Discussion and Criticism

On Culture and Traditional Chimpanzees

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A confluence of information from field and laboratory observations with chimpanzees, comparative genetic studies, and cladistic analyses of extant and fossil hominoid morphology has persuaded a cohort of behavioral scientists to declare that chimpanzees are cultural beings (see Boesch and Tomasello, CA 39:591–614, and cf. Whiten et al. 1999). Correlatively, the more humanoid chimpanzees are and the more we see ourselves as merely privileged apes, the more obscene it is to treat them as though they had fewer human rights.

Boesch and Tomasello argued (p. 591) that “culture is not monolithic but a set of processes.” Because there is “little agreement among anthropologists on precisely what is meant by the term ‘culture’ as it is applied to human social groups,” they explored how a concept of culture could be devised to embrace the chimpanzee case. There has been some confusion between product and process, with the assumptive former taken to indicate presence of the latter. Before we label chimpanzee demic traditions “cultures,” chimpanzees need to be shown to be cultural beings (Tuttle n.d.). Process is predominant in this exercise. Accordingly, one would have expected much more thorough digestion of the information that has emerged from a half-century of intensive anthropological research, particularly that of the American school (Kuper 1999, Shore 1996), on the nature of culture.

Although Kroeber and Kluckhohn [1952:357] confessed that they had “no full theory of culture,” they articulated the “central idea [that] is now formulated by most social scientists approximately as follows: Culture consists of patterns, explicit and implicit, of and for behavior acquired and transmitted by symbols, constituting the distinctive achievement of human groups, including their embodiments in artifacts; the essential core of culture consists of traditional . . . ideas and especially their attached values. . . .” I doubt that there is much disagreement among anthropologists and sociologists today that key to the concept of culture is symbols and symbolically mediated ideas, values, and beliefs, however difficult it may be to explicate the precise psycho-

logical, neurophysiological, and social processes that underpin them or to discern them from behavior, narratives, or texts.

Boesch and Tomasello (p. 610) attempted “to bridge the gap between the views of culture typical in . . . biology and psychology and to find common ground between them” and concluded that “there seems to be enough common ground concerning processes of culture and cultural evolution that investigators from many different disciplines can begin to make their voices heard in a way that results in an accumulation of modifications to the concept of culture that will facilitate everyone’s empirical work” (p. 611). Until the newly professed cultural primatologists engage the scholarly corpus of five decades of research on the concept of culture by a notable roster of cultural anthropologists (Kuper 1999), I doubt that much empirical progress can be made toward discerning naturalistic humanoid cultural capacities in chimpanzees and other nonhuman species and especially the phylogeny of human cultural capacities. While not denying the importance of understanding how animals, including people, learn and transmit behavior to conspecifics spatiotemporally, we need to focus on whether and, if so, how symbolic mediation might be involved in naturalistic behaviors of other animals.

It is unfortunate that, beginning with Tylor [1871], definitions of culture restricted the phenomenon to Homo sapiens and that many sociocultural anthropologists believe that to search for culture in other animals is futile. However, this should not dissuade us from searching for symbolically mediated, shared systems of meaning among chimpanzees and other animals. Indeed, I expect that instead of coming of age (de Waal 1999:635) cultural primatology will come a cropper unless fresh, broader interdisciplinary approaches are developed and applied in the search for nonhuman cultural (cf. symboling) beings.

To date, little more has been demonstrated than that chimpanzees have local or demic behavioral traditions that are learned somehow from conspecifics. No one has shown that naturalistically chimpanzees have symbolically mediated ideas, beliefs, and values, the sine qua non of culture (Tuttle n.d.). Indeed, one barely encounters mention, let alone detailed discussion, of symbols in the arguments for naturalistic chimpanzee culture. Instead, there is an emphasis on how behavioral traditions are learned and passed on socially and on the idea that the variations in or tangible products of chimpanzee behavior are not a consequence of physical environmental [a.k.a. ecological] influences or genetic transmission. The nature of culture itself and especially the mechanism[s] by which meanings are encoded for the chimpanzees are missing from the discussion.

While I agree that spoken language—a cultural cate-
gory—need not be invoked as the criterion for culture in other animals, nonetheless the challenge remains to discern behaviors that are influenced by symbolically encoded meaning. Combined with growing understanding of human cultural cognition, particularly from the studies of developmental and cultural psychologists (Tomasello 1999, Cole 1996), this would give us a better base for modeling the evolution of human cultural capacities and for appreciating actual similarities with and unique features of chimpanzee minds.

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On Misconceptions of Evolutionary Archaeology: Confusing Macroevolution and Microevolution

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Larson (CA 41:840–41) argues that evolutionary archaeology’s central premise is that “artifacts are part of the human phenotype and therefore subject to the same evolutionary processes . . . as any somatic feature.” He takes the inferred stability of cliff-swallow nests and the coincident morphometric stability of the species over the past 40 millennia as support for “the punctuated-equilibrium contention that most species remain in morphological stasis during their evolutionary histories” (p. 840). He therefore considers it incumbent upon evolutionary archaeologists to explain the conservatism of cliff swallows’ nest-building behavior in the face of the diversity of material cultural phenomena and sociocultural behaviors produced by New World humans in the same or much less time.

First, punctuated equilibrium is not stasis punctuated by abrupt phyletic change; it is stasis punctuated by abrupt cladogenesis (Eldredge and Gould 1972). Second, that stasis in cliff-swallow nests contrasts with diversification of prehistoric North American artifacts does not invalidate the “central evolutionary archaeological premise.” Lineages of cliff swallows and of their nests and lineages of humans and of their artifacts can evolve independently. Evolution is historical and thus contingency-bound (Gould 1986, Eldredge 1999).

Cultural transmission is different from genetic transmission. Cultural evolution can be more rapid than biological evolution, and replication can occur with less fidelity; therefore greater diversity is predictable (Dunnell 1978). That some artifact lineages display rapid increases in diversity is exemplified by computers (Neff, CA 41:427–29 and 2001), but the 1870s QWERTY keyboard is still with us despite the introduction of the more efficient Dvorak Simplified Keyboard in 1932 (Gould 1987). Different lineages evolve at different rates and through different modes. Modern Darwinian evolutionary theory holds that it could not be otherwise.

Larson suggests that our evolutionary archaeology is gradualistic. We have said that the tempo of cultural change may vary and that its mode may be anagenic or cladogenic (Lyman and O’Brien 1998, O’Brien and Lyman 2000). The rate of change is an empirical matter. Evolutionary archaeologists will often be forced to study only macroevolutionary change because the analog of microevolutionary change among organisms is genetic. Paleobiologists do not monitor genetic change directly with fossils; they assume that the changes they see in bones and teeth reflect it (Eldridge 1989, 1999). Archaeologists are in the same situation; they study change in artifacts, not in the ideas behind artifacts. Whether the change was cladogenic or anagenic is an empirical issue.

It is on this contrast between microevolutionary and macroevolutionary change that Neff misunderstands us. He says (CA 41:427) that cultural traits “can affect biological survival and reproduction” but believes that “frequency changes in cultural traits occur because of differentials in cultural reproductive success, not because of differentials in biological reproductive success.” We prefer Leonard and Jones’s (1987) term “replicative success” and disagree with the implication of Neff’s remark that changes in frequencies of cultural traits occur only because of changes in their replicative success. Neff doubts that the Cree adoption of snowmobiles either influenced their biological reproductive success or was the result of drift. The adoption of snowmobiles by Skolt Lapps took place in less than a decade and resulted in major economic, political, and social change in less than 15 years. The long-term results for these people—biological reproductive success—and their culture—artifact replicative success—are unclear (Pelto 1973).
The rate of snowmobile adoption by individuals, changes in the relative socioeconomic status of individuals, and changes in political structure are all changes in multiple cultural replicators. These replicators are archaeologically invisible in part because each such microevolutionary change occurs in a matter of weeks or months. Because archaeologists must rely on radiocarbon dates with standard deviations of multiple decades, they must focus on causes that are macroevolutionary, ultimate (Mayr 1961), and archaeologically visible. Our conviction that this will eventually produce a workable macroevolutionary archaeology is predicated on the history of punctuated equilibrium (Eldredge 1989, 1999).

Archaeologists can directly monitor the replicative success of cultural traits themselves, and “the replicative success of a particular [cultural] trait may or may not affect the reproductive success of the bearer” (Leonard and Jones 1987:214). In Dunnell’s (1978) terms, traits may be functional or stylistic. Evolution involves the transmission and replication of replicators. Units of cultural transmission can be defined theoretically as “the largest units of socially transmitted information that reliably and repeatedly withstand transmission” (Pockington and Best 1997:81). Evolutionary archaeologists want to measure “the effect of transmission on variability, [and] culture-historical types, as conceived by archaeologists, are entirely [reasonable proxies for] the unit of cultural transmission” (Lipo and Madsen 2001:100).

The replicative success of these units is what evolutionary archaeologists seek to explain. Replicators that are functional will be sorted by natural selection; those that are stylistic will be sorted by the vagaries of transmission. Whether the former replicators—as manifest in artifacts—influence the biological reproductive success of their human bearers is an empirical matter the assessment of which requires the time depth provided by the archaeological record.

Gould (1983:75) claimed that the neo-Darwinian synthesis hardened around the notion that “cumulative natural selection [working on genetic replicators] leading to adaptation be granted pride of place as the mechanism of evolutionary change” biologists “insisted to the point of dogma and ridicule that selection and adaptation were just about everything.” Under the synthesis, phyletic gradualism was the tempo and mode of all evolution (Eldredge 1989, 1999). Evolutionary archaeology is not a reworked version of the synthesis; rather, it takes both the tempo and the mode of cultural evolution to be historico-contingent (Lyman and O’Brien 1998). Gould (1995), whom Larson approvingly referenced, made the point that paleontology was uniquely set within the biological sciences to address questions regarding both tempo and mode in evolution—precisely the way we characterized evolutionary archaeology’s contribution to anthropology (Lyman and O’Brien 1998).

Larson’s and Neff’s confusion is attributable to their failure to distinguish between macroevolution and microevolution. Darwinian evolutionary archaeology is neither reductionist or extrapolationist. It is grounded in the same macroevolutionary principles as paleobiology and is geared explicitly toward reconstructing hereditary lineages and providing explanations for those lineages’ having the appearance that they do.

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More on the “Venus” Figurines

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Soffer, Adovasio, and Hyland (CA 41:511–37) provide a challenging new thesis regarding some of the features of West Eurasian “Venus” figurines. The claim that their research “informs our understanding of Upper Paleolithic ideology” and that a probable occurrence of textile use “associates these technologies with women as well as with power, prestige, and value” may, however, be in the eye of the beholder. We know little or nothing about the context of the West Eurasian figurines, and consequently it is lamentable that Soffer et al.’s research excluded an important East Eurasian site that holds some clues regarding Upper Paleolithic ideology. At Mal’ta, female and male figurines appear together in a definite context and at a date, 24,000–23,000 B.P. (Vasilyev 1993: 86–87), that falls within the time range of the figurines Soffer et al. discuss. A brief description based on the work of Gerasimov (1964), Abramova (1962), and Ozols (1971) may therefore be warranted.

Mal’ta represents a single horizon on the third terrace of the Belaya River northwest of Irkutsk. It was a campsite consisting of five lodges built of mammoth tusks, mammoth and rhino skulls, reindeer antlers, and limestone slabs, with mammoth-hide roofs covered with antlers. The secondary burial of a four-year-old boy was found in an oval pit roofed with stone slabs and antlers. Near it were five reindeer burials reminiscent of those found in an oval pit roofed with stone slabs and antlers. The secondary burial of a four-year-old boy was found in an oval pit roofed with stone slabs and antlers. The under surface is engraved with three snakes, symbolizing the World Below. The upper surface is dominated by an engraved spiral curve that begins at the edge of the plate and, in seven narrowing turns, empties into the “spirit hole.” On both sides of the large spiral are smaller ones. The spirals seem to symbolize whirlpools such as occur at the mouths of the Ob, the Yenisei, and the Lena. Representations of “spirit holes” in bone, wood, ivory, or iron are attached to the modern Siberian shaman’s drums and costume and symbolize openings to the Worlds Above and Below.

Whether the “Venus” figurines were an expression of an ideology similar to that of Mal’ta is an intriguing question. According to the ancient Siberian worldview (Schlesier 1987:19–49), on the highest level of intellectual achievement—in the vocation of shaman—women were equal to men. Whether this can be established for early West Eurasian populations requires investigations that go deeper than textile surfaces.

Reply

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We thank Schlesier for his comments and remind him that we omitted the Siberian female images from our discussion because “our examination of the originals curated at the Hermitage Museum in St. Petersburg revealed a high degree of stylization which makes for great ambiguity in interpretation of just what they were wear-
ing” [p. 534]. The Siberian evidence is far from clear-cut, and there are a number of serious problems with Schlesier’s presentation of the data from Mal’ta. These problems stem from outdated and misunderstood evidence and highly problematic interpretations. We begin by correcting a number of errors.

1. Schlesier’s reporting of numerous male figurines at Mal’ta is the first mention of them in the literature and an exercise in either wishful thinking or misunderstanding or both. Gerasimov [1964] made no mention of any male figurines, and neither did Abramova [1962, 1995]. In a recent summary of Paleolithic art from Siberia, Medvedev [1998b:134, emphasis ours] notes, “At present, the collection of Siberian Paleolithic anthropomorphic figurines contains only female images.” Medvedev and Abramova do report that there were figurines and figurine fragments other than the 30-some female ones, and both of them, along with all the other sources that we know of, always describe these other figurines as “anthropomorphic.” This term means “humanlike” rather than “male,” and the failure to differentiate the two may have been the source of Schlesier’s confusion.

Both Medvedev and Abramova also report, contra claims by Schlesier, that a number of the female figurines are depicted naked—something that can be clearly seen in figure 15 of our CA paper.

2. Whereas Schlesier calls Mal’ta a single-layer Upper Paleolithic site, recent site work has called this interpretation into question. Mal’ta was studied on and off by Gerasimov over some 30 years but very poorly published (Formozov 1976; Medvedev 1998a, b). In his synthetic description of the part of the site he excavated in 1956–57, Gerasimov [1964] reported unearthing one undisturbed in situ cultural layer containing a number of synchronically occupied dwellings. He concluded this in spite of having excavated the purported dwellings, as well as some hearths, at different levels [Formozov 1976: 208]. Much disagreement existed among the geologists working with Gerasimov about the pristine condition of the cultural layer, with many noting strong evidence for solifluction and cryogenic disturbance [Tseitlin 1979: 178]. Medvedev and his colleagues, who resumed work at Mal’ta in 1990, have substantiated these observations and noted a great deal of vertical dislocation of cultural remains [up to 50 cm] [Medvedev et al. 1998:160]. This new research, together with a review of all of Gerasimov’s sparse field documentation, has convinced them that Mal’ta represents not a single synchronous settlement but a palimpsest of repeated occupations over a number of years [Larichev, Khol’ushkin, and Laricheva 1990, Medvedev 1998a, Medvedev et al. 1998].

3. The animal burials that Schlesier mentions are news to us and will be to our Siberian colleagues as well. No such claims can be found in Gerasimov [1964], who believed that the reindeer antlers, together with the large bones of other animals, were used in the construction of dwellings. Larichev, Khol’ushkin, and Laricheva [1990: 371] do cite Gerasimov’s opinion that part of a young mammoth may have been intentionally buried near the “child’s” grave. However, since these bones were found under a hearth, we can envision a number of other interpretations [for similar features at Mezin in Ukraine, see Soffer 1984].

4. As for the reported richly adorned burial of three-to-four-year-old child, Schlesier is again working with outdated information. Gerasimov did not fully publish this feature, and restudies done by both Alekseev [1998] and other paleoanthropologists have identified the remains of two children [also cited in Larichev, Khol’ushkin, and Laricheva 1990]. This, again, warns us to be extremely careful with Gerasimov’s summary publications.

There are at least two sources of interpretations in Schlesier’s comment—his and Gerasimov’s. Since his build on Gerasimov’s, we begin with problems with the former.

Schlesier reports that the Mal’ta dwellings were divided into men’s and women’s halves. In this he paraphrases Gerasimov without a critical appraisal of the source. Gerasimov [1964:17] gendered dwelling space on the basis of unexamined assumptions that certain categories of material culture have always been associated with either males or females. He saw the right sides of dwellings as male domains because he claimed to have found the bulk of the blades, choppers, points, and ivory fragments there. To these he added the bird figurines as male-specific artifacts as well. He claimed the left sides as female domains because he found items of personal adornment as well as buttons, needles without eyes, awls, scrapers, and knives there. Limits of space preclude a detailed discussion of the serious problems with assuming that specific items of material culture always reflect one sex or the other. The literature on this subject is vast, and we direct interested readers to Gero [1991] as a useful entree into it. Gerasimov offered no explanation or justification for his assumption—it was common sense and received wisdom—and both our CA contribution and Gero [1991] discuss the problems with such a stance.

Schlesier criticizes us for not considering ideological explanations for Upper Paleolithic anthropomorphic imagery—specifically, shamanism. We explained why we did not discuss the meaning of the imagery. As Soffer and Conkey [1997] have argued at length, we consider it impossible to decipher the unambiguous meaning that a category of material culture had for its makers and very much doubt that there ever was a single meaning. Questions about meaning inevitably lead to arguments from authority, in which one simply chooses whom to believe. A clear example of this impasse is the diversity of interpretations offered for the “ivory plaque” found at Mal’ta. For Schlesier it is clearly a cosmological shamanistic object, for Larichev a calendrical-astronomic device [Larichev, Khol’ushkin, and Laricheva 1990:372], for Gerasimov something of genealogical significance [Medvedev 1998b:135], and for Medvedev a stylized map. Thus, as with the shapes of clouds discussed by Hamlet and Polonius, meaning lies in the eye of the beholder.

Schlesier [1987] has published a number of contributions on Cheyenne ritual practices in which he traces
On Population Growth and Parental Investment

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Voland et al. (CA 38:129–35) have recently presented data that seem to suggest a correlation between population growth rates and an index of differential parental investment. Seeking to augment previous work on the way sex-biased parenting may be influenced by factors such as social and economic status, they want to include difference in population growth rates as another factor that may affect a parent’s “fitness calculations” regarding the sex in which there should be greater parental investment. Data from six 18th- and 19th-century rural German populations are used, with each population divided into upper and lower classes.

The index of differential parental investment, TW, compares the female/male ratio for infant mortality, q/[q]+q[m], for the upper class with the same ratio for the lower class [i.e., a ratio of ratios] for each of the six regions. Voland et al. note that upper-class q/[q]+q[m] values are consistently higher, thus showing a class difference in degree of sex bias toward sons and daughters. When they correlate TW with the growth rate for each of these six rural groups they obtain a value of r = 0.829, which they accept as statistically significant (n = 6, p = 0.05, one-tailed). The linkage between differential sex bias and growth rates, they argue, stems from higher values of r, suggesting “expansion competition” and lower values suggesting “resource competition.” They conclude (p. 134): “It seems that social-rank effects on sex-biased parental investment can go in either direction, depending on whether the prevailing kind of competition is displacement or expansion.”

<table>
<thead>
<tr>
<th>Region</th>
<th>TW</th>
<th>Growth Rate (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Krummhörn</td>
<td>1.085</td>
<td>0.415</td>
</tr>
<tr>
<td>Geest</td>
<td>1.283</td>
<td>0.939</td>
</tr>
<tr>
<td>Moor</td>
<td>1.277</td>
<td>0.769</td>
</tr>
<tr>
<td>Leda</td>
<td>1.119</td>
<td>0.713</td>
</tr>
<tr>
<td>Leezen</td>
<td>1.243</td>
<td>0.757</td>
</tr>
<tr>
<td>Ditfurt</td>
<td>1.177</td>
<td>0.327</td>
</tr>
</tbody>
</table>

Source: Voland et al., table 1.

The essence of ethnographically documented belief systems into the deep past, all the way to Upper Paleolithic Siberia. While limitations of space prevent us from discussing the ethnography, we question the accuracy of his archaeological data. As we have shown above, great dangers lie in the use of outdated and misunderstood information.

Caveat lector!

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A closer examination of the data provided by Voland et al. makes it evident that the matter is more complex and the conclusions drawn may be premature. First, according to the data they report (table 1) \( r = 0.69 \) \( (n = 6, p = 0.13, \) two-tailed), not 0.829, and so the relationship between TW and growth rate is not statistically significant as they claim. Second, when Krummho"rn, the region with the second-to-lowest growth rate, is compared with Geest, the region with the highest growth rate, the hypothesized correlation between sex bias and growth rate does not materialize. Rather than differing substantially, these two regions have very similar ratios for each of the two classes (table 2). Third, it is implicitly assumed that the upper class and the lower class constitute homogeneous populations in the statistical sense. Concordance between model assumptions and data structure is of the prerequisites for statistical analysis that aims at results with theoretical meaning about processes that structure the data (Read 1985, Carr 1985). It is evident from the data presented by Voland et al., however, that neither class is internally homogeneous.

If the upper class were homogeneous from one rural group to the next with regard to sex bias in parental investment, we would model sex bias as \( \mu + \epsilon \), where \( \epsilon \) has a \( N(0, \sigma^2) \) distribution; that is, we would assume the same sex bias, \( \mu \), from one upper-class rural group to the next modified by an error term, \( \epsilon \), that accounts for all nonsystematic variation in the value of \( \mu \) across the different rural groups. Should there be systematic variation around the value of \( \mu \) we would explicitly incorporate it in the model.

For the data presented by Voland et al. there is systematic variation in sex bias across the upper classes for the six groups and similar variation for the lower classes. It is evident that the upper and lower classes both split into two distinct groupings (see table 2): \{1\} Krummho"rn, Geest, and Moor and \{2\} Leda, Leezen, and Ditfurt. (The division is based on the nonnormality and approximately bimodal distribution of the \( q/f/q/m \) ratios for the upper class and the lower class considered separately.) The ratios \( q/f/q/m \) for the upper class have mean values of 0.80 for the first grouping and 1.02 for the second grouping. The difference in the means is significant \( (t = 4.42, \ d.f. = 4, \ p < 0.01, \) two-tailed\). Apparently members of the first grouping are consistently favoring daughters over sons whereas those in the second grouping show no sex bias. For the lower classes the means are 0.66 and 0.87 for these two groupings, and the difference in means is significant \( (t = 5.12, \ d.f. = 4, \ p < 0.01, \) two-tailed\). Further, the average sex bias in the first upper-class grouping, 0.80, is not significantly different from the average sex bias in the second lower-class grouping, 0.87 \( (t = 1.65, \ d.f. = 4, \ p > 0.10, \) two-tailed\).

Rather then a simple upper-class/lower-class dichotomy, three of the upper-class rural groups show a sex bias in favor of females matching the sex bias of three of the lower-class groups while the remaining three upper-class groups show no sex bias. A scattergram plot of the six groups and the infant mortality rates for each of male and female infants makes the complexity of the overall patterning clearer (fig. 1): \{1\} the first rural three regions have a consistent male/female difference; \{2\} Geest has an unusually high mortality rate for both males and females, without a significant difference in sex bias; and \{3\} Leezen has very similar infant mortality rates for both sexes regardless of class. Growth rates show no particular pattern with respect to infant mortality rates and/or sex bias in mortality rates, as expected from their nonsignificant correlation with the TW index. It appears that attempting to use differences in growth rates as a unicausal explanandum for variation in sex bias for infant mortality between classes and regions is not justified by these data.

### Table 2

**Mortality Rates and Female/Male Ratio by Sex and Class for Six Rural Regions in 18th- and 19th-Century Germany**

<table>
<thead>
<tr>
<th>Region</th>
<th>Upper Class</th>
<th>Lower Class</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>N</td>
<td>Male</td>
</tr>
<tr>
<td>Krummhörn</td>
<td>307</td>
<td>0.091</td>
</tr>
<tr>
<td>Geest</td>
<td>106</td>
<td>0.250</td>
</tr>
<tr>
<td>Moor</td>
<td>118</td>
<td>0.080</td>
</tr>
<tr>
<td>Mean</td>
<td></td>
<td>0.80</td>
</tr>
<tr>
<td>Leda</td>
<td>143</td>
<td>0.070</td>
</tr>
<tr>
<td>Leezen</td>
<td>1,136</td>
<td>0.126</td>
</tr>
<tr>
<td>Ditfurt</td>
<td>376</td>
<td>0.170</td>
</tr>
<tr>
<td>Mean</td>
<td></td>
<td>1.02</td>
</tr>
</tbody>
</table>

Source: Voland et al., table 1.
Reply

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We thank Read for the interest he has taken in our report. Unfortunately, his careful analyses appear to be less secure than he supposes, in part because of statistical errors and in part because of errors of interpretation.

Read asserts that our analysis of the relationship between our (admittedly rather complex) index of a Trivers-Willard effect (the ratio of sex-biased infant mortality in the farmer class to that in the labourer class) and population growth rate is incorrect. In fact, it is correct: we gave Spearman’s nonparametric correlation coefficient (which is significant at $p = 0.042$, two-tailed), whereas he calculates Pearson’s parametric correlation coefficient. Because the sample size is very small and assumptions of normality cannot be made, a parametric test would normally be deemed inappropriate.

Read also claims that a contrast between Krummhörn and Geest fails to produce the expected difference in Trivers-Willard effect despite a difference in population growth rate. We fail to see how Read arrives at this conclusion, since the data show that they do differ considerably in Trivers-Willard values (1.085 vs. 1.283, corresponding to population growth rates of 0.415 and 0.939, respectively).

Third, Read argues that the assumption of homogeneity of variances across populations is false and therefore the statistical model used is inappropriate. This remark would be correct had we used parametric statistics, but for this very reason we in fact used nonparametric statistics that depend on no such assumptions. We would, however, argue that Read’s careful analysis of the patterns in the data supports our claim (reflected in the use of different symbols in our figure 1) that the six populations should be considered as belonging to two separate grades (which, on the basis of the habitats’ various characteristics, we explicitly identify as correlating with habitat quality).

Fourth, Read seems to misunderstand the point of our paper. Our purpose was not to explain mortality differentials within classes but rather to explore the extent to which there might be contrasting sex biases in mortality between the richest and poorest classes that were consistent with the predictions of a particular hypothesis.

Fig. 1. Infant mortality rate by sex and class in six German rural areas. UC, upper class; LC, lower class.
The local-resource-competition model of van Schaik and Hrdy (1991). His analyses of the mortality rates within each class are an appropriate answer to an entirely different [but perfectly reasonable] question that, on this particular occasion, we did not seek to answer.

Finally, Read asserts that we “use differences in growth rates as a unicausal explanandum for variation in sex bias for infant mortality between classes.” In fact, we do no such thing. At no point do we state that population growth rate is the only cause of the observed pattern of mortality differentials. We were not in the business of attempting to explain all the variance in mortality differentials within or between the six populations in our sample. Rather, as we have already noted, our purpose was to test a particular hypothesis about the possible influence of a particular variable. It is customary in science to seek to test, insofar as one can, only a single variation at a time. Such technicalities notwithstanding, we should point out that we did in fact draw attention to the possibility that the influence of at least one other variable (namely, habitat quality) could be discerned in the observed pattern of the data.

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On Models and Data in Mesopotamia

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Algaze’s [CA 42:199–233] paper takes up an issue that has been widely debated in recent years. Although essentially he returns to the same assumptions that he was using at the end of the eighties, he has changed his point of view, looking at things from the Mesopotamian core rather than from the so-called periphery. Therefore his analysis focuses more directly on the heart of the matter and brings out more clearly a number of crucial theoretical and methodological problems. I would first like to deal with the methodological ones, since they are relevant to all the others. The main point has to do with the relationship between the construction and use of models and historical research on the situations to which the models apply—“History” with a capital H, the history that reconstructs processes in addition to recounting events. Because prehistory investigates primary processes, which by definition are fundamentally unknown, a preliminary investigation of the overall reality to be analyzed must be carried out directly on the archaeological data before drawing historical or ethnographic comparisons that depend for their validity on comparable contexts. This is the only way to take advantage of all the explanatory potentialities our discipline possesses and make our models suggest something beyond what anthropology and history have to offer.

Algaze starts from the idea that the environment has played an essential part in the development of societies and stresses the environmental variability which sharply distinguishes Lower Mesopotamia from the other regions involved in the Late Uruk phenomenon. But we know so little about the actual organization of 5th–4th-millennium southern Mesopotamian societies that the most we can do is to assume that there was a generic relationship between environmental and economic factors. “Economy” is different from both technology and resource exploitation. In particular, one cannot infer that the circulation of goods and the environmental diversification underlying it almost automatically evolved to develop a technology for harnessing “external energy” through long-distance trade. In fact (1) we have no proof that the various subsistence goods circulated within the alluvium in the form of exchange/trade (though it is highly probable that they did circulate) or that long-distance trade had an important role in the centralized economies that were emerging in Lower Mesopotamia or that there was the control over trade by the elites that alone would justify the term “export-driven economy” (it may have been the elites who ordered certain commodities, but economic management is something else altogether) and (2) trade becomes an important sector of the economy when it enters the basic economic system and circuits of essential commodities. Algaze’s example of the wool imports for the textile industry in historical times is very persuasive, but it is irrelevant to the formative environments with which he is dealing. The 4th-millennium Mesopotamian or peri-Mesopotamian elites probably controlled wool through the more or less direct management of livestock (Green 1980) whose archaeological result is the strong boost given to sheep-rearing in these early centralized contexts.

This introduces the second problem: the relationship between the economy of staple products and the economy of prestige goods in early societies with elites. What
seems to dominate these archaic centralized economies, as is suggested by archaeological documentation and the Uruk tablets themselves, is control of the labor force or a substantial part of it and, through that, of certain primary inputs (land and livestock). In contrast, no documentation exists of a substantial accumulation of surplus to be reinvested in trade (for example, no large stores have been found). Administrative technology, as has already been proven from a careful analysis of thousands of clay sealings found at Arslantepe (Ferioli and Fiandra 1994, Frangipane and Palmieri 1983) in situations which are certainly structurally comparable with those found at Uruk, is essentially designed for the management and circulation of goods within the economic system. What is administered is the distribution of goods, usually primary goods, rations, or meals, and the bureaucracy, which is widely present in 4th-millennium communities throughout Greater Mesopotamia, both north and south, came into being in relation to these internal needs. The ecological diversity of Lower Mesopotamia was very likely one of the factors that stimulated the development of systems for the local circulation of products, as Algaze says, and in particular the centralized redistribution system, as Adams proposed in the sixties. But I do not think it is possible to demonstrate that this had anything to do with the creation of an economy that was strongly centered around long-distance trade. Trade relations, which likely existed, seem to have had more to do with a long tradition of contacts along the Tigris and the Euphrates since the Neolithic period, and there is no indication that in the 4th millennium they increased substantially because of developmental asymmetries between north and south (sites like Gawra in the north show remarkable “import” activities as early as the very beginning of the 4th or even the end of the 5th millennium); nor is there any reason for believing in any regional economic dependency, which is also theoretically hardly plausible in situations in which control of the hinterland was in an initial stage.

Finally, “urbanization” and “complexity” are related but distinct. The greater agricultural potential in the south was its real environmental difference from the north. This may have allowed a much higher level of urbanization, which is the crucial factor that made the south so distinctive—even though sites such as Tell Brak suggest that there were substantial forms of urban development under particular conditions even in some northern areas (Khabour). It was not “developmental asymmetries,” then, but rather different degrees of urbanization. Urbanization, however, is not an absolute gauge of complexity but only a typical feature of some so-called complex societies. When I have spoken of an “early state” in the case of Arslantepe (Frangipane 1997) (though I would be careful in using this term today) I have done so recognizing perfectly well that Arslantepe was not an “urban” center but a center with a bureaucratic-administrative apparatus and refined tools for economic and ideological control over the community that would rule out the possibility of considering it a “chief-dom”—if we still want to use these simplistic terms to refer to situations that appear increasingly complex.

Reply

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Frangipane’s comments raise a number of important methodological and substantive issues. With respect to methodology, Frangipane notes that models applied to prehistory should be derived primarily from archaeological data themselves and only secondarily from comparable ethnographic and historical sources. Alas, to restrict ourselves to models derived entirely from the archaeological evidence at hand for any one culture at any one time would be to curtail our ability to generate hypotheses likely to transcend the limitations of our necessarily imperfect data. While the degree of variability in human cultures past and present is almost infinite in detail, structural regularities between cultures at comparable levels of social development (Murdoch 1959) do allow for well-chosen comparisons across time and space. These comparisons can help generate pertinent hypotheses about the past that can then be tested against the available, often unclear, data. The model presented in my paper concerning the consequences of environmental diversity for the development of early trade and social complexity in southern Mesopotamia is simply a heuristic device intended to stimulate new research. However, it does indeed presume some cross-cultural regularities in the human will to power, the laws of economics, and the physics of transportation.

With respect to issues of interpretation, Frangipane notes that since we know little about the organization of southern Mesopotamian societies in the 5th and 4th millennia we cannot understand in detail the potential impact of environmental factors on their economy and development. She surmises that the only environmental difference of consequence affecting the development of these societies was the greater agricultural productivity of the southern environment compared with that of neighboring regions. While I fully agree that productivity was important, I believe that Frangipane seriously underestimates the many and very fundamental ways in which the southern Mesopotamian environment of the Late Ubaid and Uruk periods differed from that of the historic periods in the area. In so doing, she misjudges the potential impact of these differences on the initial developmental trajectories of human societies in the area. To recapitulate: The southern Mesopotamian environment of the late 5th and 4th millennia was one in which winter rains more abundant than is the case today were typical and in which monsoonal systems brought rains and cloud cover into the area during the summer...
months, thoroughly altering the possibilities of Mesopotamian agriculture at the time. Further, it appears that the Tigris and the Euphrates constituted a single complexly intertwined river system that was easily exploited with simple basin flow irrigation techniques and that flowed over a highly productive plain not yet degraded by substantial salinization. Finally, the southern Mesopotamian landscape of the 4th and 5th millennia was also thoroughly affected by the northward intrusion of the Persian Gulf at the time, which created a mosaic of easily exploitable biomass-rich marshes, swamps, brackish lagoons, and estuaries within short distances of early Mesopotamian population centers.

A second point of disagreement has to do with the influence of long-distance trade in exotics on the process of state formation in southern Mesopotamia. Frangipane notes that the available textual documentation rarely deals with imported commodities and, further, that control over labor and agricultural and pastoral resources is highlighted time and again in the tablets. I fully concur with her on the centrality of control over labor, grain, animals, and animal by-products to the rise of Uruk states. Where we differ is that I consider access to exotic resources equally important. The reasons for this have been discussed extensively by Mary Helms (1988, 1993), who uses a variety of ethnographic, historic, and literary evidence to show how, in traditional societies, exotic resources attesting to contacts with alien cultures are commonly imbued with ritual meaning and typically come to be seen as a direct demonstration of a leader's fitness to rule. In this manner imports become central to the very reproduction of the social order.

Another area of disagreement is that of the scale and importance of external trade in the Uruk period. Frangipane notes that we have no proof of elite control of external trade in the Uruk period and that, in any event, only trade in “essential” commodities would have been central to the economy. The absence of references to trade in the available documentation of the time that she interprets as evidence of absence is no such thing. This point is underscored by Foster’s (1977) analysis of Akkadian-period trade, based on documents from the Sumerian city of Umma. He documents whole categories of imports (including such crucial commodities as copper) that are absent from the surviving documentation but well represented in the archaeological record. Furthermore, while I consider trade in exotics to have been as essential to the political economy of nascent Uruk states as trade in essentials, it should be noted that the external trade of Mesopotamia in the Uruk period consisted not only of low-bulk, high-value preciosities but also of other commodities, such as timber and copper (for tools), that surely must qualify as essential for the maintenance of complex societies in the alluvial environment of southern Mesopotamia (Algaze 1993).

Finally, although Frangipane is correct in that our understanding of the societies of the Late Ubaid and Uruk periods in core areas of southern Mesopotamia is crippled by inadequate and unrepresentative exposures, there is some evidence that bears on the importance of external trade to early Mesopotamian elites. The Riemchenhaus structure found in the Eanna Precinct at Warka (Late Uruk: Eanna IV), discussed in my paper, for example, was literally brimming with imported exotic materials of many kinds. Was this an exceptional find that bears no relationship to elite activities at the site? On the contrary, this uniquely preserved structure (the building was buried after being consumed by fire) is likely to be representative of the wealth of imports that existed in religious/administrative quarters of Warka. If that wealth is otherwise not well attested at the site, save for scattered hoards such as the Sammelfund, it is because of the reuse of exotics in later periods and because most of the Uruk-period structures cleared in the Eanna Precinct represent but foundations of buildings that were carefully cleaned and emptied out in antiquity (Eichmann 1989).

Although I have emphasized my disagreements with Frangipane about the meaning of the available data, in fact, we agree much more often that we disagree. Her views about the importance of control over labor and agricultural and pastoral resources for understanding the processes that gave rise to early state and urban societies in the ancient Near East are precisely on the mark. Our only difference is simply that I would add trade, both internal and external, with its myriad multiplier effects, as equally important. I hope that changed political conditions in Iraq will one day allow a new generation of scholars to conduct carefully targeted research to test whether this was the case.

References Cited


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