The Quantitative Revolution and Theoretical Geography

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In the past decade geography has undergone a radical transformation of spirit and purpose, best described as the "quantitative revolution." The consequences of the revolution have yet to be worked out and are likely to involve the "mathematization" of much of our discipline, with an attendant emphasis on the construction and testing of theoretical models. Although the future changes will far outrun the initial expectations of the revolutionaries, the revolution itself is now over. It has come largely as the result of the impact of work by non-geographers upon geography, a process shared by many other disciplines where an established order has been overthrown by a rapid conversion to a mathematical approach.

Geographers may look with the wisdom of hindsight on a recent statement by Douglas C. North who points out that in the field of economic history "a revolution is taking place. . . . It is being initiated by a new generation of economic historians who are both skeptical of traditional interpretations of U.S. economic history, and convinced that a new economic history must be firmly grounded in sound statistical data."2 North's paper has a familiar ring in geographical ears, but is not primarily concerned with where the revolution is likely to lead. If the example of other social sciences is any criterion, it will lead to a more mathematical, not solely statistical, economic history.

The movement which led to the revolution in geography was begun by physicists and mathematicians, and has expanded to transform first the physical and then the biological sciences. It is now strongly represented in most of the social sciences including economics, psychology, and sociology. The movement is not yet strongly represented in anthropology or political science, and has scarcely been felt in history, although early rumblings may perhaps be heard from a new journal devoted to history and theory.3

This paper presents a discussion of the general characteristics of the quantitative movement; describes in somewhat greater detail the coming of the quantitative revolution to geography; and attempts an assessment of the value of quantitative techniques in the development of theory. Some scholars have chosen to regard the revolution in terms of a qualitative-quantitative dichotomy. It does not help to cast the debate in this form. For "what is philosophically distinctive about contemporary science is its disinterest in dubious dichotomies or disabling dilemmas,"4 which fascinate and ensnare the mind because they give the illusion of coming close to the essential nature of things. O. H. K. Spate, in his paper on "Quantity and Quality in Geography," goes so far as to cry "down with dichotomies,"5 but fails to heed his own advice and apply it to the title of his paper. Furthermore, to specify the presence or absence of an attribute or quality is merely to begin the process of measurement at its lowest level on a nominal scale. Viewed in this manner, observations of qualitative differences are but the prelude to measurements of a higher order on ordinal, interval, or ratio scales.

The quantity-quality dichotomy has also been allowed to embrace and perhaps conceal a number of related but distinct questions. These include measurement by instruments versus direct sense-data; rational analysis versus intuitive perception; cold and barren scientific constructs versus the rich variety of daily sense-experience; continuously varying phenomena versus discrete cases, nomothetic versus ideographic, and the like.

The desire to avoid this confusion reinforces my inclination to side-step the quality-quantity issue, and to view the movement toward quantification as a part

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of the general spread and growth of scientific analysis into a world formerly dominated by a concern with the exceptional and unique.

QUANTIFICATION AS INDETERMINISM

Geography has long been a "following" rather than a "leading" discipline. The main currents of thought have had their origins in other fields. The mechanistic approach of much nineteenth-century science was represented to some extent among the environmental determinists from Ratzel (if he was a determinist) to Semple, Huntington, and Griffith Taylor. They were preoccupied by the notion of cause and effect, and were constantly seeking "laws." A similar mechanistic flavour is present in much of the recent work by the "quantifiers." It is as if geography is re-emerging after the lapse into ideography which followed the retreat from environmental determinism. The quantitative revolution is taking us back much closer to environmental determinism. It is surely not coincidental that the quantitative revolution is contemporaneous with the appearance of neo-determinism in geography.

It seems clear that a strong reaction to environmental determinism has served to delay the coming of the quantitative movement to geography, and has postponed the establishment of a scientific basis for our discipline that the quantifiers hope to provide (and which the determinists were seeking, although for the most part did not find).

It is not so surprising, therefore, that the quantitative revolution was resisted most strongly by American geographers, for it was in the United States that the reaction to environmental determinism was strongest. Characteristically, the source of strongest opposition is now the source of greatest support, and the United States has achieved a very favourable balance of trade in the commodity of quantitative techniques.

Although quantification in geography has been mechanistic, new techniques being used and others being developed are in line with the contemporary trend in science in that they are probabilistic. The probabilistic approach as exemplified in Curry's work on climatic change, and Hagerstrand's simulation of diffusion offers a most promising vista for future research. As Bronowski notes, statistics "is the method to which modern science is moving. . . . This is the revolutionary thought in modern science. It replaces the concept of inevitable effect by that of probable trend." It is more accurate, therefore, to refer to some of the later examples of quantification in geography as indeterministic. With Jerzy Neyman, "One may hazard the assertion that every serious contemporary study is a study of the chance mechanism behind some phenomena. The statistical and probabilistic tool in such studies is the theory of stochastic processes, now involving many unsolved problems."10

Of great significance in the development of laws in the social sciences is the scale of analysis. As Emrys Jones explains, "The lack of stringency lies in the finite numbers dealt with in the social sciences as opposed to the infinite numbers dealt with in the physical sciences. At this latter extreme, statistical regularity is such that it suggests extreme stringency or absolute validity; while at the other end statistical variations and exceptions are much higher, and deviations themselves warrant study."11

THE END OF A REVOLUTION

Although its antecedents can be traced far back, the quantitative revolution in geography began in the late 1940's or early 1950's; it reached its culmination in the period from 1957 to 1960, and is now over. Ackerman remarks that, "Although the simpler forms of statistical aids have characterized geographic distribution analysis in the past, the discipline is commencing to turn to more complex statistical methods—an entirely logical development. The use of explanatory models and regression, correlation, variance and covariance analysis may be expected to be increasingly more frequent in the field. In the need for and value of these methods geography does not differ from other social sciences."12

Similarly, Hartshorne says that, "to raise . . . thinking to the level of scientific knowing, it is necessary to establish generic concepts that can be applied with the
maximum degree of objectivity and accuracy and to determine correlations of phenomena with the maximum degree of certainty. Both purposes can best be accomplished if the phenomena can be fully and correctly described by quantitative measurements and these can be subjected to statistical comparisons through the logic of mathematics.  

Spate, although somewhat sceptical about quantitative methods, concedes that "increasingly young geographers will feel that they are not properly equipped without some statistical nous," and adds parenthetically that he is relieved not to be a young geographer.

*An intellectual revolution is over* when accepted ideas have been overthrown or have been modified to include new ideas. *An intellectual revolution is over* when the revolutionary ideas themselves become a part of the conventional wisdom. When Ackerman, Hartshorne, and Spate are in substantial agreement about something, then we are talking about the conventional wisdom. Hence, my belief that the quantitative revolution is over and has been for some time. Further evidence may be found in the rate at which schools of geography in North America are adding courses in quantitative methods to their requirements for graduate degrees.

Many would concur with Mackay's comment that "the marginal return on arguing for the need of quantitative methods is now virtually nil." This does not deny that many ramifications of the revolution remain to be worked out. Nor does it mean that the ramifications will be painless. It is not easy to agree with Spate's argument that the need for statistical nous applies only to young geographers. Is the field to progress only as rapidly as the turnover in generations? The impact of cybernetics is already creating unemployment at the white collar level. Its impact on the managerial and professional strata is likely to mean more work, not less. It is no flight of fancy to foresee the day when geographers, if they are to remain abreast of developments, must re-learn their craft anew every decade. Nor is it difficult to see that the present generation of quantifiers may rapidly be replaced by younger men more thoroughly versed in mathematics.

Although the quantitative revolution is over, it is instructive to examine its course because to do so tells us something about the sociology of our profession, and because it provides a background for the question, "quantification for what?" considered below.

**THE COURSE OF THE QUANTITATIVE REVOLUTION IN GEOGRAPHY**

Although the origins of the revolution lie in the fields of mathematics and physics, the direct invasion came from closer to home. A list of the more important antecedents, having a direct or indirect impact on geography, would include Von Neumann (a mathematician) and Morgenstern (an economist) for their *Theory of Games and Economic Behavior,* first published in 1944; Norbert Wiener, whose 1948 volume on cybernetics emphasized the necessity of crossing academic boundaries; and Zipf, who published *Human Behavior and the Principle of Least Effort* in 1949.

Geographers began to look for quantitative techniques that could be applied to their problems, and some non-geographers began to bring new methods to bear on old geographic questions. One example is physicist J. Q. Stewart's paper, "Empirical Mathematical Rules Concerning the Distribution and Equilibrium of Population," published in the *Geographical Review* as early as 1947.

Stewart has been a leader in the development of social physics, and the declaration of interdependence signed by a group of physical and social scientists at the Princeton conference in 1949 is a landmark in the growth of the application of mathematics to the social sciences. That economists were engaging in methodological debate at this time, in a way that geographers were to do five years later, is evidenced by the Vining and Koopmans controversy in the *Review of Economics and Statistics* for 1949.

The impact of quantification began to be felt in geography almost immediately. It was initiated by a number of statements calling for quantification. Such calls had been issued earlier. For example, in 1936 John Kerr Rose, in his paper on corn yields and climate, argued that "The
methods of correlation analysis would seem especially promising tools for geographical investigation.22 This call went largely unheeded. Similar statements in 1950, however, were followed up. An outstanding early plea was made by Strahler in his attack on the Davisian explanatory-descriptive system of geomorphology,23 and his endorsement of G. K. Gilbert's dynamic-quantitative system.24

QUANTITATIVE GEOMORPHOLOGY AND CLIMATOLOGY

If Gilbert's 1914 paper was as sound as Strahler seems to think, why was it not adopted as a signpost to future work in geomorphology, instead of being largely forgotten and ignored for thirty years? The answer may be, as Strahler himself seems to imply, that geomorphology was a part of geography. Hydrologists and geologists did not direct their major interest towards such matters, or when they did they followed Davis. The followers included Douglas Johnson, C. A. Cotton, N. M. Fenneman, and A. K. Lobeck. Strahler held that they made "splendid contributions to descriptive and regional geomorphology," and "have provided a sound base for studies in human geography,"25 but they did not greatly advance the scientific study of geomorphological process. This is not to say that there was no quantitative work in geomorphology prior to Strahler.26

One immediate response to Strahler's attack on Davis came from Quam, who wondered whether mathematical formulae and statistical analysis might not give a false impression of objectivity and accuracy.27 A more violent response, however, came from S. W. Wooldridge, who notes that:

There has been a recent attempt in certain quarters to devise a "new" quasi-mathematical geomorphology. At its worst this is hardly more than a ponderous sort of cant. The processes and results of rock sculpture are not usefully amenable to treatment by mathematics at higher certificate level. If any "best" is to result from the movement, we have yet to see it; it will be time enough to incorporate it in the subject when it has discovered or expressed something which cannot be expressed in plain English. For ourselves we continue to regard W. M. Davis as the founder of our craft and regret the murmurings of disapproval heard occasionally from his native land.28

Lester King is inclined to support Strahler.

Statistical analysis is essentially the method of the bulk sample, and is admirable for the study of complex phenomena and processes into which enter a large number of variables. As yet few geomorphic topics provide data suited directly to statistical treatment, and methods may have to be adapted to the new field of enquiry, so that too facile results should not be expected. The net result must be, however, a greater precision in geomorphic thinking.29

Several geomorphologists, including Chorley,30 Dury,31 Mackay,32 Wolman,33 and others, in addition to Strahler, are using quantitative methods, and the practice seems likely to spread.

There has been little argument about the application of quantitative techniques to climatology. This branch of our subject embraces the most apparently manageable and quantifiable continuum that geographers have been concerned to study. Thornthwaite and Mather,34 Hare,35 Bryson,36 and others have been applying quantitative techniques to climatic problems for some time, and with great effect. The quality of their work has virtually silenced the potential critics.

QUANTIFICATION IN HUMAN AND ECONOMIC GEOGRAPHY

By far the greatest struggle for the acceptance of quantitative methods has been in human and economic geography. This is not surprising in view of the possibilist tradition.37 It is here that the revolution runs up against notions of free-will and the unpredictability of human behaviour. Here the comparison with physical science is helpful. Physicists working on a microcosmic level encounter the same kinds of problems with quanta and energy that social scientists do with people. The recognition of such parallels is cause for rejoicing, not for despair. To be accepted and accorded an honoured place in our society, social science needs to acquire demonstrable value as a predictive science.
QUANTITATIVE REVOLUTION AND THEORETICAL GEOGRAPHY

without a corresponding need to control, restrict, or regiment the individual. A social science which recognizes random behaviour at the microcosmic level and predictable order at the macrocosmic level is a logical outgrowth of the quantitative revolution.

The catalogue of claim and counter-claim, charge and counter-charge that appeared in the literature in the 1950's is a long one. It includes Garrison's38 comment on Nelson's39 service classification of American cities; the Reynolds40-Garrison41 exchange of 1956 on the (then) little use of statistical methods in geography; the Spate-Berry editorial exchange in *Economic Geography* in which the former reminds us that "Statistics are at best but half of life. The other half is understanding and imaginative interpretation."42 and the latter defends the quantifiers for their clear distinction between facts, theories, and methods, and in turn accuses his critics of creating a quantitative bogeyman and tilting at windmills.43 Dacey's44 criticism of Burghardt's45 conclusions on the spacing of river towns, and Porter's defence with the fable of "Earnest and the Orophagians";46 the Zobler-Mackay47 exchange on the use of chi-square in regional geography; Arthur Robinson's classification of geographers into "Perks and Pokes;"49 the debate between Luckermann50 and Berry51 on a "geographic" economic geography, and so on.

By 1956, the quantifiers were arguing with each other through the medium of the professional journals as well as with their opponents. In so doing, they occupied an increasing amount of attention and space. In 1956 also the Regional Science Association was established and gave further impetus to quantification in geography.

The erstwhile revolutionaries are now part of the geographic "establishment," and their work is an accepted and highly valued part of the field.

THE OPPOSITION TO QUANTIFICATION

The opposition to the quantitative revolution can be grouped into five broad classes. There were those who thought that the whole idea was a bad one and that quantification would mislead geography in a wrong and fruitless direction. If such critics are still among us they have not made themselves heard for some time. There were those like Stamp who argued that geographers had spent too long perfecting their tools (maps, cartograms, and other diagrammatic representations) and should get on with some real building. Stamp was "a little alarmed by the view that the geographer must add to his training a considerable knowledge of statistics and statistical method, of theoretical economics and of modern sociology. Sufficient perhaps to appreciate what his colleagues are doing so that team work may be based on mutual appreciation seems to me the right attitude."52 This seems to be another dubious dichotomy. The notion that geographers either improve their tools or engage in research with available tools seems false. Surely advances in technology are most likely to occur at the moment when we are grappling with our toughest problems. Furthermore, to argue that geographers should not use statistical methods comes close to defining geography in terms of one research tool—namely the map. One weakness of this position has been well demonstrated by McCarty and Salisbury who have shown that visual comparison of isopleth maps is not an adequate means of determining correlations between spatially distributed phenomena.58

A third kind of opposition holds that statistical techniques are suitable for some kinds of geography, but not all geography, because there are certain things that cannot be measured. This may be true for some variables. However, even with qualitative characteristics, nominal observations can be made and there is an expanding body of literature on the analysis of qualitative data.54 A variant of this argument is that the variables with which geography is concerned are too numerous and complex for statistical analysis. Quantifiers claim that it is precisely because of the number and complexity of the variables that statistical techniques are being employed.

Another class of objections is that although quantitative techniques are suitable and their application to geographic problems is desirable, they are nevertheless being incorrectly applied: ends are con-
fused with means; quantitative analysis has failed on occasion to distinguish the significant from the trivial; the alleged discoveries of the quantifiers are not very novel; and so on. That these criticisms have a grain of truth cannot be denied, but to the valid, correct use of quantitative methods (and this is surely what we are concerned with) they are merely irrelevant. Incorrect applications have been and no doubt will continue to be made, and in some cases for the wrong reason such as fashion, fad, or snobbery. More often, however, they are genuine and honest attempts to gain new knowledge and new understandings.

A final kind of criticism to note is in the ad hominem that quantification is all right but quantifiers are not. They are perky, suffer from over-enthusiasm, vaulting ambition, or just plain arrogance. To this charge also perhaps a plea of guilty with extenuating circumstances (and a request for leniency) is the most appropriate response. When you are involved in a revolution, it is difficult not to be a little cocky.

THE CONSEQUENCES OF THE REVOLUTION

The revolution is over, in that once-revolutionary ideas are now conventional. Clearly this is only the beginning. There is a purpose other than the establishment of a new order. If the revolution had been inspired by belief in quantification for its own sake, or by fad and fashion, then it would have rapidly run its course and quickly died. But the revolution had a different purpose. It was inspired by a genuine need to make geography more scientific, and by a concern to develop a body of theory. Dissatisfaction with ideographic geography lies at the root of the quantitative revolution. The development of theoretical, model-building geography is likely to be the major consequence of the quantitative revolution.

Description, or as some have said, "mere description," may be an art or at least call for the exercise of certain talents best described as artistic. Nevertheless, description is an essential part of the scientific method. In examining the real world, our first task is to describe what we see, and to classify our observations into meaningful groups for the sake of convenience in handling. The moment that a geographer begins to describe an area, however, he becomes selective (for it is not possible to describe everything), and in the very act of selection demonstrates a conscious or unconscious theory or hypothesis concerning what is significant.

In his examination of significance in geography, Hartshorne rejects the notion that significance should be judged in terms of appearance, that is, as in objects in a landscape, and establishes as an alternative the criterion that observations should express "the variable character from place to place of the earth as the world of man." In many geographic pursuits, man is the measure of significance, and spatial variations the focus. But how else can significance to man be measured except in terms of some theory of inter-relationships?

In this connection there is reason to question Strahler's assertion, quoted above, that the Davison geomorphologists "provided a sound basis for studies in human geography." The genetic and morphological landform classifications they produced may have provided a sound basis for most studies in human geography prior to 1950, but they are not truly anthropocentric. No attempts to assess significance to man were made until the work was substantially completed. This can be contrasted with Sheaffer's recent stream classification, based on flood-to-peak interval, a variable known to be of significance for human adjustment.

The observation and description of regularities, such as these in the spatial arrangement of cultural features, human activities, or physical variables, are first steps in the development of theory. Theory provides the sieve through which myriads of facts are sorted, and without it the facts remain a meaningless jumble. Theory provides the measure against which exceptional and unusual events can be recognized. In a world without theory there are no exceptions; everything is unique. This is why theory is so important. As Braithwaite puts it, "The function of a science is to establish general laws covering the behavior of empirical events as objects with which the
science in question is concerned . . . to enable us to correct together our knowledge of the separately known events, and to make reliable predictions of events yet unknown.\textsuperscript{58}

The need to develop theory precedes the quantitative revolution, but quantification adds point to the need, and offers a technique whereby theory may be developed and improved. It is not certain that the early quantifiers were consciously motivated to develop theory, but it is now clear to geographers that quantification is inextricably intertwined with theory. The core of scientific method is the organization of facts into theories, and the testing and refinement of theory by its application to the prediction of unknown facts. Prediction is not only a valuable by-product of theory building, it is also a test by which the validity of theory can be demonstrated. Scientific inquiry may or may not be motivated by the desire to make more accurate predictions. Whatever the motivation, the ability to predict correctly is a sound test of the depth of our understanding.

Given the need to comply with the rigorous dictates of the scientific method, the need to develop theory, and to test theory with prediction, then mathematics is the best tool available to us for the purpose. Other tools—language, maps, symbolic logic—are also useful and in some instances quite adequate. But none so well fulfills our requirements as mathematics.

The quantification of theory, the use of mathematics to express relationships, can be supported on two main grounds. First, it is more rigorous. Second and more important, it is a considerable aid in the avoidance of self-deception.

These points may be illustrated by reference to a paper by Robinson, Lindberg, and Brinkman on rural farm population densities in the Great Plains.\textsuperscript{60} The authors point out that the statistical-cartographic techniques which they use may be properly employed after the establishment of "tentative descriptive hypotheses regarding the variability that may exist among the distributions of an area, inferred through the study of individual maps and other sorts of data. Coefficients of correlation and related indices provide general quantitative statements of the degree to which each hypothesis is valid."\textsuperscript{60}

My submission is that the testing of hypotheses does not make much sense unless these hypotheses are related to a developing body of theory. High correlation does not necessarily confirm a hypothesis, and it is well known that nonsense correlations are possible. The authors propose rural farm population density as a dependent variable and proceed to examine spatial variations using average annual precipitation, distance from urban centres, and percentage of cropland in the total land area as explanatory variables. Having calculated correlation coefficients, the authors conclude that the general hypothesis concerning the association of spatial variations of these variables is confirmed.\textsuperscript{61} This use of quantitative techniques demonstrates rigour to the extent that precise measurements of association are made. It also demonstrates the need and possibility of avoiding self-deception.

Nowhere in the paper is it possible to find an explicit statement of theory. Nowhere are we told why rural farm population density is highly correlated with average annual precipitation. Perhaps the explanation lies in the fact that as precipitation decreases, larger farm units are required to support a farm family, owing to lower yields of the same crops, or the cultivation of less remunerative crops. This is a theory, and a test of it would be to examine rural farm population density and farm size. It is conceivable that these two variables are not closely correlated. If this is the case, the theory will need revision. It is surely not much of an explanation, however, to correlate rural farm population density with precipitation. If there is a causal relationship here, it is an indirect one and several links have been omitted.

A more logical treatment would relate farm population to farm size, farm size to yields and land use, yields and land use to precipitation; but it is by no means certain that the causal chain of relationships could be carried so far. The correlations which John K. Rose\textsuperscript{62} obtained between corn yields and July precipitation are not as high as Robinson, Lindberg, and Brinkman obtained for average annual precipitation and rural farm population. Ad-
minitely, the two studies were concerned with different measurements, in different areas, at a different point in time. Nevertheless, it is significant that the Robinson group was able to show higher correlation between remotely connected variables than Rose could show between much more closely connected variables.

Robinson's study is deficient because it is not related to an explicit statement of theory. Quantitative analysis of variables cannot be justified for its own sake. The mere restatement of accepted ideas in numerical form instead of in "plain English" is not what the quantitative revolution is about. Examination of spatial variables of rural farm population of the Great Plains in terms of an explicit theory would have led Robinson et al. to select other, or at least additional, variables than those considered. Some might argue that the hypothesis relating rural farm population and average annual precipitation is a theory. If so, it sounds dangerously like the old deterministic hypotheses and has the same quality of inferring a causal relationship without any explanation or testing of a connecting process leading from cause to effect.

CONCLUSION

Quantitative techniques are a most appropriate method for the development of theory in geography. The quantitative era will last as long as its methods can be shown to be aiding in the development of theory, and there can be no end to the need for more and better theory. It follows that any branch of geography claiming to be scientific has need for the development of theory, and any branch of geography that has need for theory has need for quantitative techniques.

Not all statements of theory need to be expressed quantitatively in their initial form. Firey, for example, has developed a general theory of resource use but without resort to hypothesis testing in a formal sense. Such statements of theory are extremely valuable, and many more of them are needed in geography. Once formulated they should not long remain untested, but the testing need not be undertaken by the same person, or even by persons in the same discipline.

The development and testing of theory is the only way to obtain new and verifiable knowledge and new and verifiable understandings. As Curry points out, "Methods of representing various phenomena of nature and speculation about their inter-relationships are closely tied together. It is too often forgotten that geographical studies are not descriptions of the real world, but rather perceptions passed through the double filter of the author's mind and his available tools of argument and representation. We cannot know reality, we can have only an abstract picture of aspects of it. All our descriptions of relations or processes are theories or, when formalized, better called models."

Curry relates model building to another element in recent geographical work—the problem of perception which may soon come to merit a place alongside the quantitative revolution in terms of significant new viewpoints.

Our literature is replete with ideographic studies. There is a strong urge to get something into the literature because it has not been described before. If these ideographic studies and new descriptions are to have lasting value, their theoretical implications must be shown. In an increasing number of cases, the relationship to theory can best be shown in quantitative terms. In some instances a simple description of an exceptional case may serve to highlight defects in theory. The theory can then be revised or modified to take account of another kind of variation not previously noted, or the theory may have to be abandoned. Theories are not usually abandoned, however, because a few uncomfortable facts do not happen to fit. Theories are abandoned when newer and better theories are produced to take their place. Although observation and description of exceptional cases may be achieved without quantification, the eventual incorporation of modifications into a theory will normally require the rigour of statistical techniques to demonstrate their validity.

There is not a very large literature in theoretical geography. Our discipline has remained predominantly ideographic.
small proportion of the large volume of central place literature can be described as theoretical. It is appropriate to speak of central place theory as one relatively well-developed branch of theoretical economic geography. A recent volume by Scheidegger has emphasized the theoretical aspects of geomorphology. Wolman comments that "the emphasis on principles that Scheidegger stresses directs attention to inter-relationships and hopefully lessens the tendency to observe, measure, and record everything because it's there." This remark can be applied with equal value to the development of theory in other branches of geography.

Geographers are now making a conscious effort to develop more theory. A recent volume on theoretical geography attempts to develop theory basic to some areas of the subject. In particular, the author presents a measurement of shape and discusses a general theory of movement and central place theory. This volume will help to focus the attention of geographers on the need for theory. Perhaps a rash of attempts to develop geographic theory will begin. Such a development seems unlikely, however. For while the use of quantitative methods is a technique that can be learned by most, few seem to have that gift of insight which leads to new theory. North comments that a difficult problem is "the development of the theoretical hypotheses necessary for shaping the direction of quantitative research." Attempts to develop theory in geography need not mean a wholesale shift in emphasis. Many an ideographic study could be of greater value if it contained but two paragraphs showing the theoretical implications of the work. This is often easier or at least possible for the author, while it is more difficult or even impossible for others who try to use the work at a subsequent time to develop or test theory. Of course, if case studies are designed with a theory in mind, it is likely that they will differ considerably from studies unrelated to a conscious statement of theory.

Theoretical geography does not mean the development of an entirely new body of theory exclusive to geography. Scheidegger has not attempted to develop new laws of physics, but has merely refined and adapted these laws to the study of geomorphological phenomena and processes. Central place theory is in keeping with some schools of economic theory. One role of an economic geographer is to refine and adapt available economic theory. In doing so he will improve the theory he borrows. If the Anglo-Saxon bias in economics has been to ignore the spatial aspects of economic activity, the geographer is one of those to whom we should look for the remedy. It need not be thought that the growth of regional science completely fills the gap. Those geographers who study drainage networks, highway networks, power distribution systems, flood problems, airline routes, social organization, and the venation of leaves all have in common a concern for a "flow" between "points" over a network of links arranged in a particular pattern. Graph theory is a branch of mathematics concerned with networks and may be adapted to fit all manner of collection, distribution, and communications systems. It is conceivable that a body of useful theory could be built up around the application of graph theory to geographical problems. This is an example of what is meant by theoretical geography. It is a direction that an increasing number of geographers are likely to follow. Let us hope that the effort will meet success.

ACKNOWLEDGMENTS

During the preparation and revision of this paper, I have benefited from discussions with Brian J. L. Berry, J. W. Birch, W. C. Calef, Michael Church, John Fraser Hart, Robert W. Kates, Leslie King, and Jacob Spelt.

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61. Ibid., p. 215.
62. Rose: Corn yield and climate, pp. 95–97, figs. 7 and 8.
64. Curry: Climatic change, p. 21.
65. One recent publication in this newly developing field of geography is Kates, Robert W.: *Hazard and Choice Perception in Flood Plain Management*. Univ. of Chicago, Dept. of Geog., Research Paper no. 78, 1962. See also papers by Ian Burton and Robert Kates, and by Robert Lucas and Dean Quinney in a forthcoming issue of the *Natural Resources Journal*.

**RÉSUMÉ**

Au cours des dix dernières années, un phénomène qu'on pourrait appeler "révolution quantitative" a transformé radicalement l'esprit et l'objet de la géographie. Initiée par des physiciens et des mathématiciens, cette révolution n'a pas changé que la géographie, mais les autres sciences également.

L'auteur décrit l'influence de cette révolution quantitative sur la géographie et ses rapports avec le déterminisme. Il est d'avis que ces transformations résultent d'un besoin d'élaborer des théories scientifiques. Les recherches géographiques feront de plus en plus usage des méthodes mathématiques et on accordera une plus large place à la construction et à l'application expérimentale de modèles théoriques. Ceci n'obvierra pas au besoin d'études idéographiques, mais ces dernières pourront être étayées par des énoncés théoriques.