Reports

A New Look at Culture and Trade on the Azanian Coast

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Recent archaeological survey and excavation on the central coast of East Africa have provided valuable new information on the culture of the early-1st-millennium inhabitants of this region, known to the ancient Greeks and Romans as Azania. According to the Periplus of the Erythrean Sea [A.D. 40–70], Ptolemy (2d–3d centuries A.D.), and Indicopleustes [6th century A.D.], the settlements of this region obtained iron objects and glassware from northern traders in exchange for ivory, tortoise shell, and spices. The region is alleged to have been under the control of the Homeritae of Arabia Felix [Casson 1989] and to have been visited by Indonesians [Miller 1969]. Pliny reports that the Indonesians brought spices from South and East Asia to be ferried to the Mediterranean through the Horn or the interior and, if they survived the five-year return journey, carried back glassware and bronze work, clothing, brooches, armlets, and necklaces [Miller 1969:156]. The documents mention Rhapta, located on a promontory and a bay of the same name, as the region’s major emporium and the Roman Empire’s southernmost market. Scholars have pointed to various possible locations for Rhapta, among them the Rufiji Delta, the Dar-es-Salaam Bay, and the mouth of the Pangani River (for discussion see Datoo 1970, Kirwan 1986, Casson 1989; Horton 1990:98–99) has recently placed it in the Lamu Archipelago or the area north of the Zanzibar Channel.

Evidence to supplement the information in the Greek and Roman literature has come from the discovery of ancient coins in various parts of East Africa—all, however, from nonarchaeological contexts. From Kimoni, north of Tanga, there are two Roman coins, one from the reign of Carus [A.D. 282–283] and one from that of Constantine [335–337], and one Byzantine one, from the reign of Heraclius [A.D. 610–641] [Chitick 1966:56–57]. A coin from the reign of Ptolemy Soter [116 B.C. to A.D. 108] was sold to a German merchant in Dar-es-Salaam in 1901. The Zanzibar Museum has a collection of Persian coins of unknown provenience dating to the 1st to 3rd centuries A.D.; others from other parts of Middle East range in time from the 2d century B.C. to the 14th century A.D.

Archaeological evidence for early-1st-millennium A.D. settlement has until recently been lacking. Chittick (1975:189) summed up the situation in the mid-1970s as follows: “Archaeologically speaking, there are no settlements known on the coast which antedate for certain the ninth or tenth century, nor any which were not already importing goods directly or indirectly from the Islamic world.” Except around such major monumental sites as Kilwa and Manda, few areas had been surveyed for sites. As a consequence, discussions of the history of Azania have long been essentially confined to the interpretation of the ancient documents [Miller 1969, Chittick 1975, Sheriff 1981, Casson 1989].

The inhabitants of Azania, described in the Periplus as “big-bodied,” have generally been considered to have been southern Cushites [see Casson 1989], and Horton (1990:96–97) has suggested that their material remains are affiliated with the Pastoral-Neolithic tradition of the interior Rift Valley of Kenya and northern Tanzania (see also Allen 1993). Chami [1994] has challenged this view, and elsewhere he [1994–95], Haaland [1994–95], and Schmidt [1994–95] have argued on archaeological evidence that farmers representing the Kwale Early Iron Working tradition living on the coast in the early 1st millennium A.D. established the foundations for the coastal Swahili tradition that had emerged by the end of that millennium. Sutton [1994–95:231] disagrees: “The date is probably just a little too early for the appearance of the makers of Kwale ware in the immediate hinterland, who as the normal argument runs would have been the first Bantu hereabouts. The Azanians could therefore have been Cushitic-speakers or they could have had trade connections with mobile groups inland who supplied ivory and other valued products.” The preliminary findings from survey and excavation on the coast in the past two years shed light on this controversy.

The archaeological data

Since 1986, archaeologists working on the central coast of Tanzania [fig. 1] have uncovered concrete evidence dating to the 1st–6th centuries A.D. Sites with typical Early Iron Working period pottery are listed with their C14 dates in table 1.

Kwale, about 30 km southwest of Mombasa, was first excavated by Soper [1967] and yielded local pottery of the Early Iron Working tradition that was designated the Kwale variant [Soper 1971]. The site has been dated to the 3rd century A.D.

Limbo, a site apparently earlier than Kwale [dated to the beginning of our era] 20 km inland and 75 km south

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of Dar-es-Salaam, has yielded many sherds of the Early Iron Working tradition, several tonnes of slag, and tuyere fragments. Study of these smelting materials indicates that the site was an industrial one, producing iron for a wider area. Several pieces of pointed iron objects were also recovered (Chami 1992).

Kwale Island, Koma Island, and Mafia Island, 4 km, 10 km, and 20 km from the mainland respectively, all have sites with Early Iron Working pottery. Only the Kwale Island site, the first Kwale-variant site to be found on the off-shore islands, has been excavated. It has proved to be a multicomponent site, with Early Iron Working materials at the bottom, 70–90 cm below the surface and dated to the 3rd and 4th centuries A.D. Associated with potsherds from this period were animal and fish bones, marine shells, and an iron object (Chami and Msemwa 1997).

Kivinja is located approximately 20 km north of the Rufiji Delta at the mouth of a small river. At high tide the river carries a large amount of silt and mud from the delta; wells dug in the vicinity by salt makers show that the deposits here are more than 1 m thick. Areas with such thick deposits are quickly overgrown by mangroves, which the salt makers use heavily for fuel. Salt is produced mainly during the dry season by people from as far inland as Kibiti, some 30 km away, and is exchanged for agricultural products and/or for cash. A survey in December 1994 revealed abundant Kwale Early Iron Working potsherds on the surface in an area extending some 3 km along the shore and 1 km inland; many sherds were observed on eroded footpaths and at the edge of the site cut by the present shoreline.

Excavation of the site produced, in addition to Early Iron Working pottery, a fair amount of pottery of a later Kwale; Kiwangwa; 3, Unguja Ukuu; 5, Limbo; 6, Misasa; 7, Kwale Island; 8, Koma Island; 9, Kivinja; 10, Mafia Island; 11, Kilwa.

**Table 1**

**Key Sites with Early Iron Working Cultural Materials**

<table>
<thead>
<tr>
<th>Site</th>
<th>Location</th>
<th>Lab. No.</th>
<th>C14 Date (calib.)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Kwale</td>
<td>lat. 4° 11’ S</td>
<td>N-291</td>
<td>A.D. 270 ± 115</td>
</tr>
<tr>
<td></td>
<td>long. 39° 35’ E</td>
<td>N-292</td>
<td>A.D. 260 ± 115</td>
</tr>
<tr>
<td>Kwale Island</td>
<td>lat. 7° 28’ S</td>
<td>Ua-10287</td>
<td>A.D. 425 ± 55</td>
</tr>
<tr>
<td></td>
<td>long. 39° 23’ E</td>
<td>Ua-10286</td>
<td>A.D. 241 ± 60</td>
</tr>
<tr>
<td>Limbo</td>
<td>lat. 7° 28’ S</td>
<td>Beta-24626</td>
<td>B.C. 90 ± 60</td>
</tr>
<tr>
<td></td>
<td>long. 39° 25’ E</td>
<td>Beta-24626</td>
<td>B.C. 93 ± 60</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Beta-24633</td>
<td>A.D. 233 ± 60</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Beta-24634</td>
<td>B.C. 167 ± 90</td>
</tr>
<tr>
<td>Kivinja</td>
<td>lat. 7° 28’ S</td>
<td>Ua-10932</td>
<td>A.D. 431 ± 70</td>
</tr>
<tr>
<td></td>
<td>long. 39° 15’ E</td>
<td>Ua-10931</td>
<td>A.D. 598 ± 70</td>
</tr>
<tr>
<td>Koma Island</td>
<td>lat. 7° 33’ S</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>long. 39° 23’ E</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mafia Island</td>
<td>lat. 7° 40’ S</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>long. 39° 40’ E</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
of Fayum in Egypt, and the pottery is a milky-white paste glazed with alkaline green and blue that was produced from the 3rd century B.C. in the Middle East (Lane 1947:8–9; Pope 1939). From this evidence it is tempting to infer that when the site was open to the sea and the river it was a port. Kivinja is the first Early Iron Working site on the coast of East Africa to yield such elaborate ceramic information and the first to produce evidence of early-1st-millennium transoceanic trade links. The glass objects imported into East Africa reported in the *Periplus* and in Pliny can now be verified archaeologically. An alkaline glass bead has recently been found in another site near the Rufiji Delta associated with Limbo-type Early Iron Working cultural materials (Chami and Mapunda, research in progress).

The finds just described strongly suggest that early-1st-millennium Azania was inhabited by iron-producing agricultural communities. Additional discoveries suggest that at the beginning of the 6th century A.D. the communities of this region were playing a major role in transoceanic trade. These communities are archaeologically identified by the pottery tradition known as Triangular Incised Ware. The site of Unguja Ukuu, on Zanzibar Island, is particularly important for an understanding of this period. First, the presence in its earliest cultural horizon of Early Iron Working pottery indicates that people had reached this island, along with Koma, Kwale, and Mafia, before the development of the more sophisticated Triangular Incised Ware tradition. Second, it has yielded a wide variety of imports from India, the Middle East, and the Roman world, all dated to the 5th to 7th century A.D. (Juma 1996), including pottery, glassware, beads, alabaster, and various kinds of metals.

The site of Misasa, seaward of Limbo on the coast, has yielded crucial information on littoral-interior links in the 5th and 6th centuries A.D. Its cultural materials are very similar to those from Unguja Ukuu, suggesting that the latter was an emporium from which traders crossed the channel to mainland settlements (Chami 1994).

Typical Early Iron Working and Triangular Incised Ware materials have also been found on Kilwa Island, separated from the mainland by a channel some 2 km wide. This site is better known for its Triangular Incised Ware component, which close examination reveals should be placed in the early phase of that tradition (Chittick 1974).

Kiwangwa, in the Bagamoyo hinterland 45 km north of Dar-es-Salaam, has yielded early-phase Triangular Incised Ware materials with fragments of imported glass and green/blue ware. Dated to the 7th century, the finds suggest that trade connections in ancient times had reached far into the interior (Chami 1994).

**Discussion**

Eastern and Southern African Early Iron Working communities have been strongly associated with Bantu-speaking people (Phillipson 1985, Soper 1971). The surviving remnants of the ancient coastal language indicate that the language was Bantu (Chami 1996), and according to Arabic documents dated to the 9th century A.D. the Kenya and Tanzania coasts were Bantu-speaking (Trimingham 1975). Now that Early Iron Working sites have been found in the interior, on the coast, and on the off-shore islands, it can be argued that the Azanians were ironworking farmers and were probably Negroid rather than Cushites. Limbo, in the heart of the central coast, has three C14 dates strongly suggesting that the early farmers and iron smelters were probably on the coast by the end of the last century B.C., and these are the people who probably initiated the transoceanic trade. It was been suggested that C14 dates are being taken uncritically to support a particular position with regard to cultural origins (see Sutton 1994–95:231), but we see no reason that several consistent dates should not be used to argue for a Bantu-speaking presence. Again, if Early Iron Working pottery is accepted as associated with Bantu-speaking farmers, we see no reason that finds of this pottery on the central coast should not be used to suggest the presence of Bantu-speaking farmers in the area.

Despite the original view that they were pirates (Sutton 1990, Horton 1990), Sheriff (1981:555) interprets the report in the *Periplus* that the people of Rhapta had sewn boats to mean that “they had dug-out canoes and small ‘sewn boats,’ but not apparently deep-sea dhows.” Drawing support from Al-Idris, who wrote in the 12th century A.D. that the Zanj had no ships to voyage in but used “vessels from Oman and other countries” (Freeman-Grenville 1975:39), Sheriff advanced the idea that the Azanians were characterized by a “low level of technology.” It is apparent from our work, however, that the Azanians had iron technology; they could have made tools to construct seagoing vessels. Moreover, it is now certain that the Azanians of the Early Iron Working culture were probably the first occupants of all the major islands of East Africa, and they must have had vessels better than sewn boats. The little sewn boats were probably used for fishing and crossing narrow channels, and given the nascent nature of the ancient economy these would have been the boats most visible to visitors.

Our own sailing experiments between the port of Kisiju and the islands of Kwale, Koma, and Mafia strongly suggest that unless the ancient seas were less turbulent few sailors would have dared to venture 10 km on the ocean in simple canoes or sewn boats (*mitetpe*). Our informants confirmed that only the very courageous would dare to cross by canoe from Kisiju to Koma, a distance of about 10 km. Currently only dhows cross between Kisiju and the islands. We sailed in one from Mafia to Kisiju in seven hours before a very strong wind. The sailors indicated that no outrigger canoe would survive the force of the wind and the waves that we were experiencing. This means that, if ancient conditions were like those seen today, the Azanians must have had bigger vessels on the model of our modern dhows to be able to cross from the mainland to Mafia and Koma.

At this juncture it is difficult to tell how far north and south these local sailing activities extended. Since the
transoceanic trade points to the north, it can be deduced that the northern coast of Tanzania and Kenya were fully involved. To the south, however, further research is required to clear the fog. Research on the coast of Mozambique indicates that it has Early Iron Working sites, but no trade goods have been found in them [Sinclair et al. 1993].

For the Comores and Madagascar, archaeological research has not revealed any cultural horizon earlier than the 8th century A.D. [Wright 1992; Verin 1975, 1986]. It is now established that the earliest ancestors of the present-day Malagasy people may have reached the island in the 10th century or thereafter. Although the report that ancient Indonesians visited the Azanian coast, combined with the occurrence of certain cultural elements in both places, has led scholars [Oliver 1966, Jones 1971] to speculate, for instance, that the present-day Malagasy are descended from them, to date there is no archaeological indication of this alleged visit. The outrigger canoes and sewn boats that occur both in Indonesia and on the East African coast as far south as Kilwa can best be explained as independent adaptations to sailing in calm waters, the ocean south of Kilwa is too rough for simple boats. If the outrigger canoe was indeed an Indonesian invention introduced to East Africa, the sailing of such vessels must have taken place at a time of year when the ocean was calm. Pliny, however, describes the Indonesians as setting out on rafts during “the time of winter solstice, when the east winds are blowing their hardest” [Miller 1969:136]. This means that they faced rough seas requiring “human courage,” and Pliny says that many of them perished.

CONCLUSION

The evidence from Early Iron Working sites suggests that, as early as the beginning of the 1st century A.D., the coast of East Africa was settled by Negroid iron-working farmers. By A.D. 200 exchange between the coast and the islands had been firmly established, and by the 7th century many sites found all over the interior, the coast, and the islands indicate the existence of a network of exchange of goods and ideas [Chami 1994–95; Msemwa 1994:13–65]. The social relations implied by these connections would have contributed to the development of similar cultural traditions over a wide area the elements of which can be observed in archaeological sites. More archaeological evidence will be required to confirm the alleged colonization of Azania by the Homeritae of Arabia.

It is now certain that both local and transoceanic trade date to the earliest 1st millennium A.D. An industrial site such as Limbo would have supplied the demands for iron of settlements on the coast and the offshore islands, where iron ore is scarce. The inhabitants of the coraline islands of Kwale and Koma might well have provided others with deep-water marine resources in exchange for mainland products such as grain and iron much as they exchange fish for grain and finished goods today. Koma and Mafia have very good soil for coconuts, their main stock in trade with the mainland today. Coastal Kivinja would have traded fish and salt with interior settlements producing iron and grains.

The transoceanic trade would have contributed to the development of a central place attracting vessels bringing foreign goods. It is apparent that the central coast of Tanzania, near or in the Rufiji Delta, should be considered among the likely locations of such an early emporium. All the indicators mentioned in the Periplus [promontory, sewn boats, archaic fishing methods, imported goods] are found there, and in the immediate hinterland there are good soils for cultivation, red lateritic soils for iron smelting, and wildlife for ivory. The ancient alkaline bead recently found in a Limbo-type site 20 km inland from the delta confirms this trade link. By the beginning of the 7th century trade with India, Mesopotamia, and the Mediterranean had penetrated farther into the interior, and at this point Unguja Ukuu was probably the trade center. By now, as Horton (1987) has pointed out, Azanian sailors may themselves have been venturing into North Indian waters.

References Cited


Changing Images of Primate Societies

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Women scientists are widely considered to have played a major role in the historical shifts that have taken place in our interpretations of primate society. Primateology is a young science, having fully emerged only since the end of World War II, and during this short period, interpretations of primate behavior and society have changed considerably [see Strum and Fedigan n.d.]. To some observers, primateology appears to be an excellent example of a science that has been “feminized.” But to what extent is the prevalent popular image of the woman primatologist in the field a recent creation of the American media? And what are the relative contributions of variables other than gender to shifts in perceptions about primate society, among them changes in theory and methods, changing societal concerns, and the interaction of science and society? How might we bring empirical evidence to bear on these issues? Have analogous shifts in the interpretation of social life occurred in related disciplines [sociocultural anthropology and archaeology, psychology, ethology and animal behavior] and in other national traditions of primateology [Japanese, Brazilian, British]? It was to begin to address these questions that Shirley Strum and Linda Fedigan

1. © 1997 by The Wenner-Gren Foundation for Anthropological Research. All rights reserved 0011-3204/97/3804-0008$1.00. The conference participants, their affiliations, and [where appropriate] their paper titles were as follows: Pamela Asquith [Department of Anthropology, University of Alberta], “Japanese Constructs of Primate Societies”; Richard Byrne [Department of Psychology, University of St. Andrews], “The Role of Imitation in Primate Cognition”; Linda Fedigan [Department of Anthropology, University of Alberta], co-organizer, “Theory, Method, and Gender: What Changed Our Views of Primate Society?”, Stephen Glickman [Department of Psychology, University of California at Berkeley], “Disciplines, Subdisciplines, and Cultural Influences: A View from Comparative Psychology”; Donna Haraway [History of Consciousness, University of California at Santa Cruz], “Feminist Science, Studies, and Primate Revisions”; Robert A. Hinde [St. John’s College, Cambridge], “Primatology and Anthropology of Multiple Sources”; Sarah Blaffer Hrdy [University of California at Davis], “Raising Darwin’s Consciousness: Female Sexuality and the Prenatal Origins of Patriarchy”; Alison Jolly [Department of Ethology, Princeton University], “The Bad Old Days of Primatology”; Evelyn Fox Keller [Science, Technology, and Society, Massachusetts Institute of Technology], “Women, Gender, and Science: Some Parallels between Primatology and Developmental Biology”; Hans Kummer [Switzerland], “Lost Topics in Primate Research” [in absentia]; Bruno Latour [Centre de Sociologie de l’Innovation, Ecole des Mines], “Primate Relativity: Reflections of a Fellow-Traveler”; Gregg A. Mitman [Department of History of Science, University of Oklahoma], “Life in the Field: The Nature of Popular Culture in 1950s America”; Brian Noble [Department of Anthropology, University of Alberta], “Leaky Visions of Gender, Nature, and Apes in 1959: The Persistence of Fossey’s Mist”; Naomi Quinn [Department of Cultural Anthropology, Duke University], “Women Theorizing Gender: The Case of Cultural Anthropology”; Thelma Rowell [West Chapel House, Chapel-le-Dale, Ingleton via Carnforth, England], “A Few Peculiar Primates”; Craig Stanford [Department of Anthropology, University of Southern California at Los Angeles], “Chimpanzees, Bonobos, and Human Origins: Empirical Evidence and Shifting Assumptions”; Karen Strier [Department of Anthropology, University of Wisconsin], “Diversity, Demography, and Population Variability”; Shirley Strum [Department of Anthropology, University of California at San Diego], co-organizer, “Theory, Method, and Gender: What Changed Our Views of Primate Society?”, Robert Sussman [Department of Anthropology, Washington University], “Piltdown Man: The Father of American Field Primatology”; Hiroki Takasaki [Laboratory of Anthropology, Okayama University of Science], “Traditions of the Kyoto School of Field Primatology in Japan”; Zu-leyma Tang-Martinez [Department of Biology, University of Missouri], “Expensive Eggs, Cheap Sperm, and Sexually Passive Females: A Paradigm in Transition”; Alison Wylie [Department of Philosophy, University of Western Ontario], “The Engendering of Archaeology: Reconfiguring Feminist Science Studies”; Maria Emilia Yamamoto [Universidade Federal do Rio Grande do Norte], “Brazilian Primatology: The Last Ten Years.” Representing the Wenner-Gren Foundation were Sydel Silverman [President], Laurie Obbink [Conference Program Associate], and Mara Drogen [Program Administrator].
co-organized a conference sponsored by the Wenner-Gren Foundation for Anthropological Research and attended by 23 scholars from eight countries. The workshop, entitled “Changing Images of Primate Societies: The Role of Theory, Method, and Gender,” was held June 15–23, 1996, in Teresopolis, Brazil. It represented the first major attempt to bring primatologists of a variety of ages, nationalities, and schools of thought together with scientists in related behavioral disciplines and with scholars in feminist, science, and popular culture studies. Particularly noteworthy is that this was the first sustained encounter, at least in the North American context, between scientists and those who study them.

To facilitate a collaborative examination of the issues just identified, we first circulated a position paper on changing ideas in North American primatology from 1920 to the present. Focusing on primate field studies and on evolutionary interpretations of behavior and society, primarily by American scientists, we asked the primatologist participants to address aspects of the history of the discipline emphasizing either a particular stage or an issue that had changed through time.

Primatology occupies a unique position in the history of science, having developed from traditions within at least three different academic disciplines (anthropology, psychology, and zoology) and having flourished within several different national/cultural settings, including those of North America, Britain, continental Europe, Brazil, and Japan. We tried to put North American primatology in context by asking whether shifts in our interpretations of primate society are unique or part of larger trends that can be documented elsewhere. Thus, experts from other fields and cultures were asked to describe developments in related disciplines and in other national traditions over the same period.

It is also important to place changing ideas in primatology within a larger context of science and society. We called on a variety of analytic expertise for an examination of the scientific process and how knowledge is constructed, an assessment of the impact of the women’s movement and the role of women scientists, and insights about the interaction of popular and scientific cultures, especially the role of the media. We asked the experts in science, feminist, and popular culture studies to address questions such as the relationship between changing ideas in science and changing facts, the effects of the woman’s movement and the presence of women practitioners on the nature of various sciences, and the role of the media in creating images of women, of scientists, and of nature.

The discussions were divided into three parts: (1) Changing Factors within Primatology—Method, Theory, and Gender, (2) The Comparative Perspective, and (3) The Larger Context. We had understood our guiding questions to be multifactorial, but in the early days of the conference we came to realize that the issues were even more complex than we had previously thought. Each of the major factors that we had identified—method, theory, and gender—turned out on closer inspection to be heterogeneous, interdependent, and controversial. There was disputation among the participants over how to define these factors, how to measure their influence, and even what words to use to talk about them. We had deliberately invited representatives of many schools of thought and had expected several opposing forces (e.g., pro- and antisociobiology, pro- and antifeminism), but we had not fully anticipated that the scientists and those who study them would hold such different views of what science is. Apparently representatives of the two groups had never been brought face to face to discuss science with enough participants to create two critical masses. As a result, the two “sides” were initially suspicious of each other, and this barrier to communication only slowly dissolved.

The opening session of the first part of the conference was intended to be a discussion of the impact of methods on the way we think about primate society. Instead, it was greatly taken up with issues of communication as participants from different disciplines struggled to find common linguistic and epistemological ground. Some time was spent trying to sort out the differences between methods, methodology, and techniques and to determine whether it is possible to separate method from theory in any general sense. For example, methods may, on the one hand, be driven and constrained by the theory that frames the study, while on the other hand they may be envisioned as expanded through the inclusion of instrumental factors such as the antimalarial drugs and rapid air travel that greatly changed the practices of primatologists. In addition to communication and definitional issues, substantive methodological matters were discussed, among them the benefits of experimental versus observational techniques, the lack of methods for dealing with stochastic processes, and the potential of new noninvasive techniques for collecting biological samples. There was much discussion of whether the observational work of Japanese primatologists was truly method-free. We began to realize that methods are inevitably situated in cultural, disciplinary, and historical contexts and therefore questions about the impact of methods on primatology are best answered when so situated.

The second session, on the influence of theory on primatology, generated less heat over the meanings and uses of terms across disciplines, but even here there were disputes. By the end of the session participants had used the term “theory” in at least 13 different ways, and it became obvious that, as with methods, theory must also be situated in time, space, and context. For example, sociobiology, ethology, group selection, and behavioral ecology had different histories and connotations for primatologists from different national/cultural traditions and even for primatologists from different institutions within North America. How and when does a theory such as sociobiology “travel” across disciplinary and cultural boundaries, and what makes it portable? Is it the criterion of testability or the perception that it is useful and productive in advancing research in a localized setting? One factor that surfaced
repeatedly in our discussions was the overriding importance of institutional location in determining how a theory or a researcher’s work is received, tested, communicated, and transformed.

We had another set of definitional and conceptual issues to address in the session on primatology of gender, namely, the meanings of “gender,” “female,” “woman,” and “feminism” for participants from different backgrounds. Keller presented a helpful distinction between three types of analyses of women, gender, and science: [1] the study of women in science (historical, biographical projects), [2] the science of gender [studies of how science has contributed to myths of gender], and [3] gender in science [study of the symbolic role of gender in science itself—the way in which dominant views of gender shape our expectations and inform science]. One point raised in this session was whether some of the scientists present were making the mistake of essentializing gender to mean sex (i.e., “biologically female”). Gender, all finally agreed, is a cultural construct not identical to woman or female. Several of the feminist scholars pointed out that gender has proved surprisingly unstable in time and space. Whereas the science scholars argued that gender is not a permanent property of an individual but rather varies across contexts and cultures, several of the scientists continued to insist that it is, and must be, possible to measure and generalize about gender within a given culture and time frame. Otherwise we are just talking about an infinitely variable phenomenon with no reference point. The deeper issue in this discussion was finding widely acceptable ways to talk about, study, and understand science.

The second part of the conference dealt with comparisons across related disciplines. The three subdisciplines of anthropology that were compared [sociocultural, archaeology, primatology] seemed to have had, for example, quite different histories with regard to women’s issues. Quinn argued that in sociocultural anthropology (unlike primatology) the feminism of the 1960s and 1970s was displaced by postmodernism in the 1980s. Wynne noted that gender issues entered archaeology much later [in the 1980s] than in sociocultural anthropology or primatology. Silverman noted that the different subdisciplines have differed in the extent and timing of the inclusion of women in their ranks. Discussion in this session also touched on why primatology has had so little influence on the other subdisciplines of anthropology and whether it is correctly located in the larger discipline. Although, as noted by Sussman, primatologists bring an important component to the study of human variability and universals, they are not well accepted by their anthropological colleagues, in Strier’s view, once they adopt biological methods and theory. Interestingly, there seemed to be more similarities between primatology and psychology or primatology and animal behavior than among the subdisciplines of anthropology. For example, according to Glickman, the history of sexual behavior studies in rats parallels some of the developments in sexual behavior research in primates, and the history of hyena field studies exhibits some similarities to that of primate field studies. Further, according to Tang-Martinez, the changing view of the female mammal (especially the female ground squirrel) in animal behavior studies is similar to the changing view of the female primate. Both have involved a move from passive to active females, from socially unimportant females to female-bonded societies, and from asymmetrical terms such as “harems” to terms that recognize the central significance of females in their societies.

The third part of the conference opened with a session examining different national traditions of primatology. Hinde, Rowell, and Kummer argued that British and continental European primatology grew directly out of classical ethnological research, even though the earliest practitioners in Britain worked and trained students in departments of anatomy, psychology, and anthropology. Unlike the Washburn school of primatology in North America, British primatology did not grow out of a desire to use primates as models for human evolution. However, Byrne suggested that Hinde and Rowell were presenting the “view from Madingley” and that British primatologists from other institutions would present a different history. He argued that all primatologists are fundamentally interested in learning more about humans. Brazilian primatology, according to Yamamoto, was at first focused on establishing and maintaining captive colonies of primates for biomedical use, but in the past 20 years conservation has been the major impetus for field studies of primates and most young Brazilian scientists studying primates are trained as conservationists. Brazilian primatologists do not yet have the impact that they desire on an international front, and in some respects their concerns [marginalization, difficulties in publishing and being cited] parallel the women’s issues in North American and European science. Asquith argued that just as “gender” and “woman” are categories that are heterogeneous and do not stand still, so Japanese primatology is not a monolithic, static enterprise. She noted that many vital insights into primate society in Japanese publications were missed by Western scientists because these findings were couched in terms that we regard as anthropomorphic and overly descriptive. Takasaki described the Kyoto school’s approach to primatology with highly evocative metaphors—they do “primatography” rather than primatology, their way of doing science produces both gems and pebbles, they regard the Western insistence on the primacy of theory in science as “extracting the nutrients from food and eating them as pills.”

The session on popular culture focused on the media’s role in creating and disseminating both scientific ideas and cultural images of primates and of women studying animals. Mitman argued that scientists need to take the media very seriously because much is at stake, including funding and the transformation of field sites into tourist sites. Haraway described primatology as a “zone of implosion” where multiple factors con-
Noble analyzed the representation of Goodall in *National Geographic* articles and Fossey in the film *Gorillas in the Mist* as images of women primatologists that influence the public and the next generation of students. Stanford described the making of the television documentary *The New Chimpanzees*, in which he participated, as an example of the way in which the producer/editor and not the scientist controls the message and the final product. Jolly argued that the threat of extinction of primates is changing primatology and described the positive role of the media in creating this shift. What we learned from this session is that we ignore the public realm at our peril and that we have social responsibilities as scientists to communicate with the public through the media. Participants concluded that because there will always be a powerful incentive to use science as represented in the media to reinforce cultural values rather than attempting to control the media after the fact it is best to include journalists and filmmakers from the start.

In the science studies session, we examined the production of knowledge in relation to different models of science with a particular focus on what happens during scientific practice rather than on the normative aspects. The resulting discussion was about how science really works, how to delineate good and bad science, whether there really is a “scientific method” and how important it is, whether scientific knowledge is cumulative, and whether there is a difference between what scientists do and what science is. Latour presented a model of science with five articulated horizons that demonstrated why the practice of science cannot and should not be viewed as isolated from the rest of the world, and participants discussed and suggested modifications to it. Mitman described the methods that historians of science use to understand how a given science has changed over time. Sussman discussed the problem of determining when we have enough data to support a theory and whether a question is answerable. Haraway helped those unfamiliar with science studies to understand the variety of work encompassed by the field by presenting three different ethnographic approaches to the study of science. There was particular interest in her presentation because her *Primate Visions* was the first and most extensive analysis of primatology from a science studies perspective. Participants concluded that the production and dissemination of scientific knowledge is a dynamic process that requires theories, methods, inscription devices, colleagues, allies, and public relations just to get started. We often assume that popularization of science is a form of dissemination and that scientists can or should control the media’s reports of their work, but this diffusionist model and the scientists’ assumptions about it may not be correct.

In a deliberate effort to return once more to the concrete, the penultimate sessions began with a round-robin question to participants: Had their ideas ever changed, and why? Many of the accounts were conversion tales in which a scientist began with a received truth or a definite, clear idea and the behavior of the subjects themselves changed the scientist’s mind, and many of the examples occurred early in the scientist’s career.

The goal of this conference was to combine our diverse expertise to come to a better understanding of how and why our perceptions of primate societies have changed. We hoped to learn from the collective effort how to ask better questions and where to search for better answers. Thus we have no “results” in any standard sense to present. It was a historic and novel event in itself to assemble scientists and those who study them to discuss matters of science, in this case primatology. Although we did not get as far in terms of concrete “conclusions” as we might have with a homogeneous group of experts, we certainly can draw lessons from our discussions and point to the ways in which different ideas about science can generate mutual suspicion and barriers to communication that can be overcome with time and effort.

The three sessions in the first part of our conference were sobering in that they required us to define our terms and to acknowledge the complexity of the issues. We learned that it takes extra work to talk across disciplines but that cross-disciplinary communication opens up new ways of looking at familiar problems. We also realized that our initial question—theory, method, and gender, what changed our minds?—had to be carefully situated. Finally, we recognized the need for a method or set of methods for determining how primatology has changed that will be acceptable to both practitioners of science and those who study science. We came to the conclusion that studying the development of a science is as complex and multifactorial as studying the ontogeny of an individual and needs to account for at least as many levels of interaction.

Several lessons also emerged from the second part of the conference. First, we found that even subdisciplines housed within the same academic department [such as anthropology] might have quite disparate histories and that members of these subdisciplines might have little awareness of the theoretical and methodological issues that interested their colleagues. Second, we discovered that there was disagreement over whether and when primatology had achieved independence from its parental disciplines of anthropology, psychology, and ethology/animal behavior. And finally, we learned once again that we must be more specific about the variables in any comparison of primatology with other disciplines because the fields of anthropology, psychology, and animal behavior are large and heterogeneous and are practiced differently in different nations and even in different institutions within North America.

During the final part of the conference, several useful suggestions were made as to how to further research on our topic, a “team approach” involving different techniques and types of expertise was one such suggestion. Participants recognized the danger of falling into the “science wars” trap, in which practitioners and science studies scholars or scientists and the media are seen as
adversaries. It was concluded that addressing complex
issues requires collaboration.
A number of challenges remain: formulating ques-
tions about changes in primatology in such a way
that the rich history of local events is not lost, determin-
ing what will constitute the evidence for change and how
it can be gathered, and encouraging the relevant experts
to communicate and collaborate across disciplinary
boundaries. Several conclusions can also be drawn. First,
this conference opened up several new ways of
thinking about the science of primatology; participants
suggested both new avenues of research and new con-
tRAINTS on how we ask and answer questions. Second,
although some of the participants felt that we spent too
much time discussing questions of science, it was clear
that we cannot understand specific patterns in any as-
tpect of primatology without a general framework for
what science is. This conference focused upon what has
happened in the past, but an understanding of what has
gone before will also allow us to move forward in the
future. And everyone was concerned about the future:
the future of primates, of primatology, of the other dis-
ciplines, and of science. We have formed an e-mail list
or chat group to continue the discussions begun in Bra-
zil. A book based on revisions of the conference papers,
continuing discussions among the participants, and our
emerging understanding of how “science” and “gender”
issues are embedded in our perceptions of the history of
primatology is in preparation.

Reference Cited
and gender: What changed our views of primate society?” in
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Contraception in Three Chibcha Communities and the Concept
of Natural Fertility

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The decline in the fertility rate of the population of Eu-
 rope at the beginning of the 19th century—the so-called
fertility transition—has been the subject of many stud-
IES attempting to understand its causes. Fundamental
contributions have been made by Henry [1961] in dis-
tinguishing pre- and posttransitional regimes and Bon-
gaarts [1978] in revising the intermediate variables of
fertility identified by Davis and Blake [1956] and sug-
uggesting that the total fertility rate of a population can
be calculated by combining its proximate determinants.

Reviewing studies of European populations, Henry
[1961] identified two different patterns of fertility. In
the pretransition stage, fertility was high and indepen-
dent of the number of previous offspring, while in the
posttransition stage fertility was low and dependent on
the number of previous offspring. Henry was trying to
obtain a baseline representing the fertility of popula-
tions that did not practice birth control with which to
compare the fertility rate of contemporary societies
(Logerst, 1990), but to his surprise he found that the
marital fertility of pretransition societies was quite
variable. This led him to reject the concept of physio-
logical fertility in favor of the new concept of natural
fertility. Although the concept of natural fertility was
first used by Pearl [1939], the exact definition comes
from Henry [1961]:

We can term as natural the fertility which exists or
has existed in the absence of deliberate birth con-
. . . control may be said to exist when the be-
behavior of the couple is bound to the number of chil-
dren already born and is modified when this
number reaches the maximum which the couple
does not want to exceed.

Given this definition, natural fertility would have been
characteristic of the pretransition period, in which the
human population was characterized by high birth and

Bongaarts (1978) considered the independence of
the number of previous offspring characteristic of the pre-
transition period a function of two of the proximate de-
terminants he identified: [1] the proportion of married
women and [2] the mean duration of the infertile period
following birth. Furthermore, he considered the depen-
dence of this variable characteristic of the posttransi-
tion period a function of these same proximate deter-
nants and of [3] the effectiveness of contraceptive
methods and [4] the rate of abortion. Thus, the transi-
tion from natural fertility to controlled fertility is re-
lected in the increase of contraception and abortion as
ways of preventing births.

The decline of fertility has thus been interpreted as a
shift from natural fertility to controlled fertility, and
this change is attributed to the introduction of contra-
ception or, as Henry put it, the entry of rationality into
the sphere of reproductive behavior. This point of view,
as well as the more explicit one advanced by Knodel
[1977], suggests that the deliberate and prolonged con-
rol of fertility within a marriage is itself a consequence
of modernization [Landers 1990]. Thus modernization
is understood as the agent of change in the fertility tran-
sition. The empirical patterns of fertility in some popu-
lations deemed “traditional” (in Latin America, Asia,
and Africa) are considered pretransitional, and therefore
their fertility is considered natural fertility [Henry 1961].

Using data collected in three pretransitional Chibcha communities in Costa Rica, this study argues for the necessity of revising the concept of natural fertility. My hypothesis is that the high pretransition or current fertility of traditional populations reflects not the absence of the notion of controlling fertility but a social, economic, and cultural choice. In the three indigenous communities studied we find contraception being practiced by young, fertile women and also reported as having formerly been practiced by women past menopause. Furthermore, there is no difference between these groups with respect to the frequency of its use, although there are differences in methods: premenopausal women use primarily modern methods, while postmenopausal women report having used traditional ones.

MATERIALS AND METHODS

The Chibcha groups studied live in southern Costa Rica [fig. 1], between 8°00′ and 10°00′ north latitude and 82°30′ and 84°30′ west longitude on the Pacific coast and in the interior [see Greenberg, Turner, and Zegura 1986; Barrantes 1990, 1993]. The choice of these groups for study was a consequence of an interest in analyzing the presence of contraception in these populations as a function of the length of contact with the white or creole community and therefore of the degree of disintegration of the traditional culture. The Guaymi community was chosen as the most isolated group, of all the indigenous groups in Costa Rica they have best conserved their traditional culture, and studies of genetic markers evidence little or no mixing with nonindigenous groups [Bozzoli 1986, Barrantes 1993, Guevara and Chacon 1982, Barrantes and Azofeifa 1981]. Within the Guaymi community itself two distinct settlements, Progreso and Villapalacios, the former more isolated than the latter, were studied. The Huetar community, the most accessible (30 km from San José), was chosen as the most integrated into creole life, continuous contact with the white community having begun before the turn of the century. It is small and moderately transculturized and hybridized with nonindigenous groups, whites as well as blacks. Finally, the Bribri and Cabecar communities were chosen as intermediate in degree of contact with the creole community and, moreover, relatively accessible. Some members of these communities have maintained their traditional ways of life as well as their scattered settlements, although a significant degree of contact with Western society is reflected in types of dwelling and in the presence of schools, transportation, and modification of their traditional customs [Sainz de la Maza 1994]. For these two communities three settlements with different degrees of isolation, Salitre (Bribri), Ujarras (Cabecar), and Cabagra (Bribri), were studied. Bozzoli [1979], among others, has proposed that the Bribri and Cabecar are best understood as a unit with regard to their belief systems and warfare, and therefore they have been treated as one. Statistical analysis [Student-Newman-Keuls test] determined with a 95% level of confidence that the Bribri and the Cabecar are similar with regard to the variables studied [Sainz de la Maza 1994].

To carry out this study, 320 Huetar, Bribri-Cabecar, and Guaymi women were interviewed with regard to their reproductive histories. The distribution of the women interviewed by community and the representativeness of the sample are shown in table 1 and their number and distribution by community and reproductive stage—premenarcheal, premenopausal, and postmenopausal—in table 2. Taking into account the heterogeneity of the census sources, the percentage of the population that the women interviewed represent has been calculated for each community. Data collection was undertaken during a period of six months—September–November 1992 and February–April 1993.

RESULTS

The frequency of use of the different types of contraceptive methods is shown in table 3, in which we observe that although the three groups behave in significantly different ways with regard to the use of these methods it is the absence of their use that predominates [fig. 2]. In each community modern methods of contraception, especially birth control pills, were the most frequently used [fig. 3]. The median age at the start of contraception varied with the community; the Guaymi began to use contraceptive methods earlier than the others. There are no significant differences in the practice of contraception—either modern or traditional—or in the use of different contraceptive methods—pills, prophylactics, etc.—and in medical supervision between premenopausal and postmenopausal women [table 4]. There is, however, a significant difference (p < 0.05) in the type of contraception used, whether modern or traditional, according to reproductive stage; the postmenopausal women had mostly used traditional methods [fig. 4].

Focusing on the women who had completed their reproductive period and therefore had a final family size, the use of contraceptives is significantly negatively correlated (R = −0.134; p < 0.05) with the number of live births. For this sample of women, differentiating four reproductive periods according to the age of the first pregnancy, we find that the amount of time between the birth of the last child and menopause decreases with increase in the age at first pregnancy [table 5]. Thus the women who had started having children later maintained their reproductive capacity until 6 years before menopause while those who had started having children earlier stopped reproducing 12 years before menopause.

DISCUSSION

Almost all of the published works on fertility in developing populations mention the absence of contraceptive

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methods and consider lactational amenorrhea the contraceptive method used in those populations that do not use other contraceptives apart from the postpartum sex taboo [Wood, Johnson, and Campbell 1985, Khalifa 1986, Garenne and Van de Walle 1989, Chimere-Dan 1990, Harrison 1992]. Moreover, delay of marriage and first childbirth are viewed as necessary for a decline in fertility [Rob 1990, Luc et al. 1993]. The general acceptance that fertility in these traditional populations is “natural” implies a belief in the absence of deliberate control of the birthrate [Henry 1961], another characteristic that seems to group these populations [Wood, Johnson, and Campbell 1985, Challis 1986, Rosette 1989, Garenne and Van de Walle 1989, Chimere-Dan 1990, Rob 1990, Harrison 1992, Luc et al. 1993] but in fact obscures the considerable variability characteristic of so-called natural fertility. Leridon (1977) found that in 23 natural-fertility populations the total fertility rate varied between 3.7 and 9.5 — this variation, as the definition of natural fertility implies, resulting from the combined effect of postpartum amenorrhea and sex taboos, early or late marriage and/or first childbirth, and different physiological conditions. In the three indigenous communities studied here, however, the practice of contraception is reported both by young, fertile women and by women past menopause. This raises the question whether contemporary or “primitive” pretransitional populations are really characterized by an absence of deliberate control of the birthrate.

The results of this investigation indicate that tradi-
### Table 1
Number of Women Interviewed by Indigenous Community and Percentage of Each Community’s Population According to the Three Most Accurate Censuses

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Huetar</td>
<td>98</td>
<td>30.6</td>
<td>56</td>
<td>29</td>
<td>–</td>
</tr>
<tr>
<td>Bribri-Cabecar</td>
<td>115</td>
<td>35.9</td>
<td>18</td>
<td>8</td>
<td>8</td>
</tr>
<tr>
<td>Bribri</td>
<td>67</td>
<td>20.9</td>
<td>–</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Cabecar</td>
<td>48</td>
<td>15.0</td>
<td>–</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Guaymí</td>
<td>107</td>
<td>33.4</td>
<td>33</td>
<td>24</td>
<td>11</td>
</tr>
<tr>
<td>Total</td>
<td>320</td>
<td>100.0</td>
<td>–</td>
<td>–</td>
<td>–</td>
</tr>
</tbody>
</table>

### Table 2
Number and Percentage of Women by Reproductive Stage and Indigenous Community

<table>
<thead>
<tr>
<th>Indigenous Community of Origin</th>
<th>Reproductive Stage</th>
<th>n</th>
<th>%</th>
<th>n</th>
<th>%</th>
<th>n</th>
<th>%</th>
<th>n</th>
<th>%</th>
</tr>
</thead>
<tbody>
<tr>
<td>Huetar</td>
<td>Premenarcheal</td>
<td>12</td>
<td>12.2</td>
<td>22</td>
<td>19.1</td>
<td>14</td>
<td>13.1</td>
<td>48</td>
<td>15.0</td>
</tr>
<tr>
<td>Bribri-Cabecar</td>
<td>Premenarcheal</td>
<td>62</td>
<td>63.2</td>
<td>82</td>
<td>71.3</td>
<td>84</td>
<td>78.5</td>
<td>228</td>
<td>71.3</td>
</tr>
<tr>
<td>Guaymí</td>
<td>Premenarcheal</td>
<td>24</td>
<td>24.5</td>
<td>11</td>
<td>9.6</td>
<td>9</td>
<td>8.4</td>
<td>44</td>
<td>13.7</td>
</tr>
<tr>
<td>Total</td>
<td></td>
<td>98</td>
<td>100</td>
<td>115</td>
<td>100</td>
<td>107</td>
<td>100</td>
<td>320</td>
<td>100</td>
</tr>
</tbody>
</table>

### Table 3
Frequency of Use of Contraception (%), Mean Age at Start of Use, and Medical Supervision (%) by Community

<table>
<thead>
<tr>
<th>Indigenous Community</th>
<th>Contraceptive Usea</th>
<th>Modern</th>
<th>Traditional</th>
<th>No Response</th>
<th>Pill Useb</th>
<th>Mean Age at Startc</th>
<th>Medical Supervisiond</th>
</tr>
</thead>
<tbody>
<tr>
<td>All</td>
<td>27.6</td>
<td>76.2</td>
<td>13.1</td>
<td>10.7</td>
<td>64.9</td>
<td>24.0</td>
<td>6.72</td>
</tr>
<tr>
<td>Huetar</td>
<td>41.9</td>
<td>97.3</td>
<td>2.7</td>
<td>–</td>
<td>61.1</td>
<td>24.8</td>
<td>7.8</td>
</tr>
<tr>
<td>Bribri-Cabecar</td>
<td>25.8</td>
<td>60.6</td>
<td>12.4</td>
<td>27.0</td>
<td>62.1</td>
<td>25.4</td>
<td>4.9</td>
</tr>
<tr>
<td>Guaymí</td>
<td>16.1</td>
<td>57.1</td>
<td>42.9</td>
<td>–</td>
<td>88.9</td>
<td>19.5</td>
<td>3.9</td>
</tr>
</tbody>
</table>

aSignificant, p = 0.0.
bSignificant, p < 0.05.
Fig. 2. Frequency of use of contraceptives by indigenous community. Left to right, yes, no, no response.

Fig. 3. Type of contraceptive method used by indigenous community. Left to right, traditional, modern, no response.

Fig. 4. Type of contraceptive method used by reproductive stage. Left to right, all women, postmenopausal women, premenopausal women.

Table 4
Frequency of Use of Contraception (%), Mean Age at Start of Use, and Medical Supervision (%) by Reproductive Stage

<table>
<thead>
<tr>
<th>Reproductive Stage</th>
<th>Contraceptive Use</th>
<th>Type of Contraceptive Used</th>
<th>Medical Supervision</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Modern</td>
<td>Traditional</td>
</tr>
<tr>
<td>Postmenopausal</td>
<td>31.8</td>
<td>64.3</td>
<td>35.7</td>
</tr>
<tr>
<td>Premenopausal</td>
<td>26.8</td>
<td>77.5</td>
<td>8.7</td>
</tr>
</tbody>
</table>

*Significant, p = < 0.05.

*Significant, p = 0.0.
gatherer nomads and sedentary farmers, might be seen as an explanation of this variation, which in 90% of cases ranges from 4 to 8 (Wood, Johnson, and Campbell 1985), but this has been demonstrated not to be so [Campbell and Wood 1988]. At the extremes of the fertility distribution, for the values outside the 4–8 interval, it has, however, been possible to classify the populations. Those with birthrates higher than 8 are considered colonizing populations—among them the Hutterites, the Mennonites, the Mormons, and even the Yanomami. Those with birthrates lower than 4 are populations characterized by pathological sterility such as those found in Sub-Saharan Africa (Caldwell and Caldwell 1983). Pathological sterility will not be considered here. It is interesting, however, to see that the colonizing populations—groups with the intention of populating, settling, and establishing themselves—tend to reproduce more than others that do not have these ambitions. If some populations respond to particular circumstances by increasing their fertility as colonizing groups do while others maintain lower numbers, it must be because they have some control over their reproduction. One might ask, then, what mechanism is controlling these variations in fertility.

According to Bongaarts (1978), the proximate determinants of fertility are the proportion of married women, the practice of contraception, the existence of abortion, the duration of postpartum infertility, the frequency of sexual relations, spontaneous intrauterine mortality, and the percentage of permanent sterility. Therefore, bearing in mind that the existence of natural fertility in traditional populations presupposes the absence of abortion and contraception, the homogeneous fertility of developed countries is the result of the combination of all the proximate determinants while the heterogeneous fertility of traditional societies results from a combination of characteristics that excludes abortion and contraception.

Fertility began to decline in Europe as a result of control of the birthrate when women had achieved the number of children desired. Subsequently, and once infant mortality was checked, control operated at all ages [Bernis and Varea 1990]. Thus there was a general decline of fertility throughout Europe as a consequence of the incorporation of women into the workforce and other social, economic, and cultural factors [Hill 1990]. This trend shows that, in the past as now, the number of children born to a woman is the result of a choice conditioned by one circumstance or another. All this brings us to the hypothesis that even in women belonging to traditional societies fertility is the result of a choice and never the consequence of the absence of deliberate birth control.

The populations studied here have a fertility level of 6.5 (Sainz de la Maza 1994), and this does not vary significantly among them even though age at menarche, age at first childbirth, or duration of lactation may do so. At the same time, as we have seen, women who began to have children later maintain effective reproduction longer than those who began to have children early, from which it can be deduced that they were practicing birth control. These data alone would confirm the presence of contraception in this supposed natural-fertility population, but they are further supported by the information gathered about the use of contraception analyzed earlier. These observations lead us to conclude that we are dealing with populations that control their birthrate throughout the fertile period.

Here the hypothesis is advanced that traditional societies have contraceptive methods—including natural medicines, postpartum amenorrhea and sex taboos, abortion, and even infanticide—and use them. Therefore the concept of “natural fertility” understood as the fertility of populations that do not control their birthrates no longer makes sense in its general application to traditional societies. It is possible that in communities subjected to severe tension and acculturation [refugee camps, etc.] situations may arise in which women are truly incapable of controlling their fertility, but even here this would be a hazardous assumption.
Whether natural fertility has ever existed is a question that Wood [1991] has characterized as difficult to answer. In the first place, he questions the definition of natural fertility, which is based on a negative logic (the absence of behavior limiting the number of children per woman) that is impossible to demonstrate in any case. He goes on to suggest that the concept of natural fertility depends on individual intentions, which are always difficult to distinguish, the more so when one is dealing with little-known populations and, especially, past populations. The data from a number of research projects being carried out at present point to behaviors in traditional societies that clearly indicate fertility control—whether dependent on the number of previous offspring or not (Felt et al. 1990; Varea 1993; Sainz de la Maza and González-Kirchner 1991, 1995). These findings give us reason to reopen the debate over the appropriateness of the concept of natural fertility as it is used at present.

References Cited


Polygyny as a Risk Factor for Child Mortality among the Dogon

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Polygyny was allowed in 83% of preindustrial societies in Murdock’s (1967) Ethnographic Atlas \(N = 849\); evidence on sexual dimorphism suggests that polygyny was also the prevailing mating system over human evolutionary history (Alexander et al. 1979). In a classic book, the economist Gary Becker (1981) presents a theoretical model in which he explains the widespread occurrence of polygyny in terms of benefits for women. In essence, he agrees with George Bernard Shaw, who wrote, “The maternal instinct leads a woman to prefer a tenth share in a first-rate man to the exclusive possession of a third-rate one.” Becker’s view is similar to the polygyny-threshold model (Verner and Willson 1966, Orians 1969) developed for passerine birds and extended to humans [Borgerhoff Mulder 1988, 1990; Josephson 1993]. In brief, the polygyny-threshold model proposes that “since polygyny must always be advantageous to males, its presence or absence must depend primarily upon the advantages or disadvantages to females” (Orians 1969). Becker’s hypothesis and the polygyny-threshold model view polygyny as the outcome of female choice, but I shall argue that female choice is inadequate to account for the maintenance of polygyny among the Dogon of Mali.

An alternative to the female-choice model emphasizes conflicts of interest between members of the two sexes over the optimum mating system [Downhower and Armitage 1971, Irons 1983, Davies 1989, Chisholm and Burbank 1991]. In humans, the conflict arises because males may benefit reproducitively from concurrent marriages to multiple wives even if the result is lower average fitness for each wife. Thus, to the extent that male strategies win over female strategies in any given society, polygynous marriages may entail a cost to female fitness. Females may experience the cost of polygyny through a reduction in fertility or an increase in child mortality. Testing for an adverse effect of polygyny on the reproductive success of females was a pri-

mary a priori objective of 31 months of fieldwork among the Dogon initiated in 1986 and currently ongoing.

Demographers have examined the hypothesis that polygyny reduces female fertility, but their findings have often been equivocal and have failed to illuminate the underlying mechanisms (e.g., Dorjahn 1958; for review see Wood 1995). In view of the considerable interest generated by the possible link between polygyny and fertility, it is curious that the hypothesis that child mortality is higher under polygyny has drawn little attention. The few studies that have addressed this possibility [Chojnacka 1980; Roth and Kurup 1988; Isaac and Feinberg 1982; Borgerhoff Mulder 1989, 1990; Chisholm and Burbank 1991] have relied on retrospective data and have not controlled for other risk factors for mortality. By contrast with these studies, the data in this report constitute a prospective, longitudinal test of the polygyny-mortality hypothesis. Through logistic regression analysis [Hosmer and Lemeshow 1989], other significant predictors of mortality, including age, sex, wealth, nutrition, and family size, were controlled.

ETHNOGRAPHIC BACKGROUND

The Dogon live in the Malian Sahel in a landscape dominated by a 260-km sandstone cliff called the Bandiagara Escarpment. The study village, Sangui, is situated on the plateau of the escarpment at 14° 29’ N, 3° 19’ W, and had a population of 460 in January 1988. At that time, 54% of the married men in the study village had one wife, 35% had two wives, and 11% had three wives. First wives enjoy the minor honor of having a sleeping room to the right of the husband’s and other tokens of esteem but no material advantages. Polygyny is strictly nonsororal, and wives are either ya biru [arranged] or ya kezu [taken from another man]. Ideally they do not reside with the ige biru [arranged husband] until the birth of two children, who will be raised by their maternal grandparents. As a young girl matures, her fiancée presents minor gifts to his future parents-in-law including small amounts of cash, firewood, cowries, chickens, and grain. He also owes limited assistance in the fields, but there is no bride-price.

The Dogon are an appropriate population in which to test for an adverse effect of polygyny on female fitness because the interests of women are subordinated to those of men in several important respects. For example, through patrilineal descent and patrilocal residence, groups of related males form powerful coalitions [Smuts 1985, Hrdy 1996]. Related women are not allowed to marry into the same patrilineage, which makes alliances among female kin difficult to maintain. Behavioral scan data also provide evidence for male dominance: women spent 21% more time working than men \(t = -5.10; \text{d.f.} = 127; p < 0.0001\), and men spent 29% more time resting than women \(t = 7.71; \text{d.f.} = 127; p < 0.0001\) [Strassmann 1996]. Although women assume the energetic demands of lactation and heavy physical labor, their diet is largely restricted to the sta-

1. © 1997 by The Wenner-Gren Foundation for Anthropological Research. All rights reserved 0011-3204/97/3804-0010$1.00. I thank Akeme Dolo and the Dogon of Sangui for their generous hospitality, K. Sanogo for authorization to conduct this study, S. Moulin and T. Stevenson for participation in the fieldwork, S. Slocum for logistical help, B. Gillespie for statistical advice, K. Hunley for assistance with the preliminary analyses, and P. Gowaty, B. Hewlett, K. Hill, and S. Hrdy for helpful comments. This research was supported by the University of Michigan, the L. S. B. Leakey Foundation, and the National Science Foundation (BNS-8612291).
ple crop, millet, and men have almost exclusive access to meat [Strassmann 1996]. Animism, the traditional religion, is a powerful vehicle through which Dogon males attempt to assert control over female sexuality. For example, threats of supernatural punishment associated with animism, as well as social reprisals, help husbands and patrilineages to enforce the menstrual taboos. These prohibitions require menstruating women to advertise their menstrual taboo status by visiting a menstrual hut. Although all female informants \( N = 113 \) viewed the menstrual taboos as restrictive and unpleasant, hormonal data revealed excellent compliance [Strassmann 1992, 1996]. Females are also clitoridectomized, which is intended to promote paternity certainty by reducing female sexual pleasure.

The greatest burden on Dogon women, however, is child mortality. In the study village, the mean number of live births for postreproductive women was 8.6, but 20% of children died in their first year of life and 46% died by age five years \( N = 388 \) [Strassmann 1992]. Improper drinking water, low levels of vaccination, and other manifestations of poverty contribute to the mortality toll by increasing exposure and lowering resistance to the three major endemic killers: malaria, measles, and diarrhea [Fabre-Test 1985].

**METHODS**

The study population included all children \( N = 205 \) aged \( \leq 10 \) years who were resident in the study village between 1986 and 1988. These children were individually known to me because I resided in the village for 30 months during the same years. The village was recensused in 1994 to determine which children had survived the intervening six-year period. Twenty children had left the village and were lost to follow-up, and 9 children lived with widowed grandmothers in domestic units that did not include any married adults. After excluding these 29 children, the effect of polygyny was tested on a final sample of 176 children, 86% of the total population aged \( \leq 10 \) at the outset of the study. Data on age at death between 1988 and 1994 were not available; therefore I coded the dependent variable dichotomously [0 if the child was still alive in 1994 and 1 if he or she had died]. Since the dependent variable was dichotomous, I used logistic regression instead of survival analysis. All logistic regressions were performed in the statistical program SPSS 6.1 [SPSS 1994].

Any independent variable that improved overall model fit based on the likelihood ratio test [Hosmer and Lemeshow 1989] was included in the final models. To test whether the logit had a quadratic rather than a linear relationship with any of the continuous variables, I added squared terms. If a squared term did not improve model fit based on the likelihood ratio test it was omitted. The values for the independent variables were obtained from a combination of direct observation (e.g., child’s sex), private interviews (e.g., parent’s education), and quantitative measurement (e.g., economic rank). To compare the economic resources (land, grain, onions, and livestock) of all families, the 540 cereal fields and 422 onion fields belonging to the people of Sangui were measured with a compass and meter tape, baskets of grain and onions were counted and weighed, and livestock were tallied by species, maturity [juvenile versus adult], and sex, as described in Strassmann [1990]. Year-to-year fluctuations in the wealth of the village from 1986 to 1994 occurred in response to changes in rainfall and market forces but had a negligible impact on the relative wealth of the families, which I expressed as a rank from 1 [lowest] to 59 [highest]. The 14 poorest families were headed by widowed grandmothers and did not contain any married adults. Since this study was about polygyny versus monogamy, no children from these groups were included in the sample of 176 children.

Polygyny, the independent variable of key interest, was defined first by the mother’s marital status: first, second, third, or sole wife. This definition excluded from the analysis 46 children whose mothers were widowed, engaged, divorced, or deceased, reducing the sample size to 130. Second, I computed polygyny as the ratio of married women to married men in the child’s work-eat group, defined as the people who cultivate the same millet fields and assemble in one compound to eat together. Members of a work-eat group defer to the same head of the family and depend on one another economically. Married women cultivate millet alongside the other members of their work-eat group (both male and female) and do not plant their own individual millet fields. The sample size for polygyny of the work-eat group was 176 children and the mean \( \pm \text{S.D.} \) work-eat group size was 15.60 \( \pm 10.64 \) persons, with a range of 3–41. Work-eat groups do not correspond to households: if a work-eat group contains several married men, their families sleep in different compounds. Cowives and their children also sometimes have separate compounds.

**RESULTS AND DISCUSSION**

Univariate results revealed strikingly lower survivorship among the children whose mothers were polygynously married \( \chi^2 = 9.51, \text{d.f.} = 2, p = 0.008, N = 130 \), particularly if the mothers were first wives. When the ratio of married women to married men in the child’s work-eat group was plotted against the number of children who lived or died, a step function was obtained with high survivorship below and low survivorship above 1.5 (fig. 1). Specifically, a total of 37 children died and 81 children survived in the groups with a ratio of married women to married men of \( \geq 1.5 \) [hereafter defined as polygynous], while only 3 children died and 55 children survived in the groups with a ratio of \( < 1.5 \) [hereafter defined as monogamous] \( \chi^2 = 15.18, \text{d.f.} = 1, p = 0.0001 \). Because the relationship between polygyny of the work-eat group and child mortality was a step function, I modeled work-eat group polygyny as a categorical variable. Whether I used the polygyny index of the work-eat group for 1988 or for 1994 had no effect on
the conclusions. The results reported in this paper are for the average index in these two years.

The two best-fitting final models are shown in table 1 \((N = 176)\). Both models contain the polygyny index of the work-eat group \((0 = \text{monogamous, } 1 = \text{polygynous})\), age of the child at the outset of the study \((0–10 \text{ years})\), sex of the child \((0 = \text{female, } 1 = \text{male})\), and the economic rank of the child’s work-eat group \((15–59)\). In addition, model 1 contains the number of children \((\text{aged } \leq 10 \text{ years})\) in the child’s work-eat group, whereas model 2 contains the dependency ratio, number of children \((\text{aged } \leq 10 \text{ years})\) per married adults in the work-eat group. Because these two variables were correlated \((r = 0.33, p = 0.000)\), it was useful to consider them in separate models. In both models, work-eat group polygyny was the single strongest predictor of child mortality in terms of effect size. In model 1, the odds of death were 7 times higher in the polygynous groups than in the monogamous groups \((p = 0.005)\). In model 2, the odds of death were 11 times higher under polygyny \((p = 0.001)\). These effect sizes could reflect my choice of a married women-to-married men ratio of 1.5 as the cut point between monogamy and polygyny. This cut point was based on the data in figure 1.

Fig. 1. Percentage of children who died and polygyny index of the work-eat group. Numbers above bars indicate sample sizes. \(N_{\text{Total}} = 176\).

Table 1

Predictors of Child Mortality from 1986 to 1994 \((N = 176)\)

<table>
<thead>
<tr>
<th>Predictor</th>
<th>Model 1</th>
<th>Model 2</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Odds Ratio</td>
<td>95% Confidence Interval</td>
</tr>
<tr>
<td>Polygyny status of the work-eat group ((0 = \text{monogamous, } 1 = \text{polygynous}))</td>
<td>7.32***</td>
<td>1.82–29.42</td>
</tr>
<tr>
<td>Age of child ((0–10))</td>
<td>0.66****</td>
<td>0.55–0.81</td>
</tr>
<tr>
<td>Sex of child ((0 = \text{female, } 1 = \text{male}))</td>
<td>2.33*</td>
<td>1.02–6.39</td>
</tr>
<tr>
<td>Number of children in family ((1–19))</td>
<td>1.25***</td>
<td>1.08–1.44</td>
</tr>
<tr>
<td>Number of children per married adults ((0.33–2.33))</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Economic rank ((14–59))</td>
<td>0.43*</td>
<td>0.32–0.85</td>
</tr>
<tr>
<td>Economic rank squared()</td>
<td>–</td>
<td>–</td>
</tr>
</tbody>
</table>

*Monogamous, ratio of married women to married men <1.5; polygynous, ratio of married women to married men ≥1.5.

*Odds ratio for an increase of 1 year in age.

*Odds ratio for each additional child.

*Odds ratio for an increase of 10 ranks.

\(p < 0.05\).

\(p < 0.001\).

\(p < 0.0001\).

\(-2 \log \text{likelihood: 135.1 (model 1) and 132.8 (model 2)}\)

Goodness-of-fit statistic: 173.9 (model 1) and 148.6 (model 2)
nificance of all the other independent variables including polygyny. Moreover, the odds ratio for polygyny increased to 8.7 (model 1) and 13.8 (model 2).

To assess the goodness of fit of the final models, I compared the observed and predicted outcomes [lived or died] for each child. In model 1, 81.8% of all children were correctly predicted, and in model 2, 79.2% were correctly predicted. Plot of the squared studentized residuals against the predicted probabilities of death also indicated good model fit. In both models, only 4 of the 176 children had squared studentized residuals greater than 4, indicating that they were not well predicted by the models [see Hosmer and Lemeshow 1989:162–63]. All 4 were children from polygynous families who had high predicted probabilities of survival due to variables other than polygyny. Consistent with the polygyny-mortality hypothesis, these 4 children actually died. The leverage statistics for models 1 and 2 indicated that no observations had undue influence on model fit.

Several variables were tested for an effect on survivorship and found to be nonsignificant (p > 0.05). One of these was age of the father or mother. Neither the number of married men nor the number of married women in the work-eat group was significant, only the ratio of the two. Children who had more paternal siblings were not at greater risk of death; what mattered was the total number of children in the work-eat group. This result may reflect competition among children or increased exposure to infectious diseases such as measles and gastroenteritis [Aaby et al. 1984, Desai 1995]. Surprisingly, child mortality was not predicted by whether or not the father or mother was resident in the village. Nonresident fathers included young men from other villages who had fiancées and children in Sangu, deceased fathers, and fathers who had left temporarily to work in the city. Children whose fathers were not resident lived with relatives: usually mothers, grandparents, or paternal uncles. If the mother was deceased or had divorced and left the village, the child lived with the father or grandparents. A total of 114 of the 176 children in the study had both parents resident in the village. The fact that residence of the parents did not predict survivorship speaks to the strength of the extended family. Whereas the women had no schooling, some of the men had a few years of primary school education, but father’s education did not predict child mortality. Many of the men had previously worked in the city for a year or two, but these experiences also did not predict mortality.

In some cases a child’s parents were in a monogamous union but other adults in the work-eat group were polygynously married (or vice versa). This raised the question whether mother’s marital status or the polygyny index for the work-eat group as a whole was more important for survivorship. To address this question, I compared alternative multivariate models that differed only with respect to the polygyny variable [either mother’s marital status or work-eat group polygyny]. In these models work-eat group polygyny was more predictive of mortality than mother’s marital status. Specifically, after controlling for the other predictors of mortality in model 2, the odds of death for the children of polygynously married women were 3.6 times higher than for the children of sole wives, but this result was not significant (p = 0.10; N = 130). When work-eat group polygyny was substituted for mother’s marital status, the odds of death were 9.8 times higher under polygyny (p = 0.003; N = 130). Moreover, the model with work-eat group polygyny had a smaller −2 log-likelihood statistic (101.3 versus 109.9), indicating better model fit. To confirm the conclusion that work-eat group polygyny is critical, I deleted from the sample the children for whom mother’s marital status and work-eat group polygyny differed. As expected, the results for the remaining children were significant and identical for both mother’s marital status and work-eat group polygyny (odds ratio = 16.18; p = 0.009; N = 111).

The comparison of mother’s marital status and work-eat group polygyny is of particular interest because it shows the importance of family structure. When members of a patrilineal extended family are economically interdependent, children are affected by the polygyny status of their own parents as well as the polygyny status of their paternal relatives.

Resource dilution. To explain why child mortality was dramatically higher in polygynous work-eat groups after controlling for confounding variables, I tested the a priori hypothesis that polygynous groups were wealthier in terms of total resources but poorer on a per capita basis [see Chojackna 1980, Brabin 1984, Oyedeji 1984, Hames 1996]. I found a positive correlation between the wealth rank of the group and the ratio of married females to married males (r² = 0.28; N = 45; p = 0.0001) [fig. 2, left]. Work-eat groups with ≥ 1.5 females per males farmed more land (p = 0.001) and produced more grain (p = 0.009) and more onions (p = 0.002) than did families with < 1.5 females per males [fig. 2, right].

Next I standardized the wealth of each family by its daily energy requirements. I computed the energy requirements of each family from the number of individual members, adjusted for age and sex, using guidelines of the FAO/WHO [1973]. The standardized wealth of polygynous groups was still slightly higher than that of monogamous groups (r² = 0.09; N = 45; p = 0.03) [fig. 3, left] because polygynous groups had more revenues from onions (p = 0.01) [fig. 3, right]. On other measures of wealth, polygynous and monogamous families were comparable. These data contradict the hypothesis that the dilution of wealth accounts for the high mortality of children in polygynous families. The dependency ratio provides another estimate of the parental resources (e.g., wealth, time) available to children. As this variable increased by one additional child, the odds of death increased by a factor of 2.9 (p = 0.02). However, as
Fig. 2. Left, regression of the wealth rank of the work-eat group on the polygyny index (number of married women/number of married men) of the work-eat group. N = 44. Groups in which an elderly widow or widower worked alone or with a grandchild are excluded. Right, mean wealth (CFA or, for land, ha) of polygynous (≥1.5 married women/married men) and monogamous (<1.5 married women/married men) work-eat groups. Error bars are 95% confidence limits. □, livestock [p = 0.222]; ■, onions [p = 0.002]; ○, grain [p = 0.009]; ●, land [p = 0.001], ◊, commerce [p = 0.148]. In 1987, 310 CFA = $US1.

shown by model 2, mortality was much higher under polygyny even after controlling for the dependency ratio (table 1).

To find out whether children in polygynous families were less well nourished, I used cross-sectional anthropometric data on 77 children aged 6 years or younger in 1988. In particular, I compared their observed and expected values for weight/height. The expected weight/height for each girl or boy was obtained from quadratic regressions of weight/height against age. These equations are as follows: weight/height (Dogon girls) = 9.17 + Age(1.53) + Age2(−0.10) [r2 = 0.74; N = 34; p < 0.0001] and weight/height (Dogon boys) = 9.47 + Age(1.98) + Age2(−0.16) [r2 = 0.71; N = 43; p < 0.0001]. If the children in the polygynous families were leaner than those in the monogamous families, then they should have tended toward negative residuals [observed weight/height < expected weight/height] while the children in monoga-

Fig. 3. Left, regression of work-eat group wealth (CFA)/energy requirements (MJ) on the polygyny index. N = 45. Right, mean wealth (CFA or ha)/energy requirements (MJ) of polygynous and monogamous work-eat groups. Error bars are 95% confidence limits. □, livestock [p = 0.967]; ■, onions [p = 0.012]; ○, grain [p = 0.567]; ●, land [p = 0.314], ◊, commerce [p = 0.793].
Fig. 5. Percentage of observations of females and males engaged in child care at various ages, from behavioral scan sampling in the agricultural fields and village (Sangui). Stars indicate significant differences between the sexes.

Fig. 4. Regression of weight for height residuals on the polygyny index of the work-eat group. N = 74.

Fig. 5. Regression of weight for height residuals on the polygyny index of the work-eat group. N = 74.

Mous families should have tended toward positive residuals [observed weight/height > expected weight/height]. As shown in figure 4, there was a very weak tendency in the predicted direction that was not quite significant ($r^2 = 0.04; N = 74; p = 0.07$). Controlling for this weak relationship had no effect on the coefficient for polygyny in the subset of observations [$N = 74$] for which weight for height measurements were available. These results indicate that the mechanism causing low child survivorship in polygynous families was probably not nutritional.

Cowife competition. The indigenous Dogon explanation is that poor survivorship under polygyny reflects competition among cowives. Cowives are not related, and the rivalry among them extends to their sons, who, upon the death of their father, almost invariably stop farming together. In addition to accusations of neglect and mistreatment, it was widely assumed that cowives often fatally poisoned each other’s children. I witnessed special masked dance rituals intended by husbands to deter this behavior. Cowife aggression is extensively documented in Malian court cases with confessions and convictions for poisoning. These cases raise the possibility that Dogon sorcery might have a measurable demographic impact—a view that is consistent with the extraordinarily high mortality of males compared with females. Males are said to be the preferred targets because daughters marry out of the patrilineage whereas sons remain to compete for land. Even if women do not poison each other’s children, widespread belief in the hostility of the mother’s cowife must be a source of stress. Stressful family environments, including residence with a stepfather and half-siblings, have been shown to affect childhood cortisol levels and can lead to immunosuppression and a high frequency of illness (Flinn and England 1995). In the United States, the risk of fatal abuse is approximately 100 times greater for stepchildren than children living with two genetic parents (Daly and Wilson 1988:89). However, regardless of whether or not female-female competition results in neglect, immunosuppression, or poisoning, the focus on cowives alone is too narrow. If cowives adversely affected each other’s offspring, one would not expect work-eat group polygyny to be a better predictor of survivorship than mother’s marital status.

Paternal investment. An alternative hypothesis is that the children in polygynous families are the victims of lower paternal investment. The monogamous fathers have a greater stake in the survival of each of their children. The polygynous fathers eventually produce a greater number of offspring, so each child is less important for the father’s total lifetime reproductive success. Controlling for age, polygynists [aged 24–69] had on average two more living offspring for each additional wife ($r^2 = 0.25; N = 70; p = 0.0001$).

What aspect of paternal care is crucial for survivorship but lacking in the polygynous families? Behavioral scan data show that the men in both the polygynous and monogamous families do very little direct child care (fig. 5), so it is unlikely that the key difference is the amount of time spent with children. Given the prevalence of parasitic and infectious diseases, differential access to medical care could have a dramatic impact on mortality. If the children in the polygynous families are less likely to be given costly medicines and other Western or indigenous treatments for illness, then this could cause the striking decrement in survivorship observed in this study. This hypothesis needs to be tested by monitoring differences in morbidity and treatment under monogamy and polygyny.

If polygynous men invest less per child, where do they divert their efforts? Children with more paternal...
siblings were not at greater risk of death, so it does not appear that polygynous men are simply spreading their investment among a greater number of existing children. Instead they may be diverting resources to mating effort [and the prospect of producing future offspring] or even to somatic effort [buying meat or beer in the marketplace].

Nepotistic investment. Yet the paternal investment hypothesis can be criticized along the same lines as the cowife competition hypothesis. If polygynously married men invest less per child than monogamously married men, then it is not clear why work-eat group polygyny is a better predictor of child survivorship than parent's marital status. A potential solution is to broaden the paternal investment hypothesis to include the nepotistic investment of relatives other than fathers. Polygynous work-eat groups may be less inclined to pay for medicines and other treatments for childhood illnesses. Such groups have higher reproductive potential than monogamous groups and are more likely to produce a surplus of sons relative to the hectarage of land to be inherited. With each successive generation, the competition for land within the family becomes increasingly intense. Some of the sons emigrate; the other children may be less indulged than those in monogamous work-eat groups, even if their own father is monogamously married. The father himself is not necessarily the one who withholds investment; instead it may be the uncle or grandfather, whoever is work-eat group boss (gimt du bangal). Thus, to explain the high mortality rates of children in polygynous work-eat groups, further study is needed to identify possible differences in the spending patterns of the men and women of these groups compared with monogamous groups. Conflict of interest between work-eat group members is another area for inquiry. Such conflicts were frequently reported and led to a high frequency of work-eat group fission (Strassmann, unpublished data).

Why do women marry polygynously? In view of the high mortality of children in polygynous work-eat groups, it is surprising that Dogon women are willing to marry into such families. But there are not enough monogamous work-eat groups for all the women on the marriage market. The number of women exceeds the number of men on the marriage market for reasons which have been widely overlooked. First, Dogon wives are on average eight years younger than their husbands. When the age-structure of a population is pyramidal, a difference in mean age at marriage ensures that the cohort of women looking for husbands is larger than the cohort of men looking for wives, forcing some women into polygyny [fig. 6] (Dorjahn 1959, Pison 1985, Chisholm and Burbank 1991). Second, a tendency for men to marry at a later age or to die at a younger age produces a surplus of widows relative to widowers. If these widows generally remarry, this will also result in a surplus of women on the marriage market (Pison 1985). The remarriage of widows will promote polygyny regardless of the age-structure of the population. Third, it is well known that a female-biased sex ratio will promote polygyny [e.g., Dorjahn 1959]. Among the Dogon, the skew is caused by excess male mortality and the departure of males to the cities in search of wage labor. Finally, even those women who enter monogamous marriages cannot assume that their marriages will remain monogamous. In this study, the excess mortality under polygyny was greatest among the offspring of first wives, most of whom had not known that their marriages would become polygynous.

Most Dogon women are in a polygynous marriage at some time during their lives, and in preparation young girls are taught to sing, “I’m not afraid of my husband’s other wife.” But interviews of all the adult men (N = 71) and women (N = 113) in Sangui indicate that women do sometimes fight polygyny with some success. The primary strategy is divorce, which both sexes agree is predominantly female-initiated. In a sample of 88 divorces, the wife said that she had been the initiator in 95% of divorces, and the four most frequently cited reasons were (1) husband pursuing wage labor in the city (25%), (2) dislike of husband (18%), (3) dislike of cowife (10%), and (4) too many children died in that marriage (6%). Miscellaneous other reasons, each of which was less commonly cited than the above, accounted for 41% of cases. The absence of a decrease in “per capita” economic status in polygynous work-eat groups [fig. 3] provides further evidence for female resistance to polygyny. Dogon informants said that men cannot attract women into polygynous marriages unless they at least appear to have sufficient wealth.

CONCLUSION

The results reported here provide the strongest evidence to date for an adverse effect of polygyny on child mortal-
ity in a human population. The odds of death for Dogon children \( N = 176 \) in polygynous work-eat groups were 7 to 11 times higher than for children in monogamous groups. By contrast with previous studies, this conclusion is based on prospective rather than retrospective data. Moreover, other significant predictors of mortality were controlled, including age of the child, sex of the child, number of children in the work-eat group, dependency ratio of the work-eat group, and economic status. Postulated explanations for why polygyny is a risk factor focus on resource dilution, cowife competition, paternal investment, and nepotistic investment. The data did not support resource dilution, but further study is needed to discriminate among the other possibilities.

Becker’s economic model and the polygyny-threshold model view polygyny as the outcome of female preference. However, the eight-year difference in the age of spouses generates a surplus of Dogon women on the marriage market, forcing many women into polygyny. Dogon men who achieve polygyny gain reproductively while their wives lose, indicating that male preference is more likely to be the driving force behind this marriage system. The practical import of this study is that polygyny is a much neglected but crucial variable in prospective studies of child health.

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Bedouin Hand Harvesting of Wheat and Barley: Implications for Early Cultivation in Southwestern Asia¹

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The food-producing transition is increasingly seen not from the vantage of the Neolithic or as a progression from one evolutionary type to another but as a set of alternative foraging strategies with selective advantages and disadvantages that vary with the socioecological circumstances (Layton, Foley, and Williams 1991:255). This view focuses attention on the constraints placed upon foragers by different circumstances. We present data on one such constraint resulting from ethnographic study among the Bedouin of Jordan. At the time of the study (1986), many Bedouin cultivated wheat and barley using local seed stocks, ard tillage, and rainfall farming conditions. More interesting, they harvested their fields by hand, only rarely using sickles. We were therefore able to compare the cost of hand harvesting with that of harvesting with various types of sickles known from the past. Harvesting by hand proved less costly than the use of early sickles, and hand harvesting of cultivated cereals was similar in cost to the harvesting of wild cereals despite the investment in field preparation. These findings have implications for the recognition of food production in the archaeological record.

THE BEDUL BEDOUIN AND CEREAL CULTIVATION

The Bedul Bedouin are the traditional inhabitants of Petra, Jordan, a locale best-known as a major urban center during Nabataean-Roman times (fig. 1). During intermittent fieldwork from 1986 to 1994, the Petra Ethnoarchaeological Project focused on the ethnography, ethnohistory, and ethnoarchaeology of the Bedul Bedouin (Simms and Russell 1997). In the first years of our study, many Bedul retained a traditional life despite the tour-...

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¹ © by The Wenner-Gren Foundation for Anthropological Research. All rights reserved 0011-3204/97/3804-0011$1.00. Kenneth Russell passed away in the field in Jordan in 1992, when most of this work was in progress or rough-draft form. Continuation of the project is in his memory. Funding was provided by the L. S. B. Lea- key Foundation, a U.S. Information Agency Fellowship, the Ameri- can Center for Oriental Research, Amman, and Utah State University, Logan. We gratefully acknowledge the support of the Department of Antiquities of Jordan. We thank K. Renee Barlow and James F. O’Connell, University of Utah, for good comments, an anonymous referee for helpful perspective, and Marina Hall for editorial assistance. Article scope, emphasis, and any errors remain the responsibility of the senior author.

THE COSTS OF CULTIVATION

The wooden ards employed in southwestern Asia and throughout the arid and semiarid regions of the world...
Fig. 1. The study area, showing Bedul fields and sources of grain samples.
have changed little since prehistoric times [Avitsur 1965; Hopfen 1969:47–55; Russell 1988:38–40, 118; Varisco 1982; Watson 1979:74–75], and the ards used by the Bedul are no exception. [Palmer and Russell [1993] review the traditional ards used in Jordan.] Of course, the earliest tillage was by hand with the help of digging sticks, hoes, and other such implements. Russell (1988:109–34) compiled a detailed survey of ethnographic and historical accounts to compute the labor costs of cereal cultivation at various levels of technology. He found that hand tillage suitable for cultivating wheat and barley requires approximately 35 hours per dunam, an area of 1,000 m² or 2.25 acres [Russell 1988:115]. This value is employed here for modeling early cultivation employing hand tillage. Bedul informants commonly reported that one could till three dunams in an 8–10-hour day with a team of donkeys. This is equivalent to 2.7–3.3 hrs./du. These observations are comparable to data on ard tillage from other ethnographic and historic sources [Russell 1988:122–23, tables 25 and 27]. For complete field preparation using an ard, a value of 20 hrs./du is used in the calculations here [Russell 1988:124].

In May 1986 we obtained quantitative data on wheat and barley yields from Bedul fields. A representative square meter of the crop in a field was totally harvested, and both clean seed [hand-cleaned by the researchers] and straw/chaff yields were measured using hand-held scientific scales. In three instances, two samples were taken in order to quantify obvious yield differences in the same field as a result of microtopographic and ecological variations. The results are presented in table 1. The Bedul considered 1986 to be an average to good year, noting that 1983 had been exceptional. Wheat and barley yields and informant evaluations about the quality of the harvest among Bedouin farmers in the Negev desert in the late 1950s as reported by Mayerson [1960:18] are consistent with our findings for the Bedul. Additional consistent comparisons of the Bedul yields with others from the region can be found in Russell [1988:71, table 3; 112, table 16].

Comparisons of the nutritional composition and energetic value of cultivated wheat [Feldman 1976:121; Aykroyd and Doughty 1970:18] and wild einkorn collected in Turkey by Harlan [1967:198] suggest that the general values have not significantly changed as a result of domestication. The energy value of the wild einkorn harvested by Harlan was 3,567 kcal/kg. Modern red winter wheat analyzed at the same time was found to have an energy value of 3,474 kcal/kg. Modern wheat contains more carbohydrates but slightly less protein and fat than wild einkorn. Analyses of two wheat samples from Bedul fields produced an average of 3,511 kcal/kg. The average of two barley samples was 3,313 kcal/kg. The nutritional composition of the Bedul samples indicated a higher protein content than is typical of modern domesticated wheats. For the calculations here, we rounded the energy values to 3,500 kcal/kg for wheat and 3,300 kcal/kg for barley.

In May and June 1986, quantitative data on the labor costs of harvesting wheat and barley by hand were recorded in fields on the plateau south of the lower Wadi Beida drainage and in the southern Petra Valley. During this period, fields were being harvested by the Bedul in groups ranging in size from the few members of a single household to groups of over 20 individuals from related families. The actual harvesting was done by adult males and females, with children, adolescents, and the elderly assisting by carrying bundles of grain to larger piles.

To obtain quantitative data, a representative focal
Table 2
Return Rates for Bedul Males Hand Harvesting Wheat and Barley near Petra in May and June 1986

<table>
<thead>
<tr>
<th>Data</th>
<th>Subject</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Subject’s age</td>
<td>barley</td>
<td>45–48</td>
<td>45</td>
<td>20</td>
<td>10</td>
</tr>
<tr>
<td>Cereal</td>
<td>wheat</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Field location</td>
<td>S. Petra Valley</td>
<td>B5</td>
<td>L. Wadi Beida</td>
<td>W4</td>
<td>S. Petra Valley</td>
</tr>
<tr>
<td>Sample</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Yield [kg/du]</td>
<td></td>
<td>67</td>
<td>40</td>
<td>64</td>
<td>248</td>
</tr>
<tr>
<td>No. of m² timed</td>
<td></td>
<td>4.1</td>
<td>0.64</td>
<td>0.65</td>
<td>2.68</td>
</tr>
<tr>
<td>Avg. minutes/m²</td>
<td></td>
<td>3.66</td>
<td>3.74</td>
<td>5.94</td>
<td>5.58</td>
</tr>
<tr>
<td>Yield [kg/hr.]</td>
<td></td>
<td>12,078</td>
<td>13,090</td>
<td>20,790</td>
<td>19,530</td>
</tr>
<tr>
<td>Returns in kcal/hr.</td>
<td></td>
<td>10.7</td>
<td>10.8</td>
<td>44.7</td>
<td></td>
</tr>
<tr>
<td>Return rate [hrs./du]</td>
<td></td>
<td>18.3</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: Calculated for barley at 3,300 kcal and wheat at 3,500 kcal.

A person was chosen and timed by stopwatch, recording the time elapsed for each square meter harvested. The purpose and nature of the recording were not revealed to the focal person, and the recorder pretended to be observing another harvester. The measurement of the number of square meters covered by a harvester was done with a tape measure after the completion of the timed observation. In four cases, harvesting labor was determined in fields for which grain yields had been previously established. Returns are based on the yield of clean grain and exclude tilling, sowing, and processing costs. The results of these four cases are presented in table 2. [We made additional observations but exclude them here because they represent instances in which the timing was interrupted or the yields from the field under observation could not be obtained. These incomplete results did, however, fall within the ranges recorded during the complete experiments.]

Subjects 2 and 3 harvested from sandy soils, while subject 4 harvested from silty, clayey loam. The standard method did not work for subject 4, who had to jerk the stalks from the hard soil a few tillers at a time. The resulting differences in the labor required are readily apparent. Subjects 2 and 3 worked at comparable paces, resulting in expenditures of 10.7 and 10.8 hrs./du, even though the density of crops being harvested was 40 and 64 kg/du respectively. In contrast, subject 4 invested 44.7 hrs./du. In addition to occasional expletives, subject 4 repeatedly expressed his desire for a sickle.

The important and often misunderstood relationship between efficiency—a rate of return [e.g., energy/time]—and abundance [productivity or yield/area] is apparent here. Subject 4 was working in a field with an exceptionally high density of wheat because it was located in the trough of a shallow drainage where water accumulated. In this portion of the field, he was also working in very hard ground. His increased time per du-
### Table 3
Return Rates for Alternative Methods of Wheat Procurement

<table>
<thead>
<tr>
<th>Method</th>
<th>Cost (hrs./du)</th>
<th>Tilling</th>
<th>Sowing</th>
<th>Harvesting</th>
<th>Threshing</th>
<th>Milling</th>
<th>Return Rate (kcal/hr.)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Wild wheat</td>
<td>45.5&lt;sup&gt;a&lt;/sup&gt;</td>
<td>5</td>
<td>37.6</td>
<td>1,986</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cultivated wheat, hand-tilled and hand-harvested</td>
<td>10.8&lt;sup&gt;b&lt;/sup&gt;</td>
<td>5</td>
<td>37.6</td>
<td>1,815</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hand-tilled and harvested with early lithic sickles</td>
<td>34</td>
<td>5</td>
<td>37.6</td>
<td>1,463</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hand-tilled and harvested with advanced lithic sickles</td>
<td>20</td>
<td>5</td>
<td>37.6</td>
<td>1,870</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ard-tilled and harvested with early metal sickles</td>
<td>20</td>
<td>5</td>
<td>37.6</td>
<td>1,932</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Note:** Energy content of wheat = 3,500 kcal/kg. All calculations for methods involving cultivation assume a yield of 50 kg/du. Estimates for tilling, sowing, harvesting, and threshing from Russell (1988:113–24) unless otherwise noted.

<sup>a</sup>Wild wheat return rate of 1.1 kg/hr. (Harlan 1967) scaled to 50 kg/du for comparison with cases of cultivation.

<sup>b</sup>Wright (1994:246, fig. 4). We use an average of her samples 0.5–1.5, representing dehusking and groat preparation. These samples reflect the greatest degree of overlap in costs for this level of preparation, ranging from 0.5 to 1.3 hrs./kg.

<sup>c</sup>Hand harvesting cultivated wheat is from direct observation of Bedul.

### Metal sickles
This relationship is stronger in the case of wheat grown on sandy soils, with Bedul return rates of 10.7–10.8 hrs./du being significantly higher than for harvesting with all early sickles and competitive with harvesting with early iron sickles (Russell 1988:116–17). The return rate (energy/time) for the subject harvesting wheat growing in hard soil was similar to that for hand harvesting, but his productivity (energy/area) would have been improved with the use of a sickle (thus intensifying production). However, with the typically lower density of wheat in sandy soils a sickle would have been only a third to half as efficient as hand harvesting. Intensification increases productivity at the expense of efficiency by adding labor and, eventually, non-human energy to the system. This raises the question what selection pressures would lead to this seeming irony.

### Comparisons
A variety of available data can be combined with the findings reported here to compare the overall return rates for harvesting wild forms of wheat (Harlan 1967, O’Connell and Hawkes 1981, Russell 1988) with hand tillage and hand harvesting of domesticated wheat (this study), hand tillage and hand harvesting using early lithic sickles, and finally animal-traction tillage and advanced lithic/early metal sickles. Data on processing costs can also be included to calculate return rates for the entire cultivation and preparation process (O’Connell and Hawkes 1981, Wright 1994) (table 3). For wild wheat procurement, tilling and sowing are, of course, omitted. For cultivated wheat the aforementioned analyses of Russell (1988) identify the costs of hand and ard tillage, sowing, and threshing. Wright (1994) carefully details processing costs by examining the various forms of milling necessary to make cereals edible. She reports considerable variability in the time required to process groats, and therefore we use a suite of her samples (3–15) bridging the processing steps of dehusking and groat preparation, yielding an average of 0.9 hr/kg. We suggest that this value approximates the midpoint of what was likely a wide range of processing costs. Further, it compares well with the processing value of 1.0 hr/kg used by O’Connell and Hawkes (1981), which was based on direct observations of foragers grinding hard seeds. Our goal is not so much to find the “real” return for early cultivators as to identify potentially robust relationships.

The return rates for wild wheat (1,986 kcal/hr.) and hand-tilled and hand-harvested cultivated wheat (1,815 kcal/hr.) are similar. This counterintuitive similarity is probably due to the offsetting of wild wheat’s higher harvesting cost (because of its weak rachis and smaller seeds) by the absence of the need to expend energy in field preparation. At the same time, the costs of intensification associated with cultivation (tilling and sowing) are offset by characteristics of domestication including larger seeds, less loss of seed because of a tough rachis, and, under favorable circumstances, the increased density of plants resulting from field preparation. The employment of early lithic sickles reduces the return rate to 1,463 kcal/hr., and this loss is not overtaken until the advent of advanced sickles employing lithic inserts in the Pottery Neolithic period.

### Conclusions and Implications
*Hand harvesting versus harvesting with sickles.* Harvesting cereals by hand is the oldest and most enduring...
method throughout the Old World (Bohrer 1972), and experimental studies have shown that it is effective (Anderson 1991, Harlan 1967). The efficiency of hand harvesting compared with harvesting with early sickles suggests that there is no necessary link between the appearance of sickles in the archaeological record and the earliest cereal cultivation. Early sickles may have been used for purposes other than harvesting grasses/cereals (see Anderson 1991, Unger-Hamilton 1989, 1991). The advantages of sickles lay in opportunities for intensification rather than increased efficiency. Early cereal cultivation in southwestern Asia, North Africa, Central Asia, and Europe took place in the light, sandy soils (Sherratt 1980:315; 1983:98) that occurred in pockets in the Levantine forests and in larger expanses in surrounding areas. As cultivation expanded into areas of heavier soils during the Neolithic, the advantages of sickles increased. Nevertheless, Natufian-age sickles are known from areas characterized by sandy soils. If sickles were used here to harvest grass seed, their primary advantage at this early date would have been in extending the harvest. Microwear study (Unger-Hamilton 1989, 1991) indicates that Natufian sickles were employed on green wheat, which would have enabled the harvest to begin early. As the grain stands dried, they would have continued to be harvested at the higher return rate afforded by hand harvesting. Thus, greater productivity would have been obtained at the expense of efficiency. In sum, sickles indicate intensification, a very different behavioral pattern from initial cultivation.

Diet breadth, resource choice, and intensification. The proposition that diets expanded to include seed resources from the Upper Paleolithic through the Epipaleolithic in southwestern Asia is supported by the 19,000 b.p. dates for wild wheat and barley at Ohalo II (Kislev, Nadel, and Carmi 1992). Seed resources are relatively high in cost (O'Connell and Hawkes 1981, Simms 1987), and once diets began to expand to include high-cost resources a host of these expensive but storable resources became available. The similarity in return rates between wild wheat and hand-harvested cultivated wheat indicates that when the diet includes expensive resources, subtle variation in circumstances may favor one or another strategy. Perhaps this is why some researchers infer general foraging while others see specialization in nuts, acorns, marsh resources, or wild cereals (e.g., Henry 1989, Shipke 1989, Bar-Yosef and Belfer-Cohen 1992, Olszewski 1993, McCorriston 1994).

Conceptualizing the move to food production in terms of alternative adaptive strategies means that with adequate data on the return rates of the alternative resources available we should be able to predict which strategies would be favored under different circumstances. For example, consider the impact on Epipaleolithic foragers of different mobility patterns: Those with higher mobility would be expected to harvest wild wheat (or nuts, or acorns), perhaps store some, and move on. Less mobile foragers exploiting areas that include cereal patches on a recurrent basis would, through repeated use, begin the shift to cultivation and increased storage (or intensification of acorn collection). Selection for stronger territorial claims should increase with tethered seasonal rounds and repeated use of patches. The management of cereal patches by cultivation would be one expression of territoriality. Thus, the differences between Levantine forests, desert plains, and wetland locales are probably important to understanding why some people continued as foragers, some processed acorns, and others embarked upon a trajectory only retrospectively referred to as cereal “farming.” That such variability in seasonal round and tempo and mode of mobility existed even as early as the Epipaleolithic Geometric Kebaran complex seems well demonstrated in recent syntheses for the region (Henry 1989, 1993). Variability in adaptive strategies would be expected over time as well, and the conditions of selection may be especially dynamic during periods of rapid climatic change such as the Younger Dryas.

The whys of the food-producing transition revolve more around such questions as what would select for intensification focused on two cereal grasses rather than other resources such as acorns and what conditions would account for the cases in which agriculture was rejected or delayed. The move toward food production began with a broadening of the diet in the Upper Paleolithic and Epipaleolithic, opening a suite of forager strategies across space and through time. Given the similarity in return rates between wild wheat and hand-harvested cultivated wheat, early cultivation could have been one of these. The rapid appearance of sickles and “‘village’ trappings in the Early Natufian period marks intensification, and it seems that this too was spatiotemporally variable. One implication of a broadening diet is a decline in foraging efficiency. Hawkes and O’Connell (1992:64) observe that “where diet is broad and handling represents the bulk of foraging effort, improvements in handling efficiency would have large effects.” Thus, when foragers are exploiting seeds, whose high cost is largely due to harvesting and processing (O’Connell and Hawkes 1981, Simms 1987, Wright 1994), the development of techniques to reduce these costs would give them an advantage. By the Neolithic period, the improvements in sickle technology discussed here may have accomplished this, but this was long after first cultivation. As for understanding why some people continued as foragers, it is possible to expand upon Hawkes and O’Connell’s observation. Intensification applied to resources already in the diet (e.g., seeds) need not represent an increase in efficiency, at least initially. Rather, the effort directed toward intensification need be competitive only with the alternative of adding more expensive (lower-ranked) resources to the diet. Both strategies—early cultivation and foraging for expensive wild resources—would be expected to occur for a while.

At this point, these inferences can only be considered hypotheses, albeit with the prospect of testing. A fuller exploration will require data on the costs and benefits of various resources. Foraging theory (Stephens and Krebs...
1986) holds great promise for these issues but has not been fully exploited in southwestern Asia [but see Rus- sell 1988, Layton, Foley, and Williams 1991, Hawkes and O’Connell 1992, Winterhalder and Goland 1993]. In order to overcome the limitations of culture-historical and post-hoc explanation typical of the literature on the food-producing transition, theoretical models will be essential. There is a need for more data on the costs as- stitute. and post-hoc explanation typical of the literature on

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