1976 Medical Research Among the !Kung. *In* Kalahari Hunter-Gatherers: Studies of the !Kung San and Their Neighbors. R. B. Lee and I. DeVore, eds. Pp. 166–194. Cambridge, MA: Harvard University Press.

Wilmsen, E.
1982 Studies in Diet, Nutrition, and Fertility among a Group of Kalahari Bushmen in Botswana. Social Science Information 21:95–125.

Winterhalder, B.
1981a Optimal Foraging Strategies and Hunter-Gatherer Research in Anthropology: Theory and Models. *In* Hunter-Gatherer Foraging Strategies: Ethnographic and Archaeological Analyses. B. Winterhalder and E. A. Smith, eds. Pp. 13–35. Chicago: University of Chicago Press.
1981b Foraging Strategies in the Boreal Environment: An Analysis of Cree Hunting and Gathering. *In* Hunter-Gatherer Foraging Strategies: Ethnographic and Archaeological Analyses. B. Winterhalder and E. A. Smith, eds. Pp. 66–98. Chicago: University of Chicago Press.

Yost, J. A., and P. J. Kelley
1983 Shotguns, Blowguns, and Spears: The Analysis of Technological Efficiency. *In* Adaptive Responses of Native Amazonians. R. B. Hames and W. T. Vickers, eds. Pp. 189–224. New York: Academic Press.

Agricultural Intensification and Women's Work

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Ember's analysis in "The Relative Decline in Women's Contribution to Agriculture with Intensification" (*AA* 85:285–304, 1983) of changing women's roles in agricultural labor organization is unconvincing. She fails to adequately define concepts, which leads to inappropriate comparisons, and she takes an ahistorical, synchronic approach which leads to viewing the process of change in isolation from the larger world. I offer the following four comments in an attempt to further discussion of this important issue.

1. The lack of a critical, comparative evaluation of the definitions, methods, and time frames used in the various studies she uses in her analysis makes the tables, each with different numbers of different cases from different surveys, very confusing. The only evidence offered in support of her central argument, that there is a relative decline in women's labor contribution to agriculture and an increase in their domestic work with intensification, is given in Table I. This table consists of a sample of 13 groups taken from a previous review (Minge-Klevana 1980). In subsequent sections she supports proposed causes for this change in women's roles by drawing on three additional cross-cultural surveys: the HRAF Probability Sample, the Ethnographic Atlas (Murdock 1967), and Nag's (1962) survey of factors affecting fertility. Mixing data from these surveys together, often in the same table, and using different sets of groups in each table (none of the five subsequent tables uses the same groups used in Table I to establish the condition to be explained) makes the argument unconvincing.

2. Although the concept of agricultural intensification is central to Ember's argument, it is never explicitly defined. She does imply, however, that intensification is to be equated with "having the plow or irrigation" (p. 287). This is not the usual definition of intensification, which centers on the increasingly frequent use of land (Boserup 1965:43), although it can include increased technical and labor inputs (Netting, Cleveland, and Stier 1980; Cox and Atkins 1979:139–140; Ruthenberg 1980:15–16). Thus, while the use of the plow or irrigation may be associated with intensification, it does not define it. Ember also states, in explanation of her assignment to categories in Table I, that people practicing hoe agriculture are probably nonintensive, although she "cannot be sure" (p. 287). While "hoe agriculture" is often equated in the anthropological literature with "slash and burn" techniques, which of course are relatively nonintensive, many people using the hoe as the major tool of cultivation have quite intensive systems. Ember does not, however, even follow her own definition. For example, the Tallensi and Ashanti are both classified as intensive agriculturalists in Tables II and IV even though neither use irrigation and neither depend on the plow. Although the plow was introduced to the Tallensi in the 1930s, it has not replaced the hoe as the primary tool in their intensive system of cultivation centered on continuous cropping of manured fields. Puzzlingly, in Table VI the Ashanti have become horticulturalists. They are in fact nonintensive, cropping a plot for three years and then fallowing from two to ten times as long (Allan

1965:226–227; Benneh 1973). The widespread establishment of a permanent cash tree crop (cocoa) in Ashanti at the beginning of this century, while an intensive system, does not entail the use of the plow or irrigation either.

No definition is given of the other major concept considered in Ember's paper, work, the meaning of which is the subject of much debate (see Minge-Klevana 1980, especially the comments).

3. Ember sees increased time required for child care, resulting from increased fertility, as the most important explanation for the suggestion that women are pulled into more domestic work with intensification. She moves directly from evidence that more intensive groups have higher fertility than less intensive groups to explanations for that higher fertility without any hard evidence to support the critical point: that more time is spent in *domestic* child care by women in intensive agricultural groups. She does cite one contrary study, but then concludes that because a cross-cultural study suggests that mothers provide more than half the care of infants, mother's work increases with increasing number of children. Again, lack of a clear definition of child care clouds the issue. Many women appear quite capable of carrying out a variety of domestic *and* agricultural work in conjunction with child care. How are such periods of combined activities to be defined, and how are they defined in the studies Ember cites? Even if intensive agriculturalists do have more children, it could as plausibly be argued that they spend less time in child care than nonintensive agriculturalists because settlements are more permanent, there are more older children and other kin nearby to help, and there may be lighter agricultural work closer to the house (see Ho 1979; Ware 1978). We need more quantitative evidence to decide how increased numbers of children may affect not only child care but amount of time spent by women in domestic work.

4. The time period for Ember's data and her analysis of it is not clear. Most of the studies cited in her tables appear to refer to a quite recent period. For example, of the 13 in Table I, 8 have publication dates in the 1970s, 4 in the 1950s and 1960s, and 1 in 1939 (Minge-Klevana 1980). For the 14 African societies cited in Table II, the dates of the sources in Nag (1962) from which the data on fertility levels come span the period 1941 to 1958. Ember states that for all but 2 of these, the ratings for the type of agriculture come from Murdock (1967) and refer to the same "dates of description and ethnographic sources." No analysis of the period described in the studies in comparison to publication dates is given, although it is inferred that most of the studies refer to the "ethnographic present" (p. 291, Table III, n.a).

Thus, although most of the studies appear to describe the recent past, Ember implies a largely premodern time frame in her analysis. For example, she argues that warfare pulls men in horticultural societies out of agricultural work (p. 297), and infers that she is discussing "preindustrial" intensive agriculture (pp. 300, 301). The strongest evidence that Ember's analysis assumes a precontact or premodern period is the absence of almost any consideration of the role of colonialism or Western-style economic development, which have encouraged intensification in ways that have affected women's roles in agriculture. Thus, if Ember's central argument is correct, and if much of the data she cites for evidence is from intensive groups affected by Western influence, it may be that women are not so much *pulled* into more domestic work with intensification as they are *pushed.* The literature on women in development abounds with examples (e.g., Ashby 1981; Boserup 1970:55–56; Pala 1976). A widely publicized World Bank study of Africa stated in 1976 that there has been

> a tendency among project planners and authorities to see African women in Western terms—i.e., essentially as domestic workers whose primary responsibility should be in the home and not in the fields. Thus, the goal of extension services has frequently been not the increase in farm-level productivity of women but rather finding ways to reduce their participation in agriculture through promotion of more homebound activities. [Lele 1975:77]

In a wide variety of situations where Western males have undertaken the intensification of agriculture, it may be necessary to look for the determinants of women's work roles in other than local, functional contexts.

References Cited

Allan, William
1965 The African Husbandman. New York: Barnes and Noble.

Ashby, Jackqueline A.
1981 New Models for Agricultural Research and Extension: The Need to Integrate Women. *In* Invisible Farmers: Women and the Crisis in Agriculture. Barbara C. Lewis, ed. Pp. 144–195. Washington, DC: Office of Women in Development, Agency for International Development.

Benneh, George
1973 Small Scale Farming Systems in Ghana. Africa 43:134–146.
Boserup, Ester
1965 The Conditions of Agricultural Growth. Chicago: Academic Press.
1970 Women's Role in Economic Development. New York: St. Martin's.
Cox, George W., and Michael D. Atkins
1979 Agricultural Ecology. San Francisco: Freeman.
Ho, Teresa J.
1979 Time Costs of Child Rearing in the Rural Philippines. Population and Development Review 5:643–662.
Lele, Uma
1975 The Design of Rural Development. Baltimore: Johns Hopkins University Press.
Minge-Klevana, Wanda
1980 Does Labor Time Decrease with Industrialization? A Survey of Time Allocation Studies. Current Anthropology 21:279–287.
Murdock, George P.
1967 Ethnographic Atlas: A Summary. Ethnology 6:109–236.
Nag, Moni
1962 Factors Affecting Fertility in Nonindustrial Societies: A Cross-Cultural Study. Yale University Publications in Anthropology, No. 66. New Haven, CT: Department of Anthropology.
Netting, Robert M., David A. Cleveland, and Francis Stier
1980 The Conditions of Agricultural Intensification in the West African Savannah. *In* Sahelian Social Development. Stephen P. Reyna, ed. Pp. 187–505. Abidjan, Ivory Coast: U.S. Agency for International Development.
Pala, Achola O.
1976 African Women in Rural Development: Research Trends and Priorities. OLC Paper No. 12. Washington, DC: Overseas Liaison Committee, American Council on Education.
Ruthenberg, Hans
1980 Farming Systems in the Tropics, 3rd ed. Oxford: Oxford University Press.
Ware, Helen
1978 The Economic Value of Children in Asia and Africa: Comparative Perspectives. Papers of the East-West Population Institute, No. 50. Honolulu: East-West Center.

Reply to Cleveland

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I wish to correct some of Cleveland's misunderstandings about my article (*AA* 85:285–304, 1983), but before I get to those corrections I want to comment on what I perceive to be our differences in attitude toward research, because I think those differences underlie his criticisms.

There is a major point on which David Cleveland and I agree—we need more evidence to test the theory I put forward in my 1983 paper, particularly more data on demography and time allocated to child care and other activities. But then I did say that in my original paper (see p. 288).

It seems to me that Cleveland has completely missed the spirit of my paper. If my words are read carefully, I think the reader can see that my conclusions are all tentative. For example, I say that the evidence is supportive, not conclusive. One might ask why I bothered to write the article if I didn't have all the data I really wanted. The answer is that some relevant data were available and I hoped that my theory and the provisional tests I made would encourage the collection of better data and the further testing of hypotheses. I am not surprised that Cleveland is not convinced by the data I presented. I am not either! If one has a scientific spirit, one is never convinced that a theory is true. If we doubt it, we should try to collect additional evidence that may falsify it. Let me turn now to some of Cleveland's specific criticisms.

Cleveland complains that my data are synchronic, not historical, and therefore lead to viewing change in isolation from the larger world. I think the problem here is that Cleveland does not understand the strategy of cross-cultural research, so let me take this opportunity to describe it briefly. The main purpose of the cross-cultural research strategy is to provide a preliminary test of theories by looking to see if presumed "causes" are generally associated with presumed "effects." If the theory predicts what turns out to be a statistically significant synchronic association, all we can legitimately say is that the evidence is consistent with the theory. But no investigator trained in research design would conclude that the theory is necessarily correct or proven true. I believe that synchronic cross-cultural studies are effective first steps for evaluating theories. I think cross-historical research should also be done to test the temporal orderings suggested by our theories. Since Cleveland thinks historical research is important, it is a pity that he didn't take the time to bring some historical evidence to bear on the theory I have proposed.

The central complaint in Cleveland's point number 2 seems to be that I didn't provide data for all the variables (e.g., fertility) on the cases reported in Table I. The reason I did not is quite simple—the data that I wanted were not available for those cases. Studies that present time allocation data, for example, usually do not present fertility data. Nag's (1962) survey is comprehensive for fertility data, but the sources usually have no information on time allocation. So, I had a choice: I could give up, or I could try to make do with what was available. The disadvantage of not having data from the same sample is that one cannot do control analyses to sort out one predictor from another. But other than that, I see no disadvantage in using different samples. If a theory has validity, it should generate supportable predictions *in any representative or nonpurposive sample of societies*, regardless of the times of description and measurement for the sample societies. Hence I fail to see why Cleveland is bothered by my taking data from the *Ethnographic Atlas* (Murdock 1967) on type of agriculture to correlate with Nag's fertility data, when I made sure that the *Atlas* rating had the same time and place focus that Nag had used for rating fertility (see p. 291 of my paper). If the data on fertility and agriculture come from the same ethnographic reports and pertain to the same group of people at the same point in time, then it should not matter whether Nag made the judgment or Murdock did for the *Atlas*—the basic data still come from the same sources.

As I noted in my paper, I allowed only three cases to depart from that requirement. These exceptions were the Ganda, Haya, and Navaho. In those three cases the *Atlas* date was earlier than the sources that had fertility information reported by Nag. However, since these cases were coded by Murdock as having intensive agriculture at the early time and since I could find no contrary indication from my reading of Nag's sources for the later time, I assumed those cases were still intensive. Also, there is some advantage to using ratings by Nag on fertility and ratings on agriculture from other sources such as the *Atlas*—the ratings of one variable could not possibly be influenced by the ratings of the other. Even if we were to exclude from Table II those cases that did not match in time or which I rated myself from sources referred to by Nag, the results would still be statistically significant by Fisher's Exact Test.

When I examined data from the HRAF Probability Sample to look at the relationship between fertility and type of agriculture, I did so not to be confusing but rather to see if the fertility result would *replicate* in another sample.

Cleveland says that I did not define intensive agriculture. That is not so. For each analysis that I reported I gave the reader an "operational" definition of "intensive agriculture." For the results that came from the Standard Cross-Cultural Sample, I described which "letters" represented intensive agriculture and horticulture, respectively. I did the same thing for the results based on *Atlas* information. Since the codes are in the public domain and since the definitions are fairly standard, I did not bother to repeat them in the paper. If Cleveland had looked up those codes in the published sources, he would have known that they are explicitly defined. Both the Standard Cross-Cultural Sample and the *Atlas* basically follow the same coding scheme. The *Atlas* (Murdock 1967:159) gives two categories of intensive agriculture. If a society is coded as "I" it refers to permanent field cultivation "utilizing fertilization by compost or animal manure, crop rotation, or other techniques so that fallowing is either unnecessary or is confined to relatively short periods." "J" refers to intensive cultivation where it is largely dependent on irrigation. The major difference in the Standard Cross-Cultural Sample is that "I" and "J" are combined into one category (which is what I did in the tables I presented).

With respect to the cases in Minge-Klevana's (1980) study, I did have more of a problem classifying some of those cases as intensive agriculturalists or horticulturalists. I did not mean to imply that the "plow and irrigation" were my sole criteria for intensive agriculture; I merely meant that if the community was described by Minge-Klevana as having "plow agriculture" or "irrigation" then I was fairly certain that they had intensive agriculture. I do not think that there is any problem with the classification of Kali Loro, Le Levron, German Swiss, Nepal villagers, Kabupaten, Medières, or German peasants. I should have spelled out my decisions for the other cases. So, for example, I classified Tenía Mayo as intensive because Erasmus (1955) discusses plowing. On the "simple agricultural" side, there is no apparent problem with the classification of the Machiguenga; Minge-Klevana describes them as having "slash-and-burn gardening." Although the Kayapo are described by Minge-Klevana as having "gardening," I know from the work of Dennis Werner (1984:150–151), who was a student of mine, that the community he studied had slash-and-burn agriculture. The Bemba are listed by Minge-Klevana as having "hoe agriculture"; Murdock and White (1969) code them as having shifting cultivation on the basis of the same authority Minge-Klevana cites—Audrey Richards. With respect to the Genieri of Gambia and the Ihangiro of Tanzania, both are described by Minge-Klevana as having "hoe agriculture." Since I did not have access to the original sources, I did not classify them in Table I as "horticulturalists"; rather I labeled them "simple agriculturalists" in comparison with the definite intensive agriculturalists in Table I. I wish I could be more sure about the Genieri and Ihangiro, but wishing will not make it so. That is one reason why we need more evidence to test the theory that I have proposed.

Cleveland says that I do not define work. It would be very nice for everyone to agree on a definition and abide by it, but the fact of the matter is that we cannot be sure that the various time allocation studies define it in precisely the same way. Again we have a choice: Do we use the data from those studies, assuming that most people mean approximately the same thing, or do we ignore them? My choice was to make use of them. It is important to recognize the problems in those studies, however, so that we may conduct more comparable studies in the future.

Cleveland seems puzzled by the fact that the Ashanti are listed as intensive agriculturalists in one table (II) and as horticulturalists in another table (VI). This should not be puzzling inasmuch as the date of description is different in each table (see the footnotes to each table). In Table II, I was using fertility data described by Nag and looking for agricultural data to coordinate with it. Nag got his fertility data from Fortes (1954), who describes the Ashanti of Agogo township as largely dependent on cocoa growing. As Cleveland himself notes, this was an intensive system. The Ashanti referred to in Table VI were coded by Murdock for the *Atlas* as having extensive agriculture, but the date for the Ashanti in the *Atlas* is 1900. The definitions of type of agriculture did not change; the time foci are different!

Cleveland seems puzzled by the variation in time focus employed in my paper and I suggest that his puzzlement results from a lack of familiarity with cross-cultural studies. Cross-cultural researchers do not use the same time frame for all societies. This is because different peoples have been described at different times by different ethnographers. I always try, as I did in this article, to note what the ethnographic present is or to refer to the sources of my data which give the ethnographic present for each case. In theory, the use of different times for different cases should not matter. If a theory or hypothesis has merit, the presumed cause and effect should be associated, no matter what the time period described for a case. I regret, however, not reiterating an important point with regard to the coding of warfare interference, which appeared in M. Ember and C. R. Ember (1971:579). In that study, warfare was rated for a period up to 50 years before the ethnographic present. Cleveland was right to note that with regard to warfare I must have been describing a premodern period. I apologize for that omission. All other data, however, pertain to the "ethnographic present." In spite of the leeway in the time period for the warfare data, knowledge of warfare did help us predict division of labor in the ethnographic present. I suspect therefore that division of labor is subject to considerable time lag; that is, even though warfare ceases, men and women may continue to adhere for some time to the old patterns.

At the end of his comments, Cleveland suggests the theory that colonialism may have led both to agricultural intensification and a deliberate attempt to push women out of agriculture. His suggestion is worth testing. It is a pity that Cleveland did not try to test it. As I said at the outset of this reply, I put foward my ideas with the intent of encouraging more research. I am not so attached to my own ideas that I would be upset if an alternative idea were supported. But I do not think that it is sufficient to offer an alternative interpretation that is garnished only with anecdotal evidence. It seems to me that Cleveland has complained that I didn't do what he would have done. I encourage him to improve on my work. We need more research, not rhetoric.

References Cited

Ember, Melvin, and Carol R. Ember
1971 The Conditions Favoring Matrilocal Versus Patrilocal Residence. American Anthropologist 73:571–594.
Erasmus, Charles J.
1955 Work Patterns in a Mayo Village. American Anthropologist 57:322–333.
Fortes, Meyer
1954 A Demographic Field Study in Ashanti. *In* Culture and Human Fertility. Frank Lorimer et al., eds. Paris: UNESCO.
Minge-Klevana, Wanda
1980 Does Labor Time Decrease with Industrialization? A Survey of Time-Allo-

cation Studies. Current Anthropology 21:279–287.
Murdock, George P.
1967 Ethnographic Atlas: A Summary. Ethnology 6:109–236.
Murdock, George P., and Douglas R. White
1969 Standard Cross-Cultural Sample. Ethnology 8:329–369.
Nag, Moni
1962 Factors Affecting Fertility in Nonindustrial Societies: A Cross-Cultural Study. Yale University Publications in Anthropology, No. 66. New Haven, CT: Department of Anthropology.
Werner, Dennis
1984 Amazon Journey: An Anthropologist's Year among Brazil's Mekranoti Indians. New York: Simon & Schuster.

Taxonomies or Twos

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Lancy and Strathern, in " 'Making Twos': Pairing as an Alternative to the Taxonomic Mode of Representation" (*AA* 83:773–795, 1981), question "the centrality of taxonomies in culture and thought" (p. 774) and assert that in many societies "alternative organizing principles are more important" (p. 774). One such alternative is called "making twos." They conducted three studies: first, a comparative study of ten societies in Papua New Guinea, next, a followup study involving Melpa and Ponam children. It was their conclusion that "Ponam children behaved as if they recognized the taxonomic structure inherent in the stimuli, and could use it to organize and improve recall; Melpa children did not" (p. 775). The results of the second followup study essentially confirmed those of the first, and in the final study only the Melpa children were tested.

Lancy and Strathern give two responses to what "making twos" is: (1) a definition and (2) a discussion of meanings in Melpa that incorporate the concept of making twos. The definition is a notion borrowed from Brown: "a concept ('binary opposition') bearing considerable resemblance to 'making twos' has emerged as an important organizing principle in the development of classification systems (Brown 1979)" (p. 781). For further discussion I add the following definitions: *taxonomy, binary structure, hierarchy,* and *hyponymy.* A binary structure is a representation of meaning such that within it the units of meaning are related to each other by an exclusion relationship. A taxonomic structure is a representation of meaning such that the units of meaning within it are related to each other by an inclusion relationship, with or without the added complexity of exclusion relationships and hierarchies. Units of meaning are said to be in a hierarchy if they are in a taxonomy, and if units included in other units are considered to be lower in the taxonomy, and those which include other units are considered to be higher in the taxonomy. And finally, a hyponymic structure is a representation of meaning such that within it units of meaning are related by inclusion and the terms related may be either in a referential or sense mode (Lyons 1968:453–456).

In addition to these definitions we must assume also that there are at least two ways in which pairing may be an alternative to taxonomizing. First, pairs and taxonomies may be functionally equivalent in the sense that one may substitute for the other so that the meanings are the same no matter which alternative is constructed or used. Or, second, pairing is an alternative in the sense that pairs are neither substitutable for nor functionally equivalent with taxonomies. They both *are* something else and *do* something else.

According to Lancy and Strathern, twos in Melpa are binary and nontaxonomic. Their conclusion rests on the results of experimental tests, especially a class inclusion test, and on an informal discussion of some expressions in Melpa that are twos.

First, I will deal with the nonuniqueness of the meaning of twos and inconsistencies in their informal discussion. The authors present pairs whose constituent terms enter into binary relationships. By virtue of their claim that these are not taxonomic relationships, it must be true that the constituents of these pairs do not enter into inclusion relationships. They present ten pairs and their underlying dimensions. At issue is the status of an underlying dimension. For Lancy and Strathern, if there is no overt lexical label in the language there is no superordinate category and, hence, no taxonomy. And, they claim there are no overt lexical labels for underlying dimensions for pairings in Melpa. But if, as the authors assert, pairings are nonhyponymic, then there can be no dimension of similarity linking the terms of a pair because a dimension of similarity is the equivalent of a superordinate category, even if only in an English metalanguage. However, underlying dimensions of similarity figure prominently in their discus-