B.L. Turner II
Clark University

Research Roles and Goals: A Commentary on the Study of Aboriginal and Peasant Cultures by Latin Americanist Geographers

Latin Americanist geographers are engaged in an ever expanding range of research which can be broadly labeled cultural ecology or anthropogeography. Just as I become confident that I have identified "basic research foci" in this sub-field, my colleagues confront me with additional new research developments. Certainly the research efforts in our sub-discipline, as pursued in Latin America alone, are of a magnitude that makes it difficult for any individual to keep abreast.

The three reviews in this section of the proceedings have covered the literature and classsified the research well. The wide range of topics with which our subfield has been engaged is clearly demonstrated. But while the topical coverage of our work is large, my attention has been drawn to at least two classes or avenues of study that are diminutive in number as judged by the bibliographies in the reviews. These classes involve research roles in interdisciplinary projects and research goals of building general theory.

I have had the fortune during the past seven years to participate in a number of interdisciplinary projects that have ranged topically from Maya prehistory to development in East Africa and that have included personnel with such far-flung specialties as ceramics, limnology, nutrition and econometrics. Each of these projects has demonstrated to me that competent synthesizers of cultural ecological materials who possess strong backgrounds in basic and field research are increasingly needed and in demand. Who fits this description better than the so-called cultural ecologists in this session? The specialists and generalists from other fields tend to have insufficient familiarity with both human and the environmental components of the problems, and tend to be less sensitive to the fine nuances that result from the interplay of livelihood systems and physical habitats. The time is ripe for the "specialized generalist" or mugwump, as Robert Kates (1967, 53) has so aptly labeled those geographers "who sit astride the social and natural sciences, mug faced toward one and wump solidly planted in the other," to take more direct and central roles in interdisciplinary projects.

Such a change does not imply that Latin Americanists of our research persuasion

have not been active in interdisciplinary work or have failed to provide quality syntheses of various man-land issues. Rather, our project participation has .been too limited and our syntheses are typically that of a secondary party to the actual research endeavor. These circumstances probably result from our legacy of individual research. But the future is clearly earmarked for group research, if only because the individual can no longer handle all of the various specialties applied in man-land problems and attempts to do so can lead to superficial assessments. One of our special roles should be as directors or central foci of such projects, be they studies of traditional Peruvian adaptation systems or small farmer development in Amazonia. We do, indeed, have special qualifications for this research role in that our training as synthesizers and manipulators of man-land data is as good as and, perhaps, superior to that of any other discipline. Recent events suggest that we are improving on this research role as several of our members have been engaged as central figures in large-scale, interdisciplinary projects such as the agricultural project in which Gene Wilken is participating. Interestingly, this work is in Africa, not Latin America.

The research effort of our sub-field as applied in Latin America has not gone unheeded, and various Latin Americanist geographers have made considerable contributions to interdisciplinary topics. We have been particularly strong in providing methodological approaches and field data and in interpreting our data in terms of specific theories. Unfortunately, we have not been as active as our sister disciplines in the direct development of general theory, an issue raised here by William Denevan. We have not held our weight in generating non-specific explanations and sustaining a measure of critique of them. We do a good job of "pecking away at the flanks" of general theory as developed elsewhere, but we have done it largely as "outside" specialists, not "inside theorists." I find this circumstance peculiar given our heritage of Humboldt, Ratzel, and Sauer, all of whom proceeded beyond description and specific explanations of events. Certainly we have splendid examples from our sub-field outside Latin America, such as the theoretical underpinnings of the work of Harold Brookfield (1972) and Karl Butzer (1980).

There are, perhaps, several reasons why this circumstance has developed. We have emphasized the role of field research that is temporally and mentally demanding. Too, the strong humanistic foundations of much of our work does not necessarjly stress theory building but focuses on other research objectives. Regardless of the reasons, it is time that a segment of our sub-field working in

Latin America enters that rich domain so dominated of late by anthropologists/archaeologists, rural sociologists, and agricultural economists. The establishment of the cultural geographer David Harris as a Professor in the Institute of Archaeology at London University may stimulate such direction in Great Britain and filter toward us.

The call for this venture comes not without a cautionary note. If I have correctly interpreted our sub-field, we have been able to maintain a high level of intellectual exchange without damaging collegiality among institutions or individuals. Indeed, at the risk of ruffling a few hairs, I suggest that in our desire to be collegial we have lost a measure of positive criticism that is healthy for any discipline or sub-field. But in the realm of general theory, a tendency exists for the establishment of schools of thought. Critique of opposing schools can become intense, if not personal, somewhat analogous to the so-called quantitative/cultural rift that once pervaded much of geography. We must attempt to overcome such tendencies. Controversy can be contained within the bounds of intellectual dialogue.

I close by offering an example of such controversy rooted in general theory and brought forth in this session by Larry Patrick. At issue is why agricultural terracing developed in Tlaxcala, which is a subset of the more general question of why agriculture is intensified. Patrick rejects the stress or pressure thesis used to explain agricultural growth in the central Maya lowlands (Turner and Harrison, 1978) as applicable to the Tlaxcalan case. In the former argument, the Maya are portrayed as expanding first across the well-drained mollisols of the uplands, practicing extensive forms of cultivation, followed by bajo or depression drainage and field raising and by slope terracing. In contrast, Patrick argues that the Tlaxcalans selected slopes for incipient cultivation, followed by terracing on slopes before other zones were utilized. He also contends that terracing developed in situ on slopes and that the practice was made possible by the development of the hoe, which allowed grass-fallow farming on the terraces.

Patrick has raised numerous issues, such as the temporal role of tools or technology in agricultural growth. I, for one, do not concur that only hoes can combat grass invasion, but this discussion (pecking away at the flanks) begs the more significant question. Does the Tlaxcalan case, as presented in this session, disagree with the principles of the stress thesis?

Our work on agricultural growth and decision making among traditional farmers demonstrates that rational input-output decisions prevail in context with local constraints, and that central tendencies in pan-regional farming responses can be explained in terms of three major decisional forces-production demand (an elaboration of population pressure), risk aversion, and least effort. I do not have time to elaborate but note, as have others, that as production demand increases the farmers' output goal interacts with numerous variables (habitat, alternative food sources, cultivars, and so forth) and results in a particular agricultural system (Turner, Hanham, and Portararo, 1977). Eventually a level of "stress" is reached such that it is necessary to increase output per unit area and time (agricultural intensification) and that continued increases commonly result in major landscape modifications such as terraces (Turner and Doolittle, 1978). This scheme in no way implies that terraces cannot develop in situ, that pressures of production over a large area must be acute, or that some sort of spread (diffusion) mechanism is involved. Rather it asserts that a general pattern exists in which farmers choose food procurement systems that are most efficient (that is, provide production demand with a good measure of security and with minimal inputs), given local constraints. The precise systems employed and their sequence of employment will vary from case to case, but the underlying explanations are similar. We should not expect the specifics of the Maya case to approximate those of the Tlaxcalan case, if only because the two areas differ considerably in physical conditions.

For the Maya of the central lowlands, the most efficient system of cultivation, as long as land was plentiful, was extensive agriculture (swidden) on the well-drained mollisols. Once these lands were filled or movement over them restricted, and production demand continued to grow, the Maya had to increase output per unit area and time. Such intensification was, perhaps, easiest to accomodate on level but well-drained lands. These lands were limited and, ultimately, intensive agriculture was undertaken on slopes, with terraces; and inundated depressions, with drains and raised fields. These techniques allowed for increased production but only at the cost of greater effort and less return per effort.

Patrick outlines a passage from extensive to intensive cultivation at Tlaxcala that apparently conforms to the basics of the stress argument but differs from the specifics of the Maya case. Slopes offered the most feasible zone for extensive cultivation because so many valleys were inundated. While production demand

was low (low stress), extensive cultivation was practiced on the slopes. However, as demand for production increased a shift was necessitated to intensify cultivation. Because of the valley bottom problems and, perhaps, spatial constraints on expansion, intensification took place on slopes, requiring terrace use. Ultimately the inundated valley bottoms were converted to intensive raised-field cultivation because of increased demand for food (Wilken, 1969).

In summary, the temporal scheme of Tlaxcalan terrace agriculture apparently conforms to the principles outlined in the stress thesis and used to explain agricultural growth in the Maya zone. The controversy here stems not so much from the generalities of the stress thesis but from its application. Clearly the specifics of the thesis will vary from one event to another, but this circumstance does not invalidate the principles involved.

Regardless of these issues, general theories of agricultural growth are elaborated by the data and interpretive challenges, and our appreciation for the complexity of explanations is enhanced. Even if I have misinterpreted Patrick's critique and alternatives, further elaborations of them, which must surely be forthcoming, will assist our thinking. This exchange enriches the intellectuality of our sub-discipline and the dialogue among ourselves and among related scholars.

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