Discussion and Criticism

On Chronology versus Geography in the Iberian Mesolithic-Neolithic Transition

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Jackes, Lubell, and Meiklejohn [CA 38:839–46] criticize two of the findings of Lalueza Fox [CA 37:689–95] and Lalueza Fox, González Martin, and Vives Civit [1996] from the analysis of physical anthropological data in the Iberian Peninsula: (1) that there was a morphological shift between Mesolithic and Neolithic populations and (2) that the Iberian populations are highly homogeneous from Neolithic to modern times, the main exception being the Basque population. To support their critique they provide new skeletal evidence from Portugal for the critical Mesolithic-Neolithic period. However, their analysis has some methodological shortcomings that seriously limit the reliability of their conclusions. First, the employment of small samples and only a few craniometric variables yields inconsistent results that do not help to clarify the issue. We feel that samples of fewer than 20 individuals [both sexes] introduce serious noise and therefore should be excluded from the analysis. For instance, Lalueza Fox, González Martin, and Vives Civit [1996] included only samples with more than 30 individuals for neurocranial variables and more than 15 for facial variables [each sex], most of them consisting of 100 individuals. Second, Jackes, Lubell, and Meiklejohn claim that it is better to use pooled male and female data, but in using our skeletal data they simply take the average of female and male sample means. This is statistically incorrect. For instance, in the Tarragona Roman population, females constitute 37.6% of total individuals, while in Medieval Muslims from Granada they constitute 50% of total individuals. Thus the “pooled” sex data of Jackes, Lubell, and Meiklejohn are likely to increase the differences between the two populations, making the craniometric variables of the Muslims smaller with respect to the Roman population. The combination of the two factors [small sample sizes and incorrectly “pooled” samples] invalidates many of the analyses the authors provide.

Despite the claims of Jackes, Lubell, and Meiklejohn, there is a clear morphological break between Mesolithic and Neolithic Iberian populations. For instance, while brachycranial individuals are commonly found in Mesolithic sites such as Moita do Sebastião [Portugal] [Ferembach 1974] and La Oliva [Valencia] [Pérez-Pérez et al. 1995], they are completely absent in Neolithic sites, where individuals are almost exclusively dolichocranial. Other differential morphological traits include, as our critics themselves mention, larger tooth size in Mesolithic than in Neolithic groups. This large dento- sition is especially apparent at La Oliva, where some dental measures are close to or even larger than Upper Paleolithic dental averages. Probably related to this trait is the marked subnasal prognathism observed in some Mesolithic individuals from Muge and La Oliva, a feature residually observed in some Neolithic individuals, such as those from Sant Pau del Camp [Barcelona] and Bôvila Madurell [Barcelona]. Tooth size and morphol- ogy have a strong genetic component and therefore have been extensively used for phylogenetic purposes. The dental and cranial Mesolithic-Neolithic disruption does not seem to be attributable to any dietary shift associ- ated with food production, since other variables more dependent on diet and nutrition, such as stature and dental caries, do not show any dramatic change during this period [Lalueza Fox and González Martin 1997, Lalueza Fox 1997].

We agree with our critics that two samples are not enough to represent the complex patterns of the Meso- lithic-Neolithic transition; in this sense, our studies may represent an oversimplification of the history of the Iberian population. However, the answer is not in- cluding small samples, which can introduce enormous biases. At least those for Portuguese Visigoths [Cunha and Neto 1953], Eira Pedrinha [Mendes Corrêa and Teixeira 1949], and Escoural [Isidoro 1981], with maxi- mum sizes of fewer than 15 and usually only 5–10 indi- viduals, are clearly inappropriate. Even taking into account these statistical limitations, however, some interesting inferences can perhaps still be derived from the discriminant analysis displayed by Jackes, Lubell, and Meiklejohn. The simultaneous representation of the first three functions is misleading, for the first func- tion alone accounts for 67.81% of the total variation. On the basis of this first function, the Mesolithic Muge and Basque populations appear to be very close, and this seems to suggest some morphological affinities be- tween a Mesolithic and a modern (Basque) population. This relationship would be surprising considering the geographical and temporal differences [around 7,000

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years and 750 km] but is perfectly plausible if the hypothesis that the Basques are a pre-Neolithic, relict population is correct. Thus, Neolithic migration seems to be the only thing that could plausibly result in a general post-Mesolithic morphological uniformization.

We agree with Jackes, Lubell, and Meiklejohn that it is interesting that the Portuguese populations [Mesolithic, Neolithic, and Visigothic] tend to cluster together. Even considering the possibility of severe sample-size biases, this clustering seems to suggest some geographic, long-term morphological affinity in Portugal. This could be related to the existence in the Mesolithic in the Iberian Peninsula of some densely populated places that experienced a more limited genetic impact during the Neolithic. Further skeletal evidence could confirm or reject the existence of such continuity in these special areas.

In our opinion the morphological evidence still suggests significant population replacement in the Neolithic. As is evidenced by the apparently contradictory data from classical genetic markers (see, for instance, Cavalli-Sforza, Menozzi, and Piazza 1994) and mitochondrial DNA (see, for instance, Côte-Real et al. 1996), a different question would be to estimate the importance of Neolithic migration. Even if rather restricted genetically, it could have had a strong impact on cranial morphology. However, we want to emphasize that we do not take a dogmatic position with respect to the Mesolithic-Neolithic transition in the Iberian Peninsula; we are ready to change the paradigm of chronology versus geography if conclusive evidence for continuity can be found.

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On “Robbing Native American Cultures”

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The forum on “Van Sertima’s Afrocentricity and the Olmecs” (CA 38:419–41) is not a forum in the usual sense of the word—on which all or many views are expressed. The only contention among the commentators is over whether such “rubbish” should be contested at all. It is also disturbing that the most important contribution, that of Van Sertima himself, was not accepted on the basis of what appears to have been a technicality.

I am also struck by the article’s emotional tone. The first word of the title is “Robbing,” and it ends (p. 431): “Van Sertima has, in effect, trampled on the self-respect or self-esteem of Native Americans by minimizing their role as actors in their own history, denigrating their cultures and usurping their contributions to the development of world civilizations.” The same emotional tone pervades the comments. Michael Coe, for instance, begins by stating that Van Sertima’s claims belong in a “historical dustbin” (1997:432). This is not to deny that Van Sertima himself puts passion into his historiography. But the forum, presented as a collective voice of reason, seems in fact to have been a passionate attempt to stamp out heresy rather than a measured response to a serious challenge to the academic disciplines concerned with ancient America.

There is also a failure to recognize that Van Sertima is fundamentally unlike some Native Americans and radical relativists who object on principle to “Western” scholars’ applying their modern “objective” methods to what they see as sacred objects. Van Sertima is attempting to play the scientific game, using argument and ordering evidence in very much the same way as defenders of the status quo.

Van Sertima’s ideas are also unlike those of, say, Velikovsky in that they do not go against the canons of natural science, nor do they involve massive catastrophes or extraterrestrial intervention like those of Von Däni-
ken. There is nothing inherently impossible in there having been contacts between Africa and America at any time in the past 5,000 years.

This leads to a second issue. It is disappointing not to find any introspection as to why Americanists should find the possibility of pre-Columbian contacts between Africa and America beyond the pale. At one level, there is the objective fact that, with the exception of the Inuit, American societies were and are very different from any found in the “Old World.” There is also no doubt that the Atlantic and the Pacific are substantial barriers and that any contact across them must have been intermittent. Nevertheless, can we be sure that these barriers were impermeable?

At the level of the sociology of knowledge, there is the natural scholarly tendency to maintain that academic institutions hold a monopoly of wisdom. In many or most instances, this is indeed the case. However, there have been issues on which, in retrospect, one can see that outsiders were right and the professionals mistaken. For instance, while today we believe that William Jennings Bryan was wrong to attack Darwinian evolution, his bimetalism and attack on the gold standard are now accepted as having been correct. At the time, however, they were denounced as absurd by every academic economist. Similarly, the shady entrepreneur Heinrich Schliemann and the architect Michael Ventris founded Mycenaean studies in the teeth of the Classical Establishment. Thus, the facts that Ivan Van Sertima was trained in Swahili lexicography, not Mesoamerican studies, and belongs to a subculture that sees itself as persecuted do not in themselves mean that he must be mistaken. It is his ideas not his status or his background that should be confronted. Such a confrontation between academia and outside subcultures is not always a waste of time. In some cases it can be very fruitful. For instance, the sociology of Durkheim and Weber, though not admitted as such, was an academic reaction to the Marxism being taught in the Socialist party and party schools in Germany.

There are, of course, specific reasons that Americanists should react so vehemently to this particular challenge. American cultures seem to provide a wonderfully well-defined academic turf whose barriers it would clearly be painful to surrender. Beyond this there is the traditional anthropological attachment to purity and the deep romantic belief that a culture is somehow diminished by mixture. Such views, however, go against the modern anthropological interest in cultural contact and hybridity. A picture of sporadic trans-Atlantic contacts and cultural exchange among the continents in no way undercuts the brilliance or originality of American cultures. It is particularly striking that Mesoamericanists studying the wonderful cultures derived from many different American civilizations as well as post-Columbian European and African ones should be so passionate in their defense of the purity and autochthony of early American civilizations.

While genuinely champions of American cultures, modern Americanists are also heirs to the strong scholarly tradition that sees Europe as the continent that connects all the others and specifically considers Europeans the first people to sail to America. The survival of this tradition can be seen in the extraordinary tenacity with which archaeologists clung to the Clovis Horizon and its corollary that the ancestors of the Americans walked across Beringia. What is more, now that the Horizon has been breached, the assumption persists that the crossing took place during some previous Ice Age. Given the fact that humans reached Australia over water some 40,000 years ago and the impassability of an “ice-free corridor” during ice ages, it would seem much more likely that a crossing at such high latitudes was made by water during a warmer period.

The notion that only Europeans could have sailed to America is not opposed by the acceptance of the Viking voyages to Vinland, which Haslip-Viera and his coauthors cite as an example of their open-mindedness (p. 428). As my former colleague Tom Lynch put it, he as an Americanist possessed a hierarchy of disbelief on pre-Columbian contacts with America: European, trans-Atlantic, trans-Paciﬁc Asian, and ﬁnally African. In fact, trans-Atlantic contacts at low latitudes are as easy as or easier than the other two.

Another area where some introspection among the authors and contributors would be welcome is their requirement of “proof” or “certainty.” Haslip-Viera and his colleagues frequently use the argument from silence, arguing that the absence of any extraregional material object found in a controlled archaeological dig invalidates all claims of contact (pp. 422, 428). This is a standard that is frequently required of challenges to the status quo (though less often of defenses of it). Proof is very difficult to obtain anywhere outside of formal systems of mathematics and is certainly unattainable in prehistory. All one can hope for here is competitive plausibility. None of the contributors mention the articulate—and to my mind convincing—arguments against the necessity of proof with regard to transoceanic contacts made by Ekholm (1971:45–60) and Kelley (1971:61–65). To take a historical example, it is now accepted that Vikings settled in Newfoundland in the 11th century A.D. Was it permissible to postulate such settlements before Ingstad’s excavations at Anse aux Meadows? I would argue, given the textual evidence and the technical and geographical plausibility, that it was.

When assessing possibilities of transoceanic contacts one should consider the competitive plausibility of both diffusion and isolation. Van Sertima applies Tylor’s three criteria of arbitrariness, collocation [at least in the larger sense], and gradual development in the source culture. For instance, he attempts to link the Olmec heads to the earliest American pyramids and to von Wuthenau’s terracotta ﬁgurines of African appearance, none of which is a cultural necessity. For contacts in the 2d millennium A.D. he links historical reports of Abu Bakari II’s voyage into the Atlantic with the pre-Columbian introduction of American cotton and maize to Africa, very early reports of African blacks around the
Caribbean, and the archaeological reports of African skeletons in pre-Columbian contexts in the Virgin Islands [1992b:33].

Having read most of Van Sertima’s work on the subject and after a couple of conversations with him, I have formed a very different view of his motivation from that expressed in the forum. I have never heard him assert, as Ann Cyphers suggests, that the ancient people of the Americas could not have attained such a level without aid from elsewhere [p. 433]. His view is that African intervention was contingent not necessary to the development of American civilizations. To be presumptuously psychological, I am inclined to believe that the American component in his ancestry and his boyhood among Americans in inland Guyana may have inclined him to welcome ideas that linked his two continents. Van Sertima’s passion is not anti-American but anti-European [see, e.g., the tone of his attack on what he sees as the lack of Viking influence on American cultures [Van Sertima 1976:77]].

Although all hypotheses of non-European contact have been generally rejected by professionals, those between America and Asia have not been attacked with the violence used against Van Sertima. One reason for this is clearly that the proponents of Asian connections with America have tended to have acceptable academic credentials. The genuine forum given in CA to Jeffreys’s [1971] proposals would seem to bear out this view. Even so, Van Sertima’s appeal to nonacademic audiences should not be taken as grounds for dismissing his views without a hearing. His proposals are serious and deserve to be debated rather than denounced.

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On Network Analysis: The Potential for Understanding (and Misunderstanding) !Kung Hxaro

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Schweizer’s social network analysis (CA 38:739–52) of gift giving among the !Kung San (Ju/'hoansi) demonstrates most elegantly how individual strategies, guided by basic cultural rules, coalesce to form a regional system. Complex connections in the network that defined description with simpler analytical techniques are brought within reach. Hours upon hours of sitting in the sand and recording the sources of possessions—from arrows to underpants—are projected into a grand finale in Lothar Krempel’s multidimensional diagrams. The publication of such tools in a journal that reaches out to a broad audience is most welcome.

Though in possession of powerful technology for communication, ironically, researchers are not as connected as Kalahari Bushmen. I had extensive discussions with Thomas Schweizer when I handed the data over to him and commented on a draft of an earlier publication [Schweizer 1996]. However, I did not see a draft of this article that takes quite a different direction until CA arrived in my mailbox. Here I would like to comment on his results one by one, questioning aspects of the analysis and interpretation and suggesting that the potential of the tools presented may be far greater than Schweizer demonstrates if quantitative and qualitative analyses are more systematically integrated.

1. The data. I gave Schweizer two separate data sets; he used one in his article, but I will outline both and refer to both in my comments. The first was a tally of

1. For consistency I have used the same spellings of !Kung (Ju/'hoansi) by Schweizer to denote his use of the Ju/'hoan language [1996]. This has been accepted by the Nyae Nyae Farmers’ Cooperative. Correct spellings of words and place-names are placed in brackets after each word when it appears for the first time.
all possessions of the 73 people in my sample, how or from whom each possession was obtained, and attributes of the giver—age, sex, location, and kin relation to ego. The people interviewed represent approximately 50% of the residents of Xai/Xai [Kae/Kae], 10% of the population of the Dobe area, and 5% of the population in the many villages at Tsumkwe (Tjum!kui). Many givers of hxaro (xaro) gifts were therefore people not included in my sample. The second data set, sent later, was a list of all hxaro partners of the 73 !Kung in my sample and their attributes. I tried to verify these partnerships carefully by recording the history of exchange of each dyad and cross-checking the relationship with both partners whenever possible. The data analyzed by Schweizer come from the first set and include only possessions that were received as hxaro gifts. Those which were made, purchased, or obtained from non-!Kung were omitted from the analysis.

2. Density. The finding that the hxaro network is sparse is significant in that it contrasts with the density of kinship network, formed by three overlaid systems of kin classification that make it possible to classify a very wide range of people as kin, as demonstrated in Lee’s (1986) superb analysis. However, classification does not specify obligations to kin beyond close family members and affines. From the !Kung viewpoint, then, people live in a dense web of kinship—hxaro simplifies by specifying which of this myriad of relations one is responsible for, whom one “holds.” The sparse network of hxaro is thus a product not of a widely dispersed population and walking distances, as Schweizer suggests, but of intentional placement of obligations. Comparison of the hxaro network with webs of food sharing would almost certainly yield different densities. Hxaro builds sparse networks to allow people to redistribute over the resources of the region, ties of food sharing create community among people living in one place—both residents and visitors—and are dense within a given location. In short, the !Kung kinship network is dense; hxaro is one way of placing more binding obligations.

3. Connectedness. Schweizer’s measure of the connectedness of the hxaro network underlines the efficiency of hxaro as a system for risk pooling. In a highly connected network, information about social and natural resources flows freely and facilitates the redistribution of people over resources. Two other features of hxaro may make it even more connected than the analysis indicates. First, spouses always do hxaro (Wiessner 1982), not only consanguines; for some gifts received from consanguineous kin [but not all] to be passed to a person’s spouse and on to his or her kin is a very important obligation of hxaro. Second, even though connections further information flow, they do not imply any obligations regarding assistance between those who are indirectly connected. To channel obligations in this highly connected network, !Kung concatenate partnerships into long chains that criss-cross the Kalahari (somewhat like the Orokaiva networks described in the comment by Hage and Harary). These chains have a prescribed course and are reproduced through time (Wiessner 1986), giving people access to the resources of those who are beyond the bounds of ordinary kinship reckoning.

4. Equality. The data that Schweizer analyzes are not suitable for examining equality of participation in hxaro. Hxaro is delayed, alternating reciprocal exchange, and therefore the gifts possessed by a household at any one point in time do not reflect overall participation in the system. Centrality in the network can be measured only by number and location of hxaro partners or by the flow of gifts to and from a household over a year or longer. On any given visit it is not unusual to find that the “big wheels” of hxaro have little left in their houses, for success in hxaro comes from giving rather than retaining. The data set listing number of hxaro partners does indeed indicate considerable inequality (Wiessner 1982), with some !Kung having fewer than 5 partners and others more than 30. However, individuals identified as central by their number of hxaro partners do not correspond closely to individuals identified as such in Schweizer’s analysis of number of gifts present in a household at the time I did the tally of possessions. Such results would be predictable in all networks of reciprocity where a primary value of goods lies in their capacity for underwriting social ties via exchange and where influence comes from distribution of material goods rather than their retention.

5. Nonsymmetry. Schweizer’s conclusion that hxaro is often not symmetrical (A gives to B, but B does not reciprocate) is baffling in view of his awareness that reciprocation in hxaro must be delayed. To reciprocate immediately indicates a desire to balance a partnership so that no obligations remain and it can be canceled. Alternating delayed reciprocation means that in a dyad, A and B, one would expect only one partner to possess a gift from the other. Furthermore, since hxaro gifts travel along chains of partners, from A to B to C to D to E to F and back, it should not be unusual for neither A nor B to possess a gift from the other at any given point in time. In view of the structure of gift giving in hxaro, Schweizer’s results show the opposite of what he argues: that reciprocation occurs at a much higher rate than would be expected. That is, 48% of the time both A and B possess gifts from each other, while predictions from the above rules would be that this situation should rarely occur. I can suggest two explanations for this, both of which could be tested. First, in hxaro partnerships between individuals who have strong emotional bonds, for instance, mother and daughter or sisters, gifts are exchanged more frequently than convention requires out of spontaneous expressions of affection. Here emotions cause behavior to deviate from the cultural norm. Second, high frequency of giving can be linked to the recent flood of store-bought goods into the hxaro system with the establishment of stores in the study area and opportunities to earn cash. The relatively easy access of some !Kung to such goods bred jealousy that
was appeased by their circulation through **hxaro**. The high rates of giving found in the 1970s thus may be a good indication of increasing embeddedness in the regional and national economies of Botswana and Namibia.

Schweizer cites as further evidence for nonreciprocity the fact that in the list of **hxaro** partners, 15% of partners were named by one partner in a **hxaro** dyad but not by the other. This is indeed the case, though such exceptions were not “undetected” inconsistencies but points of inquiry during fieldwork. Lack of mutual acknowledgment of **hxaro** partnerships occurred when adolescents first entered **hxaro**, when elders who were no longer physically mobile passed on relationships to their children, or less frequently when new partnerships were initiated or old ones, which were no longer of interest to either party, dwindled. In these cases, one partner, usually the younger, listed the relationship as **hxaro** while the other said that he or she would wait and see what developed. The instability that occurs is therefore related to the waxing and waning of relationships with the life cycle.

In short, for most of the above points I feel that Schweizer’s analysis and interpretation from the viewpoint of network analysis fall short of their potential or lead to untenable conclusions. This is to be attributed not to shortcomings in the tools themselves but to the pursuit of “straw men” of density, reciprocity, and equality constructed rather superficially from the forager literature to demonstrate the power of the method. Implicit in this approach is the assumption that once qualitative background has been presented, quantitative analysis can take over. But qualitative and quantitative data are complementary and should be intertwined at every stage of the analysis and interpretation for optimal results. Once analysis and interpretation have been fine-tuned to conventions of the specific culture in question, the results can be used to shed light on broader issues.

6. **Assessing embeddedness.** !Kung **hxaro** over the past 100 years has been both a system which enmeshes the individual in a regional network of other !Kung to cope with social and environmental risks and one which provides points of articulation with the economies of surrounding populations. Though it is the topic of his paper, Schweizer dismisses the second issue by saying that unfortunately we do not know how **hxaro** and related institutions of !Kung society were affected by developments at the regional and national levels in this century. But this is simply not the case [Smith and Lee 1997, Wiessner 1994, Wilmsen 1989]. Here I will elaborate on this point not as a critique of Schweizer’s work but to give a glimpse of the potential of network analyses for studies of change through time.

Ethnohistorical and archaeological studies of **hxaro**, though yielding no quantitative data, can be used to establish a baseline for tracing developments in embeddedness. Early in the 20th century, **hxaro** chains are said to have stretched across hundreds of kilometers of Kalahari to export desert products and import desired trade goods, particularly pottery, beads, and metal. This means that the “outer network” portrayed in figure 1 of Schweizer’s paper was more developed in the past to tap into the trade of southern Africa. It is probably for this reason that !Kung **hxaro** has features atypical for forager exchange systems: semiformalized partnerships, concatenation of exchange partnerships into chains with a prescribed course, and inheritance of partnerships [Wiessner 1994]. Between the 1930s and the 1950s, when desired goods were brought within reach by pastoralists who settled permanently in the study area, and in the 1960s and 1970s, when stores were established at Tsumkwe and Qangwa, the importance of the outer network declined. Starting in the 1960s, !Kung from the Nyae Nyae area were encouraged to settle at Tsumkwe by the South African administration, attracted by possibilities for wage labor, agricultural programs, a school, a store, a clinic, and hopes for an easier life. The **hxaro** chains that used to link villages of the Nyae Nyae area with each other and points afar were then rewound through the many camps of Tsumkwe, circulating goods to appease jealousy, mediate conflict, and dampen the development of social inequalities.

Some of the developments of the 1970s are indeed reflected in the data set on possessions but unfortunately in the very portion omitted by Schweizer, namely, the 31% of possessions not received in **hxaro** but manufactured, purchased at stores, or received from pastoralists or anthropologists. Inclusion of these data in the analysis would link the !Kung **hxaro** network to regional and national economies, as would data on the source of cash used to purchase store-bought goods. Some questions regarding the effect of other economies on !Kung **hxaro** could then be measured. For example, what is the relation between participation in the **hxaro** and involvement in the broader regional economy? Do !Kung who have cash incomes or !Kung who work for pastoralists participate more or less actively in **hxaro**? Further, what is the effect of the influx of less durable store-bought goods on patterns of **hxaro** exchange? Are more traditional items such as beadwork made with ostrich eggshells or glass beads and arrows circulated differently from store-bought goods?

During the years that have passed since the 1970s, !Kung of the Nyae Nyae area have seen equally great economic change: the formation of the Nyae Nyae cooperative [Biese 1990, Lee 1993, Marshall and Ritchie 1984], the resettlement of Tsumkwe residents on their traditional lands in permanent, sedentary communities, the provision of reliable water, government rations, and pensions, local schools, crafts marketing, and agricultural programs. Preliminary analysis of data comparable to those collected in the 1970s suggests that the input from national programs and international donors has had a marked impact on **hxaro**. The breadth of individual networks has decreased, the average !Kung of the 1990s having 7 partners whereas the average !Kung of the same population in the 1970s had 16 [Wiessner 1997]. **Hxaro** chains are vanishing as material goods become readily available in the area. The data on source of...
sions today are received in *hxaro* in comparison with some 65% in the 1970s. However, the percentage of possessions purchased or obtained from non-San has remained virtually unchanged. Despite narrower networks, no goods have moved into the category of commodities to be bought and sold or of private property to be retained. The primary value of material goods as a currency to create, maintain, and reproduce social relationships through gift exchange appears to have held fast. Nonetheless, there are indicators that some of the former rules of exchange are being altered. While virtually all material possessions obtained from other !Kung in the 1970s were given as *hxaro* gifts, today 14% of possessions are received in casual giving between !Kung friends who cannot be readily identified as kin (Wiessner 1997). Individual action is thus breaking with traditional norms which stipulate that intra-!Kung gift giving should be embedded in kinship and *hxaro*. This trend towards separation of giving and kinship, if continued, will represent an important restructuring of !Kung social relations, one which is well-suited to the mobility and casual contacts of life today.

Comparison of data from the 1970s with those from the 1990s will raise many questions: How do permanent settlement and resource security affect degree of embeddedness of individuals in the !Kung population? What is the impact of increased integration into the national economy on former exchange systems? Will the networks of Dobe, /Xai/xai, and Nyae Nyae !Kung begin to take on characteristics of networks of surrounding populations, or will they continue to maintain a character of their own? To answer these complex questions, qualitative techniques and basic descriptive statistics will give clues but fail to grasp the broader picture of change. The tools presented by Schweizer, by contrast, provide a means for precise, systematic, and encompassing description and comparative analysis. For these issues, their potential is indeed great.

**Reply**

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I welcome Wiessner's comments on particular interpretations advanced in my reanalysis of her data, the new hypotheses she develops in response to my analyses, and her interesting sketch of changes in *hxaro* in the 1990s. Since she had commented extensively on and much improved my earlier restudy of her data (Schweizer 1996), I had not expected that including basically the same formal findings in my CA article would prompt additional comments from her. Had I known that switching the conceptual framework (from social network thinking as in my earlier paper to embeddedness and globalization in the present one) would elicit more in-depth remarks, I would have seized the opportunity to learn more about the case prior to publishing my analysis. Since my reanalysis of Wiessner's data is not yet finished, I shall take up her hints in future work (and make sure that I get her comments on the first draft!). I fully agree with her that formal and qualitative analysis are complementary. In the *Kung* reanalysis, because of lack of fieldwork in the Kalahari, I can only play the quantitative part, which may create the false impression that quantitative tools take over qualitative analysis. In my view there should be constant feedback between these different but complementary approaches.

This brings me to a major methodological insight that I gained from Wiessner's comments on my reassessments of her data and from work on ethnographic data that I collected myself in Java: analyzing ethnographic data of one's own, one is in a privileged situation compared with the secondary analysis of data obtained from others. One can draw on very rich background knowledge to guide one's imagination and rule out many possible interpretive hypotheses as implausible. In the case of secondary analysis the background knowledge is thin and one is not guided by deep local knowledge. Therefore one will come up with many interpretations that do not impress firsthand observers because the findings are well-known; some hypotheses drawn from ethnographic sources will look like straw men, and some effects will be easily explainable by local experts as special cases. Still, most restudies of extant data with new methods and new theories, even by researchers who have not been “out there,” can lead to useful results. Since ethnographers tend to overestimate pattern, formal analysis is a crucial test of the validity of interpretations, and it can reveal pattern that has been overlooked by the ethnographer [e.g., Hage and Harary 1997, Houseman and White 1998]. Although she corrects some errors of mine, Wiessner seems to be convinced of the usefulness of social network analysis and even envisages more in-depth insights from its proper application—as do I.

Now, the field of Bushmen studies and Wiessner's research in particular are difficult terrain for secondary analysts. First, the ethnographic evidence has been deeply debated and assessed in the very competent scientific community specialized on the Kalahari (see Kent 1997 on the present state of knowledge in this field), and therefore it is hard to come up with new findings. Second, Wiessner's extensive and excellent fieldwork has already combined qualitative and quantitative approaches. In this situation social network analysis cannot yield a quantum leap of understanding but can only add some new brushstrokes to an already well-painted image. For instance, we can visualize the *hxaro* network and provide some formal underpinning to the net-
work concepts used in Wiessner’s analysis by computing graph-theoretic indices. This is what I did in my earlier paper (Schweizer 1996) and in my CA article. Since Wiessner is the ethnographer, I fully accept her factual revisions of some of my interpretations. However, I take the opportunity to clarify three points that are of more general interest:

1. Hxaro goods and non-hxaro possessions: In my brief reanalysis of the !Kung case I focused on hxaro and not on the Western goods that !Kung receive as payments, commodities, and presents from the outside world because it seemed to me more convincing to engage in a counterintuitive argument and demonstrate that even in the most traditional part of this society world-system impact is massive. The embeddedness of !Kung in the outside world of course becomes even more visible when we take into account the whole material sphere, including non-hxaro goods bought in the market.

2. Graph-theoretic indices and the special role of asymmetry and centralization. When a network analyst assesses a substantive problem at hand, it is standard procedure to compute some basic formal indices. We want to know, first, whether the network is connected (does it contain segregated segments, or even isolates?). This is an assessment of general flow in the network. Second, we look for the overall density to assess the volume of flow and the extent of cohesion among members of the network. Third, we study the centrality of individual points and the overall centralization of the network as indicators of inequality in the extent of flow and the capacity to disturb flow in the network. Fourth, we measure the (a)symmetry of flow in the network to investigate reciprocity. Once these formal indices have established a basic technical understanding of the network one can proceed to the crucial task of interpreting the formal findings substantively. Here ethnographic background knowledge but also data on comparable systems of exchange (as used by Hage and Harary in their comment on my paper) come into play. Clearly, given Wiessner’s data collection procedure for her first data set (only in-coming presents were counted) and the delayed nature of hxaro exchange one would expect a high degree of asymmetry. This is not really the case, and Wiessner is right that one should try to explain why so many “symmetric” ties occur. In contrast, for her second data set, covering long-standing hxaro partnerships, one would expect symmetry, but there are some asymmetric exceptions, which I noted. In her comments she explains why these exceptions occur. My main methodological point is that one needs to compute all these basic indices to arrive at a sound formal grounding irrespective of the substantive relation that is being investigated. Older social network studies pulled out only a few of these indices, such as density or centrality, and therefore tended to be biased, whereas I would propose to assess the formal properties of a network very broadly even when we know that asymmetry is not the key issue for the data at hand. As for the two data sets that Wiessner has collected—in the paper I focus on the possessions data set, and she suggests that I would bet-ter have studied the second data set for some of my questions—let me state that they are not massively different. When one concentrates on the 54 individuals represented in both data sets, the correlation between the two matrices is $r = .66$, which is quite high. As Wiessner has expected and I have already said (p. 745), the pattern is tidier in the cognitive partnership data set than in the behavioral possessions data set. But the patterns are rather similar: actors possess on the average about 5.5 gifts in the partnership data set compared with 3.7 in the possessions data set, and the centralization of the network is higher, about 15% compared with 10%. This is still not a very high degree of inequality in the sense of differences in hxaro activity among actors and fits the image of the !Kung as a sharing-oriented foraging society.

3. Changing hxaro. There is a slight misunderstanding on this point. I know that there are historical data on the development of this institution and social change among the !Kung. What I wanted to put on the agenda is a need for longitudinal network data, that is, the collection of exchange and partnership data for the same set (or a subset) of actors at a second point in time. With such data one could really assess stability and change in a process model and test whether the central actors at the first time are indeed peripheral at the second—as one would expect under the norm of sharing. Fortunately, Wiessner has just completed such a re-study, and I am looking forward very much to seeing her report and eventually being able to add a temporal dimension to my network restudy of her data—which will include more intense communicative flows between us in the course of data analysis.

References Cited


### Calendar

**1998**

**October 12–16.** 5th Congress of the Latin American Association of Biological Anthropology/6th Luis Montané Symposium on Physical Anthropology, Havana, Cuba. Write: J. Martínez Fuentes, Facultad de Biología, Universidad de La Habana, La Habana 10400, Cuba.

**November 12–15.** American Society for Ethnohistory, 1998 Annual Meeting, Minneapolis, Minn., U.S.A. Write: Jean O’Brien-Kehoe, Department of History, University of Minnesota, 614 Social Science Tower, Minneapolis, Minn. 55455, U.S.A. [obrieo002@maroon.tc.umn.edu].

**November 19–22.** International Union of Pre- and Protohistoric Sciences, InterCongress Meeting, Commission 4, Data Management and Mathematical Methods in Archaeology, Scottsdale, Ariz., U.S.A.

**Write:** Keith Kintigh [kintigh@asu.edu] or George Cowgill [cowgill@asu.edu], Department of Anthropology, Box 872402, Arizona State University, Tempe, Ariz. 85287-2402, U.S.A.


**1999**

**January 10–14.** Fourth World Archaeology Congress, Cape Town, South Africa. Theme: Global Archaeology at the Turn of the Millennium. Write: Carolyn Ackermann, WAC4 Secretariat, P.O. Box 44503, Claremont 7735, South Africa.

### Errata

In the Discussion and Criticism item on treponemal disease by Heathcote et al. in the June issue, two corrections should be made: On p. 360, col 1, first full paragraph, point (5) should read “claiming support for the Columbiaan theory on the New World origin of syphilis [see Baker and Armelagos 1988].” In the list of references, p. 367, col. 2, the Price et al. article is entitled “Parasite Mediation in Ecological Interactions.”

### Institutions

The University of Arkansas, Fayetteville, announces a new interdisciplinary Ph.D. program in environmental dynamics, focusing on human-environmental interactions within recent Earth history. The program emphasizes interdisciplinary regional analyses of geophysical, biological, climatic, and sociocultural factors. Appropriate techniques such as GIS, GPS, remote sensing, computer modeling, and computer cartography are integrated into the program. Master’s graduates in anthropology, geography, geology, biological sciences, agronomy, and related fields are encouraged to apply. Outstanding B.A./B.S. holders may be admitted directly into the program but must complete M.A./M.S. requirements in anthropology, geography, or geology. For information and application materials write: Program Director, Environmental Dynamics Program, Old Main 525, University of Arkansas, Fayetteville, Ark. 72701, U.S.A. [endy@comp.uark.edu].
Prizes

The American Society for Ethnohistory reports the awarding of two annual prizes: For the best book-length work in ethnohistory, the 1997 Erminie Wheeler-Voegelin Prize was awarded to Kathleen J. Bragdon (College of William and Mary) for Native People of Southern New England, 1500–1650 (Norman: University of Oklahoma Press, 1996). For the best article in ethnohistory, the 1997 Robert F. Heizer Prize was awarded to Tamara Giles-Vernick (University of Virginia) for “Nalege ti guiriri (On the Road of History): Mapping Out the Past and Present in M’Bres Region, Central African Republic” (Ethnohistory 43 [1996]:245–75).