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QUESTIONS, TOOLS OR PARADIGMS: SCIENTIFIC GEOGRAPHY IN THE 1980'S

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ABSTRACT

Contemporary geography seems condemned to ever-increasing proliferation of methodologies, techniques and subfields. It is argued that this is due ultimately to the dominance of the paradigm of explanation. Frustrated with their apparent inability to develop theories of the landscape itself, geographers have been driven increasingly to the study of the processes which affect landscape, and therefore into the domains of other disciplines. There are no obvious reasons in principle to expect a reversal of this process, and no evidence of a reversal in practice.

There is no difficulty in defining the subject matter of geography, but the existence of subject matter is not sufficient for the survival of a discipline. Survival will ultimately be determined by application, which is the unacknowledged paradigm of the majority of current activity, and which leads to a definable and limited set of tools and methodologies.

INTRODUCTION

The late 1970's and early 1980's were a period of great insecurity in many departments of geography in North America. The staff of the UWO department had been cut from 25 in 1975 to 19 in 1981, and enrolment in geography courses had been dropping at roughly 10% per year for some time: morale was generally low. The department at the University of Michigan was being closed, and we were acutely conscious that our Vice-President (Academic) was the brother of the University of Michigan President. University faculty are always sensitive to the standing of their discipline among the higher ranks of their administration, and this was never more true than in November 1980 when the President of the University of Western Ontario made his opening remarks to the first joint meeting of the East Lakes division of the Association of American Geographers and the Ontario division of the Canadian association.

He remarked that as a bioscientist with little knowledge of geography as a discipline he had read the conference program with interest, and then made two points. First, the university campus was an unusually attractive one and it was in fact possible to canoe through it on the Thames River in something approaching wilderness surroundings, as he had done on several occasions. Second, the titles of many of the papers suggested to him that the group of geographers present could make significant contributions to a number of major disciplines, including economics, psychology and engineering. While it was undoubtedly well-delivered and entertaining, the speech merely confirmed the suspicion of many of those present that there was little clear understanding of the nature of their discipline in the minds of university administrators.

The low morale of the early 1980's has now largely disappeared. Enrolments in university geography programs are buoyant, at least in southern Ontario: departments are growing again, or at least able to offer junior positions: and both the need for geographical education and the work of geographers are receiving significant attention in the media, thanks to a large extent to the decision to expand the functions of the AAG Washington office. Finally, the much feared chain effects from the Michigan closure have not come about. Yet although the discipline may be more confident, and professional geographers more respected, geography is surely no less fragmented, and the public no less confused about what it is that professional geographers actually do.

These problems are acutely apparent in two particular aspects of current university curricula. In the early days of quantitative geography it was reasonably easy and straightforward to design an introductory course in quantitative methods: the contents consisted of a basic introduction to statistics with special attention to those aspects, such as the Poisson distribution, which had clear application in geography. Numerous textbooks appeared along those lines, and sessions were organized at national meetings to discuss the associated pedagogical problems.

After thirty years of quantitative research in geography the situation is very different. Point pattern analysis is out of fashion, along with many of the multivariate techniques which were prominent in the literature of the late 1960's. But few of these have been so discredited as to justify dropping them from the curriculum. Meanwhile the literature has moved on to new fashions: a contemporary introductory course must surely devote some time to interaction modelling and the associated research of the 1970's. In the hard sciences old theories fade away, to be revived only as strawmen or in philosophical discussion: how many undergraduate physics majors have read the literature of

phlogiston? But geography ... theories for ever: will the day ever come when Christaller will not be part of every urban-economic text?

In the absence of any clear criteria for selection, each university's introductory quantitative course increasingly reflects the particular interests of the department's faculty rather than some disciplinary consensus. And the process which operates within the quantitative subdiscipline also operates between subdisciplines: with more and more recognized branches of geography to be taught, there is increasing variance between departments not only in the content of quantitative courses but in whether undergraduates receive any quantitative training at all.

If there is increasing confusion about the appropriate contents of the quantitative curriculum, the same is surely true of the courses in the philosophy and methodology of geography which many departments require as part of their graduate program core. To the older Hartshorne/Schaefer/Bunge debate which so dominated these courses in the early 1970's we must now add Phenomenology, Marxism and every other methodological option which geographers have explored or embraced in recent years. To the student the result is increasingly incoherent, and by emphasizing such widely disparate options, increasingly irrelevant.

Like the search for empirical laws, the search for methodology can be both deductive and inductive. One can take an existing methodology, developed perhaps in the context of some other discipline, and attempt to show its relevance to geographic thought, or its consistency with the approach taken by many geographers, or the benefits it would bring to the discipline. Or one can examine the current practice of professional geographers, and distil its methodological essence. To do the latter would be to suggest that methodology should be an <u>a posteriori</u> structure erected on research activity, rather than an <u>a priori</u> and directing force. It would seem to be the appropriate way to proceed in the current atmosphere, given widespread respect for geography and the activities of certain geographers but no clearly focussed concept of what geographers as a whole actually do.

To pursue the analogy, deductive theories of the methodology of geography are no longer empirically valid, if they ever were. Attempts to devise a uniform, consistent focus for the discipline have been met instead by increasing fragmentation both in subject matter and philosophical approach. The unfortunate result is a growing pedagogical confusion. Yet the discipline is evidently alive and flourishing. The purpose of the remainder of the paper is to attempt a reconciliation of this apparent contradiction. To do so, however, we must first examine certain aspects of current methodological thinking.

EXPLANATION IN GEOGRAPHY

The Quantitative Revolution of the 1960's is widely recognized as bringing about a major change in geographic thought and in the nature of the discipline as a whole, as reflected in its literature and teaching curricula. By emphasizing numeracy, however, the term is misleading: the major impact was not in the introduction of mathematics and quantitative methods but in the new emphasis on the use of appropriate tools in the pursuit of the goal of scientific explanation. Geography was painted as a discipline concerned with the identification and empirical verification of laws which would provide answers to its particular set of questions, those having to do with the locations of events on the surface of the earth.

The new emphasis on scientific explanation was laid out in Harvey's (1969) definitive Explanation in Geography, which became the textbook of numerous graduate methodology courses. In an influential essay in 1972 Sack examined the question of whether a scientific explanation could ever be said to be in any sense distinctly geographic, and concluded that

> The questions that geographers ask do not require explanations that can be called peculiarly or uniquely geographic. Laws that are appropriate to geography are laws that are appropriate for other questions as well (Sack, 1972, p. 78).

Thus geography, in common with other scientific disciplines, would be defined by a unique subject matter coupled with the goal of explanation.

The thinking of the late 1960's had an enormous influence on the discipline, appearing as it did to provide a clear and consistent answer to its traditional insecurity. Yet less than twenty years later we find geography and geographic thought more fragmented than ever. The failure of the explanatory, theory-building paradigm to unite and strengthen the discipline appears to be due to two basic flaws, and their immediate consequences. First, although there is a well-defined set of questions which are undoubtedly geographic in nature, this is not a sufficient condition for the existence of a discipline. Second, by emphasizing explanation as the dominant goal of all geographic inquiry the paradigm assumes that explanation is both possible and desirable. These issues are pursued in the next two sections.

SUBJECT MATTER

There are many ways in which one could classify subject matter, depending on the criteria used. So although it is

possible to identify a set of questions which are geographic by virtue of their emphasis on location, it is not at all clear that this aspect is of sufficient importance to justify the establishment of a separate discipline, any more than the existence of a set of questions about cities justifies a discipline of urban studies. And because different disciplines are likely to use different criteria in their own taxonomy, conflicts will inevitably arise: are questions involving location and social class to be part of geography or sociology?

The existence of a discipline is thus determined not only by a common goal and assorted taxonomies of subject matter but also by factors of social significance and historical inertia. Perhaps more importantly, any scientific activity requires a set of tools with which the subject matter can be manipulated and analyzed, and in which its practitioners must be trained, and this can have an enormous influence in cleaving one discipline or in moving two together. The development of x-ray techniques led to radical changes in the role of mineralogy as a separate discipline and its relationship to geology and physics.

Since no explanation can be said to be specifically geographic, in pursuing the goal of explaining their subject matter geographers have been led into the development of so many varied tools that it is no longer clear what is necessary for an adequate geographical training. Yet without a unique training it is difficult for any discipline to maintain a unique claim to its subject matter. Geographical questions will only be referred to geographers if the latter can demonstrate some superior ability to answer them. (It is of course often argued in this context that geographers are unique in the breadth of their training).

Two examples illustrate the difficulty. Geographical analysis has always been important in epidemiology in the clues it can offer for the causes of disease, and this has become increasingly true with the growing emphasis in recent years on environmental causes of cancer: yet few geographers have been able to establish personal reputations in this area which extend beyond the discipline itself (the work of Cliff and Haggett, 1984, is a notable exception) and few epidemiologists would acknowledge geography as a necessary part of their own training. Again, interplanetary missions by Viking and Mariner spacecraft have returned enormously valuable data to earth on the physical geography of the planets: yet geographers have clearly failed to demonstrate a superior ability to analyze and interpret these data in competition with other earth scientists.

In summary, then, geography's claim to its subject matter is not unique or absolute, but must be established in practice through the development and mastery of unique tools and techniques. But in emphasizing explanation, geographers have been led into such a vast and varied array of tools that no mastery is possible. This leads us to the second flaw in the paradigm, the emphasis on explanation.

THE NATURE OF GEOGRAPHICAL EXPLANATION

Explanation has been defined as any satisfactory response to a 'how' or 'why' (or, in geography, 'where') question. Difficulties arise immediately as to the meaning of 'satisfactory', which implies something emotional, subjective and personal rather than objective, absolute and scientific. Carried to an extreme, it might suggest some sort of intellectual Darwinism in which geographers are driven by a peculiar form of natural curiosity to seek answers to 'where' questions. However society's willingness to support a particular form of scientific activity is driven not by the strength of the natural curiosity possessed by its practitioners, or their numbers, so much as by political consensus.

The acknowledged scientific principle of parsimony, which requires that the simpler of two theories be chosen when both are empirically valid, would indicate that satisfaction is related to the desire to simplify: that an event is explained when it is shown to be required by some general principle or law. It follows that the search for explanation may be futile in cases where laws are not simple and therefore not satisfying to the seeker.

Unfortunately the potential for this in geography would seem to be high. In both physical and human branches an explanation of the current landscape requires an analysis both of the processes which have acted to shape it and of the initial form on which those processes began to operate. In the physical case those laws are ultimately derived from the elementary laws of physics, but operate in exceedingly complex situations. For example an engineer wishing to predict the behaviour of a fluid flow is unlikely to rely on the Navier Stokes equations, which are simple but virtually unsolvable except in the simplest conditions. Instead he or she will make use of empirical formulae and rules of thumb which provide reasonable predictions but cannot be said to embody an understanding or explanation of the system.

In human geography there is no equivalent to the well-understood laws of physics which underlie most of physical geography. In a field like the study of human spatial behaviour, both the initial conditions and the processes which have operated on them to produce the present landscape may be more complex than the phenomena they seek to explain, making the search for explanation doubly futile.

The term science has a number of connotations, many of which have nothing to do with explanation and theory-building, such as the use of mathematics and statistics, and

concern for objectivity and reproducibility. A number of disciplines are accepted users of the science label but do not have explanation as their dominant paradigm: examples include management science, library science and engineering science. Recently Casetti (1983) and others have argued that the term scientific geography, which implies the coupling of scientific method and approach with geographic subject matter, might be a satisfactory umbrella term for a wide range of useful and respectable activity within the discipline, by including much more than the theory-building paradigm.

This point becomes particularly apparent when one considers the history and successes of nearly thirty years of theory-building in geography. To do so we must first distinguish between theories of the landscape itself, and theories merely of the processes which influence the landscape. For example a theory that under certain conditions streams erode their beds cannot explain the Grand Canyon until it is coupled with a number of other statements about the landscape of the Colorado Plateau; otherwise all stream channels would be expected to be grand canyons.

At geographical scales the landscape clearly lacks parallels to the empirical regularities observed in physics and chemistry: there are no equivalents to the constant boiling point of water or the gas law. So with one or two notable exceptions there are no examples in geography of inductive theory-building from observed regularities. Instead early theory-building was dominated by social physics, which asked deductively whether social laws existed as crude parallels to physical laws. The gravity model of human interaction was hailed both as a remarkable fit to reality, with consequent potential as a predictive planning tool, and as a first law of geography.

As a law, however, the distance decay or gravity model was different in kind from its parallel in physics in being approximate rather than deterministic. An exact or deterministic law can be refuted by finding an empirical instance or more likely a pattern of instances where its prediction is incorrect. But when the law is claimed merely to be approximate, this criterion for validity is clearly inappropriate, since no empirical outcome is absolutely inconsistent with the law. For example if a gravity model predicted 100 trips between city A and city B per day and only 10 were observed, we would almost certainly search for specific factors which might explain this particular circumstance, rather than reject the law.

The standards of empirical verification are clearly very different in a discipline where all laws can be expected to be approximate or probabilistic, and yet some degree of verification is absolutely necessary for any empirical science. It is unfortunate then that methodology in geography is still based on the exact science model, and has not developed formal methods for probabilistic laws. One minimal standard is to require that the proposed law outperform previous, and presumably more naive, laws. In other words it should be possible to reject the naive stochastic process, but not the proposed one, at some agreed level of significance.

What, then, is the appropriate null or naive hypothesis for the gravity or distance decay law? Let us focus on the distance term alone, in other words a negative power or negative exponential dependence of interaction on distance. The answer is clearly not a total lack of dependence on distance. That would lead to the expectation that the interaction with places should be constant with distance, which is absurd. Wilson (1970) was one of the first to show that if one assumes only that the total or average trip distance is known, the most likely pattern is a dependence on distance as the negative exponential. Since it had already been established empirically that the constant of dependence was not universal and that the adherence to the law was not sufficiently exact to support the selection of one function, negative exponential, over the other, we must conclude that the gravity or distance decay law is itself the appropriate null hypothesis, and adherence to the law no more than one would naively expect.

Unfortunately this is not the only example of an empirical law which on closer examination proves to be in effect irrefutable. The Horton law of stream number is on first encounter a remarkable empirical regularity, and it was not until 1966 that Shreve (1966) obtained the important result that channel networks combining with topological randomness would follow the law. The rank-size rule is another apparent regularity, until one looks for an appropriate null or counter hypothesis. Another example is presented by Goodchild et al. (1981).

Two further methodological problems are worth mentioning in connection with stochastic or inexact theory-building and verification. In the point pattern analysis literature which flourished in the late 1960's and early 1970's the conventional criterion for acceptance of a model was non-rejection: the model itself is treated as the null hypothesis in a goodness of fit test, and must not be rejected at a certain level of significance. This approach is very likely to lead to Type II statistical errors, or acceptance of the null hypothesis when it should be rejected, when the amount of data is insufficient. In fact the likelihood of verifying the theory according to this criterion is highest when the least amount of data is gathered. Second, it is common to find more than one stochastic process leading to the same expectations: in the case of the negative binomial distribution Boswell and Patil (1970) describe no less than 13 equally parsimonious but distinct stochastic processes.

In summary, there are several objections to the explanation paradigm in geography, when discussion is confined to explanations of the landscape itself, rather than of the processes which influenced it. There is no reason to assume that explanations if found will be satisfactory, since it is quite possible that they will be more complex than the phenomena they seek to explain. And since they will almost certainly be probabilistic, they will encounter the problem of poorly developed procedures for verification which have led to significant mistakes and misinterpretations in the past.

CONSEQUENCES OF THE EXPLANATION PARADIGM

The previous section focussed on objections in principle to explanation of spatial phenomena as the dominant paradigm of geography. There seem to be two possible responses to this problem: to direct curiosity not at the landscape itself but at the processes which shape it, or to move away from explanation as the ultimate goal. In this section we argue that there has been a tendency in geography in the past twenty years to follow the first option, and examine the apparent effects on the discipline.

The set of processes which influence the landscape is very large: in fact the only possible exclusions are because of scale, or because the domain of operation is extra-terrestrial. But all economic processes are claimed by economics, all social processes by sociology, all chemical processes by chemistry. What then distinguishes a geographer interested in explaining the economic processes which influence the landscape from an economist interested in explaining all economic processes, if virtually all economic processes influence the landscape? Perhaps a unique set of priorities, but that is a weak basis for a discipline.

In the late 1960's economic geography had reached the reluctant conclusion that the classical location theories, particularly Christaller's, had little validity as models of the economic landscape: hexagonal arrangements of towns and villages clearly did not exist even in the flattest parts of the Midwest. Both the physical constraints of uniform density of demand, ubiquitous travel etc. and the behavioural processes assumed by the models were inappropriate to real landscapes. Given correct information about the physical constraints, techniques could be developed to apply valid rules of behaviour and obtain predictions about the locations of activity: the missing elements, it was argued, were the rules describing individual and entrepreneurial behaviour. So behavioural geography was born with the intention of providing the essential components of an improved economic geography.

In practice, behavioural geographers found much useful material in certain parts of psychology. Multidimensional scaling was discovered during the development of techniques for the analysis of shopping behaviour (Rushton, 1969), and adapted and applied to a whole suite of geographical topics (see Golledge and Rushton, eds., 1976; Golledge and Rayner, eds., 1982 for reviews). But twenty years later, although a great deal has been learned about spatial perception and behaviour, it is hard to see solid evidence that this activity has resulted or will ever result in improved models of the economic landscape.

Similar points could be made about a number of other subfields of geography on the borders of major disciplines. Indeed it is hard to imagine any field of human endeavour which could not be combined with geography as the basis for a potential specialty group, in the pursuit of a particular class of processes with influence on the human or physical landscape. Each one could likely be justified in terms of an eventual return to a better spatial theory. But the effect on the discipline as a whole is without doubt one of ever-increasing fragmentation. As geomorphologists delve more and more into physical processes and economic geographers become better and better economists it becomes increasingly difficult to argue for cohabitation.

ALTERNATIVES

We earlier identified three fundamental components of scientific activity: the subject matter, the tools which have been developed for its analysis, and the paradigm which drives the system. Geography has a clearly defined subject matter, and a set of questions which can be asked, but this is not sufficient in itself to ensure the continuation of the discipline. Rather, it must be demonstrated that the set of tools and theories in which geographers are trained give them a superior ability to answer geographical questions, and there are plenty of examples to demonstrate that this is not always the case. In other words the pragmatic and real divisions between disciplines result more from a natural taxonomy of techniques for analysis and problem solving than from natural breaks in subject matter.

No discipline can survive in the current climate of post-secondary education without the support of society as a whole, and this support is clearly based on perceived relevance and social importance. In recent years geographers have made distinct progress towards establishing the relevance of the discipline, whether altruistically or for reasons of self-preservation. Applied geography is now an accepted concept, both as a specialty group with a highly active membership and as a specialized undergraduate stream

on many campuses. Application is often viewed as following rather than leading pure research, but harsh reality suggests the opposite. With a few limited exceptions the vast majority of all research activity has always been funded because of the promise of eventual application: even the most fundamental particle research in physics is no exception. There is no doubt that real, practical problems exist within geography's subject matter. The crucial question is whether others, such as epidemiologists and geologists, are not equally well equipped to solve them: if they are, then the funding available for geography as a discipline will be very limited, as it is at present.

The conclusion seems inescapable: that application, specifically prediction and prescription, should be the dominant paradigm of the discipline, rather than explanation. The tools, methodologies and theories taught would then be the most applicable set, rather than an ever-expanding and unprioritized collection. And we would avoid the problem of infinite fragmentation in pursuit of process. This is not to say that other paradigms are not possible and reasonable, or that application is not the de facto paradigm in many parts of the discipline today. The problem is more a failure to bring current thinking and teaching in the philosophy of geography into line with reality and long-term practical necessity. If geography has a secure future it lies in developing and applying a useful set of tools and theories in the solution of spatial problems, and in training others in their use.

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PATTERN, PROCESS AND A GEOGRAPHIC LANGUAGE

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INTRODUCTION

Goodchild (1983, 12) has written, "Mounting frustration with complexity of spatial distributions has led many geographers to concentrate on processes in both human and physical branches." He expresses doubt as to the ability of geographers to find or create a "theory of the landscape". Presumably this would be a single highly integrated formulation capable of explaining (the word is used advisedly) every distribution, whether human or physical, and the interrelationships among these. Indeed, it is much easier to deal with a single process and hope that someone will later integrate one's findings into a larger structure of knowledge, but the separation of geographic pattern from geographic process is an arbitrary division. Perhaps the division results from limits, not on human abilities to assimilate large volumes of information, but on the ability to simultaneously use large volumes of disparate information. In any case, this separation has increased the fragmentation of academic geography and leads geographers into some relatively unproductive practices.

Perhaps our apparent inability to integrate pattern and process more fully is the result, not of anything inherently intractable about our object of study but only of our technology. Much of scientific discovery has been dependent on technology. The discovery of microbes did not occur until the microscope was available, similarly, the invention of the telescope facilitated a vast number of "breakthroughs" not particularly dependent upon insight. In many cases, discovery is the consequence of two essential components. One is the observational powers of the individual, the other is the individual's being in the right position, technologically and spatially, to observe the phenomenon of interest. The lack of either of these ingredients will halt the scientific process.

We should, then, consider the technology we use, the tools we have at our disposal in the pursuit of theory. More specifically I would like to consider three areas: 1) what we aspire to as geographic theory; 2) the tools we have for representing geographic theory; and 3) a type of representation which might serve geographers well.