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A. Theoretical Considerations.

One of the principal contributions of cultural and social anthropology has been an unending stream of holistic "ethnographies" and "community studies," which purport to sum up the entirety of the lifeways of a specified group or community. Whereas other disciplines have limited their focus to special aspects of the human phenomenon, anthropology has retained the ambitious (perhaps naïve) goal of dealing with the totality.

This holistic outlook usually translates itself into the authoring of a document with a fairly standardized Table of Contents, one chapter for this, and another chapter for that domain of life. And field work often turns out to be the attempt to gather enough material to write a presentable, decent-size chapter on each of these areas. This done, the ethnography has been accomplished.

Despite its drawbacks this tradition has been of value to behavioral science, in keeping alive concern with the whole and concern for what is local and "down to earth." Given the state of the art, it would have been difficult to do much better. But it has been illusory holism, disjointed, and of necessity exposed to subjective biases as to what is reported and how much page-space is dedicated to each "domain."

Many anthropologists have now repudiated, either implicitly or explicitly, this concern for the whole. One ten-

dency has been to define the totality in terms of mental entities, and ethnography turns out to be the intricate analysis of conversations with informants. Observation of community events becomes a secondary concern, carried out tangentially to the more important task of finding out what informants think, or not carried out at all.¹ Other anthropologists, while retaining interest in observable events, restrict their observation to one domain of a community's life and criticize the goal of holism as being now unrealistic, given the changed nature of local communities.²

But despite trends toward the study of "mental" phenomena and the study of limited, selected domains within the community, the tradition of holism and of concern with external events runs deep and is unlikely to disappear from the scene. Rather, given the obvious difficulties associated with the increasing involvement of local communities in world-wide currents, we may hope for the development of new concepts and tools capable of dealing with the whole, not the abandonment of the goal.

One step in the development of such an instrument is The Nature of Cultural Things. In this work, Harris develops a model set of operations for observing and coding the events which occur as individuals behave. A firm commitment is made to viewing the behavior of individuals as the bedrock data, and to maintain a clear conceptual distinction between the verbal and non-verbal subsets of this behavior stream, though recording both.

The ethnographer is surrounded by individuals in different types of motion acting on material objects and interacting with other individuals. The behavior stream of individuals--including the verbal signals emitted to each other and to him the ethnographer--is the fundamental data which the ethnographer has at his disposal.

Given the nature of the data, the task is to develop a data-language which, though dealing with higher-level phenomena, remains faithful to this bedrock source of data, the empirical behavior stream of real-life individuals. The language which traditional social science teaches to its neophyte practitioners is to a large degree a universe of discourse filled with time-hallowed lexical items whose definitions, i.e. reference--is often a matter of uncounted hours of heated discussion.³ The difficulties associated with these lexical items--of which "culture" itself is a prime example--is frequently attributed to their "abstract" nature.

But this is doing an injustice to the process of abstraction. Abstraction is a systematic, step-by-step logical operation anchored in bedrock empirical referents. When hours and books must be dedicated to discussing "what" a term is, we are dealing not with an "abstract" term, but with a referentially empty one. And the danger is that the neophyte field worker's involvement in an academic hierarchy, under specific scholars, will result in a greater commitment to certain labels and to devising

possible ways of applying them to his community, than to observing and recording the behavior which is unfolding before his eyes. In short, addiction to an unsound data language can effectively prevent perception of what is happening on the ground.

The operations presented in the Nature of Cultural Things are an attempt to devise a coding scheme--i.e. a data language--~~which~~ remains anchored in direct observation of the commonplace details of daily life, but which permits systematic construction of "higher order" phenomena. These operations permit the researcher to deal with higher order questions, but prevent his eyes from losing focus on the rich, event-filled behavior stream unfolding in his community.

The mandate to observe and record daily events is certainly nothing new in anthropology; but the mandate has been given a high level of systematization in the Nature of Cultural Things; and the lines between this and other approaches to fieldwork are more clearly drawn.

But it is one thing to agree with the set of operations, and quite another to convert this model program into a feasible field endeavour. The experience of doing fieldwork within the observer-oriented frame of reference proved to be quite different from working under a mentalistic frame. My first contact with Pino Tumbao had come in 1967, when I gathered data (for a senior Honors Thesis in

the Department of Social Relations at Harvard) on the attitudes of the people toward different aspects of their life. Though the transcribed interviews permitted me to make statements about how informants said they felt and thought about many things, I had very little notion of specifically how they stayed alive, because the theoretical frame of reference with which I went to the field did not motivate the type of question that would have given that information, nor sensitize me to the events occurring around me.

During this past summer, however, my attention was called to quite different types of phenomena. Summed up simply, the stance taken in The Nature of Cultural Things makes one equally sensitive to what people are actually doing as opposed to what they are saying. This distinction should be so obvious that it need no repetition. But a perusal of most ethnographies will show that the distinction has not always been kept in mind. And adherence to this obvious distinction resulted in a very different type of fieldwork from that done in my first summer in Pino Tumbac.

Essentially, one's pivoted to a different class of phenomena. One is extremely alert to micro events which before passed unnoticed--changes of clothing, food-giving and food-lending between households, subtle types of observably deferential behavior by peasants to the powerful people in the community. Since, in the actonic framework, such events constitute the bedrock data, they must be recorded as accurately as possible.

But note taking then becomes an almost constant and tiring activity; a pile of notes rapidly accumulates; and the question begins to loom; where are all these notes leading to? Even when one is convinced that this type of information is the backbone of a valid ethnography, the question of what to do with the voluminous material can be perplexing.

But the same program which encourages the collection of behavioral data also provides the model for ordering this data. For in reality the superficially complex helter-skelter jumble of unknown people running around doing unfamiliar things with unrecognized objects is on a fundamental level quite strictly ordered. The task is to bring this chaotic jumble--as reflected in unorganized field notes--into conceptual order. Whereas anthropologists of an emic bent insist that the key to ordering life in the community is to get a glimpse of the cognitive map of the people involved, the observer-oriented program looks for the key in terms of longer regularly recurring behavior chains. Each of these micro-events regularly occurs in conjunction--and in frequent logico-physical concatenation--with a limited number of other micro-events. And the initial chaos of these micro-episodes gradually takes on an ordering in terms of a finite number of higher-order chains in which they regularly occur. To find out the meaning of an event, then, find out the longer sequence of which it is regularly a part.

Once these short-range chains have been isolated, they provide the basis for constructing longer, higher-level entities called scenes. But throughout the entire abstracting and coding process, a firm commitment is retained to the empirically and intersubjectively observable episode as the basic constituent of the data. At any given point in the ethnography, and any level of abstraction at which the discourse is proceeding, the reader should be able to work back to the individual behavior which constitutes the bedrock data. That is, the reader will know exactly what on-the-ground observable class of events the ethnographer was referring to when he makes this or that assertion.

If this program is carried through faithfully, the product will be an ethnography in which even higher-order "abstract" concepts can be traced back to the actonic data which supposedly underlie it. This commitment to episodes as the basic data is founded on the conviction that the nature of the target phenomenon demands this type of handling; that failure to anchor one's ethnography in this bedrock actonic data will result in pages of abstractly empty jargon which--though elegant, titillating, and in conformity with current social science fads--have little to do with the real-life behavior occurring in the community, which in the final analysis is all that the ethnographer had at his disposal.

In line with the ethnographic program of the Nature

of Cultural Things, the focus of observation and recording became the behaving individual. The sum total of the movements by which an individual interacts with the environment is his behavior stream. One of the central tasks of the ethnographic endeavour is to make statements about the behavior stream in his community. It is supposed that the behavior stream of individuals in the community, though at a minute level of observation is completely unique, nonetheless can be segmented in such a way as to isolate and highlight crucial similarities with the behavior stream of other individuals in the community. It is the task of the ethnographer to discover and report these commonalities in the behavior stream of the different individuals in the community. It is further supposed that, though each behavior stream observed will manifest similarities with every other behavior stream--such as eating, sleeping, defecating, walking, talking--there will appear gross differences in which, for example, the behavior of some individuals will be directed habitually toward one group of actone objects (such as machetes, crops, live animals, etc.) whereas that of others will be directed habitually toward other groups of actone objects (e.g. pots, pans, brooms, ovens, etc.). It will be useful to isolate these regularly recurring differences and construe actor classes in the community on the basis of these empirically observed similarities and differences.

The different classes of individuals represented in the verbal repertoire of the community (such as hombre, mujer, agricultor, comerciante, quemadora de casabe, rezadora, alcalde, and many others) provided helpful maps to guide initial observation, but on closer observation were found occasionally to cloak important differences. Such, for example, is the case with peasants who gave credit to poorer neighbors but who were called agricultores as well, there being no commonly used term in the verbal repertoire of the community to refer to the individuals who engaged in this special and economically important activity. Thus the final establishment of these actor classes is on the basis of observations of the behavior stream, not on the basis of the terminological practices of the community. This stance is in contradiction to that of those ethnographers who elicit these local verbal items, label them "cognitive categories," and insist that the ideal ethnography proceeds on the basis of these folk labels (which supposedly give access to the mind of the people).

Not only do differences in the behavior stream suggest the profitability of establishing different actor classes. But the behavior stream of a given individual may be profitably segmented into different domains, primarily on the basis of changing involvement with different groups of action objects and different individuals. Some such distinction has been at the basis of the traditional seg-

menting of an ethnographic report into chapters on "Economics," "Family Life," "Religion" and the like.

But these traditional ethnographic chapter headings must be treated in the same way that the verbal labels of the community as to actor-class were treated: they are initial common-sense guides to start observing and sorting one's field notes. But the question as to the final categorization of the behavior stream and the table-of-contents of the ethnography is hopefully determined by the results of observation of one's community.

From watching what different individuals were doing morning, noon, and night, and from noting the topics which occupied their conversations with each other, it became clear that most of the time the adult males of Pino Tubbao were involved in some endeavour which, in the context of logico-physical prerequisites, formed part of the behavior chains terminating in the production, harvesting, transportation, and exchange of cows and pigs. Even in conversation scenes, when the signal exchange concerned objects and events not immediately present to the speakers, almost inevitably the topic of conversation was some aspect of the agricultural or livestock sequences. It was quite obvious that in terms of sheer man-hours, both of physical activity and verbal interaction, this class of activity in the farmers' lives far outweighed all others. Even at night, in the patio or the family bohío (house, aboriginal name), some aspect of the

cultivation and animal-raising chains were being carried out or talked about. It was a rare occasion when the adult male residents of Pino Tumbao were not involved, either physically or verbally, with their crops and their animals.

This fact should have great relevance for the "Table of Contents" of the ethnography of Pino Tumbao. In contrast to data from my first field trip, in 1967, the information I gathered on this field trip was heavily concerned with the material aspects of life in the mountain community. In terms of man-hours of involvement, or of conversations, areas such as fiestas and religion do not appear to play the important part in the life of Pino Tumbao that they apparently play in some other Latin American communities, judging from the attention they have been given in other ethnographies. This impression can be interpreted in at least two fundamentally different ways:

1. Since Pino Tumbao was recently an unsettled demographic frontier and has recently been hit by several natural and governmental blows, economic struggles do in fact engage more of the time of the mountaineers than of other peasants.

2. Or: the choice of focus in an ethnography is predominantly a matter of personal preference of the ethnographer, and bears no necessary relation to the focus of life in the studied community. This would mean that the predominance of economic data in my field notes is a result of my bias, just as the predominance of ritual matters in Herskovits' Life in a Haitian Valley (which is geographically very close

to Pino Tumbao) is a result of a bias on his part.

There is as yet no objective technique for comparing differential importance of different spheres of activity in two different communities. Nor is there any reliable guidelines for allocating page-space in an ethnography. And so the end product appears to be up to the tastes and biases of the individual ethnographer, and ethnography appears doomed to unavoidable subjectivity.

But I believe an argument to the contrary can be made. The heavy predominance of notes concerning the economic processes in Pino Tumbao was in a sense the product of an actonic, observer-oriented approach. Since the time limitation imposed the need for strategic selection of events and processes to observe and record, the heavy preponderance of economic activities and conversations, already alluded to, signalled this sphere as being of central importance ethnographically. When the principal research instrument is the interview with an informant, the ethnographer enjoys the luxury of a choice as to which domain he will elicit information on. But when the principal tool is the observation of the behavior of people in their daily rounds and the conversations of the people with one another, the choice is not as great. If one is purporting to do an ethnography, one is bound to dedicate a great deal of attention to those matters toward which the members of the community dedicate a great deal of time and energy in their daily lives. If the people are involved with farming and

animal-raising, and talking constantly about that, to focus on fiestas, religion, cognitive structure, or the use of "usted" is to risk missing the central sequences around which the lives of the people are constructed.

To deny that these latter are legitimate fields of inquiry, or to propose that 80 pages out of 100 must be on economics if eight-tenths of the people's waking hours are spent in these activities, would be dogmatic nonsense. But given the nature of the data--the behavioral events which commonly predominate the scene in Pino Tumbaco, an ethnography that does not give a full picture of the long linked chains involved in conuco making, coffee growing, extensive livestock grazing, and cooperation between households, etc., can be accused of having missed the point.

It was this conviction (or bias, some would say) which gave the data collection its specific orientation. Needless to say, selection had to be made as to which behavior chains and scenes were the critical ones for observation and recording. My initial attempts to record everything possible soon fizzled out, and a more realistic program gradually emerged.

The concept which came to the fore to dominate my perception of the affairs in the community was that of the nomothetically occurring nodal chain. At any given moment, in a scene of any complexity, the behavior of each individual can be placed on some regularly occurring

obtain. The uncounted thousands of episodes which constitute the behavioral repertoire of any adult individual occur not randomly but in fairly regular sequences, and under fairly regular scenic settings. Thus it is possible to bring very elaborate behavioral repertoires into manageable conceptual order by placing the behavior of a given individual at a given moment in its nomothetic context. The questions to be asked of a given behavior segment are: what are its nomothetic antecedents and what is the nomothetic node which in this community ordinarily follows upon this behavior sequence? More simply: what is in the process of getting done?

The adoption of this frame as a guide to observation had very noticeable results. Gradually what was originally a confusing kaleidoscope of innumerable disparate events began to fall into place, and a picture of the community's life emerged in which a finite number of dominant material behavior chains absorbed and marshalled the activity of the individuals in the community. The comprehension and accurate description of these dominant sequences begins as a hazy first approximation, but eventually the principal plot of life in the community becomes clear enough so that every individual can be given his proper place in the cast, and every episode or minor nodal chain can be located in the dominant chain of which it is a part.

As might have been expected, the behavior chains which

emerged as dominant were those which concerned the manipulation, production, exchange, and consumption of certain key economic objects--in this community the products of the conuco, as well as unfenced mountainsides of semi-wild coffee, certain nondomesticated vegetal objects in the floral environment, and two principal classes of livestock. The inhabitants of Pino Tumbao practice a surviving, though greatly modified, version of a mixed productive system whose defining elements are shifting cultivation and extensive livestock grazing, which has well nigh disappeared from the island of Hispaniola, and which has survived in modified form only in out of the way, hard to reach mountain districts still beyond the ever-advancing frontier of more modern permanent-field agriculture and enclosed livestock breeding. Because their productive system is mixed and complex--the strugglings of migrants who have been driven to agriculturally marginal highlands where their productive technology is only partially suited to the changed environment--they have been forced to modify their economic chains learned from their parents. The technological sequences which they learned from their fathers and grandfathers, and which they universally report to have provided a well-being that contrasts sharply with the difficulties of today, are no longer adequate and have been modified and supplemented with more recent endeavours.

As is evident from the above, the investigation was

not limited to collecting information on the present situation. The current productive system in the hills is understandable only in the light of its historical antecedents. In line with the cultural-historical approach, as exemplified in the cooperative research endeavour which produced Steward's The People of Puerto Rico, a knowledge of the historical influences which have molded and modified the economic systems on the island is crucial to a realistic grasp of the present. Just as individual episodes must be contextualized into their longer chains, and these into their scenes, so also the totality of the present system in the hills must be related both to its immediate and to its more distant antecedents on other parts of the island if it is to be properly understood.

The life cycle of a resident of Pino Tumbao is in large part a shifting cyclical involvement in these different sequences, mentioned above. The ethnographic task was viewed as having as its first stage the adequate description of these short and long-range sequences. There are no shortcuts to understanding the "Dominican campesino," it was felt. Genuine comprehension of "life in Pino Tumbao" has as its skeleton a step-by-step grasp of the cyclically re-occurring dominant atonic sequences which, by virtue of his birth in a given geographical niche, in a given decade, and in a given social group, the mountaineer of Pino Tumbao re-enacts day after day, year after year. These horticultural sequences and animal-raising sequences constitute the central axis of the serrano's life, and provide the framework

within which many social-organizational and other "non-economic" phenomena fall into place. Though this issue raises ideological bristles and is at a level of discourse ~~that~~ "proof" or "refutation" are scarcely possible, many salient features of kinship, compadrazgo, political activities, and the ethos of neighborliness lose their status as exotic curiosities when recorded in ~~h~~their real-life setting and given their proper ethnographic lodging as supporting links in the behavior chains which insure the physical survival of the men of Pino Tumbao.

Because of the centrality of these chains, the ethnographic endeavour involved a study of serrano horticulture, coffee growing, and animal raising. The description of the conuco--the shifting cultivation plot--will constitute the bulk of this present paper. The goal was to produce a description in which not only the structure of the commonly practiced technological sequences would be represented, but also the quantification of certain aspects of the process, such as man-hours involved in different tasks, typical crop yields, and the like. Because such information usually involved the questioning of informants, some brief comments must be made about the use of verbal data.

Because the observer-oriented approach to ethnography emphasizes so frequently the recording of behavior as opposed to tête-a-têtes with informants, it can be (and frequently

is) misconstrued as an injunction to avoid talking with people in the field, to merely stand back and take notes. Nothing could be further from the truth. What is implied in the observer-oriented approach is a different use of verbal behavior from that approach, advocated by many ethnographers, where the informant's version of events becomes the principal object of data-gathering and the final touchstone of the accuracy of the ethnography.

The observer-oriented approach utilizes verbal data in a completely different fashion. Cooperation must often be recruited from informants in the initial stages of isolating and describing the dominant behavior sequences and scenes in which the members of the community spend a large part of their time and energy. "Dígame, qué es eso?" "Qué está haciendo ahí?" "Porqué tiene que hacer eso?" "Cómo Uds. le llaman a esa cosita ahí?" Confronted with an unfamiliar maze of activities and objects, the ethnographer will need the verbal help of informants as he begins to unravel the tangle into a finite number of orderly chains. I doubt that even Super-Ethnographer, when he finally makes his appearance in Academia, will be able to piece together the events occurring in a foreign community without a good knowledge of the local language and the aid of very patient informants who are willing to explain many things.

But this type of verbal interaction is predominantly an aid to placing together and isolating behavior chains that are occurring on the ground. Because of practical

likitations the ethnographer would be foolish to attempt to figure out everything in the community on his own--just as he would be foolish to try to find his way in a strange city without the help of ~~street~~ maps or of passers-by. But this is quite different from saying that the purpose of questioning is to elicit this type of information as the central data. For the actor-oriented ethnographer, the "map" is the object of study. For the observer-oriented ethnographer the "map" is a useful tool to finding out something else. To say that the informant is not necessarily right is not to say that he is always wrong or that his opinions and explanations should not be constantly solicited.

But the final criterion for placing a behavioral event should not be the actor's explanation of his activity. If the ethnographer's insight into the workings of life in his community is genuine, he will hopefully be able to see the basic context of a given action even where for one reason or another the informants' explanation tends to cloak this. One example of such a situation occurred one morning at the home of my hosts in Pino Tumbao.

Scene. The patio, outside the kitchen, behind the house. Tuesday, ten o'clock, A.M. I came into the patio and two young men, X, about 18 years old and Y, sixteen, were standing in the patio conversing with doña Caridad (the woman of the house). We greeted. The two youths seemed to recognize me, but I couldn't place them. In as tactful a way as possible, I asked them what they were doing here on this fine morning, etc. "They're paying me a visit," doña Caridad told me. "They're paying you a visit?" "Yeeees, they're visiting me. That's the way we are here. When we feel affection for a person we visit them in their house." The two

youths seconded her explanation. The practice of house visiting is very common here. I left the scene for a few moments. When I came back, the two youths were at the small pigsty near the front gate, taking out some tiny pigs which Gerardo (a neighbor from across the river) had brought there the day before.

At that the meaning of their presence in the patio became clear. The day before, Gerardo, a distant neighbor, had found some recently farrowed pigs of a sow belonging to him, who had been foraging on the mountains. He brought them to the house of my hosts, since the man of the family knew how to castrate and earmark. They performed these operations on the preceding day, and the sow and litter stayed in the pigpen during the night. The two youths, one of whom was related to Gerardo, had been sent by the latter from some distance to tie up the sow and bring her and her litter to his place. Thus the two youths were actually there on an errand.

Viewed in terms of a chain of physically prerequisite nodal chains leading up to some higher order effect, their behavior was one of transporting valuable objects from one location to another, and their interaction with doña Caridad was a prelude to this activity. The basic "purpose" of their presence in the patio--if we look at the concatenation of logico-physically dependent links in a long behavior chain concerned with the care and disposition of livestock--was the transportation of the pigs. Yet they had stated their purpose as being one of a neighborly visit. There was no deceit involved; further inquiry elicited the information I was looking for. But the common verbal pattern of making

protestations of neighborly esteem had come to the fore when I first asked for an explanation. And this version cloaked (not in any sinister sense) the meaning that was eventually given to their presence there.

This is not to deny that neighborliness is a social reality. But it would be missing some very significant points, in terms of the economics of pig raising and the social organization of the community, to code the event as a case of "neighborly visit," or "manifestation of spirit of respect for elders" or some such--as one would be obliged to do if the informant's explanation is the final word. And to insist on these latter interpretations would also be missing the empirical fact that "neighborly visits," and "payments of respect to elders" do not usually occur at 10:00 A.M. on Tuesday morning.

To repeat and sum up: though informants must be constantly asked for explanations of what is happening, there is no guarantee that their explanations will always hit the point. The ethnographer's eventual unravelling of community events will hopefully reach the point where he can place an event in spite of the explanations he is given by informants.