper suggests, its early use in geography was as much prescriptive as descriptive. The extent to which the movement actually was revolutionary is a matter for debate, as is Kuhn's view about the nature of change in science. What is more, there appears to be a mismatch between Kuhn's conception of science and the views of the revolutionary geographers about their own work. They seem to have subscribed to the idea that science is a rational and cumulative enterprise, which deals objectively with testable propositions about the real world. Kuhn challenged this idea. As Searle (1996, pp.11-12) puts it:

> Kuhn sometimes seems to be arguing that there is not any such thing as the real world existing independently of our scientific theories, which it is the aim of our theories to represent. Kuhn, in short, seems to be denying realism.

He then adds (*ibid.* p.12):

> Most philosophers do not take this denial of realism at all seriously. Even if Kuhn were right about the structure of scientific revolutions, this in no way shows that there is no independent reality that science is investigating.

Whilst the quantitative revolutionaries were happy to appeal to Kuhn's ideas to justify their attempts to transform the discipline, few if any shared his relativism. Behind the rhetoric of scientific revolution - derived from arguments about revolutionary change *within* a science - was a more gradual but in some ways more profound transition from an unscientific to a partially scientific geography.

As for 'positivism', it is seldom clear what various users of the term have in mind, apart for their disapproval. In the philosophical literature, 'positivism' tends to be used, if at all, as a contraction of 'logical positivism'. The nature of this school is neatly summarise by Solomon (1997, p.720):

> The main thrust of logical positivism is its total rejection of metaphysics in favour of a strong emphasis on science and verifiability through experience. The method of the logical positivists, accordingly, is strongly empiricist (they actually called themselves "logical empiricists")…

In addition to the rejection of metaphysics, the logical positivists had a clear view about ethical and aesthetic statements. They thought (Pettit, 1993, p.9) that:

> Evaluative propositions did not serve, or at least did not serve primarily, to essay a belief as to how things are; their main job was to express emotion or approval/disapproval, much in the manner of an exclamation like 'Wow!' or 'Ugh!'

The logical positivists, then, were concerned with 'how things are' and they took the view that evaluative statements do not help *with this task.* But there is a great deal of sloppy reasoning between that observation and the notion that 'positivists', in some ill-defined sense, are not concerned with matters of conscience or social justice. And the reasoning is worse than sloppy when it comes to suggesting, as some recent geographical writing appears to do, that positivists, *qua* positivists, have been complicit in crimes against humanity. The logical positivists were certainly acquainted with crimes against humanity but in a somewhat different sense (*ibid*, p.720):

> Against the horrendous mythologies and superstitions propagandized by the Nazis, using the old metaphysics as a tool, these philosophers used the clarity of science to dispel non-sense and to defend common sense. Accordingly, the group was broken by the Nazis ...

The central feature of 'positivism' in geography, in the minds of its critics, appears to be the empiricism of the logical positivists. This ties in with the notion that geography in the quantitative revolution was monolithically inductivist (see above). Thus, the terms 'positivist' and 'quantitative' have come to be used more or less interchangeably by the critics, with both failing to capture the heterogeneity of the 'scientific' movement in the discipline. However, it is not just the empiricism of the logical positivists that the critics wish to carry over into their notion of positivism. It is the failures of logical positivism as a philosophical doctrine.

In a conversation with Brian Magee, A.J. Ayer, the man who did most to propagate logical positivism in the English-speaking world, was clear about its inadequacies (Magee 1978 p.131).

> MAGEE But [logical positivism] must have had real defects. What do you now, in retrospect, think the main ones were?
>
> AYER Well, I suppose the most important of

the defects was that nearly all of it was false.

The critics of positivism in geography would like to be able to claim that this observation may be extended to the foundations of the quantitative revolution and its modern manifestations. Pickes, for example, seems to think that the intellectual battle has been won by critical theorists but that the quantifiers have failed to accept defeat. He says (Pickles 1995 p.25) that for some scholars, apparently including himself,

> GIS represents a reassertion of instrumental reason in a discipline that has fought hard to rid itself of notions of space as the dead and the inert, and, as Soja (1989) has argued, to reassert a critical understanding of the sociospatial dialectic.

But this will not do as a mapping of the wider philosophical debates into a geographical context. Logical positivism has not been abandoned in favour of the critical doctrines of the so-called continental philosophers. On the contrary, it is the analytic tradition, in which logical positivism played a central part, that has come to dominate the philosophical landscape. According to Searle's essay on contemporary philosophy in the United States (*op. cit.* p.1),

> Without exception, the best philosophy departments in the United States are dominated by analytic philosophy, and among the leading philosophers in the United States, all but a tiny handful would be classified as analytic philosophers.

Magee and Ayer make a similar point at a personal level. Logical positivism may have had its day but the general view of the world implicit in it is alive and well:

> MAGEE So, a former Logical Positivist such as yourself, although you now say that most of the doctrines were false, still adopts the same general approach; and you are still addressing yourself to very much the same questions, though in a more liberal, open way?
>
> AYER I would say so, yes.

Thus, to understand the shortcomings of scientific work in geography, it is more instructive to look at the changing view of science within the analytic tradition than to turn to the philosophically eccentric positions of various critical theorists.

Returning to the Pickles quotation, one might argue that the attempts to 'reassert a critical understanding of the sociospatial dialectic' are intended to undo the very thing that logical positivism did succeed in doing – undermining the old metaphysics - but I do not want to pursue that line of argument. Rather, I want to conclude this section by asserting that the blanket attachment of the title 'positivist' to scientific work in geography does not serve to undermine the philosophical foundations of that work. Scientific geography continues to derive philosophical support from the analytic tradition, notwithstanding the demise of logical positivism, and that tradition is the dominant one in philosophy.

To summarise, the 'quantitative revolution' was neither quantitative (if that term is used to mean inductivist) nor revolutionary (in the Kuhnian sense). The heterogeneous body of work that comes under the rubric of the quantitative revolution and/or geocomputation is best described as being 'scientific'. It is not the case that the supposedly 'positivist' geography of the quantitative revolution has been weeded out by critical theorists, only to start spreading again through the development and use of GIS.

The scientific approach to geographical problems was and is firmly rooted in the analytic tradition of philosophy. Rather than turn to critical theory to understand the shortcomings of scientific geography, it is helpful to consider the changing notion of science within the analytic tradition and the changing role of computation in science.

Science and computation

It was noted above that the geography of the quantitative revolution exhibited a range of activities that gave the discipline some resemblance to the (then) standard model of a science. Specifically, geography became increasingly concerned with, on the one hand, data exploration and inductive reasoning and, on the other, model building and theory development. The prevalent notion of science owed much to the model of physics. Science was thought to be truth-seeking, cumulative, and objective. It was believed that as

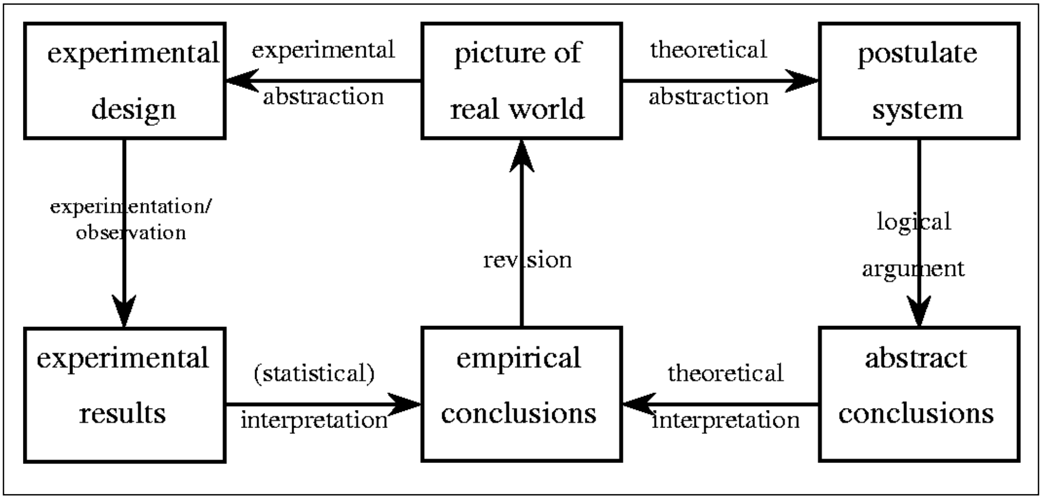

Figure 1. The scientific method in diagrammatic form

our understanding of various systems increased, so did their predictability. The process of scientific advancement was thought to consist of interrelated cycles of rationalist and empiricist endeavour (see Figure1). Computation entered the process both in the analysis of observational and experimental data (the left hand cycle of Figure 1) and through the numerical solution of mathematical problems for which analytical techniques were inadequate (a possible strategy on the right hand side). But computation was seen primarily as a means to an end, not as part of the intellectual milieu shaping the way in which scientific problems are conceived. The social context of scientific endeavour was one of optimism about the benefits that science could bring. Consequentially, perhaps, the sociology of science was not of great interest, certainly not in a geographical context.

I want to consider some of the changes that have occurred in this view of science and its social circumstances but first I want say something about computation. Three examples should serve to illustrate the range of social and intellectual purposes to which computational devices have been put. All three examples are of significance in the history of geography.

The first is the chronometer, specifically John Harrison's four chronometers H-1 to H-4. Sobel's entertaining book *Longitude* tells of the trials (literally) and tribulations associated with Harrison's attempt to solve the problem of calculating a ship's longitude at sea. The problem was one of such importance in the early 18th century that the British Parliament, in passing the Longitude Act of 1714, offered a prize £20,000 for its solution. Two strategies came to the fore: the astronomical ideas of the scientific establishment; and Harrison's idea that it was possible to make a clock of great accuracy with which the true time could be carried from the home port. Solar observation could then be used to establish local time and the time difference used to calculate the change in longitude. The astronomers believed that no one could build a clock of sufficient accuracy. They thought that the problem would be solved by producing tables of data relating to the position of the moon relative to other celestial objects at given times and at given longitudes for years into the future. The battle, which raged through the second and third quarters of the century, provides a useful case study of the sociology of science. Sobel (1995 p.9) observes of Harrison that:

> His every success … was parried by members of the scientific elite, who distrusted [his] magic box. The commissioners charged with awarding the longitude prize – Nevil Maskelyne [the fifth astronomer royal and Harrison's principal rival] among them – changed the contest rules whenever they saw fit, so as to favour

the chances of the astronomers over the likes of Harrison and his fellow "mechanics". But the utility and accuracy of Harrison's approach triumphed in the end. His followers shepherded Harrison's intricate, exquisite invention through the design modifications that enabled it to be massed produced and enjoy wide use.

Harrison's chronometers were mechanical computers dedicated to the task of measuring longitude. They are thought of as scientific instruments but are not scientific in the sense that they facilitated either the inductive or deductive processes of scientific development represented in Figure 1.

This is not true of the Varignon Frame. It can be thought of as a mechanical computer, again dedicated to a particular type of task. But the task may be thought of as belonging to the right hand side of Figure 1. The Frame computes solutions to what geographers refer to as Weberian location problems (see Wesolowsky (1993) for an interesting account of the genesis of this class of problems). That is, it provides a mechanical method for obtaining numerical solutions to a mathematical problem and, by analogy, identifies the implications of a set of assumptions about industrial location under specified initial conditions. The simplicity of the assumptions and conditions has the effect of detaching the process from the inductive, left hand side, of Figure 1; the assumptions and conditions are not capable of being true of many or any real systems so there is no sense in trying to test them. The reason why they cannot be anything but simple is, of course, the computing technology. Given the absence of an analytical solution to the general Weber location problem, a mechanical analogue computer may be used. However, as well as allowing a solution to be found, this approach limits the way in which the problem may be conceptualised.

There is an interesting parallel with the notion of market equilibrium. The idea that price and quantity in a market is determined by the intersection of supply and demand schedules is a construction rooted in 19th century computing conditions. The simultaneous solution of two equations provides answers to questions about a market that would be difficult to generate otherwise, given those conditions. But in a modern computing environment, there is not need to assume away the whole, messy, multi-agent process of market interaction. I will return to these observations later. Meanwhile, I want to consider, very briefly, some of the aspects of the changing picture of science noted above.

At the time of the quantitative revolution, one of the objections frequently raised against the use of the scientific method to study social phenomena was that it entailed a mechanical view of the world. There was some truth in this charge. Physics was the model science and mechanics is a branch of physics. Our understanding of the universe was built on a clockwork conception of the heavens. Much of the mathematics that was available, including the calculus of Newton and Leibnitz, was forged in the study of physical phenomena. And, of course, some of the approaches that were adopted were directly mechanical – like the use of the Varignon Frame. It is not too difficult to object to the employment of scientific methods in a social context when physics is the inspiration, as it was in the gravitational and thermal models of migration of Ravenstein (1885) and Hotelling (1979). But the rise of the biological sciences has altered our conception of what constitutes a science, undermining this source of objection; the intellectual distance from ecology to population geography seems less than that from physics. Indeed, as the social sciences have developed alongside the biological, the intellectual traffic has not been all one way, as it was with physics. Darwin's debt to Malthus is well know (see for example, Bronowski 1973) but it is not the only example of the biological sciences borrowing from the social; the theory of games is a more recent example of some importance (see, for example, Nowak and May, 1992).

Closely connected with the scepticism about physics as a methodological beacon, was the notion that social life does not have the predictability and, therefore, the controllability of the physical world. This belief has been undermined not so much by successes in the social arena as by the growing realisation that aspects of the physical world are fundamentally unpredictable. Interest in catastrophies and bifurcations, in fractal geometry, and in chaotic behaviour, has spanned the scientific spectrum and this interest has helped to make it clear that, if there is a methodological cleavage between the social and physical sciences, it does not centre on predictability.

One of the other sources of this supposed cleavage is the problem of objectivity. It has long been argued that objectivity is possible in the physical world – in the study of objects – but not in the social world. But history suggests that the distinction is not so clear. The example of the longitude problem is helpful here, notwithstanding the fact that it is as much technical as scientific. The identification of the longitude problem as being worthy of study was clearly social, and the assessment of the empirical claims made by Harrison and his opponents was scarcely objective. There was as much attachment to belief in a cherished theory - and as much chicanery to sustain that belief - as might be found in any strictly social context.

The debate about objectivity shades off into the debate about truth. The revisionary metaphysics of the postmodernists is sceptical about claims to both. But when it comes to truth, the objectors have a serious obstacle to overcome. Scruton (1994 pp?) puts it this way:

> Nietzsche … argued that there are no truths, only interpretations. But you need only ask yourself whether what Nietzsche says is true, to realise how paradoxical it is. (If it is true then it is false! - an instance of the so-called liar paradox). [GAP?] Likewise…Foucault repeatedly argues as though … [t]here is no trans-historical truth about the human condition. But again, we should ask ourselves whether that last statement is true: for if it is, it is false…A writer who says that there are no truths, or that all truth is 'merely relative' is asking you not to believe him. So don't.

Despite counter-attacks such as this, relativism has been a mainstay of critical approaches in geography. It has taken the subject in two directions - towards a change of context and towards a change of focus.

A standard philosophical distinction is that between the context of discovery and the context of validation. Questions related to the former belong to the sociology of science; they deal with the circumstances under which particular problems and ideas have become objects of study. Questions related to the latter are methodological; they deal with the so-called logic of justification – with arguments about the reliability of knowledge claims. Philosophical concerns in geography have shifted under the influence of relativist thinking from the context of validation to the context of discovery. In 1969, Harvey's *Explanation in Geography* concentrated on methodological issues; his presumption was that there is a real world out there, which is knowable, provided certain methods are employed. Relativist dissent from this position shifted the debate to the context of discovery so that, for example, interest in Weber's theory of industrial location (such as it was) moved from the theory's propositions to its social origins and uses.

The change of focus brought about by relativist thinking has been from the general to the particular or, to use the terminology of an old debate, from the nomothetic to the ideographic. The postmodern enthusiasm for the recognition of alternative voices and the celebration of difference is underpinned by a rejection of the idea that there is a single truth, independent of the observer. This rejection relies on a rather loose usage of the term 'truth'. It may be that different individuals and groups have different perceptions of some object or phenomenon and that we cannot talk about which is the 'true' perception. But that does not mean that true propositions about what these different perceptions consist of cannot be formulated. It is important to note that this is not a repudiation of the idea that alternative voices should be heard and differences celebrated. It is a repudiation of the idea that these objectives are incompatible in principle with a scientific conception of the pursuit of knowledge. The extent to which they are compatible in practice is, at least in part, a computational issue.

Models

It should not be assumed from the foregoing argument that the notion of 'truth' is unproblematic. Indeed, in recent decades, there have been important advances in dealing with this notion in both science and philosophy. In science, the dominance of Boolean logic, in which the only truth values are 0 and 1, has been reduced by the development of fuzzy logic, with its continuum of truth values (for a basic introduction with geographical references see Macmillan 1995). In philosophy, the notion of truth has been at the centre of increasingly sophisticated criticisms of realist beliefs. These criticisms have led Aronson *et al.* (1994) to mount a rescue of realism based on a re-orientation of the debate away from the truth of propositions towards the verisimilitude of models. The increasing importance of models philosophically has not been reflected in geographical work.

One of the difficulties surrounding model use in geography is that the nature of models and the purposes of model building are widely misunderstood, even amongst those who promote their use. As I have droned on at some length on these matters in other papers (see, for example, Macmillan 1989, 1996), I will confine myself here to one point. It is often said in introductions to modelling, that models involve simplifications of reality. This is true but unhelpful. First, all attempts to characterise the world, including ordinary language descriptions, involve simplifications. There is nothing peculiar, in this respect, about model building. Second, the simplicity of a model, or an ordinary language description, depends on the purposes of the author. To make this point whilst teaching I tend to pick an everyday object, like a waste bin, and ask students to describe it. As often as not, they launch into a rather complex account: 'It's a truncated cone, inverted with an open base, made of metal, painted grey, etc.'. They sometimes look puzzled when I give them my description: 'It's a waste bin'. But they see the point when I indicate the purpose of the description: 'Throw this in the inverted, truncated cone for me will you?'. My simple description is adequate for the purpose of using the waste bin. Map making is equally purposeful. The purpose of the London Underground Map is to help travellers navigate. The representation of the system is simple in order to facilitate this task – nomenclature and topology are represented accurately but nothing else is. But there are other maps of the Underground, such as those used for engineering works in the tunnels, and these attempt to represent accurately those features that are required by the engineers. The complexity of models, like that of maps, is a reflection of the purposes of their authors and users.

There are, however, technical and intellectual constraints on the achievement of these purposes. It was noted above that the Varignon Frame computes solutions to Weberian location problems. The Frame is a representation – a model – of an economic landscape. It is a 'simplification' of the landscape not because simplicity best serves the purpose of emulating the industrial location decision problem but because the computing technology will not allow greater sophistication. Similarly, but more subtly, the notion of market equilibrium embodies an intellectual constraint imposed by 19th century computational capabilities.

GIS and explanation

This brings me to the nature and use of GISystems. What are we capable of doing with this late 20th century computing technology? If one believes Taylor and Johnston, we cannot use it successfully in an explanatory context. They argue (*op. cit.* p.61) that:

> quantitative procedures, and hence GIS, … cannot produce substantial answers to the question 'Why?'

They base this view on Sayer's (1984) notion that mathematics is an acausal language. I have taken issue with this claim before (Macmillan, 1989). If we regard 'cause' as 'sufficient condition' (see, for example, Hospers 1967 p. 279-320), then a set of mathematical relations with an appropriate empirical interpretation can be construed causally. This is precisely how the causal explanations of the physical sciences are formulated: a set of equations, say, represents a set of law-like generalisations; a set of parameter value assignments constitutes a set of condition statements; and a solution statement represents the statement of the event or condition to be explained.

GISystems *should* be used in producing substantial answers to the question 'Why?'. They allow a representation of spatial systems which is substantially better than that embodied in the Varignon Frame. They provide a less-constraining computational environment. They certainly do not provide a *non*-constraining environment and some of the critical comments that have been made about data-led GIS work may be thought of as highlighting some of the constraints that undoubtedly operate.

Taylor and Johnston further question the possibility of using GIS for explanatory work by arguing (*ibid.* p.57) that

> The original 'quantifiers' attempted to … [develop] deductive theory but … it is just this aspect of quantitative geography that has been severely castigated by GIS proselytizers …

That is a fair comment, taken in isolation, although it is a little surprising to find the proselytizers called on in support of a case that is largely directed against them. But as a line of argument it is not persuasive. The fact that Openshaw sees science as consisting only of the inductive half of Figure 1 does not make it so. And the suspicion that Openshaw can see more out of his one methodological eye than many can with two does not alter this conclusion.

It is certainly the case that much GIS work has been data-led and that a good deal of it has been applied. But it is also true that there has been a fair amount of theoretical endeavour. Goodchild (1995 p.46) notes that

> An environmental modeler will likely write his or her model in source code, typically FORTRAN or C, but may well maintain a GIS, linked to the modeling system, to preprocess data, and to analyze and present the model's results. This type of GIS use probably characterizes the majority of efforts in environmental simulation modeling...

Theoretical endeavour of this kind bridges the gap between pure and applied geography, to which Taylor and Johnston allude. That gap, as indicated above, is largely computational in origin. A rich system of conditions, on which law-like statements can operate, allows theoretical ideas to be applied.

Social change

There is a greater continuity here than Taylor and Johnston would allow. What they see as a 'tension' between pure and applied work in the quantitative revolution does not look like that to me. For my own part, starting work in geography too late to be a revolutionary, theory seemed to be a necessary pre-requisite for application. Indeed, the thing that was applied *was* the theory. I became interested in theory development because of my interest in applications and many others did the same. Of course, the social climate was one in which it was thought desirable to provide scientific support for rational decision making in the public interest. Much computational model building was predicated on this idea. But societies change.

The culture of the times, in many parts of the world, swung against what might be called the planning perspective. From the right, it was not just planning and the social democratic notion of market intervention that came under attack - it was the notion of society itself. From the left, the supposed irredeemability of capitalism led to a similar conclusion – the idea of rational decision making in the public interest was a snare and a delusion. But, as I have just said, societies change.

In Britain, in much of Western Europe, in the U.S. (arguably), and in many other places, the intellectual leadership of the right has waned. At the same time, the dramatic collapse of communism has done little to further the claims of the left. Geography as a discipline has become somewhat eccentric in its continuing interest in Marxist thinking - much of the rest of the academy has moved on. To be sure, the new world is not the same as the old, either materially or intellectually. But the old idea that science can serve society, and serve in the study of society, has re-emerged, battered but unbowed.

Geocomputation

Where does this leave us with regard to the nature of geocomputation? I don't propose to dwell on what it consists of historically or currently but I will venture an opinion on what it could or should be. The foregoing arguments suggest that it should be a set of activities, conducted in or around a computationally sophisticated environment, in which the geographical sciences are developed and applied. Taking GIS to be the paradigmatic example of a computationally sophisticated environment, this means that we should be using GIS for theory development both inductively and deductively. That is, we should use GIS in an inferential mode but we should also be concerned with building models in a GIS environment – an activity that is theory-led rather than data-led. Indeed, GISystems should become the laboratories within which the two scientific cycles of Figure 1 interact fully for the first time in a geographical context.

This is not to say that application should be neglected. Theory and application should be related cyclically in what might be thought of as an orthogonal relationship to that shown in Figure 1. Theory should inform application and application should inform theory. In both cases, verisimilitude should be a watchword, although there should be an economy of design appropriate to the purposes of the exercise.

Clearly, geocomputational exercises should have explicit purposes and they should be conducted in the knowledge that those with whom we interact have their own purposes, including those that supply data and consume advice. Also, the form in which advice should be offered is of considerable concern. In applied work, we should not behave as if we were producing a product for a consumer, where the product consists of a single forecast and an optimal prescription based on that forecast. It would be more consistent with our contemporary understanding of science to build a model with which users can play, on the understanding that it can yield useful insights about real decision problems but that those insights are limited by the verisimilitude of the model (see Macmillan 1996). In theoretical work, we should take up our own purposes, the traditional purposes of the academy. For those of us concerned with society, we should be prepared to assert that our purpose is to understand, however hard that may be.

As for the critics, they might well claim that this is a pious hope, given the history of what they might see as data-led, theory-free, ethically neutral work in GIS and related fields. I prefer to think of it as a challenge to a new generation to see that the promise of geocomputation is fulfilled.

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