

their analytic tools. As a result, one finds significant criticisms made in Europe unaddressed by the Americans, and one finds sophisticated methodological advances advocated and practised only by Americans.

We subsequently decided to test more systematically this impression of American and European ethnocentrism. Our indicator of such ethnocentrism was the proportion of citations by a scholar of American or European "academic nationality" to co-nationals. We expected to find that Americans most frequently cited other Americans and Europeans other Europeans.

A systematic and searching review of the literature was conducted using a snowball sampling technique. Ultimately, the sample generated approximately 2,000 references from 50 authors. Each reference was coded for (1) the academic nationality of the author and (2) the academic nationality of those cited. Academic nationality was primarily established by place of academic training. Thus, individuals who received their academic training in the United States or Europe exclusively were classified as American and European respectively. Individuals educated across continents were classified as Euro-Americans, as were those trained in Canada, Israel, and South America. Those who were trained at South African, Australian, Indian, and New Zealand universities were classified as European.

Table 1 presents our major findings. Of 637 citations made by Europeans, fully 79% are to other Europeans. This is strong support for our impression of a schism. While Americans appear to be somewhat less ethnocentric in this field, for whatever reason,² over 60% of the citations by Americans are to other Americans. The Euro-Americans split their citations, with more falling to Europeans (48%) than to Americans (35%). The relationship between the academic nationality of those cited and the academic nationality of the citing scholar is fairly clear and moderately strong.

In an area of universal significance such as the theoretical literature dealing with revenge, such a covariation needs explanation. We suggest that, aside from the simple, structural and crucial qualification of differential access to sources (i.e., some are not available in any given library), there may be other, and in some ways more serious, factors to be taken into account. We have suggested that the questions asked and the methods favoured appear to differ from Europe to the United States and that in this context it becomes understandable that Europeans tend to cite Europeans and Americans Americans. Scholars cite the work they deem most relevant to their own, which in practice tends to be work conducted on the basis of the same premises, the same assumptions, and the same scientific canons (Kuhn 1962). This, in turn, leads us to conclude once again that science in practice may fall far short of Merton's (1957) norm of "communality" (international access to and sharing of findings).

TABLE 1

CITATIONS TO THE FEUD LITERATURE BY ACADEMIC NATIONALITY OF AUTHOR AND OF SOURCE CITED

ACADEMIC NATIONALITY OF SOURCE CITED	ACADEMIC NATIONALITY OF AUTHOR		
	European (N=14)	Euro-American (N=16)	American (N=20)
European.....	79% (502)	48% (159)	28% (148)
Euro-American.....	7% (48)	17% (56)	11% (62)
American.....	13% (87)	35% (125)	61% (328)
Total.....	100% (637)	100% (340)	100% (538)

NOTE: An additional 466 citations could not be classified. A complete bibliography of the literature reviewed is available upon request.

²One of the 20 Americans in fact accounts for over half of the citations to Europeans by Americans.

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The Acquisition of Quiché (Mayan)¹

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Between January 1977 and May 1978 I recorded and transcribed the utterances of seven Quiché Mayan children living in the predominately Indian town of Zunil in the western highlands of Guatemala (90% Indian according to the 1973 census). I made a longitudinal study of three children (one boy, two girls) over a period of a year. These children ranged in age from two years to three years when I began working with them. I visited them in their homes for a one-hour play session every other week. In addition to these subjects I visited four other children and recorded four hours of their speech to insure the generality of the study.

So far I have only completely analyzed the record of one child, but the analysis shows that very broadly the picture of grammatical acquisition that Brown (1973) has outlined for English holds for Quiché as well. My subject, Al Chaay, began with utterances consisting almost entirely of adjectives, nouns, and verb roots. Over a period of nine months, she added more and more grammatical morphemes, such as aspect, person markers, and articles, to her speech and used them more and more consistently. Figure 1 gives Al Chaay's development in terms of mean length of utterance, comparing it with the

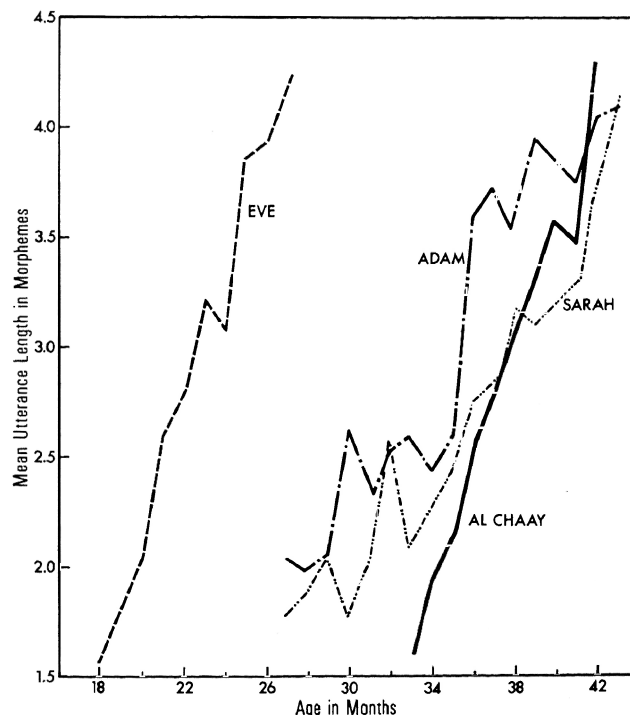


FIG. 1. Mean length of utterance and chronological age for Brown's (1973:55) subjects and Al Chaay.

¹I am grateful to the Organization of American States and to the Wenner-Gren Foundation for supporting this research.

TABLE 1

ORDER OF ACQUISITION OF SEMANTICALLY COMPARABLE
GRAMMATICAL MORPHEMES IN ENGLISH
(BROWN 1973) AND QUICHÉ

ENGLISH	QUICHÉ
2, 3. <i>in, on</i>	1, 2. Existential copula, <i>pa</i> 'in, on'
6. Possessive	3. Possessive
7. Uncontractable copula	4. Third person regular
9. Past regular	5. Past regular
10. Third person regular	

development of the three children in Brown's study. It shows that the course of language development is much the same in Quiché as it is in English.

The similarity in the acquisition of Quiché and English seems to hold at the more basic level of the individual grammatical morphemes. Brown (1973:274) found that his three subjects learned 14 grammatical morphemes in approximately the same order. Table 1 shows the mean order of acquisition of the morphemes in Brown's study and the order in which Al Chaay learned a set of semantically comparable grammatical morphemes in Quiché. (The numbers for the English morphemes give their order of acquisition among the 14 morphemes that Brown studied.) The Quiché morphemes are, of course, only approximately semantically equivalent to the English morphemes. The Quiché copula, in particular, is distinct from the English copula both in meaning and in the fact that it is stressed. Such similarity in acquisition order despite the tremendous differences in the formal properties of English and Quiché morphemes is especially surprising.

There are other parallels between Al Chaay and children learning English. In Quiché all nouns are marked for person. The person markers for subjects and objects are prefixed to the verb root (*A* stands for absolutive, *E_v* for ergative with vowel root, *E_c* for ergative with consonant root, and 1,2,3,4 for person):

- (1a) *k-in-e-k in* (2a) *k-at-inw-il-oh at in*
asp-*A*1-go-term I asp-*A*2-*E_v*1-see-term you I
(1b) *kinek* 'I go' (2b) *katinwiloh* 'I see you'

Person markers for possessor nouns and objects of prepositions are prefixed to the possessed nouns and the prepositions respectively:

- (3a) *qa-naan uj* (4a) *q-uuk' uj*
*E_c*4-mother we *E_v*4-with we
(3b) *qanaan* 'our mother' (4b) *quuk'* 'with us'

A later rule of pronoun drop then deletes all nonemphatic personal pronouns. (Craig [1977] discusses a similar set of person-marking rules for the Mayan language Jacaltepec.)

Al Chaay approached this learning problem by first using the independent personal pronouns to mark person on verbs, possessives, and prepositions (the correct form is given in the *b* portion of each example):

- (5a) *tij at* (6a) *paj uj* (7a) *uj mal*
eat you face we we because of
(5b) *k-a-tij-oh* (6b) *qa-palaaj* (7b) *q-umal*
asp-*E_c*2-eat-term *E_c*4-face *E_v*4-because of
'you eat' 'our face' 'because of us'

She was thus using a rule for expressing person in new environments in which the independent personal pronouns were no longer appropriate.

The main reason for studying language acquisition in a non-Indo-European language is of course that the role of syntax, semantics, and phonology will be highlighted differently than in English. In this way the relative influence of syntax, seman-

TABLE 2

PERSON MARKERS IN QUICHÉ IN ORDER OF PERCEPTUAL SALIENCY

	VERBS	NOUNS
Most salient	<i>E_v</i> 3, <i>E_v</i> 4	<i>E_v</i> 1, <i>E_v</i> 3, <i>E_v</i> 4
Intermediate	<i>E_c</i> 1, <i>E_c</i> 2, <i>E_c</i> 3, <i>E_c</i> 4	<i>E_c</i> 1, <i>E_c</i> 2, <i>E_c</i> 3, <i>E_c</i> 4
Least salient	<i>E_v</i> 1, <i>E_v</i> 2	<i>E_v</i> 2

tics, and phonology will become apparent, and we can begin to speculate meaningfully about the ability children bring to the problem of learning any human language.

Quiché almost seems to have been designed to exhibit the role of perceptual saliency in language acquisition. Its system of stress is extremely regular compared with that of English. The main word stress in Quiché always falls on the last syllable. This system of stress placement interacts in a complex way with the perceptual saliency of the person markers discussed above (/ marks a syllable boundary and ' marks the syllable receiving the main word stress):

- (8) *w-úuk'* (9) *a/w-úuk'* (10) *r-úuk'*
*E_v*1-with *E_v*2-with *E_v*3-with
'with me' 'with you' 'with him/her'
(11) *nu-/ndán* (12) *a-/ndán* (13) *u-/ndán*
*E_c*1-mother *E_c*2-mother *E_c*3-mother
'my mother' 'your mother' 'his/her mother'

In (8) and (10), the person marker combines with the word to produce a single stressed syllable. In (11), (12), and (13), the person marker has the form of an unstressed syllable attached in front of the word. In (9) the person marker is split by the syllable boundary; the latter part combines with the word to form a stressed syllable, while the initial part forms a separate, initial, unstressed syllable. There is no simple correspondence between the person-marker sets and perceptual saliency.

Stress and syllable boundary seem to be the main factors governing the perceptual saliency of the person markers. On this basis the person markers can be ordered according to their perceptual saliency as shown in table 2.

The Al Chaay data confirm the hypothesis that perceptual saliency determines person-marker acquisition orders; Al Chaay learned to use the person markers in the order predicted by their perceptual saliency. I am currently analyzing the data on the other children in my study in order to see how they acquired the person markers.

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CRAIG, C. 1977. *The structure of Jacaltepec*. Austin: University of Texas Press.

DISCUSSION AND CRITICISM

On the "Shantytown" Conference Report

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We wish to register some strong objections to Lloyd's report on the Wenner-Gren symposium on "shantytowns" (CA 20: 114-17). Some of these were presented at the very initiation

of the organization of the symposium two years ago (e.g., to the term "shantytown," to which I return below), and some were formulated with respect to drafts of the report and submitted by various persons to the report writers. Most of the objections are not ours alone, but those of several of the symposiasts, and involve quite basic issues raised before, during, and after the symposium but neither responded to nor really reported in the symposium statement. Here we shall state these objections in briefest possible form. More extensive statements can be found in a vast literature to which the symposium report seems also not to respond, though it has existed for quite a number of years.

First and foremost, we reject out of hand the term "shantytown" as a generic designation for several reasons: (a) It has no sociological significance whatever. (b) It obscures a process of evolution out of "shantyness" which has been widely reported in detail for Latin America, various parts of Asia (especially the Philippines and Malaya), and parts of Europe (e.g., the *casa clandestina* areas of Portugal). (c) It obscures any significant criteria important in the formation of the areas referred to, whatever their evolution or nonevolution. The obscurantism of the term, then, is both scientifically and politically irresponsible—a point raised again and again over the last two years with respect to the title and conception of the symposium by several of its members, while both verbal and photographic data were presented by a number of the members to show the generally misleading character of the term.

Second, the report contains a number of wholly false, partly false, and/or misleading statements about "shantytowns":

a) They are *not* identical with urban poverty (a proposition repeatedly disproved in the literature).

b) They are *not* identical with "informal-sector" labor absorption (notably not so in some areas of Latin America and in Portugal). Incidentally, informal and formal sectors are not only *not* clearly bounded domains with respect to occupations and labor absorption (as was mentioned repeatedly in the symposium), but markedly not so with respect to *persons* who may work in both sectors at once, as in Peru and other countries, as part of life strategies (somewhat like academics' getting contracts in addition to their fixed salaries).

c) There is no *necessary* relation between "shantytowns" and rural-urban migration (again, it is repeatedly demonstrated in the literature that early phases of rural-urban migration may go to inner-city slums rather than to squatter or "unregulated urban" settlements). In some of the areas referred to, the populations are largely born in the city being considered, and the rest are born in other towns and cities of the country; that is, they are not rural people at all. Obviously, immigration from rural areas (almost certainly connected with capitalist concentration of resources) is involved in city growth, but it does not therefore follow that it is rural-urban migrants who go to "shantytowns"—demonstrably, in some cases, it is urban-born people, fed up with inner-city slum dwelling, who go.

d) It has *not* been demonstrated that there is "an insufficiency of stable wage employment" for cities with "shantytowns" any more than it has for New York or London, with very large informal sectors (e.g., the "lump" in London, operating especially among Irish immigrant labor in the construction industry from early in the last century till the present). It is, in fact, known that the larger the city, the more it is able to absorb large numbers of people in various forms of work as well as to absorb large perturbations of its labor force at any given time (e.g., when the Brooklyn shipyards were closed in New York, practically all the thousands of workers put out of work were reabsorbed within a few weeks). This reabsorption occurs, especially perhaps, through informal-sector work, which exists in all (capitalist?) cities.

e) The contrast made at the symposium between Spanish-Portuguese and British-French settlement patterns in ex-co-

lonial countries had to do with the *delimited* character of the squatter settlements in the towns of the former in contrast to the *undelimited* character of "shantytowns" in the latter (specifically in British Africa). The argument was based on the evolution of social stratification from a class system of an epifeudal mercantilist capitalism of the former countries at the time of colonization to that of an industrial-finance capitalism of the 19th-century British variety, with corresponding differential results of settlement pattern after conquest.

f) No one claimed that the urban poor were a reserve army of the unemployed (the correct term, taken from Marx by Latin American marginality theorists), though *some*—those who are *not* actually the proletariat—*may* be. This confusion parallels the statement that the urban poor were seen as a homogeneous category, which was simply false for a number of the participants.

g) It can be shown only with the greatest difficulty, and often not at all, that "shantytowns" are direct consequences of political decisions. We can show this for Portugal, but there the decisions have nothing to do with migration or even with allocation of resources to middle-class housing, but rather with the deliberate maintenance of shanties. We *cannot* show *anything* like this for Brazil or Peru. Incidentally, all these settlements have middle-class and stable-working-class residents.

h) We can show for a number of cases that it is at least misleading and, in fact, often false that political concerns "among the poor" come from national leaders' legitimations. Rather, these concerns tend to be formulated in terms of their own class and subclass interests, mixed with various cultural backgrounds.

i) Not all the Latin Americanists present saw the connection between squatter settlements and society in terms of marginality—A. Leeds's paper specifically and explicitly rejected the idea, in detail, as did also parts of Uzzell's. *Some* Latin Americanists, especially those who are Latin Americans, *do* make such a connection.

j) E. Leeds's paper was primarily about *South* Portugal (the Alentejo) and migration to greater Lisbon (suburbs or no), not chiefly about the north and migration to France. The two are quite different problems with different dynamics; she was concerned with the origin and growth of squatter or shack areas in Lisbon, not with *bidonvilles* in Paris, which have disappeared altogether in recent years.

k) Some papers and much of the discussion continually and in detail delineated linkages between macro-study and small-group study, in some cases even trying to eradicate the dualism.

What is troublesome about all the major misstatements and omissions is that we get the sense, in reading the report, that much of the discussion both before and during the meeting never took place. Much of the dialectic of the discussion, the broadening of issues and approaches, and the richness of ethnographic and conceptual detail have been lost in the report of a symposium which the report writer himself created.

Reply

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Anthony and Elizabeth Leeds register "strong objections" to my report on the shantytown symposium, suggesting that these are shared by other participants. I should like to reply to each point raised by them. First, however, I should explain that the initial draft of the report was sent to each participant for comment; a few replied, all save the Leedses raising small points of detail. Later, each participant was asked to provide a commentary to my report, but since so few replied it was decided

to incorporate the points made into the final report. Wherever feasible this was done (some participants raised issues not explored in the symposium, and it did not seem fair to include these). The issues raised by the Leedeses were thus considered and incorporated, but many of their points—like those raised in their comment here—seemed to lack substance. With their exception, the participants have expressed their approval of my report.

a) As the report indicates, terms such as “shantytown” and “squatter settlement” (both very widely used in the literature) are contentious and imprecise. Some prefer one term, others another. The symposium participants had no difficulty in communicating with each other. In particular, none denied the possibility of development within the shantytown, though several felt that the levels of development reached, for example, in Peru, were near one end of a continuum which embraced, at the other extreme, continuing squalor.

b) The report makes it plain that no congruence was assumed between the shantytown and urban poverty or the informal sector. Scholars approach the problems defined by them with different frames of reference—geographers and planners are more likely to take the territorial unit, while economists and sociologists will categorise differently. The section of the report dealing with the informal economy specifically notes the movement of persons between wage employment and self-employment; no time duration is specified—the movement may occur within the course of a day.

c) The report does not state that there is a *necessary* relation between shantytowns and rural-urban migration. Yet it is agreed that the majority of their residents were born in the countryside. This does not deny the fact that many migrants settled first in inner-city slums (a feature common in Latin America, though not in Africa) or that many shantytown residents are city-born.

d) The report makes it clear that many informal-sector activities are productive and, indeed, that the informal sector is not a feature confined to developing countries. It is established beyond doubt, however, that the flow of migrants to the city (or, again, the output of primary schools) is generally greater than the creation of new wage employment in the formal sector (manufacturing industry, the public services, etc.). Many of those in the informal sector are minimally productive, especially in such occupations as petty trading, casual labour and portage, etc. Those so occupied usually seek wage employment; to the extent that they cannot obtain it, there is insufficiency.

e) Anthony Leeds raised, at the symposium, the contrast between settlement patterns in Spanish and Portuguese ex-colonies and in those of Britain and France. Time prevented a full discussion of this topic, but participants were sceptical of the validity of the proposition; it seemed to ignore the complexity of settlement patterns within Africa.

f) Participants certainly discussed, though without agreement, a variety of terms, such as “reserve proletariat,” etc. As the report indicates, the emphasis on macro-social processes *tended* to lead to discussion of the urban poor as a homogeneous category, but the participants did stress historical and cultural differences. The later section on the informal economy makes very clear the heterogeneity of this sector, even in economic terms.

g) The report does not state that shantytowns are the *direct* consequence of political decisions. It states that policy decisions relating to economic development, to the allocation of resources (especially for housing) among classes, and to the improvement or eradication of shantytowns all contribute to the development of the shantytown. The paper given at the symposium by Portes and my own experience in Peru amply confirm this proposition.

h) The sentence in the report about political legitimization relates the development of political consciousness among the poor to the degree to which political leaders seek legitimization by the poor; it suggests that legitimization is in fact often (or usually?) directed towards the middle classes.

i) The report states that Latin Americanists in general (including most Latin Americans) couch their discussions in terms of “marginality.” It states that while participants in the symposium (Latin Americanists included) rejected the idea of cultural marginality, many continued to use this term in other senses. As recorded in the report, Kapferer produced a novel use of the term in his paper on Zambia.

j) Elizabeth Leeds’s paper gave equal weight to explanations of the migration of persons from southern Portugal to Lisbon and of the migration of persons from rural areas in the north to France. Most of the discussion centered upon the latter. The report cites this paper in the context of migration studies and makes no mention either of the squatter areas of Lisbon or of the *bidonvilles* of Paris. In common with most other papers, this one provided material germane to the discussion of issues raised in the symposium on successive days.

k) The report clearly reflects the interest of participants in this symposium in uniting macro- and micro- approaches. This was demonstrated by the fact that, whilst almost all the participants had at some time been engaged in micro- studies, their discussions so often focussed upon the macro- level as they sought to relate their findings to broader issues and policies.

It is with regret that I feel that the comments by Anthony and Elizabeth Leeds result from a rather perverse reading—or on occasion a misreading—of my report. It is not easy to summarise so many papers and a week’s debate in so few words; each participant would have expressed himself differently, singling out certain issues as more exciting than others. The points raised by the Leedeses, however, seem to add no new dimensions to the report.

On the Concept of Mode of Production

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It is disappointing that Godelier’s incisive if terse essay (CA 19:763–68) provoked comments from only 5 of the 40 persons to whom it was sent. In a relatively short piece by CA standards, Godelier has established a bench mark for theoretical work on key issues in anthropology and, in so doing, has helped to bring Marxism from the periphery to rejuvenate the idle core of contemporary anthropology. My comments concern the concept of mode of production in Marxist analysis and some general epistemological issues that derive from this concept.

Godelier has taken some pains to delineate as carefully as possible what he means by a mode of production; this thinking, of course, has evolved through time. In his *Perspectives in Marxist Anthropology* (1977) and in the present essay, Godelier argues against the proliferation of modes of production in Marxist analyses (e.g., the so-called hunting, farming, and pastoral modes), as he considers these phenomena in terms of different forms of labor process within a mode of production. Godelier’s criterion for determining whether two or more of these phenomena comprise different labor processes within a single mode of production rather than different modes of production is whether they are combined within the same social relations of production (p. 765).

This argument has some appeal in the abstract terms for

which Godelier is well-known, but it runs into some problems in specific historical cases, especially cases in which the superstructural apparatuses are those of the state. In contemporary nation-states, Godelier's perspective apparently would lead to the acceptance of state borders as the bounds for production relations, regardless of what kinds of units (e.g., ethnic groups, regionalized production) the nation-state incorporated. Perhaps one can identify "regions" within state borders, but what other kinds of distinctions are possible?

Godelier's approach may be contrasted with the world-systems approach of Wallerstein (1978:7), who argues that the appropriate unit of analysis is not the mode of production or the nation-state, but the modern world system, or capitalist civilization, which, he claims, has bred many nationalisms. In other words, Wallerstein has inverted the usual order of relationship between the nation-state and the international system, in a way similar to Fried's (1975) inversion of the accepted order of relationship between the tribe and the state. What is significant about Wallerstein's work for present purposes is that he places the discussion of modes of production in a hierarchical and historical perspective. Consequently, an emergent capitalist mode on the periphery of the world system need not be identical to an earlier capitalist mode in the core, any more than a secondary state is identical to a pristine state. Similarly, Wallerstein's approach helps place in sharper focus the subordinate modes of production in the capitalist periphery. As increasing attention is paid to the unevenness of capitalist development and the ways in which precapitalist or non-capitalist modes are maintained, transformed, or even created by capitalist relations (see Cook and Diskin 1976, Duncan and Rutledge 1977), the terminology for describing these arrangements must become more precise. Perhaps "mode of production" is not the best term to describe internal variation in dependent nation-states on the capitalist periphery, but what are the alternatives?

Along with Beckford (1972) and others, I believe that the concept of "agrarian capitalism" merits further attention (see Wessman 1978a, b). What are needed, I propose, are intermediate concepts that deal with significant variation *within* modes of production. What are the different forms of the labor process to be called? For agrarian formations, the concept of "agrarian structure" has some merit, as Stavenhagen (1971, 1975) and others have shown, but it is not at all clear what to do with variation within other, nonagrarian formations.

A case in point is Sahlins's (1972:41) "domestic mode of production," which he proposes as a generic term for *all* precapitalist modes. What these modes have in common is that they are "organized by domestic groups and kinship relations" and are structurally underproductive. Sahlins's view of the domestic mode is one of structured underproduction which follows a more or less faithful relation between household composition and labor capacity and intensity, as affected by structural constraints, such as kinship and polity. The result is a series of internal contradictions involving the provisioning household and the dominant institutions, an apparent paradox of affluence and reciprocity in humble surroundings.

In general, anthropologists have accepted rather uncritically the notion of a domestic mode of production. Godelier mentions it in his *Perspectives* (1977:81). The notion has a number of difficulties which I plan to go into elsewhere, not the least of which is the articulation of the domestic mode of production to the capitalist mode through, for example, labor migration, as in the case of the Gwembe Tonga (Scudder 1962: 156-58). Certainly the positing of a *single* domestic mode of production strains Sahlins's credibility. Yet I am sympathetic with his attempt to delineate modes of production that are not just precapitalist but also noncapitalist. The problem is

partly one of limiting one's focus to the patterns within a mode without adequate concepts to account for internal variation.

Anthropologists have prided themselves on their development of the concept of culture, but the concept is essentially static and internally undifferentiated. Godelier's notion of ideology points the way to more sophisticated conceptualizations, but the differences between Claessen's and Pi-Sunyer's responses to his discussion of the notion indicate that a great deal more conceptualization remains to be done. It is disappointing, if not surprising, given Godelier's fieldwork interests, that he has not chosen to develop the concept of "hegemony" in his analyses.

Gilmore raises the complaint that Godelier "does not tell us how to apply his ground-breaking insights elsewhere" (p. 769). After years of being alternatively inspired and perplexed by Godelier's insights, I have concluded that they are not applicable in a *specific* sense. He points the way in a large number of important areas, but the application of these insights is another matter. Even Godelier's own empirical analyses are not without their problems (e.g., see Bradby 1977 concerning his analysis of Baruya salt currency [Godelier 1971; 1977:127-51]), and his reinterpretations of other anthropologists' work (e.g., on the potlatch, the African "cattle complex," the Mbuti Pygmies [see 1977:40-41, 51-61]) are oddly reminiscent of cultural-ecological analyses. One anthropologist who admits to having been influenced by Godelier is Cook, but Cook's (1970, 1976) economic analyses draw at most a *general* influence from his work. It is not that Godelier's ideas are flaccid—Cook's work alone is evidence that they are vital and instructive—but that they are abstract and programmatic rather than concrete and analytical. The transition from abstractions to specific historical instances is difficult at best, and it is to Cook's credit that he has made this transition. However, Gilmore is expecting too much.

Furthermore, I find it curious that Godelier uses the term "history" in his title, for his concepts are not posed as historically specific ones. I remain unconvinced, for reasons given above, that there is much in the line of historical insight in Godelier's work.

Marx's achievements in synthesizing social and economic analysis, in demonstrating the simultaneous constraints of social and material forces, cannot be overestimated, nor can the impact of the European intellectual and social environment on Marx and Godelier be denied. Marx demonstrated how one carries into a given situation the totality of his or her experience and historical tradition. His synthesis has been difficult to emulate, partly because of the times and partly because our intellectual tradition manifests the alienating and individualizing tendencies Marx warned about. In our society, in which radical scholars do not enjoy the kind of cultural support that is common in France and other countries, there is apparently an overwhelming temptation to attempt to write *the* history of anthropological theory or to do *the* ultimate piece of fieldwork, as if one were allowed but a single chance. Consequently, attempts to repeat Marx's accomplishments in the context of late 20th-century North America often dissolve into mysticism (that is, the posing of completely untestable propositions) or lapse into positivism (that is, the consideration of only those phenomena that can be concretely measured). Marx was dialectical, and he took his work seriously. These two aspects of Marx's approach are essential for anyone who pretends to continue the work he began.

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Prizes

■ The attention of persons interested in African studies is drawn to an annual prize being offered by the Amaury Talbot Fund. The Trustees invite applications for the prize, which for 1977 amounted to £173.45 after allowing for fees and expenses in connection with the award. (At the time of this publication, entries for the 1978 prize are still being judged.) The prize will be awarded to the author or authors of the work which, in the opinion of the judges, is the most valuable of the works of anthropological research submitted in the competition. Only works published during the calendar year 1979 are eligible for the award. Preference will be given to works relating in the first place to Nigeria and in the second place to any other part of West Africa or to West Africa in general. Works related to other regions of Africa are, however, eligible for submission. All applications, together with two copies of the book, article, or work in question, must be received by January 31, 1980, by the Trustees, Barclays Bank Trust Company Ltd., Central Administration Trust Office, Radbroke Hall, Knutsford, Cheshire WA16 9EU, England. Please quote reference number 61/888. Entries will not be returned to candidates but will be at the disposal of the judges.

For Sale

■ *The Proceedings of the 8th Pan-African Congress of Prehistory and Quaternary Studies, Nairobi, September 5-7, 1977* (Nairobi: International Louis Leakey Memorial Institute for African Prehistory, 1979), containing some 100 illustrated

papers and other Congress matter. The volume may be ordered for Ksh. 380 (Kenya only) or U.S. \$55 from PACQS, % National Museums of Kenya, P.O. Box 40658, Nairobi, Kenya. Air-mail rates will be sent on application. The price outside Kenya is subject to fluctuations in the rate of exchange.

■ *Directory of Anthropologists and Anthropological Research in Aging*, third edition (Chicago: Loyola University of Chicago Department of Anthropology, 1979), including information on 120 anthropologists working in this field. Copies may be obtained at cost for U.S. \$1 from Christine L. Fry, Department of Anthropology, Loyola University of Chicago, Chicago, Ill. 60626, U.S.A. Checks should be made payable to Christine L. Fry.

Our Readers Write

It is encouraging even to see the title "Systems Theory in Anthropology" (Rodin, Michaelson, and Britan, CA 19:747-62), for anthropologists as a whole have not yet incorporated "systems" thinking into their world view. Yet the developments in scientific philosophy which have made possible the things which the conference participants report constitute nothing less than a major revolution now taking place. Current science is revising itself away from 19th-century thought categories, just as 19th-century science revised itself away from those of the 18th. Before launching into deeper water, congratulations to symposiasts and authors. At last we have something of a notion concerning how anthropologists are using "system." It registers an advance over a recent past in which it was not being used at all.

Yet now I must side with those commentators who find the treatment superficial and misleading, for (a) there is no such thing as "systems theory," (b) nor are we helped when "it" is further described as a "compendium of approaches, theories, and methods"; (c) on the contrary, "system," however used by various sciences, is a metascientific concept, and recognizing this fact makes all the difference in the world. Most of what I shall be saying will revolve around this proposition.

To (a) first, however: "Systems theory," no; "systems philosophy," yes. The body of literature about this is already large and growing; I need not expound. Instead, I'll simply mention that a 19th-century atomism promoted "reductions," some of which have been amply justified, others not. "Form" was an axiom, "relation" was assumed but not rationalized; inquiry concentrated upon what we would term today the "information of the parts." In contrast, in our current part of the 20th century, "form" is a problem, "relation" is a prime and demanding of rationalization, and we query an "information of the relations between parts." Here is the polar difference between atomism and holism: "the whole is 'more' than the sum of its parts." No doubt some of the conference participants would agree about this, at least in some degree. Yet they expound "systems theory" in terms of how to apply it ad hoc.

This leads us into (b): "Systems theory" is stated to be a "compendium of approaches, theories, and methods." This is very untidy. A "compendium" by what basis or logic of classification? "Approaches, theories, and methods." If I appear rude, I am apologetic, for it is unintentional. This "compendium" has a grab-bag quality; it is not just an assembled armamentarium. On the contrary, "system" is a definable meta-scientific term.

A parable may serve to clarify: The conference participants