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Notes Toward a Theory of Applied Anthropology

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This paper urges applied anthropology to return to its early concern with anthropological and sociological theory. That early work not only was closely tied to theory but also made significant contributions to it. The author suggests that the time has come for practitioners to develop a coherent theory of practice and offers some particulars on what such a theory should contain.

Key words: theory and application, history of applied anthropology, agricultural policy, Tlingit land use

When Ed Lehman created the byzantine organization under which the American Anthropological Association (AAA) now operates, the Society for Applied Anthropology’s (SFAA) officers said, “Include us out.” I don’t blame them and probably would have done the same. Yet, I think it was a sad mistake—for intellectual rather than institutional reasons. I had been a member of SFAA from nearly its beginning but left the organization when this happened—not out of pique, but because it reminded me that the articles in Human Organization were no longer useful to me after I turned more to theory. I did not share the antagonism and mutual suspicion that then prevailed, as shown by the statement of policy I made when nominated for president of the AAA. I said the association should promote the role of applied work and wrote: “We need to develop a climate where anthropologists will find their way into policy-making positions.” (Goldschmidt 1974:21). (Lehman said this would destroy my candidacy but he was wrong; the anti-applied sentiment was articulate but limited to a self-appointed priesthood.) We did several things on my watch, not the least of which was to publish, with the help of Peggy Sanday, The Uses of Anthropology (Goldschmidt 1979). The “subtext” of this book was that our history provides a charter for the legitimacy of the practical use of anthropology. Let us stop to consider this history.

Historical Considerations

The sentence that closes the first textbook in anthropology reads as follows: “Anthropology is essentially a reformer’s science.” This assertion by Tylor (1889:453) should be emblazoned on the escutcheon of the Society for Applied Anthropology. Perhaps Tylor was thinking of Morgan’s concern with the conditions of the Iroquois and, contrariwise, how much we had actually learned from them. Perhaps these words had been the inspiration for James Mooney (1896), who went to the plains of North America to

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examine the Ghost Dance movement. It was the first anthropological field trip aimed at solving a problem in the administration of a native population. Or perhaps it stimulated the anthropologist and colonial officer, Captain R. S. Rattray, to convince the Home Office that the Golden Stool of the Ashanti was not a piece of furniture but a sacred symbol of nationhood (Smith 1928). And a half century later Ruth Benedict (1946) gave a similar piece of advice regarding the emperor’s symbolic role in Japanese nationhood in her *The Chrysanthemum and the Sword*.

I like to think the first use of cultural knowledge to formulate social policy was drawn up by arguably the most significant committee of intellectuals and activists ever assembled to solve a social problem: the framers of the Constitution of the United States. They were not academics, let alone anthropologists, but men steeped in the knowledge of social history, amateur students of comparative society using data from classical times and the turbulent events of their recent past; knowledge they assiduously used to create institutions for an effective social order.

Our origins were more mundane. I was not present at the creation, which I date to the research at the Hawthorne plant of General Electric. There they discovered the informal ad hoc social systems that greatly influenced the behavior and satisfaction of workers—a system that neither management nor union had been aware of. Go back to Roethlisberger and Dickson’s *Management and the Worker* (1939) and read the description of the social controls in the bank wiring room. It is a powerful lesson in how society works, and it influenced social theory.

The Hawthorne Plant study influenced me directly—and indirectly through the works of Lloyd Warner and his students of American community life. Some of the earliest and best of this was by Burleigh Gardner, a largely unsung anthropologist, who had been my teaching assistant when I was an undergraduate at the University of Texas before he studied under Warner and collaborated with Alison Davis in the study of Biloxi, Mississippi, I think it was. I believe he was the first to earn his living as an anthropological entrepreneur. (As an aside, I should note that liberals attacked Gardner for recommending changes that increased worker satisfaction. They said this undercut the unions, forgetting that there are other satisfactions in work than pay.) These applied works were rooted in social theories from France (Durkheim) and Germany (Weber) and from the more down-to-earth Chicago sociologists (Thomas, Park, and Wirth). They injected this all-important social dimension into anthropological thought. Boasian anthropology, for all its breadth, was woefully ignorant of sociology and the issues surrounding social organization. It is a lesson in the interaction between application and theory.

Another influence came about the same time. It was drawn from psychology and contributed both to anthropological theory and the emergence of applied anthropology. Anthropologists turned to the ethnology of child rearing to explain the subtle out-of-awareness aspect of culture. They were seeking to demonstrate the validity of Freud’s theory of the sexual stages of infancy and, in the process, amassed data that overturned it. Their work had a salutary influence on the psychiatric practice that had inspired it as well as on attitudes about child rearing in America. Margaret Mead’s influence on Spock was arguably the most widely effective and significant bit of applied anthropology ever. With some embarrassment I recall my initial reaction was: “This is nonsense; children just grow up.” I soon saw differently. Studies sponsored by the Indian Service and the Committee on Human Development at the University of Chicago by Erik Erikson on the Sioux and Yurok (1963) and by Laura Thompson and Alice Joseph on the Hopi (Thompson and Joseph 1944) became major influences on my theoretical outlook. In my use, this involves shifting the oedipal complex from involvement with the id, as Freud had it, to involvement with the ego, the sense of self. I see this as the perfect paradigm for the relationship between theory and practice. Anthropologists had discovered that cultures varied in their deep psychological outlook. They came to assume, rightly, that this had to be a product of child rearing and applied the Freudian psychoanalytic theory of sexual development in the child. Their work proved the theory wrong. So the next step should be to correct the theory.

Stop to think about this a moment. I have just argued that an anthropological test of a psychological problem is resolved by applying sociological concepts. Some 50 years ago I made an analysis of the Hupa-Yurok social system using that multidisciplinary paradigm (Goldschmidt 1951). It is wrong to think of applied and theoretical anthropology as distinct enterprises; they should be mutually supporting. I urge you to close the gap and be more interactive with the discipline that gave you the wisdom with which you face the daunting task of telling people in the world of practical matters how they can better their condition. I want you to make your findings more readily available to anthropology in general. It will make your lives easier and perhaps richer if you keep the connection alive; it will certainly make ours richer.

**Application as Validation**

But most important of all is the fact that application demonstrates the validity of theory. This is vital in all the sciences. Science has conquered faith-based understanding by being better at solving practical problems. Can you imagine economics without involvement in the markets; research in medicine and health having no contact with pharmacists? The skepticism and doubt inherent in scientific inquiry are less comforting than blind faith and can be overcome only with proven results. This is the case in all scientific explanation, but it is most vital with respect to the social disciplines because our work cuts closer to the soul, because our successes are less self-evident, and because we cannot engage in experiments. This last means that we must get our data where we can, and one way is to use the natural experiments that circumstances throw our way. Studies in applied anthropology can be a major source for such windfalls. I suspect that
applied anthropologists cumulatively do more original field research than all of us in the academies do—except, perhaps, for the graduate students. Many of you have a corpus of knowledge and a rich lode of case material that can support, correct, or refute theory, and it is a great loss when it is moldering in your files.

That excellent compendium by Elizabeth Eddy and William Partridge (1978), now nearly a quarter century old, had a section headed, “The Dialogue Between Theory and Application.” When you read Connie Arensberg and Freddy Richardson and Ward Goodenough, you see minds that show the influence of theory on their thinking and their applied anthropology as contributions to that theory. When I asked Barbara Pillsbury, my friend and mentor on applied matters, to bring me up-to-date, she sent me to the NAPA Bulletin, The Unity of Theory and Practice in Anthropology: Rebuilding a Fractured Synthesis (Hill and Baba 2000). Despite the promising title, I did not find the sense of dialogue I want.

I know it takes two to tango and that the problem lies as much with the theorizers as with the appliers. I have said elsewhere that anthropological theory has lost its way (Goldschmidt 2000). Neither the postmodernist cant nor the pseudobiology of the evolutionary psychologists can be of any possible use. If paradigms are out, as Marcus and Fischer (1986) allege, then you can’t understand why things happen, and if you can’t understand the causes how can you possibly know how to correct them. At the opposite end, the biological determinism of evolutionary psychology would make eugenics the only cure for social ills (see, for example, Ridley 1997).

These theories, however, were preceded by the theory of cultural ecology. The concept of ecology was developed by evolutionary biologists to express the process of adaptation as an element in the evolutionary process. Julian Steward (1977) saw it could apply to cultural adaptation as well. Unfortunately, the scientific use of the term has been undermined by its sociopolitical uses, which have led to a tendency to make simplistic causal statements. Roy Rappaport’s (1968) classic study that explains pig rituals as ecological adaptation is but a “just so” story. It is reminiscent of Dr. Pangloss explaining the nose as existing to support the eyeglasses. But real ecological adaptation is a true dynamic in social life. My work in East Africa, based on my evolutionary theories (Goldschmidt 1959), showed the social consequences resulting from changing technology under natural conditions (Edgerton 1971; Goldschmidt 1976, 1987). True ecological adaptation is a powerful tool for understanding the dynamics of social relationships in modern life. The invention of the typewriter must certainly have had a major impact on office life late in the 19th century; the computer certainly did late in the 20th.

Notes for the Theory Volume

At this venue I am preaching to you applied anthropologist and not to theorists, and in the remaining time I want to talk about what you should be doing. The first step is to develop a theory of applied anthropology, and the first step in doing that is to write a book (or books) developing such a theory—and I am sure that at least one of you will. So what I am going to do in the rest of my time here is to set forth some ideas for what this book should contain. Admittedly, it is easier to make suggestions for a book than to write one, but I am certain whoever picks up this challenge will get a measure of fame and fortune. And a lot of heat.

The first point is that we are talking about applied cultural anthropology. Each of the four fields has an applied side: physical designs airplane seats (under contract with the Kalahari Airlines, I suspect), salvage archeology works to get in the way of new construction, and applied linguistics decodes Sony manuals. This shows that applied anthropology is not a fifth field but an aspect of the existing ones.

The next thing is to find out what we as anthropologists do that distinguishes us from what other disciplines do when they are called in to advise on organizational problems. Why ask us? What special things do we bring to the table? We do not solve engineering problems or wage disputes. We are asked in when there is some failure in human relations, some breakdown in organization. Whatever the specifics may be, the situation to be studied always involves populations with internal variation, where some of the people engaged in the joint purpose have expectations and an outlook that is different from that of others in the enterprise; one that can lead to misunderstandings and conflicts that threaten the overall functioning of the institution.

Our chief stock in trade is culture. But culture comes in many shapes and sizes, and it is our chief task to be clear just what in culture we are dealing with. This, in turn, means we must face a practical matter. Much of what we do involves exotic peoples, that is, peoples with manifestly different customs and daily lives from our own. We are often called in on problems that involve tribal people as well as nations with different practices than our own. Under such circumstances our expertise may simply be taken for granted and usually our role under such circumstances is clear. But we are also increasingly dealing with aspects of our own society and this gets us into a very different kind of involvement, into the minutiae of culture.

Let me illustrate with two examples. When I went to Alaska in 1946 to record the land rights and uses of the Tlingit and Haida (Goldschmidt and Haas 1946), I had the rather straightforward ethnographic task of learning what lands were owned by whom and how they were being used. My client, the Bureau of Indian Affairs (BIA), needed information about native perceptions of rights to the use of the land and the uses to which it was put. The result was a monograph that established the legitimacy of property claims made by Tlingit clans or houses. What was straightforward ethnography to me was alien to the BIA (despite a century administrating native affairs), and so I had to interpret native customs. I had to show that Tlingit clans and houses, though built on kinship, could be seen as corporations, that their leaders were
chief executive officers (CEOs), and that potlatches were not just give-aways but also served as “contracts” in which the guests who got the gifts were witnesses to “deeds” often recorded on totem poles.

In contrast, when Bob Edgerton (1967) studied the lives of the mentally retarded at a psychiatric hospital, he not only had to describe their culture—he had to discover that they had a culture. Bob told me he was the first social scientist at that institution ever to have gone to the back wards to talk to patients and observe their behavior—itself a good reason to use us. Bob found that the daily life of these mental patients was dominated by efforts to protect their sense of self against the onslaughts brought on by their own incompetence and by the demeaning (note, demeaning, not cruel) treatment they receive from their keepers. He describes in poignant detail the wiles and small deceptions these mentally challenged young men and women used, like wearing a broken watch and pretending it had just stopped when they had to ask someone what time it was so they could cover for their inability to tell time. After release from the hospital, the stigma of hospitalization became a major threat to this image. Castration not only deprived the men of a vital function; it also lessened their self-esteem and inflicted a stigma that became impossible to hide. Bob says that these mental retarded persons wore The Cloak of Competence (to use his title) as false as the emperor’s to preserve their sense of self. His book has had a major impact on the treatment of the mentally retarded; it is deservedly a classic. It is also a prime example of an applied work in relation to theory, for Bob was working out of the social theory I had developed in Man’s Way (Goldschmidt 1959), and I was gratified he found it useful.

Some Theoretical Suggestions

This leads us directly into examining what anthropological theory is relevant. I don’t expect you to be surprised that I turn to my own theory on the person in cultural definition (Goldschmidt 1990, 1993). I see culture as sets of socially induced motivations, as being those aims and desires that are not directly tied to the instinctual urges for food and sex; they come instead from the demands or expectations of the community as they have been mediated by parents and others. They are the desire for the rewards of acceptance, praise, or honor for acts, behavior, performance, or accomplishment. These responses serve the ego and the sense of self. They nourish the symbolic self, just as food nourishes the body, and they are equally important to the continuing health of persons as they move through life. I believe most of the problems you must face when you try to rearrange matters in an institutional setting involve in greater or lesser degree some impairment of such forms of gratification.

Since the primary sentiments underlying such motives are laid down in infancy, they are deep-seated and indestructible. In tribal and other homogeneous societies, the infantile experience tends to be uniform because parents transmit consistent signals as to what is expected and, at the same time, these very virtues are constantly being reinforced by the shared myths, rituals, law, and the like. This consistency (it is never uniformity) lays down deep-seated attitudes that set a characteristic tonus to such homogeneous cultures, rendering them highly resistant to change and forming what historians call “national character.”

Indeed, in America (and increasingly in all modern nations) immigration from diverse homogeneous cultures has made our nation into mosaics of such diverse cultures. Not a small part of your problems involves sorting out the various cultural predilections and the way they can breed conflict. This is not merely a matter of understanding and tolerance; it involves the differences in the needs and capabilities of different peoples. Some cultures are clearly pre-adapted better to the demands made in our society than are others.

But the kind of subculture that Edgerton found does not have that deep and consistent background of such ethnic communities. We are here dealing with matters so different that we must stretch the meaning of culture to apply it. Yet I do believe it is proper to say that the mentally retarded persons Bob studied, the group in the bank wiring room, and many other studies can best be understood in terms of culture, however situationally defined and transitory they may be. We must be aware of the different implications such ephemeral cultures have from the tribal cultures of anthropological tradition. But when we look at what motivates the people in such subcultural situations as the bank wiring room, we learn that “the bottom line” is not always calculated in cash. This I believe to be the most important lessons from anthropology, and it has a major factor in solving problems in applied anthropology.

This recognition of alternate personal aims leads us to the consideration of the context of social events and the concept of holism. Certainly a chapter of our book must be devoted to these perceptions. We take holism so much for granted that we tend to overlook both its importance and its absence from other disciplines, especially economics. It is as central to our viewpoint as culture itself and, indeed, culture is a holistic perception. I have already pointed out that my own theory of human behavior must combine sociology and psychology to make sense of the world as I find it. But let me show how holism can affect practical matters.

My study of social adaptation of cattle-keeping peoples in Africa was initiated to test the theory of cultural adaptation (Goldschmidt 1959), but it proved relevant to the “pastoralist problem.” Nomadic herders graze their livestock on arid lands unsuited to more productive uses. They are mobile, independent-minded, difficult to count or to tax, frequently smugglers, and given to fighting. Nations have disliked having them in their midst for at least four millennia. (I am not making this up. I had been visiting two graduate students studying Kenya’s efforts to settle the pastoral Maasai and stopped on my return home to visit the excavation of a 4,000-year-old city on the Euphrates. A paleographer working on the documents of a contemporary neighboring state told us that that city-state had collapsed when it antagonized the herders in their hinterland by trying to settle them down.)
For a conference on pastoralism in Nairobi, I looked at the efforts that had been made over the preceding several decades to solve the pastoral problem (Goldschmidt 1980). Colonial administrations, independent African governments, international banks, and nongovernmental agencies, all repeatedly came up with the same "solutions"—none of which worked. Mostly they tried the "obvious" one of digging wells to pump water. This, it turns out, makes things worse. The only successful program I found was dipping the animals to kill the ticks. The herders took to it, but in the end it proved tragic. Decreasing livestock mortality increased herd size, causing overgrazing that left the land and the people worse off than before. The holistic view that comes naturally to us led to a solution that lay elsewhere in the social fabric; namely, to improve market facilities to drain off the excess animals. They knew about this need, but never seemed to connect the two.

I spoke earlier of anthropology’s entry into the study of American towns. Following the model of the Lynds’ (1929) famous sociological study, Middletown, a spate of such studies was made; ethnographic descriptions that treated the communities much as if they were tribes. My research on Wasco, California, was part of that tradition (Goldschmidt 1947). I did not go as an applied anthropologist, but it did give me some insights necessary to the anthropological approach that should get a chapter in our proposed book. This is to recognize that things are not always what they seem. We anthropologists tend to look under the surface of events for latent functions and deeper meanings. I found hidden realities under native misconceptions and am sure many of you have had similar experiences. Two of the major institutions in Wasco did just the opposite from their stated purpose (Goldschmidt 1995). The mutuality and social unity that the Christian ethic propounds was not found in Wasco’s Protestant churches. Instead, the several sects formed a veritable stairstep of social standing, ranging from the disenfranchised Okie evangelists shouting their faith in tents on up to the leading merchants’ and bankers’ sedate rituals in elegant temples. (Though there was only one Catholic Church, the priest said he held separate masses for the Mexican migrants and the town’s elite.) On the other hand, the schools, whose chief function might be said to sort out people in terms of their capabilities, proved to be the one unifying institution in the town, bringing Mexicans, African Americans, and Okies into direct, though tenuous, contact with the elite.

Such disconnect between reality and perception can be important. I am a card-carrying member of the Sapir-Whorf thesis and convinced that people regularly treat the world in terms of the words they use. In Washington, issues are allocated according to the verbal definition of the situation. Farm problems must be solved in the U.S. Department of Agriculture (USDA), even when the problem derives from tax or labor policies. Uncovering the falsifications hiding undesirable reality, however, may lead to unpopular solutions. My California research undermined the myth of the “family farm” that the industrial elite used to its advantage. This led me into some unwanted notoriety. The story is a warning.

Caveat

Farming in America has had a dynamic history: from pioneering homesteaders to struggling yeoman to harassed commercial farmer and now, increasingly, to industrial agriculture. California’s agriculture was industrialized early in mid-19th century by a combination of environmental and historic factors: a soil and climate that made it possible to raise a great variety of high-value specialty crops, the Spanish heritage of large tracts in private hands, the availability of abundant cheap minority labor released by the completion of the transcontinental railroad, and the marketing opportunity made possible by those railroads. This led to agriculture production dependant on masses of low-paid labor, supplied by a series of ethnic minorities that were being induced to come to California to maintain the supply of cheap labor, culminating (at the time of my study) with Okies. Financial and other urban corporations dominated both the farming enterprises and the local small town, promoting policies to their advantage under cover of the myth of the family farm. The most crucial of these policies was getting farm labor exempted from the provisions of the National Labor Relations Act, Roosevelt’s important innovation for protecting the right to unionize. This denial has preserved the disadvantaged status of farmworkers to this day and supported the continued immigration of low-paid workers, many, of course, from Mexico.

After I made the Wasco study I was asked to do my first piece of applied anthropology, investigating the effect of large farms on the quality of rural life (Goldschmidt 1946). Should the acreage-limitation law be applied to the lands made irrigible by the massive Central Valley Project (CVP) then under construction? This law limited the size of farms that could get water from the project to 160 acres, the traditional size for homesteaders and veterans’ grants. It applied to water developed by the Reclamation Service, which financed the CVP. Putting water on arid land increases its value tenfold. Southern Pacific, the Los Angeles Times-Mirror Company, and other powerful interests owned giant tracts of land in the project’s target area and would—and did—benefit greatly from this public largess. These interests fought the rule—and they fought me. To answer the question as to the virtue of applying the acreage-limitation law, I compared a town with family-size farms to one surrounded by great holdings, both with industrialized farming. In every measure I could devise, I found colossal differences in the quality of life between them. The Associated Farmers, then the cover organization for the industrial interests in California’s agriculture, began attacking me while I was in the field, an attack that found its way to the halls of Congress and the offices of the secretaries of agriculture (Goldschmidt 1978). They tried to stop the research altogether; induced the USDA to forbid me from doing a second study of all the towns in the target area; and kept it from publishing the study. It was rescued by Senator James Murray of Montana and published by his Senate Small Business Committee. Senator Downey of California spent the remainder of his public career attacking
the bureau for which I worked and closed its Berkeley, California, office (leading to my departure for UCLA), and ultimately destroyed the only social research agency in the USDA. To this day it has none worthy of the name.

I wonder how many of you have had similar heart-rending and anxiety-provoking experiences. Perhaps I am accident prone; just a troublemaker. My Alaska work was certainly not popular with white Alaskans. The World Bank was incensed at my paper on the pastoral problem, but it did undertake an in-house study of the issues I raised. I feel strongly that the very nature of the work we do and the egalitarian values we bring to it are apt to incur a lot of displeasure and even hostility, often from the very people who hired us. Perhaps our book should carry a warning: Caveat lector: Not for the weak of heart; applied anthropology may be dangerous to your health.

These are but some suggestions as to what should be covered by a treatise on the theory of applied anthropology. They do not exhaust my repertoire and each of you will have your own ideas on what it should contain. They are offered to inspire in you the need to pursue such a course by showing how important such a work can be for both yourselves and for theory itself. They have been made less with the thought that each item is essential than with the conviction that the whole needs to be done.

Some Final Thoughts

Underlying everything I write is the assumption that people everywhere work for psychological rewards that give them a sense of self. It is an anthropological truism that such rewards vary widely from culture to culture. Some are for instant gratification; some may only be received in some distant afterlife. You are not free of this burden. Nor am I. And so I want to conclude with some observations on my own sources of gratification; the rewards for which I have devoted a lifetime in the study of anthropology. Being here in Merida with you and receiving this reward has made me reflect on such matters, and I want to share with you some of these intimate thoughts. They relate to this matter, of theory versus application.

Wherever my soul, if an atheist can be said to have one, ends up in the postmortem world, it will rest in peace only if my theoretical contributions find their way into the canon of anthropological lore. Only if it can torment future generations of graduate students by having them read my works—even if they are misreading them—will my soul find true peace. I am not sanguine about this. So many who have preceded me to an anthropological purgatory like Clyde Kluckhohn, Ralph Linton, and Pete Hallowell—all heroes of mine—already seem to be lost from the must-read list. Nevertheless, in some strange, ethereal way, I find the business of theory the most compelling font from which to shape my self-image in the everlasting future.

But in the real world, in the here and now of gratification, I find that my forays into applied anthropology have given me the most intense pleasure. When Tom Thornton wrote me that he was finding both Indians and administrators poring over xeroxes of xeroxes of that half-century old mimeographed report on Tlingit land use and rights, my ego was given a real boost, knowing that I had done something truly worthwhile. Learning that I had contributed to the welfare of individual Tlingit Indians and to the tribe as a whole let me know that my work was not wasted. That the old monograph was edited, expanded, and gussied up by Tom and published by the University of Washington Press (Thornton 1998) just adds icing to that cake.

Learning that Kendall Thu and Paul Durrenberger, inspired by As You Sow (Goldschmidt 1947), were investigating the problem of industrialize agriculture that I had tackled a half century earlier gave me even greater pleasure. The publication of that work in 1978 inspired a cottage industry among rural sociologists and anthropologists to test what they called the “Goldschmidt hypothesis.” It was interesting to see that none of the sociologists, working mostly with census data, seemed to get my real point—but anthropologists did. Especially Thu and Durrenberger (1998), and others like Laura DeLind and Dean McCannell. Now watching Kendall take on the Farm Bureau Federation and the Smithfield Packing Company, makes me feel that my suffering was not in vain and reminds me that the power of the pen is real; that at least occasionally research has consequences for which one can be proud. I wish I could tell this to my source of inspiration, Paul Taylor, and he, in turn perhaps, to his, the eminent labor economist, John Commons. And so on.

Perhaps also Gamio, Palerm, and the other pioneers of applied anthropology here in Mexico, who long ago tried to ease the indigenes of Chiapas into the modern world, are celebrating these days, knowing that what they were doing 70 years ago is finally paying off.

Both my soul and my ego would be gratified to learn that my words here have inspired a work of the kind I have envisioned and, thus, reawakened a constructive dialogue between theory and application.

Notes

1Readers should know that what they read here is not exactly what I said in Merida. Just as I was castigating the deconstructionists and the evolutionary psychologists, the lights went out and for the next half-hour or so I extemporized. The lights went on again in time for me to read the concluding paragraphs.

2I use this classic case as a synecdoche for the informal subcultural entities that anthropologists and sociologists have found repeatedly. For those unfamiliar with the case, it was a room where workers assembled devices in collaboration. They were paid piecework rates and preserved the pace by inducting each newcomer into the group’s expectations about work behavior. Such novices had to choose between being rate busters or gaining acceptance in this little community of workers.

3The myth of the family farm is the perception of agricultural production as being in the hands of innocent and needy farmers, each operating a small parcel of land as a family enterprise and using only casual “hired hands.” This was a reality for much of America in the 19th
century, but not in California or the antebellum South and some other places. This myth formed the basis for exempting farm operations from many rules and policies that industrial operators did not like. These policies gave advantages to industrialized operators and ironically destroyed the remnants of the reality that underlay the myth. We now see this taking over farming in the American heartland. The myth is now being used as an argument favoring elimination of estate taxes, even though it is well known that no farm has ever been lost on account of such taxation.

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