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Urbanization and Economic Development; Research Directions and Needs

Our efforts to suggest research and identify priorities for the study of development may be considerably enhanced if before we begin we consider some very difficult but fundamental questions related to the context in which we are operating. It should be clear that our conceptualization of what constitutes "development" will be central to our deliberations. While it is not my intention to offer specific definitions of this term, the issue is worth considering. In fact it is imperative that in our attempt to specify research priorities, we remain cognizant of the fact that terms like "development" are highly subjective and that even the most careful of social scientists may unwittingly be defining "problems of development" in a highly biased manner. That this is the case may be seen from the tendency on the part of social scientists from the allegedly "developed" nations to base their indices and measures of development upon the degree to which various nations approach or diverge from the so-called "norm" of western nations. The very criteria of development are often those indices of economic, social, cultural and political life which are so highly prized in "developed" nations. It is becoming increasingly clear that what we have generally considered as "development" is not necessarily acceptable to nor desired by some segments of the world's population. They have begun to question whether a nation which tolerates such a high level of infant mortality, the existence of large areas of hunger, such high incidence of crime and violence, or which for a considerable period of its history has disenfranchised a large segment of its population and whose industries have been permitted to contribute to pollution to an alarming degree, is in fact an appropriate model of "development". These people are increasingly concerned with the high costs of "development" both in material and human resources. The view is often put forward that "development" can no longer be measured in the crude economic terms which it often has been, but must be expanded to incorporate indicators which reflect social well being and the quality of life. It is quite conceivable that our conceptualization of the "problem", when viewed from a third world perspective, may be totally inappropriate. Obviously, specifying research priorities, if such is indeed the case, is highly unlikely to lead us in desirable directions. If we are to do more than

study the manner in which other nations compare to our own notions of development, then a serious question arises as to "whose development" we are talking about and attempting to understand. Until recently, and it is by no means completely changed, few felt impelled to ask those whose "development" was being studied how they perceived the problems. Undoubtedly, our understanding of developmental problems will be enhanced by bringing a number of perspectives to bear on the issues. This suggests that our search for research priorities would be considerably aided by the active participation of colleagues from the nations whose development we propose to study. This may allow us to perceive sets of national goals and problems, and define research tasks which are largely ignored or unknown at present.

Directions for research

Having to some extent qualified the context in which my remarks will be made, let me turn to the question of how we may proceed to define priorities for research. One might be tempted to suggest that we know so little about the development process that we need every study that could conceivably be completed. However, it is clear that in light of the limited number of geographers and the even more limited financial resources available, it will be necessary to order our priorities for research. If our efforts must be so ordered, then I would argue that the system of priorities should be based upon the likely contributions of the research to two broad areas: 1) the articulation of a theoretical framework within which to study the spatial aspects of development (a major academic concern); and 2) the formulation of research strategies which are likely to yield policy implications (a major human concern). Most desirable would be research likely to contribute to both areas. Perhaps the satisfaction of one implies that the other will be satisfied.

Within this broad conception of priority research it may be possible to offer specific suggestions as to the types of study which are likely to be promising in this regard. One major area of concern for future geographic research should clearly be on the relationship between urbanization and economic development, not in the limited, myopically statistical sense of correlating indices, but more directly to the relationships between the form and function of urban centers and their role in the development process. In this regard, it is clear that we will not be able to understand urbanization and development by studying only cities. Cities must be viewed explicitly as only one component of a complex set of interrelated

elements which together find expression in the spatial pattern of development within a region. Considerable theoretical insight might be gained from an examination of the extent to which the spatial inequities in the pattern of development (which are characteristic of most nations) are related to the role and function of the urban system. Such city systems may be viewed as the physical manifestations of the organizational principles governing the conduct of social and economic activities. One might suggest for example, that in those areas where long periods of national development have fostered the close integration of even the remotest areas of the country, urban systems are generally well integrated, highly differentiated with respect to function, and tend to exhibit some degree of hierarchicalization. Even the form of the urban system may be affected by such organization with the capital city or major city typically located at the center of the urban system with outlying centers well connected with good transportation. On the other hand, systems of cities which develop to effectively organize the production and export of primary products are characterized by quite different forms. This is especially true in those instances wherein the development also takes place during a period of colonialism such as that which marks the major periods of urbanization in many of the third world countries. The criteria under which the urban systems began to develop and take form were primarily established to serve colonial ends. In large measure then, the present urban systems of third world countries are composed of centers which have developed to effect efficient organization of an export-oriented colonial economic system. It is quite likely that with independence the goals, values, and plans of national leaders will differ radically from those of the colonial regimes they have superseded, although this has not been true in all of the countries of the third world to the extent which might have been expected. Consequently, one must ask whether the urban systems which have developed under one set of goals or criteria can effectively be used to attain those of another. Such research would not only significantly contribute to our understanding of the relationships of interest but would certainly yield important implications for policy.

In a more general context, our efforts should be directed toward examining those mechanisms by which developmental impulses are transmitted from areas of growth to those of economic stagnation. A good portion of our research efforts then must center upon gaining greater insight into the processes by which economic development is initiated and transmitted in a spatial context. Of primary concern in such an endeavor would be an analysis of the mutual impact of patterns of urban settlement and economic development upon one another.

That is to say, the city and systems of cities must be viewed as both cause and consequence of development. Urban centers must be viewed as both molding and reflecting the spatial pattern of development.

Concern with this type of problem stems from the fact that we are aware that cities reflect the social and economic milieu in which they have grown and that the type, numbers, locations and functions of such urban centers are affected by the social and economic conditions of the nation. In reviewing the research needs in this general context, the Resources for the Future staff (Resources for the Future, 1966) suggests that:

"While we can identify the way in which changes in the national economy influence the pattern of settlement, the consequences for national economic development of the characteristics of the settlement pattern are also of great national significance. At any one moment, the settlement pattern may impose certain kinds of constraints on the succeeding stages of economic development: e.g., it determines the scale of markets for goods and services, the degree to which labor specialization is feasible, and the effectiveness with which capital is employed."

This suggests that during certain periods of development the spatial organization of urban systems may impede the continued development of the country. In a more general sense, what is required is more complete understanding of the effect which different spatial structures have upon the initiation and spread of development. If we are prepared to agree that the spatial organization of social and economic activities may affect the rate of development, then it is conceivable that development efforts might be accelerated by actions designed to reorganize activities in space.

There is another area of interest which might be considered. That is, the continued concentration of people into often overcrowded, fast growing urban centers. Our interest in conducting research into this process of concentration need not be undertaken so much from the need to document the extent, intensity, -- or rate of growth but rather to examine carefully the spatial implications of such a process. Obviously, the redistribution of populations from rural to urban areas impacts directly upon the "pattern" of human resources, but to what extent does this redistribution of population increase the level of social, economic and political interaction normally associated with developmental

change? More research will be necessary before we can properly determine whether this urban growth is associated with any fundamental changes in the social, economic and political relationships among the populations involved, or whether it simply amounts to the location in close physical proximity of rural to urban migrants operating essentially in the same manner in which they had prior to migrating -- a substitution of urban subsistence for rural subsistence. It is clear that increases in size and levels of concentration of urban populations do not necessarily indicate increased interdependence. Additional research activities in this general area might allow us to differentiate urbanization which is taking place as a result of agglomeration from that which results in what has been termed agglutination, that is "the compressing into physical proximity of what remain essentially discrete population groupings" (Hauser, 1965).

The research effort which has already focused upon the growing urban centers in developing nations has made us aware that they often are a mixed blessing to the countries in which they are found. On the one hand, there are the benefits which accrue from the increased concentration of activities and people, such as scale economies while on the other hand, there are considerable diseconomies, in the form of overcrowding, urban congestion or increasing crime rates. This has led to the identification of alternative hypotheses concerning the role of urbanization as a strategy of development, not simply a concomitant of the development process. Terms such as "over-urbanization", "hyper-urbanization," and "under-urbanization" have been used to describe the relationship between levels of urbanization and levels of development. Perhaps some of our research should be directed towards determining to what extent and in what context urban centers function as "development generative" mechanisms and those in which they act as "development constraining" mechanisms.

At this juncture it might be appropriate to offer the suggestion that in defining research needs and directions we attempt to incorporate a much more "human" element into our research designs. By "human" element we do not mean in the restricted sense of calling for more research on the topic of human resources in which people are of concern primarily because they represent grist for the mill of development, nor just increased concern for education because a projected production function calls for the input of so many technicians at some future date in order to maintain a hoped for rate of growth, but rather to make a genuine effort to make our studies more human. Our studies should be more human in at least two respects; 1) the encouragement of research focused on

some of the aspects of development which are of immediate human concern especially the impact on life styles or quality of life, and 2) the encouragement of more explicitly behavioral modeling strategies in development research. In regard to the first point, geographers could contribute considerably to the understanding of the impact of development upon the spatial extent of social networks, or the relationship between disease and development where efforts at development have had or are likely to have a disruptive effect upon the local ecological balance, or the nature of regional inequities in the distribution of development gains. With respect to the second point, in attempting to formulate models of the development process, a more direct attempt could be made to incorporate increasingly "human" assumptions, a suggestion which would seem to be in-line with the general increase in interest in behavioral research being evidenced by geographers. This increased interest in behavioral research among geographers might appropriately find some of its first application in studies of economic development which are directly concerned with the progress of people throughout the world.

Data and research priorities

There is another issue of a general nature which I would like to introduce, the constraint which data availability puts upon our definition of models to effectively pursue research tasks and our ordering of research priorities. It is often the case that one of the first complaints one hears from geographers who begin to undertake research within a development context is that there are no data. Although this lament has been heard in countless geography departments, what is generally meant is that data are not readily available which are suitable to the task which interests the researcher. In many instances this leads to the curtailment of a legitimate research topic. How important should this constraint be in our identification of problems? What role should data play in our attempts to formulate significant lines of inquiry into the development process? Where in the line extending from formulation to execution of research design should the consideration of data availability enter? Most would agree that consideration of that particular issue should only be taken up after the line of inquiry has been clearly delineated and the problem well formulated so that the nature of the data required can be clearly specified, I would suggest, however, that this is not often the manner in which consideration of data enters the problem formation process. In fact, data often serves as both carrot and stick for the researcher. Research problems are designed around some new data set that has been

discovered or research projects are bent so as to accommodate the data which are available. I would submit that research designs which are formulated solely on this basis amount to nothing less than academic imperialism. Researchers who search out available data without a basic commitment to or concern with the problems faced by developing nations do a disservice both to the profession and to the host nations.

If we allow the availability of data to direct our research, it may seriously curtail our efforts to understand the geographic aspects of development by 1) diverting our attention from other more pressing and basic research tasks in terms of the needs of the developing countries, and 2) divert attention from the very necessary task of theory development. It is most unlikely that there will be any "quick and dirty" way of acquiring a good theoretical base for our studies and that effort can only be adversely affected by undue consideration of data availability.

There is a further consideration of this nature which might be entered here. That is, if in the process of structuring a research design the researcher discovers that the data necessary to execute the research are not available, should this be the end of that research effort or could we suggest that the research design, particularly if it is likely to provide policy implications related to the development effort, might be used to effectively argue that the class of data in question should be collected perhaps on a continuing basis. This suggests that research activity may well have payoffs in ways other than directly through the completion of research tasks in that such information might have considerable impact in the planning of census activities of various governmental agencies. In fact, if we are interested in being active participants rather than passive observers of the development process, we might consider the possibility of giving some priority to studying precisely the data needs and capabilities of developing nations. Perhaps a full scale investigation is warranted into the question of which data are basic to the effective planning and execution of development strategies. Are we, as social scientists, accustomed at times to gloomily predicting the failure of development plans due to inadequate information, prepared to meet the challenge of specifying the "minimum required sets of data" for development planning? I am suggesting that what was once considered a liability in research might be turned to useful purpose and might provide an important area for those Geographers interested in data collection systems to make a contribution of considerable consequence.

A comparative perspective

In the light of my research as an Africanist, I believe that many scholars who choose to conduct their research in the countries of Asia, Latin America, or Africa are drawn to those areas by a genuine interest in, and fascination for, their distinctly different, often unique features because they appreciate the diversity among peoples and regions. Yet few, I think, are not impressed by the striking similarities they find in certain patterns which seem to recur in a variety of cultural and economic contexts. An example which comes readily to mind is that of the operation of periodic markets, a basic institution in the articulation of internal trade in many developing countries. Even a cursory examination of the literature on such markets is sufficient to suggest marked similarities which exist between marketing systems in areas such as Korea, China, West Africa or sections of Latin America.

The adoption of a cross-cultural or comparative perspective on the various problems of development may considerably enrich our research efforts. It is often through the identification of spatial regularities or the recognition of persistent relationships that theoretical generalizations may be formulated. Such comparative studies provide one method by which to identify consistently recurring patterns as opposed to those which seem culturally specific.

Summary

The comments and suggestions presented above are intended to initiate discussions which will aid in identifying research needs and ordering the priorities for future research. In this task, I suggest we would be well advised to re-examine our conception of development before proceeding to finalize our definitions of research needs and priorities. The discussions of research topics in this paper are thus meant to be suggestive of the directions in which research might move rather than definitive statements of priorities. The research suggested centers upon the relationship of urban growth and economic development, particularly the impact of alternate spatial structures of urban systems upon the transmission of economic development.

Within this general context, several other suggestions were offered. The first was that studies dealing with the human aspects of development should receive some priority. Second, that in our attempt to define research tasks for future studies of

development our emphasis should be upon research directed toward the formulation of theoretical statements concerning development and toward the specification of policy alternatives. While concern with data must still play an important role in the completion of these research tasks, it should not be permitted to direct our efforts. Finally, it was suggested that the adoption of an explicitly comparative perspective in studying development may aid in identifying those aspects of the process which are not culturally specific, and thereby aid in theory construction.

Obviously, these observations are only a few of the many which might have been made. However, they are offered as a basis for a discussion which, if successful, will begin to outline a framework for designating research needs and priorities.

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