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The cover illustration is from a stained glass window in the Director's office, Department of Geography, Clark University.
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Since the last issue of Monadnock appeared, some changes have taken place at the Graduate School of Geography. While maintaining the research and teaching focus of our five streams of study, we are expanding our cartography/remote sensing and geographic information system curriculum. We have also begun to upgrade our computing facilities for faculty and graduate students, while the University has created a "Rainbow Room" (Digital Equipment Corp. personal computers) for student use. The graduate program now begins with a "core" cluster and concludes with the public presentation of the dissertation defense. Also, we have added new faculty members in the areas of urban and physical geography and resource management.

The research component of the School remains diverse and strong. The faculty and students continue to obtain major research awards, including MacArthur and Guggenheim Fellowships, National Science Foundation and National Academy of Science grants, and major contract awards with the U.S. Agency for International Development. Dissertation research is going on in such diverse areas of the world as Japan, Bangladesh, Sudan, Zimbabwe, and Kenya.

Several faculty members have received major appointments. Our past Director, Len Berry, is now Provost of the University, William Koelsch has been appointed University Historian, and Susan Hanson has been elected as sole editor of the Annals of the Association of American Geographers. In addition, Martyn Bowden has received a University award for outstanding teaching.

In summary, the School remains an active, fluid, and hectic place. We seek to retain our uniqueness among geography graduate programs, while reaffirming our commitment to the discipline.

Finally, we say goodbye to Kirsten Johnson, who is ending her five-year position in resource management. Kirsten symbolizes the community of the program. She arrived some fifteen years ago as a student and leaves as a professor. During her tenure she conducted research in Mexico, Nepal, and Zimbabwe, taught a large number of students, and acted as tutor, counselor, and friend to many of us. We wish her and her son, Nico, happiness and success.

B.L. Turner II
Director
The Atwood Lecture Symposium:
Geography and Praxis

MICHAEL EBBAR: MODERATOR

The editors of Monadnock would like to see Monadnock take a more active role in helping to foster intra-departmental dialogue. We thought this a particularly appropriate objective in relation to Julian Wolpert's Atwood Lecture. In his lecture he touched upon a large number of issues and a wide range of problems in the field of geography. Some of these were of a larger, macro-social scale, but others have direct relevance to what we see happening in our department. We have noticed over the past several years that one of the unintended consequences of the stream approach at Clark has been an unfortunate curtailment of dialogue between members of the various streams. Graduate students in particular tend not to have much dialogue, either intellectual or social, with people involved in other kinds of work. This tends to breed a kind of intellectual isolation that is irksome to what graduate school should be about. We assume too often that a person's inclinations to do cost benefit studies or input-output kinds of work determine a priori that he or she has no sensitivity to landscape aesthetics, or that if somebody is working in physical geography he/she is not interested in Neo-Marxist debates on underdevelopment. Those kinds of artificial barriers do little to foster dialogue among different people.

Wolpert's talk on practical contributions that geographers can make towards solving social problems addresses some of these issues. He seemed to say that we have a lot of answers to a lot of social problems and that we should not hide our heads in shame. This clearly raises a number of questions; the most important of these is: Do social scientists have answers to problems? To address that question we have assembled a panel of distinguished faculty. The panel was originally to consist of four people: Dick Peet to address issues from his Marxist perspective, Chris McGee to address some of the issues from a humanistic perspective; Bill Turner to talk about the methodology of social science and how that relates to the topic, and Bob Kates to give us his insights, partially based on his experience as an active member of the National Academy of Science, and the international social scientific community. Unfortun-

ately Bob could not be here, but for the happiest of reasons — while we are speaking he is participating in his grandchild's birth. This panel will therefore be composed of Bill, Chris and Dick. Each will talk for ten to fifteen minutes, after which we will open the discussion for a general dialogue.

DICK PEET:

I shall keep my remarks fairly simple, blunt and straightforward. Firstly, we are geographers here. The function of geography is the study of two of the sets of relations that bind a society together. They just happen to be absolutely fundamental relations. They just happen to be also the source of some of society's most significant problems, including the problem of the onset of depression that we now enter into. Those two relations which geography looks at are the relation with nature, the source of our material existence, and the relation across space, the distribution of people and activity in space. Now these two relations should not, from a Marxist dimension, be seen as just abstract relations. They should very definitely be seen as a relation between society of a certain nature and space. And they should be seen as a society of a certain kind producing its space. That is, different kinds of society, different forms of society. Society is motivated by different urges that will produce space of a very different one from the other. So for us, as Marxists, I think the position has to be that we see these two relations related to the total set of relations that bind the society together, that make up the structure of human existence.

Now, we need to see these two relations as part of this structure of relations; two relations that are bound in terms of totality. Everything is related to everything else. I think that we attempt to put these relations in some kind of structured order. We claim that some of these relations are more profound than others. Some of them are more basic than others. And for us, the social relations of production are the determinant relations — the things that structure all other relations. By the social relations of production I mean basically the class relations of the society, who owns the means by which we produce our existence. For us it is the most profound question you can ask of any society. The relations between society and nature and the spatial geography of that society are determined by who owns the society. So in other words, we would want to relate those two profound questions to the questions of the social relations of production.

These two particular relations happen to be in a state of profound crisis right now; happen to be leading to a host of social problems which we would argue are produced by capitalist social relations of production. The relation with nature is in a state of increasing crisis and obviously has been for the last fifteen years or so. We would argue that the contradictory relation between capitalist profit making and Nature is the source, the essential source, of environmental problems. And space is in contradiction.
That is, what we have going on right now is a massive movement of locations of economic activity in space from centers where the social relations of production are in contradiction and the class struggle has emerged and blossomed and matured leading to unionization. Workers are merely getting back a portion of what they produce, leading to high wages and the high cost of production, in turn leading competitive capitalists to move the location of production in space, ultimately leading to a massive depression in the old centers of capitalist production. We would argue too, that that is one of the essential causes of the present depression. Economic activity is being moved from places where workers earn a decent wage which they then spend and keep the economy going, to areas where workers are underpaid and simply cannot afford to buy the commodities produced by capitalism. In other words, the spatial arrangement of capitalism, especially the changes of the spatial arrangement of capitalism, are a major cause of this economic depression. In an attempt to escape from the contradiction with labor, it produces the economic contradiction - the depression into which the world economy is being plunged.

There is a lot to do. There is a great need for these good ideas that Julian claimed we have in such profusion. However, I would argue that the question in this colloquium was not posed correctly. I don't think we are short of good ideas. We are not short of good ideas but let us just ask what is a good idea? Good for whom? Is there just such a thing as a morally good idea - good for everyone in a class-divided society, in a society where the interest of one class is in opposition to the other? And can we talk about our society? Can we talk about our environment? Can we really maintain that there are good ideas for our society and our environment, when it isn't our society nor is it our environment? Good for whom? And to whom are these ideas supposed to be given? To the Capitalist State? Is the Capitalist state some kind of neutral arbitor that settles the problem of Capitalism in the interest of all? Or is it exactly what I said it is? The Capitalist state is the arm of the owners of the means of production and does it not act in their interest?

Is there such a thing as a good idea in this general sense of the word? I would argue there isn't. There are only good ideas in a kind of society, for a fragment of the society and we should be aiming our good ideas at the masses of the people, that is, at the working class and the peasantry of the third world. So let me conclude: the shortage is not in good ideas, but in a society that will use good ideas. What we should be doing is helping to form such a society. If you trace geographic problems to their origin and you find their origin deep in the structure of the society then you are forced, in your role as an academic, to solve those problems at their source. That means a transformation of society. It forces those academics who traced the origins of the problems to societies' structures into changing that structure. It makes the academic participate in the process of revolutionary change.

MODERATOR:

Thank you Dick. The next speaker will be Bill Turner.

BILL TURNER:

What I have to say will be brief. Firstly, I think it is pre-sumptuous for social scientists or geographers, to assume that they have solutions to practical problems. At least, we may have temporary solutions, guess solutions or attempts at solutions. To say that somehow we do have solutions assumes that somehow we have perfect knowledge, and of course I don't think anyone would agree that we have perfect knowledge. But given that, let me go on to make a few statements about solutions, knowing full well that I mean temporary solutions, fragmented solutions.

There is, as yet, nothing unique about geography or about what geographers do that leads us to the solutions of problems. However, that is not to say that the geographers can not contribute towards creating temporary solutions. How do we go about doing that? I suppose I would argue that what we do most frequently is the easiest, and that is to play critic. It is easy to say what is not the solution because we can base that on past experience and draw upon that experience and say that did not work at that place, at that time. That places the geographer or the social scientist in the role of the critic.

The second, much more difficult way, of course, is to attempt to create or express new, or reemphasized, or rearranged temporary solutions. If we are going to do this, we must keep increasing our knowledge and understanding of why events occur. In this case we are looking at social events. That leads us to the realm of explanation, the realm of theory building. What I have seen in geography during the fifties and the sixties and the seventies, is not the creation of new theory (with some exceptions) but by and large geographers telling ourselves how poor our original theory or theories were, and how complex the real world is. This has caused befuddlement, and, in many respects, the abandonment of theory building. Now I state that as a generalization, and I can find some glaring exceptions. I would say that for geographers or social scientists to truly make an impact on temporary solutions in the future, there must be a coalescence of those who will attempt to build theory, will attempt to explain, as opposed to the practitioners of the explanation.

I see our department now as, in many ways, a fragmented example of that. We have practitioners and we have people attempting to construct some types of explanations which the practitioners may use. I don't always see, as I think that Michael was suggesting, discussion and give and take between those two groups. Which leads me to my final comment. If we are going to deal with explanations for social events, if we are going to have temporary solutions for social events, we ultimately must address the question of behavior.
Here I find geographers (again I will make a generalization to which there are some glaring exceptions) are derelict in terms of thought and consideration. I am not suggesting that we need to be psychologists or sociologists, but in order to deal with issues of society, we must have some firm understanding of the fundamentals of human behavior.

What I see amongst geographers up to this point are two basic thrusts in that regard, and I am going to grossly categorize them. The first is the economic, traditional economic and neo-economic group. Which often makes implicit assumptions about individual autonomous rational behavior, as if we do act in that way and as if we do exist as individuals. In contrast, we see perhaps a more structured and, perhaps more formal argument from the Marxist group in which the structure of the individual units becomes much more important in explicating behavior. The problems with the first are so numerous that I don't need to go through them. The second, has fundamental problems as well, if only that sometimes the structural argument leads us to a misunderstanding that the structure itself is doing the acting as opposed to the individuals who are within that structure. Somehow, we have to reach, I would argue, a melding of structure and individual to form some basis of assumptions about behavior that are superior to those that are underpinning the various lines of gross arguments that I have just produced. And if we are to have temporary solutions, the solutions must come from that kind of approach, melding the work of the individual interested in explanation and the individual interested in the application of that explanation. They need not be the same but they need not be divergent.

CHRIS MCGEE:

Actually I am glad everyone else was brief because I intend to be somewhat long winded. I find myself in agreement with both Dick and Billie on basic principles which I guess is unfortunate because we could have a more vociferous discussion if we disagreed. But I come to that from a much different angle, I think, and I guess that I need to context both of their conclusions. Dick's, with regard to the possibility of ever escaping moral judgment; Billie's, with regard to the possibility of ever actually achieving explanation. So my argument is a little bit complex.

I think that we are supposed to be commenting on the question: "Does geography, as social science, have solutions to practical problems?" I would point out in the first place that the question is a non-sequitur, since social science really has nothing to do with practical problems. Social science is increasingly a major source of such problems. But the relationship ends there, bears no direct connection with the resolution. That is not an indictment of social science - the science itself is innocuous - but of social scientists, whose findings are usually elaborate rationalizations of their own prejudices and dogmas. And I do not mean to suggest that they have a monopoly on that sin. I come to these positions through two simple exercises: one in characterization, and the other in definition. But let me interject at the outset that since geographic methodology is almost entirely derivative, I will dispense with any consideration of geography as social science, and direct my remarks toward social science itself. Those aspects of economics, anthropology, sociology, and political science, that affect geography are inseparable from their fields of origin so that my simplification should not substantially alter my conclusion. I should also add that I am speaking about the relationship between social science and a subset of all practical problems that I want to designate human problems. At least, I assume my category to be a subset. But frankly, I do not know what 'practical problems' means. But I justify the distinction from human problems on the ground that as opposed to the whole range of difficulties that 'practical' is probably intended to include, there exists a much narrower spectrum of the only problems really worthy of the name in any genuine social studies. I will come to how I define that smaller group shortly. First I want to discuss the character of social science as opposed to social studies.

If we understand the term 'science' naively as the systematic pursuit of knowledge, social science is that. It is possible, as Thomas Kuhn points out to have individuals who pursue knowledge systematically and still to come out with an unorganized hodgepodge of information. That is to have scientists, but no science. But, clearly, social science has developed into what Kuhn labels 'normal' science, a field or fields of inquiry in which nearly all practitioners are indoctrinated by exposure to an accepted stock of standard learning. I do not think that the growing schism between conventional or reactionary and radical normalities controverts that statement. Regarding the radical alternative, I am not prepared to say whether the basic intellectual training is yet sufficiently uniform to be regarded as standardized and I am not sure that that is desirable. What the schism does show, however, is that still sticking with Kuhnian anthropology, social science has not matured. It has not been unified, even in any of its component disciplines, by the universal acceptance of a single paradigm or model to guide inquiry.

Here I might digress long enough to point out that one social scientific area of geography, urban geography, came as close as any has ever come to becoming a paradigmatic science when it was swept by an infatuation with central place theory. Central place theory turns out to be logically unacceptable because it contains unfalsifiable premises. In any event its utter domination of one branch of geography has carried the discipline, much less all the social sciences out of the pre-maturity phase of competition among alternative paradigms. All that unified social science then, apart from its tropical discreteness from natural and physical science and the humanities, is its general history as a field endeavor.

The salient facts of that history are these: First, social science as we know it took shape during the eighteenth and nineteenth centuries beginning with the French physiocrats. Second, the fundamental metaphor in physiocratic thinking, and throughout the
Enlightenment, was mechanistic. Third, the only serious alternative to mechanistic thinking in contemporary science and philosophy was organismic, first Hegelian and then later in Darwinian biology. Fourth, the social sciences perforce absorbed either the mechanistic or the organismic metaphor, or both, in the course of their emergence. It's not really critical which metaphor a particular social science may have absorbed more completely. Both direct intellectual inquiry toward the identification and description of immutable natural regularities, toward the discovery of laws. Some of the difficulties with the law seeking effort in social science are familiar. People individually display relatively little uniformity in behavior and outlook at least compared to other organisms or with components of inorganic nature. Students of the behavior of fleas for instance, by observing large numbers of flea hops are able to confirm repeatedly that the vectors of flea hops are calculably random.

Social scientists, however, retreat from even higher levels of aggregation than do students of flea behavior. The requisite scientific regularity of human phenomena emerges only at a level of generality that is somewhat completely abstracted from the life of the individual as to be meaningless in application to the specific social actor. But the weakness of social studies, I think, can be more effectively established on other epistemological grounds. The dogmatism in social science has been widely recognized as dubious so that a more cautiously labeled pursuit of explanation a la Billie is often invoked in substitution. Explanation, in turn, means general causation, which phrase condenses three broad criteria of scientific validity. It requires, first, that some interactive relationships should be recorded. That interaction should, secondly, be observably regular, repeated as nauseum in a large number of events. Finally, the description of it should be phrased in terms that lend it general applicability. The usual standard of generality is the predicted accuracy of the description. And I would stress that all of this is, in fact, near description. An initial observation of interaction, which typically depends upon temporal sequence or spatial proximity, almost always relies upon some inference regarding the motives, intentions, or purposes of social actors. The demonstration that no inference can be a product of logic is as old as Kant's critique of pure reason. What should concern us more is that inferences do invariably arise from moral judgments. What had been glorified by the rubric moral explanation, may I think be as clearly understood as simple interpretations.

Now what I would stress about interpretations is that they are only rarely preclusive. Any number of them, even several mutually contradictory ones, can often fit a given event or phenomenon. Thus the choice among them evolves upon a moral judgment of their worth in terms of some arbitrary criterion of aesthetic, rational, or other value. In short, the choice among interpretations depends upon prejudice, and it lies at the heart of social science. Clearly, if prejudice were less important than it is, considering the human welfare is at stake, some great paradigm already would have won acceptance in social science.

Against that background I can finally come to the issue of what constitutes human problems and what social science may have to do with them. Human problems are essentially complications that individuals encounter in the conduct of their daily lives. These complications are, in turn, products of perception, or if you want, of consciousness. I am not persuaded by assertions that consciousness and objective structural condition are intimately linked. I am especially unimpressed by the contention that they are linked in such a way that consciousness may be epiphenomenal and therefore discardable. At any rate, I am encouraging here a stronger orientation for social psychology. This may seem disingenuous since I have deliberately excluded psychology along with psychiatry from my list of social sciences. But I simply do not think that psychologists have made very many worthwhile contributions to our understanding of ourselves or of humanity at large. I can be even stronger with psychiatrists. I think they have actively retarded the pursuit of knowledge. Fresh starts can be made in the nominal arenas of psychology and psychiatry without reinventing very many wheels. As for the remaining behavioral disciplines that we term social sciences, (once again I mean principally economics, anthropology, sociology, and political science) they are anything but behavioral. There, practitioners study human motives, intentions and purposes much less often than they assume them away. If we accept that those are the reasons for material actions, then social sciences assume everything social away with them. It would not do to concoct structural configurations of society and to run on about human life in those terms. We thereby learn nothing about life as it is experienced by individuals. And it would not do any better to ascribe some bourgeois economic rationality to all social actors simply in order to get with the real interest in prices or terms of trade or the beauty of hexagonal lattices. Remember that the social in social science ostensibly refers to human society and that humanity is precisely what is lacking.

The main defense that I would anticipate being offered against this line of reasoning would concentrate on my definition of human problems themselves. I would counter only that any science that personifies society or at least statistical aggregates, is scientific only in the most meritricious sense of the word. In fact, it is a brand of scientism. The percentage of unemployed workers in an economy or the number of teenage mothers are not problems in their own rights. The problems that they in fact only represent are the material difficulty and psychological humiliation inherent in having no job in a society that values and rewards subservience to an employer; the material difficulties and psychological humiliation of a teen-age mother raising children in a society that requires marriage before childbirth, and at present frowns on marriage much before a woman reaches her arbitrary maturity. To create jobs or to make available contraceptives and abortions does not solve these problems. We are justified in understanding them to wax and wane in their acuteness. We are entirely unjustified and categorically wrong in understanding them to be solvable by palliatives that work only in the aggregate.
In the end, I think that this latter error is chiefly one of self-delusion. I'm not willing to ascribe overcynicism to most of its perpetrators. But two aspects of that self-deception are especially disturbing in my estimation. One concerns our eagerness to appraise the human condition without actual reference to humans at all. If this is essential to the pursuit of science, then science is not worthy pursuing. The other issue is to me a corollary of that. I want to borrow this once more from Thomas Kuhn, who points out that aesthetics are among the principle criteria of merit in so-called scientific endeavor. It seems to me that the unesthetic noisiness of individual behavior is the real reason for the exclusion of people from social consideration. At any rate, if aesthetic merit is the issue, I wonder whose aesthetics and why. The general answer to that question is ours, of course, and precisely because it is ours. Ours occurs here to those of us who are of Anglo, Celtic, Germanic, Western European descent. If we accept Wilbur Zelinsky's argument, based on James Lemon, that the former complex of peoples constituted one cultural realm, then we are a distinct group. Most definitely we should not expect our prejudices to conform to those of other peoples—excepting, of course, those whom we may have indoctrinated. Therefore, any generalizations that we can make about the problems of life in another society, I think, are open to very serious doubts concerning their validity. Certainly I think our greatest error would be to deceive ourselves that no matter what our orientation, we are not simply imposing moral judgment on the lives of other people when we attempt to manipulate societies social scientifically.

MODERATOR:

Thank you. We had planned to open up the floor immediately to questions. I think, however, that perhaps we should have a short response by Bill to Chris' presentation if he would so choose.

BILL:

Well, a great deal could be said. But I think I'll just make one quick comment. Hasn't Chris argued exactly the way that we generally and most frequently do? We are extremely good at being critical of what has been done, which is what Chris has offered us. He has not given one iota of a clue as to where we go. What is the alternative?

CHRIS:

Well, I think that I am suggesting, Billie, that there is no alternative. What we have to do is to accept that we are making moral judgments rather than making objective or abstractive...

BILL:

But the criticism went deeper than that. We accept that we make moral judgments. That doesn't bother me in the slightest. What bothers me is to tell me that the structure of the explanations pursued are inadequate and then leave me without an alternative to pursue.

CHRIS:

Well, Billie, I think you have this compulsion to be able to do something. If you can't do anything, then you have to accept that.

MODERATOR:

Let's open this up now to questions. One of the intentions of this symposium was that graduate students in particular would address some of these issues in the context of their studies here at Clark. Address the questions to whomever you want to answer.

Question:

Can we really talk about a methodology of science on social science in the abstract? I think it imperative to place it within the context of the production and reproduction of knowledge, and seriously question the constraints surrounding the things we learn.

CHRIS:

The real question here is whether we are learning anything at all. And I think that is open to very serious question.

BILL:

I don't know quite how to respond to that. I think it is easy to be a cynic.

CHRIS:

I'm not a cynic, I'm a fatalist.

BILL:

But one is simply either satisfied with the argument or one isn't. And if one is not satisfied with the argument then you pursue different forms of logic than those Chris is suggesting. I know that doesn't answer your questions. I would not be satisfied with
unstructured explanation. It simply does not satisfy me. That gets into the question of the psychology of satisfaction. Why is Chris satisfied with one form of critique and one form of description and one form of, it is difficult for me to call it explanation, I'll call it explication?

CHRIS:

I would call it interpretation.

BILL:

All right -- interpretation. Whereas other people are not satisfied with that. And yet other people are not even satisfied with the second form and must go on to a different stage.

DICK:

(the question) wasn't addressed to me but -- One of the things about...the production of knowledge is, that when you are faced with the kinds of problems the capitalist society is faced with now, which are problems of human survival, there are a couple of devices you can use to escape from doing anything about those problems. They're so bloody obvious, and why do you waste your time quivering around the edges of how to approach those problems all the time? That is one thing you can do. You can quiver around the edges of how to ask the question and never ask the question. Or secondly you can become a cynical fatalist and that is just as effective a way of escaping practical questions making question-asking an action. I really think that that is the essence of the two positions that we have heard. They are the product; they are ideologies, protected ideologies produced by the profoundness of the crisis which world capitalist society enters into.

Question:

Chris, don't you think that it is helpful to define different fields of science, and then determine what is a function of which science, and which sciences are more important?

CHRIS:

Well, my point is that these ideological categories are completely subsumed in more important intellectual similarities. So that I, frankly, don't see the difference. There is simply a different prejudice as I have described it and I don't mean to be too derisive using that term, because I've described to everyone the use of prejudice. But I simply don't see that it is a real alternative.

BILL:

A few comments, I'll respond to that and I want to respond briefly to Dick. Number one is that I would be very picky and argue that we are using the term science very nebulously and perhaps incorrectly here. I would disagree vehemently with Chris' use of the term. Science means something, specific. It does not mean any accepted body of interpretation. I don't think we need to get into that for a moment but nevertheless I reject the idea that any form of accepted interpretation -- however culturally it may be defined -- does not in itself constitute a general understanding of science. I also think that it is not important to be a science. And it upsets me that everyone has to say we are being scientific about this and that. The second comment is that while in spirit I would perhaps agree with Dick, there is a danger in what Dick is suggesting. It is the danger of the zealot -- the danger that somehow you know you have the answer. I don't mean that one doesn't try to have the answer, that one also must make the effort to be constantly aware that the solution that one is attempting need not be the correct one. Let me put it this way. Once you step out of theory building and work at apply your theory, staunch in the belief that your solution is the correct one based upon your theoretical constructs, that is when you are in severe danger. On the other hand, there is severe danger in being, as Dick pointed out, the fatalistic critic arguing that you cannot step out because any solution you undertake is bound to be incomplete, fraught with dangers, and so forth.

DICK:

I don't claim that we have an exact blueprint or carefully laid out program, that we know exactly how society should be structured. What I am arguing is that we should enter into a process -- the revolution means entering into a process -- by which the people, everybody, collectively owns the means of their existence and solves their profound difficulties, their profound problems, in a democratic way; where the owners of the means of production, the people, are doing what needs to be done in a very practical way who understands and can solve social problems better than those directly experiencing them? What I am arguing is that we should enter into a process by which we regain control over the direction in which society moves -- and that would mean owning that society, owning and controlling the very basis of society which is the productive apparatus of the society. I am not arguing that we have a set of pre-scribed solutions, but I am saying that we should enter into a process which we collectively control and as part of achieving control of course we argue -- and when we have achieved it of course we argue all the more.

Question:

Can we arrive at a set of criteria for determining useful ideas, for deciding upon which of them are good?
DICK:

Well, I didn't argue really that we have good ideas, I said we need a society in which we can evolve and use good ideas. Actually I don't think the main problem is a lack of "good ideas", but rather getting a society in which we control the direction of our society's daily movements and have the might of the mass of the population who right now are precluded from ideas. They don't have ideas, their ideas are imposed on them, their education is such that they have no independent mind. When the mass of the population thinks for the first time in a society where they massively control that society, the "good idea" part will be the least thing - achieving that essential structure is the issue.

CHRIS:

I have a problem with this whole line of exchange because I would turn Billie's criticism around and say that he criticized me for not offering an alternative. Now, he criticizes me for misusing science. I am the only person here who has defined what I mean by science. I would challenge Billie to tell me what he means by that. This, after all, is at the heart of the question of whether science can produce an evaluation of good.

BILL:

I think it is erroneous to define science as a systematic analysis of phenomena. Science is really based fundamentally on the structure of the arguments that such phenomena is put in. It depends upon both the structure of the argument and the acceptance that the standard definition is part of the structure of the argument and the acceptance that the standard definition is part of the structure of the argument. If there were no acceptance of standardized definitions there could be no science. One would have only individual interpretations.

Question:

Are you using "paradigm" as a way of looking at a problem?

CHRIS:

No, Kuhn uses them in a special sense, which is the reason why I dwell for a little while on the influence of central place theory on urban geography. He is not using paradigm in the generic sense, he is using it in a special sense's application to a way of pursuing inquiry in a scientific view.

MODERATOR:

I don't think that we should carry the discussion in the direction of the definition of science and Kuhn and paradigms - this is not a symposium on the philosophy of science. I think that we are getting off the topic and should redirect the focus of our questions.

Question:

Isn't the issue of hand the relationship of social science and technology rather than social science on geography as "science"? What we are debating is the role of academic inquiry towards effecting change.

DICK:

According to the formal agenda, we are to discuss "how we as social scientists can bring to bear the knowledge of analytical tools and methods in behavior and social sciences upon the nation's major problems in efforts to understand them better and..." (I think this is the key phrase) "and to assist in their alleviation," especially the word alleviation. I mean that is exactly the function of conventional social sciences - to alleviate social problems. Alleviate, not solve, alleviate. That is diffuse, that is do enough towards solving the systems of social problems so that they do not erupt into revolutionary problems. And that is the ideological function of conventional science. That's using it as a manipulative technology. And I think that I would agree that you can go far more - you can go beyond that, in fact you really have to go beyond that since you realize that is what you are doing. And that is what function of your life as a "scientist" is. You basically, then, reach a staff of crisis in your academic life or research and you have to make a decision at that point. You say "that is enough for me, and can I live with myself if that is sufficient - is that a good enough life for me to live, and will I, at the end of it, be satisfied being a person who has helped to alleviate problems? Will I look back and say that was a good life I've just led?" Or you can say "Christ, if I take that position, life is going to be hell, it is going to be one long struggle to keep my job, I'm never going to get major research grants (but then you would be surprised at what you can get). I'm never going to be a member of whatever caste Julian is a member of. I am always going to be somewhat suspect in my department and in my university. But I will be free to pursue the problem to its roots and do what I can about getting there." And you must eventually face that issue and you solve it of course in your own way.

CHRIS:

Well, I think that Ron hit my position on the head when he
Family, Community and Place: 
Worcester’s Puerto Rican Emigrants

Daniel Joseph Amaral

Historically, interest in the study of migration, like interest in the study of population in general, has waxed and waned according to variations in the economic importance of population transfers. In the past thirty years, migration has been a topic of considerable importance, especially in economics and sociology but geographers also have focused some attention on the subject. Throughout the human sciences, the resurgence of interest in the movement of people is the result of post-war economic growth in the industrial nations and of "development" on the periphery—in the colonies and former colonies. Knowledge and understanding of migration, as of other aspects of population dynamics, is necessary for planning and for the control of emigration and immigration. The study of migration as an aspect of public policy is not new however; interest merely varies with economic conditions. Even the most rabidly laissez-faire economists of the eighteenth century agreed that government must control the movement of labor.

Since social scientific interest in migration is born largely out of the needs of the economic system, most theories about the motives for migration derive from high simplistic metaphors of causation, and seek to "explain" locational change in terms of one or two variables. Recent economic theory initially tried to explain movement as the response of the classic "Rational Economic Man" to wage differentials. The general inadequacy of such theories prompted the creation of slightly more sophisticated models. These either appended additional economic incentives onto the "wages" explanation or rationalized deviations from expected behavior by invoking the concept of "bounded" or "intended" rationality. Thus,

Editors' note: Daniel Amaral, a highly esteemed member of the Clark community as well as a promising scholar, died in the summer of 1981. In his memory we have excerpted this paper from his dissertation, "Family, Community and Place: The Experience of Puerto Rican Emigrants in Worcester, Massachusetts".
migrants' failures to behave according to the rules of classical economic rationality were attributed to lacunae in their knowledge about potential destinations.

Despite the fact that nearly all of the theories of motivation are formulated in terms of a simplistic "push-pull" metaphor—which characterizes locational decision-making as affected by "pushes" at original location and "pulls" at potential alternative locations—they invariably concentrate on "pull" factors. Thus, superior wages at destination points, the greater likelihood of employment there or the attraction of "modern" amenities—often referred to as "city lights"—are the most frequently cited causes of migration. The brutal "push" factors which have resulted from the destruction of agricultural economies throughout the Third World have been largely ignored. Unbalanced and exploitative growth, promoted with little understanding of overall social consequences, is called economic "development", and migration to urban areas or to foreign industrial centers is seen as a positive—if sometimes overenthusiastic—response to opportunity.

Just as economics predominates in rationalizing motivations for the movement of populations in terms of the dominant economic values of the industrial nations, so sociology predominates in rationalizing the impacts of movement on migrant populations in terms of successful, non-disruptive adjustment to urban or foreign society. The difficulties which migrant groups frequently have are attributed to changes in environment and social structure rather than to sheer economic struggle. The economic factors which are supposed to have motivated migration are frequently ignored by investigators once the migrant group has taken up residence in its new location. Problems of social management—often of underemployed populations—take on greater urgency.

In brief, mainstream theories about the motives for emigration and about its impacts are tremendously narrow-minded, viewing the phenomena from the perspectives of the dominant economic system and of the society which costs the migration. The poverty of the theories is matched only by the poverty of the millions of rural, urban and international migrants who inhabit the slums of a great majority of the world's large cities.

This study began with the idea that the understanding of migration would require a radical departure from the very narrow perspectives of mainstream models of migration. At minimum, a study of migration should take into account the historical and material conditions under which the migratory movement occurred and the relationship of individual to collectivity in the decision to migrate and in the change of environments. In order to avoid the motives and impacts of migration these conditions must be taken into serious account in explaining migratory processes.

Puerto Rican migration is a classic example movement resulting from the processes of colonial agricultural exploitation and neocolonial industrial exploitation. Emigration has been encouraged in order to relieve the problems caused by large unemployed and underemployed populations on the island. On the mainland, Puerto Rican emigrants have contributed to the well-being of certain segments of the private agricultural and industrial sectors by forming part of the low-wage labor supply. Researchers have frequently blamed the difficulties and problems of Puerto Rican emigrants on a combination of migratory uprooting and a failure of community structures. It has been argued that the Puerto Rican family, because of the frequency of common law marriages, does not provide sufficient support to emigrants in their efforts to accommodate themselves to new ways of life. Similarly it has been said that the weakness of Puerto Rican Catholicism has prevented the formation of extramariial community structures which might aid in the transition to mainland life. These factors and the uprooting from home environments are thought to contribute to a crisis of identity among Puerto Rican emigrants. Such explanations, suffering from the myopia common to studies of migration's impacts, ignore the importance of economics in determining the behaviors of the emigrant population.

Puerto Rican critics of colonialism and neocolonialism on the island are also concerned about the impacts of emigration. Rather than attributing the emigrants' problems of adjustment to a crisis of identity, they attribute the problem of identity to the economic exploitation of Puerto Ricans as a subject people who are not in control of their own political and economic fortunes.

The convergent interests of mainstream researchers and of Puerto Rican researchers who are critical of the status quo suggested the following research questions, which are aimed at elucidating the importance of community, place and identity in Puerto Rican emigration:

1. To what degree do Puerto Rican emigrants feel themselves uprooted when they come to the mainland?

2. To what degree do feelings about nation and place play a role in the Puerto Rican emigrant's formulation of his/her sense of community and of identity?

3. To what degree do variations in integration into the economy affect the sense of community and of identity?

The answers to these questions could not be determined in a single study of less than unimaginably massive proportions. The purpose of the present study was to find suggested answers—answers which might contribute to the formulation of testable hypotheses or simply of other, better-honed research questions. The study was conceived of as an initial attempt to approach Puerto Rican emigra-
migation research are again in error—this time in oversimplifying the range of economic motivations for emigration.

It has often been suggested that urban amenities and services—or "city lights" as they are sometimes patronizingly called—are a kind of "added factor" in the decision to emigrate. The role of medical care, the only service of any importance as a factor in emigration in this study, was not so much that of an "added factor" in an individual's decision-making process, but that of a factor which became viable only after extended family members had emigrated and settled on the mainland. Thus, analysis of the motivational importance of urban amenities revealed that they, too, had little "pulling" power for the study participants.

A corollary to the idea that urban amenities and services might be a factor in emigration is the notion that extending them to rural places might stem the tide of rural emigration into Third World cities. The evidence of this study is that, in the absence of basic economic opportunity, amenities and services have little ability to hold a rural population in place.

Family and Community

Social scientists have frequently advanced the thesis that high welfare dependency rates, vulnerability to drug addiction, mental health problems, and juvenile delinquency among Puerto Rican emigrants derive from structural weaknesses in the Puerto Rican family and other communities. The alleged weakness of the family is attributed to the prevalence of common law marriage, which is associated with deviations from the model of strict patriarchal authority thought to have been universally prevalent among European emigrants. This view is an unsatisfactory application of an ethnocentric and highly idealized model and takes little notice of the concrete reality of Puerto Rican family life.

In general, the common law marriage is a deviation from formal systems which surrender absolute authority to the male. The form allows women rights of property ownership, a certain amount of personal freedom and some leeway in making decisions about the future of the family. The common law marriage was probably a very effective adaptation to a world in which men were forced to range far and wide in search of work. In any case, it is not prima facie a "weak" form of family structure, nor does it signify dissolution in the bonds between men and women. Among the study participants both common law and institutionally sanctioned marriages occur. Common law marriages are treated with every bit the same degree of seriousness as "formal" marriages. They receive community sanction which is conferred through a set ritual of parental consent.

For the emigrants in this study, extended family has tremendous importance in mediating the entire process of emigration. Segments of extended family follow each other from place to place. They
provide each other with information, material support and emotional support. Extended family relationships mean that all of the study families are still tied, in very real ways, to Puerto Rico. In many cases, extended family provides havens both on the mainland and on the island, and individual families move back and forth between them. This is particularly true in the cases of the parents of several of the emigrants interviewed. On the whole, emigration—as it is represented in this study—is neither an individual activity nor the activity of nuclear families. As noted earlier, emigration is, for the study participants, a complex, ongoing process in which move entails move and decision affects decision. The entity which is in constant migratory flux is the organism of extended family.

For most of the emigrants, church membership defines the social world beyond the extended family. Churches provide social contact, modest material support for new migrants and a charitable or proselytizing orientation toward non-members who are in distress. For practicing Catholics, church membership acts to reinforce identification with tradition and island religious custom and encourages the public display of Puerto Rican identity in activities like the annual celebration of the Feast of the Virgen del Carmen.

Studies which have claimed that Puerto Rican families and communal groups suffer from inherent structural weakness—such as those of Fitzpatrick (1971) and Glazer and Moynihan (1963)—are generally lacking in experimental evidence. They present speculative hypotheses based on the association of elements of circumstantial evidence—statistics on social deviancy and fragments of information on community structures of Puerto Rican migrants in New York. This study does not offer statistical proof of the viability of the Puerto Rican family and of Puerto Rican social organizations; it is, after all, a qualitative study. It does provide empirical evidence that these Puerto Rican social institutions are not congenitally flawed, however. It must be strongly suggested that the social problems of Puerto Ricans are either not problems at all—e.g., common law marriage—or that they are a reaction to exploitation, poverty and lack of opportunity rather than the result of weak Puerto Rican social institutions.

**Place, Nation and Identity**

Answers to questions regarding the emigrants' feelings of uprootedness and their feelings about nation and place must be placed together from scattered comments on a variety of matters—land ownership, amenities and services, work and religion. In material terms, none of the emigrants is wholly uprooted from Puerto Rico. Family and land ownership or rights to parcelas tie them to the island. Practically all of the emigrants exhibit—in their expressions of feelings, values and fantasies—mixed attitudes about the mainland and the island, their respective people and ways of life; outright rejection of either one or the other is rare.

The disastrous changes in the Puerto Rican rural economy brought on by colonial and neocolonial exploitation resulted in the displacement of sharecroppers and other people from the land and increasing difficulty in making a living. Because most of the emigrants have personal experience with these conditions, the ownership of land—especially of agriculturally productive land—signifies security to them. Few of them own good land, but almost all of them own small lots in Puerto Rico or hold usufruct rights to parcelas. When the emigrants themselves do not have access to land, very close relatives do. Because the parcelas are a place to stay in Puerto Rico, they play an important role in binding families together and in binding families to the island. To a substantial degree, the parcelas allow the emigrants to maintain close contact with their places of origin. Thus, it cannot be said of these emigrants that they are wholly uprooted from the island; their connections are both firm and viable.

Although one participant offered a whole-hearted reaffirmation of the superiority of traditional Puerto Rican culture and of her own past, and another seems to want his family to become as wholly North American as possible, the study participants generally demonstrated ambivalence about traditional—"modern" and Puerto Rican—North American dichotomies. To a substantial degree, the particular form with which ambivalence was expressed was consistent with religious belief and overall family integration into the non-agricultural wage economy—two factors which are themselves largely interrelated.

In exploring the relationship of religious belief and economic integration, it should first be noted that only active church members had ever labored in anything other than agriculture. Most of the active churchgoers who are able to work and/or their spouses were employed at the time of the interviews. Overall, however, the work histories of the traditional Catholics consist of agricultural jobs and show longer and more frequent periods of unemployment than do those of the revivalist Christians—Catholic, Evangelical and Pentecostal. It must be stressed that the employment histories of all of the study participants are marred by serious injury, illness or episodes of unemployment. On the whole, however, the revivalists are much better integrated into the non-agricultural wage economy than the traditional Catholics.

Almost all of the revivalist Christians and one of the traditional Catholics—Luz Perez, who, with her husband, has been long away from the agricultural lifestyle—expressed fantasies which combined life in the campo with the amenities of urban existence. On the whole, their commentaries on the rural Puerto Rican environment were very positive and demonstrated a nostalgic preference for the campo over the city. The commentaries frequently articulated a desire for the peacefulness and cleanliness of the campo and sometimes evoked images of the fecundity of the earth.
Several members of the revivalist groups and Luz Perez showed evidence of alienation from their fellow Puerto Ricans. Having espoused many of the values of the "modern" culture and having achieved comparative success, they tend to blame other Puerto Ricans, who have not done so, for being "animals" or shiftless no-accounts. Similarly, Puerto Ricans who have succumbed to drugs, drink or crime—the frequent escapes from poverty and joblessness—are tagged with moral blam. Thus, acceptance of the values of the North American culture, idealization of the rural Puerto Rican environment or of Puerto Rico itself and a degree of alienation from Puerto Ricans who have not "made it" are convergent phenomena in the revivalist interviews. This convergence is consistent with a religious belief system which subordinates traditional values, thus easing the transition to "modern" economic, social and cultural styles.

With the exception noted above, the traditional Catholics show very different patterns of social, cultural and environmental identifications. Only one of them, Josefina Diaz—a woman past sixty years of age—reveals a preference for rural Puerto Rican life. But her preference is not idealized; it is couched within a total affirmation of traditional as opposed to "modern" values. In this group, a total rejection of the Puerto Rican campo for social, economic and cultural reasons is common. Although the fantasy of owning a good, rural, single family dwelling appears in the commentaries of some of these migrants, the fantasy location is Worcester.

Despite the rejection of Puerto Rican rural life and the acceptance of certain aspects of mainland urban life, the traditional Catholics give evidence of very strong identification with other Puerto Ricans. The active church members engage in a variety of charitable, social and cultural efforts in favor of the Puerto Rican community. The only critical political analyses to appear in the entire study occur within this group. One analysis is of the injustice of agricultural wage labor in Puerto Rico; the other is of Puerto Rican social and economic status on the mainland. Finally, members of this group express very positive attitudes to the social aspects of living among many Puerto Ricans in an urban environment. Thus, the traditional Catholic emigrants in this study, whether active or lapsed, show—in contradistinction to the revivalists—weak identifications with the campo and strong identifications with other aspects of social and cultural tradition and with other Puerto Rican emigrants.

One final matter deserving discussion here is distinctions among the emigrants in their identification with the United States and Puerto Rico. The most forthright identifications with the United States occur among Evangelical Christians. Some of the Charismatic Catholics, perhaps sharing in some of the traditional orientations of other Catholics, showed considerable preoccupation with their own identities as Puerto Ricans. Much of their feeling was related as a rejection of North American racism. It is difficult to ascribe these distinctions to religious difference, however, since the

Charismatic Catholics are younger than the Evangelical Christians and have had considerably more experience with a variety of mainland institutions, including schools.

In summary, the emigrants in this study are not materially up-rooted from Puerto Rico. Owned land, parcelas and extended family all mean that each of the emigrants has important and viable connections with the island. These connections are, in fact, utilized in more or less frequent visits, extended stays and return migrations. Identification with the island, other Puerto Ricans and traditional culture varies among the emigrants, however. Variations are related to integration into the non-agricultural wage economy and religious affiliation, two factors which are themselves interrelated.

Implications

Theory:

This study was not designed to produce statistical proof of simple cause and effect propositions. It was designed to explore a particular migration experience in a broad manner, allowing the study participants to tell, as naturally as possible, the relationship among the relevant events and phenomena. The study, then, provides a glimpse into the Puerto Rican emigrants' view of the processes which have led to their current emigrant status. As such, it reveals something of their values, attitudes, feelings and beliefs. It also says much about the concrete and tangible facts and events of their histories, however. None of these is explored at the scale which would be possible in a quantitative study, but the relationships among them are visible as they never are in quantitative studies or in collections of quantitative studies. Such relationships are the stuff out of which theories about phenomena like migration ought to be built. Needless to say, theories would be best generated out of an accumulation of studies like this one. Currently, however, the theories which inspire many migration studies seem to spring, full-blown, from nowhere.

The data of this study suggest the amendment of several theoretic propositions which are typical in migration studies. The idea that migration is basically an adjustment to economic (wage) disequilibrium in a relatively homogeneous system would seem, for example, to be best applied to relatively homogeneous systems; e.g., the market for white collar labor within the United States. The idea of emigration exemplified in this study is rooted in the antagonistic interactions of two different systems. The economic burden within the weaker of the two creates the conditions for emigration.

Most theories of migrant adjustment to the host society tend to rest on the proposition that migration is a one-shot occurrence initiated by a single decision-maker. The data from this study show just how complex and social the process is stretching over many
years, with many return migrations, individual sorts of family moves and relocations. Of course, these characteristics depend on a variety of external conditions. Air fares, the political status of Puerto Rico, the land redistribution program—all of these have a profound effect on the experience of the emigrants in this study. Nonetheless, Puerto Rican emigrants are not unique in the matter of multiple migrations and continuing ties to home. Other migrations, including the post-war European labor migrations, for example, have similar characteristics. Of course, this suggests that migration is not a single unitary phenomenon, and that the contingent conditions under which migrations occur are quite important in understanding them. Although there may be some very interesting rules which underlie all migratory processes, the study of migration as it relates to policy would do better to reimmerse itself in the detail and complexity of the subject rather than to employ the theoretic oversimplifications which have dominated mainstream research.

Future Research:

The questions about Puerto Rican emigration which motivated this study have not been wholly resolved. The possible answers which have emerged require verification across a broader spectrum of the Puerto Rican emigrant population—in a variety of mainland locations, perhaps. For example, the relationship of integration into the non-agricultural economy and religious belief needs research to determine its dimensions and characteristics. Similarly, the consistency of attitudinal differences among emigrants of different church membership might be further explored. Besides these obvious "next steps," there are a number of other directions which future work along this line of inquiry might take.

The data of this study raise a set of interesting questions regarding attitudes, verbal imagery and the relationship between them in the accounts of emigrants. For example, to what degree is the rejection of social identification with fellow countrymen accompanied by idealizations of the home environment or place? Are the relationships apparent in this study coincidental or are they common phenomena? Similarly, the consistencies and patterns of both environmental and social imagery and their relationship to economic integration might usefully be explored. Linguistic analysis of interviews would probably be an effective technique in investigating these questions.

Finally, a major matter of interest which is only touched on in this study is the variation of experience, belief and feeling among the different generations of emigrants. In this study a broad spectrum of ages was represented so that generational differences were visible to some degree. Nonetheless, in-depth studies of single nuclear families or, preferably, of multiple segments of extended families, using the techniques and insights of this study, would probably open entirely new perspectives on the emigrant's experience of migration is precisely what is needed to correct the oversimplifications and interpretative biases which predominate in much of migration research.
The First Skyscrapers: The Newspaper Buildings of New York

Mona Dowosh

New York City, the capital of capitalism by the late nineteenth century, provided the setting for the first skyscrapers. The city's rise to dominance in the decade following the Civil War was based on its commercial and financial supremacy. The artistic and cultural communities of such cities as Philadelphia and Boston followed reluctantly in the footsteps of their business class. But the monumental, horizontal architecture of cultural institutions had no place in a city whose inhabitants prided themselves on their economic success. In New York, monuments to that success took the vertical form. While other cities were devoting their energies to civic concerns, and constructing museums and opera houses, New York concentrated its creative, architectural energy on building its commercial towers.

Traditional explanations for tall building development posit the primacy of technological and economic forces. In other words, scholars have assumed that the imposition of steel-frame construction on buildings at the peak land value intersection created the first skyscraper district. However, research on the time and place of that development in New York shows that this assumption is incorrect. The first buildings to pierce the Manhattan skyline were located six blocks north of the peak land value intersection, and were completed fifteen years before the adoption of the steel-frame. This article will examine a specific group of early tall buildings, the newspaper buildings of New York, and will show that particular situational and symbolic factors, not technological and economic forces, determined the course of their development. These buildings were constructed as monuments to, and symbols of, the success and power of the newspapers they housed.

In 1834, the New York Sun began publishing the first affordable daily newspaper of general interest in New York City. A large and competitive market for this type of newspaper developed almost immediately. By 1840, thirty-five papers were printed daily in New York, and by 1855, almost every family in New York bought a daily newspaper. The popularity of the metropolitan press was not restricted to New York, but was a national phenomenon, attributable to the new market created by the longing of urban masses for identity. The metropolitan press pioneered journalistic practices that satisfied people's need for information about the bewildering place they found themselves in, the other inhabitants, and themselves.

The national census of 1880 reported that the growth rate of the new industry was not equaled by "any other phase of industrial development in the United States."

The growth of the newspaper industry took a unique geographic form in New York. Since the early nineteenth century, New York newspapers had been located along Printing House Square, a plaza created by the intersection of Park Row, Nassau and Spruce Streets, and City Hall Park. The location was practical given the need of the newspaper industry to be close to both the political and financial core of the city, at City Hall and Wall Street. The boom of the newspaper industry in the nineteenth century created a need for new structures to house the expanding offices and printing plants. In addition to providing ample space, Printing House Square was an ideal setting for exhibiting these new structures. City Hall Park provided pedestrians with a place from which to view the encircling buildings, and the open space of the Park allowed the full height of the buildings to be appreciated.

The first newspaper to take full advantage of this situation was the New York Times, under the leadership of Henry Raymond. In 1858, the Times moved into a new building at the corner of Beekman Street, Nassau Street, and Park Row. It was considered a magnificent structure, particularly since newspapers traditionally were housed in dilapidated quarters. The Times Building was a New York showplace, and visitors thronged to the new structure. The five story building rose to the then staggering height of eighty feet, and as a contemporary pointed out, from the top floor, "the upper part of New York is spread out before the eye in one grand panoramic view." It was thought that the expense of the building would ruin the fledgling Times, but it was a promotional success, and began a trend of associating architectural height with journalistic success that would continue for over fifty years.

New York's skyline began to take a new shape in 1875, with the completion of the Tribune Building, the first commercial building to rival Trinity Steeple for dominance of the skyline. The Tribune became a leading newspaper in the country shortly after its inception in 1841. It had occupied several different buildings on Printing House Square, but none gave such obvious evidence of the achievements of the paper as the 'new' Tribune Building. The building, designed by Richard Morris Hunt in the Second Empire Style, measured 260 feet from the sidewalk to the top of its iron and granite tower. In order to insure the association of the newspaper with the tall tower, the words 'The Tribune' were cut into
granite blocks on all four sides. The building's design focused a viewer's eyes up to those two and a half foot long letters, located directly above an illuminated clock. The tower with its spire clearly proclaimed the supremacy of the Tribune in the world of New York journalism.

As a pioneer structure, the Tribune Building provoked strong reaction. The awareness that a commercial building rivalled Trinity Steeple in height created some anxiety among conservative New Yorkers, as shown in an illustration in Harper's Weekly of 1875 with the caption, "Tribune Building Overtopping Trinity Steeple..."12 A View of Trinity Church in the Immediate Future, if they go on putting up stores in the style they are now building..." (Fig. 1). More often, however, the building was heralded as an architectural marvel. Montgomery Schuyler commented, "The new Tribune Building was the wonder of New York that generation ago of which we were speaking... The wonder by reason of its altitude."13 Potter's American Monthly in its article, "The Palace Building of the New York Tribune," called it "the most convenient and well-appointed edifice in the country..."14

The Tribune Building continued to loom unrivaled over Park Row until 1889, when the World Building was constructed on the corner of Park Row and Frankfort Street. The building housed Joseph Pulitzer's New York World, and "signified that the newspaper had taken its place in the front rank of American journalism."15 Pulitzer bought the paper in 1883, and changed the small, democratic press into an affordable and popular newspaper, that could boast a record-breaking circulation of 250,000 by 1886.16 Part of Pulitzer's successful formula was his switch to a sensational style of journalism, and an expert handling of the visual qualities of the newspaper through the use of large headlines, illustrations and political cartoons. Therefore, when Pulitzer's World Building opened in 1889, the New York Times, and its 1858 building had given way to a taller structure in 1888) had been topped by the World, decided, under the direction of Mr. Och, to move uptown, to Broadway and Forty-second street. The New York Times was not the first newspaper to move out of the Park Row area; the New York Herald had set the example in 1893, moving up to Broadway and Sixth Avenue. But the Times was the first newspaper to change locations as part of the direct height competition with the World.19 Not only was the twenty-three story Times Building, completed in 1904, taller than the World Building, it was located in an area where no buildings exceeded five stories in height. By constructing such a tall building in the middle of Manhattan, the New York Times succeeded in drawing considerable attention to itself, and in shaping its reputation as a quality newspaper.

It was hard to build and hard to finance, but it was a magnificent signpost calling attention to the paper, at a point which was soon to become the center of midtown business and the night life of the city. Everybody in New York saw the Times Building when they came into mid-town districts; it was a standing reminder that the paper was doing great things.20

An even greater advertisement for the paper was provided by the Board of Aldermen of New York City when they gave the name Times Square to the area between Forty-second and Forty-third Streets on Broadway. In response to the increasing heights of buildings, the Tribune, whose structure had once dominated the skyline, felt forced to double the height of its building since it was "impossible, in fact, to leave it at the old height without falling out of the commercial competition."21

The Times Building is perhaps the most obvious example of a skyscraper whose raison d'être runs counter to traditional explanations...
of skyscraper development. Mr. Ochs risked the short-term financial security of his paper for the long-term dividends that he hoped would accompany his magnificent new building.22 The Times Building was twenty-three stories high because it was meant as a form of promotion, and as a monument to the paper. Its height was certainly not a reaction to intensive land use.

To understand fully the vertical expansion of cities, therefore, requires a symbolic interpretation of tall buildings. The first skyscrapers of New York were proclamations of legitimacy and supremacy. The newspaper buildings were indeed functional structures, but they were built tall because they were meant to be advertisements of, and monuments to, the success of the papers. Although the newspaper industry is only one of several industries that found its expression in the construction of tall buildings, it exemplifies a characteristic common to these businesses: their decisions to build tall were based at least as much on a promotional impulse and notions of corporate imagery, as on an evaluation of land economics.

Footnotes


2. The peak land value intersection in New York in the late nineteenth century was the corner of Broad and Wall Streets. Richard Hurd, Principles of City Land Use (New York: The Record and Guide, 1928), p. 158. The first tall buildings were located in the City Hall area of Manhattan.

3. The Tower Building, completed in 1889, was the first steel-framed structure in New York City.


6. Barth, City People, p. 59.


9. Ibid., p. 159.


22. Ochs was forced to borrow a large sum of money from the Equitable Life Assurance Society to complete the tower of his building, but he felt it worth the risk since "to leave the tower unfinished was only a proclamation to the whole town that he had bitten off more than he could chew." Davis, History of the New York Times 1851-1921, p. 325, Berger, The Story of the New York Times 1851-1951, p. 157.
within a specific social and historical context. Simply put, the choice of method will inform the results of a study. Moreover, while it is often recognized that any methodology is only as good as the person carrying it out, it is less often the case that a researcher considers how her/his biases and values affect the research process. A researcher, particularly working in other-cultural settings, does well to recognize, if not explicitly state, his/her values and the biases inherent in the research process.

Second, if a research effort is grounded in a specific social context, and environmental knowledge is particular to that context, methods appropriate to the study of one culture may not be valid in another culture-setting. These implications call into question most of the methods adopted from cross-cultural psychology for use in environmental perception studies. Moreover, for comparative studies of environmental knowledge it may be more useful and valid to compare data from separate inquiries which have been collected in a rigorous and culturally specific manner rather than adopting a strategy in which a common set of methods is used across cultures.

In the context of these broad considerations, the methodology which I present here is of significance for four reasons:

First, it is a methodology for the study of children's environmental learning, knowledge and interactions. Its focus is, therefore, both knowledge and behavior as integrally related but separate entities. That is, while I agree that the analytical distinction between culture as knowledge and culture as behavior is a useful one, I think it is a false and potentially troublesome dichotomy. Following the anthropologist James Spradely, I define culture as a system of meaningful symbols in which culture can be seen as the acquired knowledge that people use to interpret experience and generate social behavior. My methodology, then, was one designed to provide information on both knowledge and behavior.

Second, it is a methodology for the study of environmental cognition in other-cultural settings. For this I developed an essentially ethnographic approach which views both knowledge and behavior as cultural phenomena.

Third, the methodology is an eclectic one. That is, in order to counterbalance the weaknesses inherent in any single research method or type of approach, I used a branching sequence of interrelated methods in my study of children's environmental knowledge and interactions.

Finally, I did not presume a uniformity in the backgrounds of the study participants but rather built into my approach means for an analysis of distinctions in results. I anticipated and found, for example, distinctions based on gender and the social position of participants' families, but I also discovered the significance of birth order on children's environmental knowledge and interactions.

Thus, in my study I tried to develop an approach that would be at once socially grounded/appropriate to the study of children/and valid in an other-cultural setting. I will turn now to a description of the study itself and a discussion of the particular methodology developed for the study of children's environmental learning, knowledge and interactions in a transitional economy in rural Sudan.

Environmental learning, particularly in agricultural economies such as those found in Sudan, is an essential aspect of socialization. In order to analyze the relation between the content and acquisition of environmental knowledge and social reproduction in this social context, I sought information on the content of children's environmental knowledge as it is acquired and used in the activities of work, play, and formal learning and in the settings of the household, peer group, and formal education. The work called for a set of complementary research strategies to provide information on children's behavior, the structure and content of their knowledge, and how these have changed over the last two generations.

The research took place in a village of almost 350 households along the Dinder River in the Blue Nile Province of central Sudan. From December 1980 until October 1981, I lived with an extended family of six households. At the outset of the work, I conducted a village-wide census which elicited basic demographic and socioeconomic information. On the basis of this census I selected the sample population of 10% of the village ten year olds; a total of 17 boys and girls.

Until 1971 the village was characterized by the subsistence production of sorghum and sesame complemented by animal husbandry on a small scale. Since that time the village has been incorporated in a state-sponsored irrigation scheme geared to the commercial production of cotton and groundnuts. The changes brought about by the scheme have altered not only the nature of local agriculture, but the social relations of production associated with it as well. The theoretical goal of my research, then, was an analysis of environmental knowledge as an integral part of social reproduction in this changing production system as selected in the sample population's knowledge and interactions.

The antecedents of my approach are to be found in the work of the Place Perception Project at Clark University almost fifteen years ago. Most of this research was concerned with children's spatial learning and place perception. Studies by James Blaut and others of children's mental maps and understanding of maps and aerial photographs indicated that these skills are developed informally in children prior to the linguistic skills associated with formal education. In his work on place experience in a New England town, Robert Hart further pursued the study of children's geographic learning. Hart examined experiential learning, informal sources of geographic information, and children's affective response to the St. Vincent Island in the Caribbean; Ben Wisner extended the work
of the Place Perception Project to children's learning of environmental processes and the human manipulation of these processes. Wisner relied primarily on observation and found children engaged in a wide variety of environmental manipulations. Moreover, he found an emphasis on environmental learning within the family.

Building on this early work and adapting some of its methodology, my research focused on children's learning and knowledge of (1) local resources, (2) environmental processes, and (3) how to interact productively with the local environment, for example the learning and knowledge of agricultural skills and animal husbandry practices.

A branching sequence of complementary methods will counterbalance the weaknesses inherent in using any single research method. The approach included methods of observation, verbal techniques, demonstration exercises, and interviewing and surveying strategies to establish the social and historical context of the work. The methods used to provide information specifically on children's environmental learning, knowledge, and interactions are described below.

Participant observation was important to the work. Participant observation of everyday behaviors is a standard technique of anthropology and well suited to work amongst children. I used observation in two ways during my year-long stay in the village. First, random observations for short durations were used to establish the general pattern of activities of children in the sample population. These observations were continued throughout the field period to ensure that the full range of children's work, play and formal learning activities was documented and that the activities characteristic of each season and village setting were included. Second, children's specific work and play activities were observed repeatedly and at length. For example, I accompanied children for long days shepherding, fetching water or collecting firewood, and watched them engaged in dramatic play or in the rough and tumble of some of their games.

These experiences resulted in observations such as the following abridged selections from my field notes:

On this particular morning Awatif* and three of her friends (all approximately ten years old) set off for the tulih (a stand of tulih, Acacia Senegal, trees) at 6:30 and arrive there about a half hour later. They bring along rags to roll on their heads to rest the wood upon as they carry it home, and rope to tie the wood together. Within the tulih area the girls collect branches and sticks usually from trees that have been felled for charcoal production. They make three separate trips to different parts of the site, each time collecting full arloads of sticks and branches. The girls worked swiftly and easily, sticks and branches neatly over the outstretched rope, and this was obviously a familiar task to them. They brought their arloads to a central site after each foray. After the third trip each girl sorted her own wood, piling the then in pairs rocking the wood back and forth with their feet to pack it tightly, they tied the wood into large but neat and manageable bundles. They rolled the rags and placed them on their heads and then lifted the wood bundles by putting their heads down on the bundles and straightening up with the wood on their heads. They walk straight and tall as they head back for the village.

These boys play "tenancy" as well as "store" or "subsistence field" frequently. First they made the fields by raising squares of dirt and plowing them into rows with the miniature tractor they had just made from found objects. After the rows were complete they fashioned tassanets, the raised earthen mounds running between groups of rows which control the flow of water from the canals to the crop rows. The boys then planted groundnuts by sticking date pits lengthwise into the rows. They store these hundreds of date pits behind a house near their play area. After the fields were planted in groundnuts, the boys watered them. They usually sprinkle sand on the fields to signify watering them, but today they had a small vial of water which only wet about a third of two rows. They are well aware that the water in the real tenancies comes from the canals and irrigation ditches and seem to employ this knowledge by watering only between the rows as if the water had flowed there from the canal. Next they began to weed the fields and thin the crops using miniature versions of the short handled hoe used in the local fields. They each made a hoe using thick grass stalks and small pieces of scrap metal broken into a wedge shape. The weeding completed, the boys harvest the groundnuts by picking the date pits and piling them in the center of the field. They fill tomato past cans with the pits to represent the sacks filled with groundnuts at the end of the harvest. They cart their crop on the tractor to a storehouse in the village some distance from the fields.

We started getting ready to move on and as the shepherds and their flocks broke up and went separate ways, the shepherds had a chance to show their stuff. We were parting ways with two to three others and all of the boys worked together to round-up and divide each flock. It is a wonderful and totally crazy thing to watch, each boy runs around yelping and whipping the animals in and out of place. The shepherds fly between the collective flock, each cry out his version of the unique calls made by shepherds to get the sheep and goats in with the right
group and moving in the right direction. They move at lightning speed and the marvelous thing is how they know their own and each other's animals. I asked them about this later on and they said they know them by their faces and colors and because they have known each animal since it was born. The rapid-fire round-up of the flocks requires real teamwork. The boys work together and coordinate their movements and actions all the while shouting orders back and forth to catch that stray or push this one in the opposite direction. The whole process took about thirty to forty minutes after which we were again on the move towards another depression. Here, we joined up with a couple of other boys and their animals and moved, herded, walked, etc. a short time to the next well watered depression where the boys let the animal graze freely.

The combination of random and directed observations provided a complete picture of the activities of ten year olds in the village. Moreover, these observations often were documented on Super-8 sound film. My intention was to build a record of the children's activities both for later analysis and as a document.

While observation can tell us a good deal about behavior, it tells us little about the meaning of particular behaviors or interactions as they are experienced. Moreover, although observation was of enormous use in informing me of processes the children had mastered, and how these were learned, it was less directly useful in providing information on the content and organization of children's environmental knowledge. For this information, I used verbal and demonstrative methods.

The verbal method upon which I relied most heavily was the ethnosemantic interview. The method, in my case directed at eliciting taxonomies of environmental phenomena, was pioneered by Harold Conklin and Charles Frake in the mid-1950s as a means to elicit the shared knowledge of a culture group as it exists for the members of that group. The technique involves conducting a series of open-ended interviews which are designed not only to enable the participant to express his/her knowledge, but to reveal the ways and rules by which that knowledge is organized. This process is time consuming both because of the need for several interviews which can be quite lengthy and because each interview must be analyzed semantically before the next one is conducted. In my case, I conducted from two to six interviews, each of which lasted between one and two hours, with each of a sub-sample of five children. Each child produced a taxonomy of local plants and three of them also developed taxonomies of places in and around the village and the uses associated with them.

One child, for example, developed a taxonomy for "things that grow from seeds in the ground". The taxonomy included the categories of trees, grasses, vines and cultivated plants and was contrasted along dimensions that included whether they were planted or not, whether they had blossomed or had ears, whether they were small or large, whether or not they had thorns, whether or not they were a food source, whether or not they were used as fodder, whether or not they were desirable in cultivated areas and whether or not they were used as fuel.

The children expressed the content of their knowledge as they organized it as not as a structure which I might impose upon them. As research participants, they framed the categories and explicitly stated the attributes of and hierarchical relationships between the terms of each taxonomy. For these reasons ethnosemantic interviews are preferable to general testing methods or standardized interviewing strategies for research in other cultures.

As a counterbalance to the heavy reliance of ethnosemantic interviews on verbal ability, I used three methods which encouraged the demonstration of environmental knowledge as well as verbal expression.

*Child-Led Walks: In the child-led walk, I asked each child to take me where s/he chose and to show me anything s/he considered important. The walks invariably led outside of the village limits into the scrub surrounding the village, the river bed bounding it, or to the nearby irrigation canals and fields. The walks were a fun opportunity for the children to demonstrate their extensive knowledge of the local environment. The children identified particular environmental features such as plants or soil types along our route. I structured the situation as each walk progressed by asking the children to identify and explain any uses of every plant that we came upon.

All of the children were able to identify at least ten plants and give a range of appropriate uses for each one. Many of the children had an almost encyclopedic knowledge of local plants and resources. Not only did these children identify virtually every plant that we came across, but they were imaginative and extraordinarily thorough in setting forth the locally accepted uses of each one.

*Landscape Modeling: In order to elicit the children's knowledge of village geography and the human-environment interactions within it, I asked each child to model the village out of dirt, water, sticks, thorns and grass on a 10' x 5' area. For many children this technique was an excellent opportunity to demonstrate their knowledge of physical features and processes. These children built houses, plowed fields, dug irrigation ditches and got the river to flow. Other children seemed baffled by the exercise and uncomfortable digging-in and manipulating the available media. The results then were tentative sketch maps in the dirt outlining a few houses and the major physical features of the village.
*Geo-Dramas: After the children deemed that their models were done, I asked them to use a set of miniature farm animals, trucks and people whom I had clothed laboriously in Sudanese style, to show me life in the village. Again, some children took to these "geo-dramas" with great enthusiasm, but a few seemed overwhelmed by the perfection of these foreign toys and were inhibited in manipulating them. As with the landscape modeling, I interjected questions as the children acted out the patterns of everyday life. For example, the children invariably put the animals in the truck to take them to market, and I would always ask which market they went to and what price they got for a sheep, goat or cow. In this way, I was able to gather significant information on their understanding of environmental processes and interactions, and only the setting sun or calls to come home could end the game.

As the observation of the boys playing "fields" might have indicated, the behaviors associated with both the landscape modeling and geo-dramas were not alien to these children. In addition to "fields" (subsistence and irrigated) the children play "store" and "house". In each they act out in miniature the roles and responsibilities associated with each context or setting. The fit between these customary play activities and landscape modeling and geo-dramas as research methods, not to mention the fun of them, no doubt contributed to the high quality of information they provided.

In addition to these methods focused on eliciting children's environmental knowledge and documenting their environmental interactions, I conducted "oral geographies" with many of the children's parents and grandparents to discover how their own childhood interactions with the environment compared (or had changed) with their environmental goals or their children and grandchildren. Because the sample populations was drawn from families with low, middle and high degrees of integration with the irrigation project and the cash economy it represents, I was able to hypothesize about the changes in environmental knowledge, learning patterns and activities taking place as a result of the ongoing socio-economic transition.

I present this approach as a valid alternative to most of the methods used in research on environmental cognition and behavior. Each of the methods, with the exception of participant observation, undertake to discover the content and rules for organizing the collective knowledge and information processing structures of a particular culture group, in this case ten year old children from a rural village of central Sudan. None of the methods impose or search for any predetermined cognitive categories. I argue that this approach is central to any work on environmental cognition, but especially so when this work is undertaken in non-western settings. Those methods which impose categories external to the participants such as the tests common in cross-cultural psychology, almost always show the non-western culture to be at a disadvantage. This is not surprising since western standards are used to make the judgments. The methodology I adopted attempts to avoid this problem by eliciting information on what phenomena are significant for a culture group and the means they use to organize this information.
The Search for a Neutral Vocabulary for Explanation in Geography

Dale F. Cope

The persistently illusive nature of laws for the social sciences has contributed, over the past hundred years, to what Campbell (1982) calls "physics envy" on the part of many social scientists. This continuing yearning for the certainty that characterizes classical mathematics has called forth a variety of responses from those of us who seek a rigorous language, structure, and syntax for the purpose of scientific explanation in such disciplines as geography, economics, sociology, and political science. Some members of this community have sought to release themselves entirely from the constricting bands of scientific explanation; others have sought to modify the strictures somewhat in order that they might continue to participate in the game under rather looser regulations. A third response has been to use the vocabulary of physics as a rhetorical device to shore up the uncertain structures of social theory. The attractions of the language of physics are particularly apparent to geographers seeking laws for explanation of events in space; terms such as 'entropy', 'diffusion', and 'force' are so beautifully precise and rigorously defined in the language of physics, that the search in geography often may appear to be for the man/nature interactions that will fit the terms, instead of for the precise terms that will explain the geographic event.

So, we are borrowers in the social sciences; borrowers of theories, terms, laws, and languages of explanation. And in our search for a precise language with which to shore up our tentative nomothemism, we have been variously characterized as "... the children of the philosophic revolution, some huddling together on islets too barren for intellectual subsistence ..." (Cucelia and Golledge, Unpub.), "... prisoners hanging our heads against the ceiling of language ..." (Olsson, 1975), and "... not playing the game, but aimlessly moving the pieces" (Wittgenstein, 1932). Despite scepticism about the validity or possibilities inherent in our search for a neutral explanatory language for geography, however, we should continue the quest.

Part of the problem for geography, it occurs to me, is that from the earliest efforts of our disciplinary forebears we may have inherited a propensity for employing emotive, or at least highly descriptive, language. The ancient geographer, after all, was charged with the task of describing vast areas of Terra Incognita to his eager listeners and with holding their attention in competition with soothsayers, mythellers, and dreamologues. The exploratory tradition served to heighten the finery of the language employed; not only had grandiloquent predictions to be made concerning what glories might be discovered beyond earlier limits of exploration, but also it became contingent on returning adventurers to describe the sights and sounds and resources of new worlds in a style designed to entertain the common man and ensure continued funding of voyages of discovery. Thus, the early language of geographers was necessarily seductive, hyperbolic, image-laden and descriptive. Given this heritage, it is not surprising that some of our colleagues persist in employing descriptive language in their geographical writings; there appears to be some concern that if we lose our wider public audience, then we may be left talking only to each other, and in what sort of language shall we talk?

The basic issue addressed in this paper arises from the dichotomy that science is universal in its principles and results, but that language is accepted generally as one of the surest signs of a common culture. Given this dichotomy, it is easy to understand what Wittgenstein meant when he suggested that "... if a lion could talk, we could not understand him for we would not know the world in which he lives" (Wittgenstein, 1968). As our language and our use of language change over time, so does our view of the world. For example, until August, 1982, the book KEEP YOUR POWDER DRY was filed under "Cosmetics" in the Beirut library; now it has been refiled, correctly, under "Military History".

Philosophers and scientists have been wrestling with the problems of national languages and their limitations for hundreds of years, and along the way many have proposed the establishment of some form of auxiliary international language that would serve to aid in the promotion of cooperation and understanding in matters of scientific enquiry and explanation. Descartes predicted in 1629 that logic and linguistics would coincide in science, and suggested that a well-made language should have the systematic rigour of a science (Guerard, 1922).

The goals of most proposed artificial auxiliary languages incorporate the slippery notion of neutrality and, although almost every philosophical language has remained an elaborate intellectual pastime and logical exercise rather than a useful instrument of communication, the ideal of a logical grammar combined with an empirical vocabulary persists (Steiner, 1975). Some philosophers have advocated a return to Greek (Guerard, 1922). However, although Greek seems an excellent language in terms of neutrality, it has unfortunately become a byword for incomprehensability. Leiniz' ultimate goal was an a priori philosophical language, but he advocated the provision of a rational grammar made up of Latin elements (Black, 1962). Certainly our writings in explana-
tion and prediction and our translations of notational sentences might benefit from adoption of the Latin "ve" and "ut" to represent exclusive and inclusive "or". We might then be rid of the ambiguous construction "and/or".

Various other efforts in the search for a neutral language have concluded that if we could devise a language in the method of chemical nomenclature, in which each word contains its own definition, we might make a significant contribution to the search for neutrality. In the symbolism of chemistry, all existing bodies can be at the same time represented and defined by the symbols of simple bodies and the ten numerals. So, it seems that if we could reduce all the facts of life to a small number of primary ideas, then all other ideas could be expressed as a combination of these. However, many analysts of language systems have rejected these schemes as impractical: as the foundation for a language since the number of primary ideas may be infinite and certainly arguable.

Other philosophical languages based on classification posit that the form of a word will not give its analytical definition, but rather suggest its place in a system of divisions and subdivisions in a method similar to the bibliographic classification of Dewey (Guérard, 1922). Numerals, for example, might be given a phonetic equivalent. However, it is difficult to imagine that any memory but that of a computer would be able to master the resulting endless categories.

One fascinating example of a proposed auxiliary language is the invention of Solresol by Francis Sudre, a music master, who was struck by the fact that music was already an international language and that the notes of the scale were known by a definite syllabic value. So, with these seven syllables, Sudre in 1817 proceeded to make up a vocabulary via classification. "Do" indicated a class; man, moral or physical. "Dodo" indicated a subclass; a grown man. "Dododo" represented a subdivision; a grown man who was a farmer. By shifting the accent from one syllable to another, Sudre was able to form with a single stem the verb, the noun of the thing, the noun of the person, and the adjective corresponding to a given idea (Guérard, 1922). The disadvantage which Sudre was not able to overcome was, of course, that one sounded like an idiot speaking Solresol. However, given pleasant world surroundings, the disadvantage might be offset by the fact that one could hum one’s hypothesis and be universally understood. And the use of Solresol might bring a whole new meaning to the idea of “fine-tuning” a theory.

One further example of a synthetic language run riot with cases, tenses, moods and persons, was Volapuk (folks speak), an artificial language that arrived full blown in the form of revelation during the sleep of one Fr. Schleyer in 1879. By 1884, the language had spread throughout Europe and the world, for reasons obscured by time, seized upon particularly by geographers (Guérard, 1922). By 1889, at the Third Congress of its advocates, Volapuk was the only medium employed, even by the waiters at table. However, by 1900, the language had virtually disappeared. Perhaps the fact that a Volapuk verb could take 505,000 different forms contributed to its demise. Other entrants in the race to find a universal neutral language are numerous, including the well-known Esperanto and the geographer Vidal de la Blache’s contribution of a universal analytical language.

However, what do these ramblings in verb forms and pasigraphy have to do with the search for a language for explanation in the social sciences? The generally accepted notion that scientific works are composed of two parts—the most important part being written in a universal code, with national languages filling in the gaps—provides one connection. And the fact that Leibniz cautioned that one must not engage in a science without at the same time giving a definition of the terms provides another (Leibniz, 1915). Yet, definitions phrased in national languages tend to be emotive and slippery both in time and space, and we may often find ourselves employing the dead metaphors of a previous world view in our explanations. The earth as Mother Body gave us foothills, headlands, mouths of rivers; terms which geographers use casually now, having forgotten their original heartenings.

In addition, the national and emotive meanings of words are in constant flux and, as Gunnar Olsson (1975) has pointed out, each national language "... tends to illuminate some aspects of reality and leave others obscurred". For example, the first lessons in the Germanic Volapuk (supposedly universal) instructed the learner to translate the sentences: "The Fatherland governs man's actions," and "The Fatherland has been involved in many wars of liberation". So, we can see that any national language which we as geographers choose to employ will in itself reflect both the world and our view of the world.

It appears from the literature that many geographers employ long-running metaphorical language to illuminate the problems involved in searching for an appropriate explanatory language for geography. It becomes a Wittgensteinian situation of the language we are talking in, and the language we are talking about. Olsson's metaphor of language as prison walls is probably the best known; others have employed the metaphors of islands (Goulbourn and Collinge, Unpub.), chessboards (Wittgenstein, 1932), houses of cards (Gellner, 1977) to describe the bounded nature of language. I have chosen, for the remainder of this paper, to employ the metaphor of footwear, viewing language within the metaphor as both constraining and adaptable. The aspect of constraint comes into play when we realize that any item of footwear necessarily contains the foot, although allowing for varying amounts of toe wigging within the confines of leather or canvas. The adaptability factor is evident in the choice of footwear for particular events in which we might choose to participate. The metaphor seems to serve nicely the issue of discarding the descriptive language of our disciplin-
ary forebears as we would discard their muddy boots for sneakers, the race to explanation being to the swift.

Let us say that social scientists are entered in a series of Olympian efforts called Explanation. We geographers must first 
decide, in our quest for an explanatory method and language, whether we will participate in a multiple event such as the 
Decathlon, a series of related events which increase in spatial scale (dashes, sprints, races), or in a single event with some 
hurdles thrown in. And we must remember that no all languages (like footgear) are equally suited for all events.

So, let us assume that Deductive Nomothetic Explanation is the 
Gold Medal Methodology in a single event called science and that this event is a marathon with some very high hurdles. Our foot-
gear had better not be slippery or loose for this event, or we shall surely fall down at the first hurdle. Our shoes should be 
predominantly designed for our purpose; however, this restriction does not necessarily mean that they cannot also be aesthetically pleasing.

To review the standards for the Gold Medal, we can go into 
training by stretching back to Auguste Comte and his positive 
philosophy. Despite the fact that Comte came to positivism only 
after a severe nervous breakdown, his methodological precepts have 
led, through a labyrinth of translations and permutations, to the 
Hempel-Oppenheim schema of DNE. Comte's five precepts:

- direct experience of immediate reality;
- common, replicable observations;
- formal construction of testable theories;
- technical utility of science concerned with means;
- and, science progressing by gradual unification of theories;

were originally applied to logical positivism in geography by 
Schaefer and Bergmann. So, we have done well to choose these 
men as our coaches for this DNE event; they, together with Hempel 
and Oppenheim have done considerable reconnoitering for us and 
have sketched out the chart of the race course (see Fig. 1.)

Having trained sufficiently, we think, to stay the course, 
with the Gold Medal gleaming in the distance and Galileo and Comte 
trotting along beside us in spirit, we are off at the crack of the 
pistol. Our shoes are lightweight, precision-made, yet quite 
ordinary sneakers. In this event, our challenge is to explain a 
pattern of movement between places.

1. \( \pi = 1000 \)
2. \( \phi = 6000 \)
3. \( d = 14 \)

We believe that we are wearing just the right DNE footwear so 
that we know what these hurdles are saying (and they mean) and we clear them, musing about populations and distances 
and places on a uniform plane.

The next hurdle looms up ahead, taller and narrower than the 
previous two. It is called General Law Statement. Someone in 
the crowd (a Mr. Newton, who is wearing a tee shirt on which is 

**Figure 1. The course of the DNE Marathon**

<table>
<thead>
<tr>
<th>Event</th>
<th>Antecedent</th>
<th>Conditions</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \pi = 1000 )</td>
<td>( \phi = 6000 )</td>
<td>( d = 14 \text{ KM} )</td>
</tr>
</tbody>
</table>

\[ I = f(\pi, \phi, d) \]

Law Statement

21 Miles

The Symmetry Thesis


D.N.E.
printed "^2\text{Gal}/d^2) hands the physicist, who is in the lead, a cup of Catoraid, and he is on the hurdle with a foot to spare. We experience a moment of physics envy, but grit our teeth and jump the hurdle almost as well. It appears to us to read:

$$I_{ij} \equiv f(P_i, P_j, D_{ij})$$

The physicist puts on a burst of speed; in his head atoms of something or other move obediently in a frictionless space with a precision and predictability that is elegant and simple. We fall back because our shoelace has become untied while we were thinking about people and places and about how some folks will drive ten miles out of their way to save 5c on a pound of bananas. But we manage to gather up our residual effort and push on.

The next twenty-one miles of the race are hurdleless and our shoes stayed tied while we think about various translations of the law of inertia. We've cleared and come up with logarithms of observed interaction intensities and some explanatory variables and think about the fit of our model to the antecedent conditions and to the event. However, the final two hurdles appear to have been moved over from some equestrian event, and we wonder if we can possibly leap them. The penultimate hurdle is labelled The Symmetry Thesis and two people in the crowd are holding a banner which reads:

EVERY ADEQUATE EXPLANATION IS POTENTIALLY A PREDICTION
EVERY ADEQUATE EXPLANATION IS POTENTIALLY AN EXPLANATION

The physicist, still in the lead, leaps this hurdle neatly, then turns and leaps it backwards before continuing the race. Pompous! The crowd roars, but our Coach Hempel has advised us that the direction of the inference should proceed from given explanandum to explanans and that we need only clear this hurdle in one direction. But, which direction leads to explanation?

On the flat space between the last two hurdles we catch up with the physicist who has become bored. At the finish line, we both clear the hurdle marked DNE at almost the same instant. We believe that we may have won. "4200 interactions," we shout, and whisper, "more or less." Yet, it is not our flag being raised on the center pole; it is not our anthem being played. Who has won? The physicist with his symmetrical hopping? Or was it the Russian in CST galoshes? Or the anthropologist clutching his single general law about a certain taboo interaction?

The judges are hanging around our neck the Bronze Medal for statistical inference. But, wasn't that listed as a separate event - Probability Pole Vaulting?

The questions for geographers (and for social scientists in general) have been and continue to be: should we have even been entered in the nomothetic events, as Schaefer advised? Can we even qualify? Or should we, instead, confine our endeavors to some sort of intramural idiographic events in which gold medals are awarded for comprehensive categorization and for illuminating descriptions of the world around us? Should we fire our most rigorous trainers and replace them with a fellow like Gale (1972) who might allow us to run in fuzzy bedroom slippers and not be quite so exacting in our training regimen?

Perhaps we should be satisfied with our Bronze in the DNE Marathon, satisfied with having beaten the sociologist. The point is that our loss to the physicist (for it was he who really won) has to do with our language, our footgear. Our loss may be traced to the fact that, although we thought ourselves properly suited up with DNE sneakers, we were actually hindered by our hiking boots. Some of our coaches in the early days of our long-distance running were, if we think back, of the "muddy boots a geographer makes" school of thought and truly believed that the heavy clunkers of exploration and description would suit in the race called the science of geography.

The language of description involves some very heavy prose, indeed, and Symanski (1976) was probably correct in disqualifying Meinig from nomothetic events. The point is that Meinig should have disqualified himself by announcing that in his heavy wooden clogs of descriptive, "motive style," he could not possibly be a contender in the DNE Marathon.

So then, what is the proper footgear (language) for participation by geographers in the DNE event? Many of us would advocate ordinary, prosaic sneakers, well-worn and yet well-fitted to the task at hand. Even if the rigorously precise language of a positivist method of reasoning is the last element of this philosophy with which we are left, then we should cling to it very tightly indeed. As Coucelis and Colledge point out in a yet-to-be-published article, "... positivism has set standards of clarity, consistency and rigor in the development of argument and the conduct of inquiry that are unparalleled in the history of human thought." The language of deductive nomothetic explanation must be as neutral and non-emotive as we a discipline can agree upon. Logic should provide us with our grammar and empirical evidence our vocabulary. And yet, some one once said that the "music of fantasy wears sensible shoes." And, just as I though that I had the thrust of this paper sorted out in my head, I attended a lecture by Dr. Mary Hesse of Cambridge University who argues that scientific discourse should grow by "metaphorical extension of natural language and to change partly as a result of the reinterpretation of natural language, all the while remaining apart from and superior to natural language itself." Professor Hesse also cautioned that above all we must not use 'theory-determined' language in science; i.e., we must not employ state-
ments whose terms presuppose the truth of the theory under test (Hess, 1982).

So, it appears that the issues involved here in the search for appropriate language are far more complex than any simple metaphors of marathons and footgear, prison walls and banging heads, or even islands and archipelagos. The questions we must answer about our use of language for the purposes of deductive nonomothetic explanation, and about our participation in that methodology of science in general include:

- In a world of extremely complex man/environment relationships in which behavior is a critical and inexact variable, does the logic and language of DNE itself become idiographic? In other words, do we lock our explanation in time and tie it to a static society?

- How are we to translate theories about unobservables into laws that talk both about and in observables?

- If we are constrained to speak of explanation only in terms of those concepts that have empirical meaning or content, how are we to explain behaviour and the decisions that people make according to concepts that have no empirical meaning, such as faith, perception, potential?

- If we seek to employ models founded on DNE as bases for problem solving in modern society, will acceptance of the falsification thesis put us in the position of constantly trying to destroy our own credibility? And, what if our reasoning is internally consistent, but ethically objectionable? (Olsson, 1975)

One British sociologist (Ford, 1975) offers the opinion that it doesn't really matter whether social scientists adopt a scientist or a journalistic style of rhetoric but that we must try to avoid the "... obvious bad faith of mixing the two". I would agree that we must resist the temptation in geography to capitalize on the authority of our expertise in order to carry home the thrust of an argument or explanation which "... does not have the energy in and of itself" (Ford, 1975).

If we do choose to enter ourselves into the event called social journalism, we then must not expect to place in the DNE Marathon, and it would certainly be very bad form to pop out of the bushes near the end of the race and lay claim to a medal without having jumped any of the hurdles or gone the distance.

The point is that in choosing our event, we choose our language. The question of whether the social sciences can stand up to the rigours of the DNE Marathon remains essentially unanswered, although there are many would-be marathoners who would seek a relaxing of the rules and precepts which govern the course of the event. There are even those among us who would argue that merely perceiving what it might feel like to participate in a marathon should qualify them for entry. I would argue that if geographers accept the challenge to engage in the event called scientific explanation, then we accept as well the tiring distance of the race, the significance of the hurdles, and the illusion of the final leap.

Of course, there are other arguments, other logics, other metaphors, and other languages. To review just one other form; Olsson (1975) would argue that there is (or should be) an event called practical inference, for which geographers may be especially well qualified. The logic employed here is deontic or ethical, Aristotle replaces Galileo as our spiritual running partner, and our footgear is normative rather than empirical.

The schema of practical inference is presented by Olsson in a form similar to the Hempel-Oppenheim schema, except that, for Olsson:

I1, I2, I3, ... Ik refer to statements of current intentions

M1, M2, M3, ... Mr refer to statements of available means for bringing about the intended result.

A refers to the plan of action to bring about the intended result.

In Olsson's practical reasoning even we are, therefore, focussing on what we should do in order to achieve a stated end. We take the I and M statements as givens and search for the appropriate A. Within the rules of this event, we are not required to explain the world as we find it, but to describe the world as found and manipulate it by various means in order to affect change, or to maintain the status quo if that is the A.

But is this science? Or is it enlightened social engineering of which we may approve if we happen to be enlightened in the same manner? Olsson has criticized our borrowings from the law of gravity for producing models in geography that do not merely reflect reality, but create it. For example, I have been a party to the design of highway routes in accordance with the model: lijef(Pi, Pj, Dij) and have seen that we tend to constrain individual behaviour to fit the model, although perhaps in the aggregate there is the illusion of choice. However, as an engineer, I took no interest or responsibility for the
residuals of the regression equations, as a geographer I would argue that we must search for other explanations to cover them.

Olsson, however, in his schema for practical reasoning replaces the "is" statements of logical positivism with "ought" statements of prescription. He argues that instead of striving for truth preservation in the manner of classical logic, one should seek "satisfactoriness-preservation" in the realm of human action, employing a dialectical logic. For Olsson, the semantics of physical objects and existence might be replaced with a language which talks about (and in) relations and subsistence.

In conclusion, I would tend to side with Schaefer (1953) in his plea that we not give up the attempt to explain because we cannot explain everything. And I would also agree with his cautionary note that difficulties arise only "when the descriptive component is rationalized into the idiographic method which is then conceived as coordinate with that of explanatory science." That situation is very like the contestant jumping out of the crowd at the final hurdle. And yet, are we any closer to discovering the boundaries of that language which will serve us best in our quest for explanation in geography? Since there is no neutral vocabulary or pure algorithm for theory development in geography, then, as Kuhn (1962) argues "... debates over theory choice cannot be cast in a form that fully resembles logical or mathematical proof". So our language must be prosaic, non-notational (for the most part) and we should, as Kuhn suggests, persist in a disciplinary search for precision, shared values, and agreed upon meanings. The precepts of positivism provide us, I would argue, with the proper degree of bounded opportunity for this effort.

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