SI for chapter 22.

SI 22.1. Hunter-gatherers and African environments. What about Miombo woodland?

I have described the Hadza habitat as rich. It has had about the density of large animals that would be expected from its 500 mm pa rainfall. Its high seasonality ensures that plants produce extensive underground storage organs, and well protected, long lasting fruit (Baobab, Grewia). Can we place Hadza country into the general picture of environmental variation among hunter-gatherers, especially in African environments?

Foley (1982) pointed out that the hunter gatherers that we know about in Africa are mostly from extreme environments: San foragers in near desert, and Pygmies in high rainfall dense lowland forest. We have no information on foragers living in the wide band of intermediate rainfall between 500 and 1500 mm pa. Foley suggested that this was because farmers and herders had taken the most productive habitat. He suggested that herders would have occupied the habitat most productive of large ungulate biomass, and excluded foragers from this habitat. We therefore may have observed foragers in habitats at the low ends of the distribution of prey abundance, and may therefore have under-estimated the importance of hunting and the wealth of habitats that foragers formerly occupied.

Using the Standard cross-cultural sample Porter & Marlowe (2006) reported that world wide, warm-climate foragers did not on average live in less productive habitats (lower net primary productivity "NPP") than farmers and herders. Porter & Marlowe's world wide warm climate foragers include Mbuti pygmies, several South American forest dwellers, and in savanna just 2 Australian groups and 3 African savanna populations. These foragers live under NPP that range from about 189 g/m2/yr) to 1730 g/m2/yr. The mean is similar to the mean for farmers. This world wide warm climate sample shows a normal distribution of rainfall. But the African foragers on record comply with the bimodal distribution of rainfall that Foley reports. Foley's observation about foragers remains true for Africa. They occupy the wettest, and the driest extremes and are missing from the mid-range climates. But in contrast to Foley's data, Porter & Marlowe found herders living in similar environments to dry climate foragers. Examples would be !Kung and Herero, Hadza and Datoga, and Akie and Maasai. The siting of rain gauges in high altitude locations favored by European colonists probably accounts for the difference

between early and recent measurements in herder habitat. If the herders are living in habitats of maximum large ungulate production, so are some of the dry country foragers.

A historical explanation for the difference between Foley's and Marlowe & Porter's data on herders is suggested by the surprising record for Hadza rainfall in the Standard cross-cultural sample (Porter & Marlowe 2006 table 1). Hadza are noted as living, not in 500 mm pa rainfall but at 1214 mm pa. This rainfall characterizes nearby administrative centers at much higher elevation. Rainfall is generally higher at higher elevations in east Africa (chapter 2). No records were available from the Eyasi basin when Murdock & White compiled the sample that Porter & Marlowe used. The same sampling problem may have affected Foley's herder sample. Foley's (1982) herders were a selection of the, at the time best described and mostly East African herder societies. Because Foley (1982) and Porter & Marlowe (2006) apparently reach contradictory conclusions about herders, I re-examined the Foley forager and herder rainfalls, using sources such as www.aridland.go.ke, a Kenyan government web site that deals with arid and semi-arid districts and includes district profiles with rainfall figures. Probably because early colonial settlers gravitated to the higher altitude, higher rainfall arable locations (some of which were dry season refuges for herders), and because records are now available for long runs from more remote stations, the updated figures are much lower than in Foley (1982: fig 4). The new figures conform closely to Porter & Marlowe's herder sample. An alternative historical explanation might be that herders have been displaced from their high rainfall dry season refuges in recent decades.

We now have information on more forest pygmy populations than when Foley wrote but we still have the gap he described. The lack of African forager data at rainfall between 500 and 1500 mm/yr is real. This intermediate rainfall range includes the well - forested Miombo woodland, and we have almost no information on African foragers in this habitat, a glaring gap in our knowledge. Did foragers ever live in this habitat? If so, what was it like for hunting and gathering?

Miombo, well frequented by Tsetse flies, is not noted as herder habitat (except where they can maintain a more open version by frequent burning). The predominant agriculture in Miombo is intermediate fallow slash and burn. Miombo is an extensive part of African savanna today and probably at many times in the past. It has higher rainfall, 700mm pa and more, similar to much of Tanzania's best farmland but it has poor soil, apparently due to the underlying rock.

Biomass of large herbivores increase with rainfall, and can be very high in lowland or montane forest (Coe et al. 1976, East et al. 1984). But East points out that above 800-1000 mm most of the additional biomass comes from Elephants and Buffalos, two very large and dangerous species. He shows that the density of other large mammals decreases beyond 800-1000 mm. McNaughton & Georgiadis (1986), although warning against the neglect of soil differences, and biased selection of study sites, appear to concur with the relative paucity of large (non-Buffalo) ungulates in the intermediate rainfall areas, the Miombo woodland. They also remark that with the exception of these

two, large game are poorly represented in lowland or montane forest. Smaller and more solitary mammals are more abundant in forest.

But plant foods are probably more important than fauna when we are thinking about sustainability of savanna forager populations. Underground storage organs USOs (and perhaps seeds with hard shells and a worthwhile store of nutrients) are abundant where the plants face a severe seasonal water shortage. They may be instrumental for feeding foragers through the long dry season. Some Miombo woodland includes areas of strong seasonality. Wickens(2008) remarks that Baobab is less often found associated with Miombo than it is with the lower rainfall areas. Peters (1987) shows distribution of some edible nut bearing trees (especially Porinari) that might repay careful mapping against Miombo tree species. Sclerocarya birrea is another important nut bearing tree (Marula nut). Was Miombo woodland a poorer environment for hunter-gatherers?

Laden & Wrangham (2005) quantify the long discussed (Headland 1987, Bailey et al. 1989) impression that plant foods of the kinds used by hunter-gatherers are less abundant in forest than in savanna. Laden & Wrangham report that forest environments carry about 100kg/km^2 of USO and a mere 24 edible species, which they contrast with their summary of Vincent's (1985) estimates of the enormous abundance of tubers (40,000kg/km^2) and their count of 80 edible species in savanna. Laden & Wrangham consider the forest estimate to be too large because it included forest edge. Laden (1992) had observed that USOs taken by Efe were all of genus Dioscorea and found in a very limited range of habitats. Yasuoka (2006) found Baka pygmies going on long safaris in which they subsisted successfully on Dioscorea but he later found that the productive patches used by these foragers were growing on deserted village sites. However, it seems clear that Pygmy occupation of forest substantially antedates agriculture (Mercader & Brooks 2001).

USOs may be scarce in the forests but there is little information from the intermediate rainfall areas. How does the density of USOs change as we move from wooded savanna to the higher rainfall Miombo woodland, and beyond 1000mm/yr of rain toward the 1500mm of the forests? The abundant literature on Miombo as a local resource is disappointing in method and results. Some give species lists but these include for example Adansonia as a source of fibers, and Grewia as used for bows, with no mention of their value as foods. Other than Hladik (1984) cited by Laden & Wrangham, the only quantitative source on USOs comes from research on Mole Rats, famed for the variety of their social systems. Sixteen studies assess USO by excavating .5 m square test pits around Mole Rat burrows but to a depth of only 20 cm. This is deep enough to find several of the tubers that Hadza use but not the abundant and bulky //ekwa (Vigna esculens) which was not notable in the species lists of the local resource reports. A useful compendium is Lovy, Skliba et al. (2012 table 4), who in their figure 2 report a principal components analysis of the food and climate measures. Their own data collection in habitats they describe as Miombo woodland with rainfall over 1000 mm/yr include one of the lowest, and the highest food biomass. At 362 g/m2 the highest is close to Vincent's //ekwa at 5.2 metric tons /ha but a fraction of Vincent's 63 metric tons per ha of the

shallow Shumuko (Vatovea pseudolablab). The mean for the Mole Rat sites with rainfall over 1000 mm/yr is 2370 kg/ha. About half Vincents //ekwa estimate.

The information that I have located on Miombo and the areas of intermediate rainfall do not justify the belief that this was a habitat of great opportunity for hunter-gatherers, or any richer than Hadza country. But the information also does not completely support the idea that it was a poor habitat for hunting and gathering. We need quantitative foraging data gathered in a range of Miombo woodland habitats with the help of competent foragers and knowledgeable botanists.

The question of the Miombo woodland remains. Peters (1981, 1987) has given us a list of plants used by contemporary inhabitants. But we know little to nothing about the economics of their use, their abundance, size, processing time and acquistion time, or patchiness. Patchiness and size and abundance may be indicated in botanical literature. Distance between food and dry season water is important in dry savanna but given the abundant streams described by the fieldworkers, likely to be unimportant in the forest environments. Miombo woodland at the wet end of its spectrum has abundant water, at the drier end of its spectrum, location and abundance of dry season water may have been important. Nutritional information is becoming more abundant as African food scientists look at more local foods (some references in chapter 2). One might get good approximations for acquisition and processing rates by studying country people (farmers or herders) who use these resources, although they may use them at times when farm food is scarce and not at times when the resource is most rewarding or least costly. Likewise, if anywhere in Miombo woodland people are hunting with traditional weapons (no guns) it might be possible to get more information on the methods and economics of hunting that could illuminate the possibilities for hunting and gathering in this habitat. Miombo woodland is an important habitat from the evolutionary or archaeological point of view. It covers huge expanses, and is the habitat of the "savanna" Chimpanzee populations that have been studied. It is also important because it stands between East African and south African savanna.

Zoologists have looked at the possible role of changing habitat in the areas now characterized by miombo in speciation and sub-speciation (Lorenzen et al 2009). Today Miombo woodland separates eastern African savanna from southern African savanna. In slightly drier times the two savanna areas may have been contiguous. In much drier times, what is today miombo may then have become a savanna refuge. If miombo is a poor habitat for hunter-gatherers, it may have acted as a barrier, generating separation of east and southern African populations, or mingling of them, depending on the changes in its distribution. Early migrations between south and east Africa, and the presence, absence, or extent of "Khoisan" people's in east Africa, may become easier to understand if we can learn more about the Miombo woodland as a habitat for hunters and gatherers.

SI 22.2. Men in competition, recent literature.

Recent and lively literature has discussed many aspects of sexual selection and the value of measurements such as OSR, standardized variance in RS, and adult sex ratio (Kokko & Jennions 2008, Kokko et al 2012, Liker et al. 2014, Szekeley et al. 2014, Schacht et al. 2014). Much of this literature is aimed at accounting for the amazing variety of mating systems found in the animal kingdom as a whole, or even more widely to account for mating systems in all organisms, and for the origins of anisogamy. When we study humans we may be allowed a few phylogenetic "givens". Women get pregnant and have long gestations. They tend to remain infertile for a time while suckling offspring. There is a period during which sexual activity will not produce conceptions. Males in contrast can father a child whenever a fertile female is available to them. A further constraint in the human life history has been attended to for example by Marlowe (2000), Marlowe & Berbesque (2012), and Coxworth et al (in press). The potentially fertile portion of the life span differs between human males and females. Women are fertile from about 15 to 45 or so. Men from a little later in adolescence until much later in middle and old age. The result is likely to be a very skewed Operational Sex Ratio.

Operational sex ratio (OSR) has been used in different ways, some of which may resemble an outcome of competition as much as a cause. For example, when I applied Clutton-Brock & Isvahan's (2007) 25% rule to Hadza data, the key age stratum becomes 25-55, the ages at which a quadratic fit to male fertility passes 25% of its peak. But this measure shows us the age of men who succeeded in competition, not the number who could gain by success in competition. We should not a priori ignore the vigorous and very eager 18-25 year old young men, nor the still vigorous and interested 55-65 year olds (waning prospects for most of the evaluating women, perhaps less impressive rivals for younger men). If the 25 year olds vanished, would the 20 year olds not try to jump in?

It is important to discriminate between a competitive situation, for instance where there is a higher ratio of fertile males to available conceptions, and the many and varied conceivable responses to the situation. Some early expectations have been overturned. Notable is the conclusion that where many males compete for few females, desertion by males exposes them to tougher competition than in a population with an excess of females. Liker et al (2014) list several studies which suggest that human populations conform to this expectation, much as does Schacht et al (2014). When fertile females are scarce it may pay males to remain with (guard) a single female more than it pays to attempt to mate with multiple females, who are sought by a number of unattached males who become more numerous the more males attempt the desertion strategy (Houston and McNamara 2002).

When the competitive situation is severe there may be a variety of results, and the competing sex may evolve a variety of individual strategies. The literature has shown that a competitive situation does not necessarily imply more desertion, poor treatment of females, more male-male fighting etc. Alternate strategies include floaters, sneakers, facultative growth (Orang and Squirrel monkey), and in the Hadza case as suggested in

chapters 15 and 21, expert hunters, ordinary Joes, charmers, wage earners. In other populations (with the "where have all the males gone" phenomenon), issues like migration to the city should be approached as a response to competition not as a mystery "given". But, if some young men decide to migrate to town and earn their fortune, their absence changes the situation for the men who stay at home, likely to be those who already inherited fields or herds.

Wood & Marlowe 2013 ("W&M") argued that Hadza men's hunting was as would be expected if men were optimally providing for their children, and did not provide support for the "show-off" idea. Hawkes et al. (2014) dispute this, arguing that when they substitute W&M figures into the analyses conducted in Hawkes, O'Connell & Blurton Jones (1991), the picture remains the same. This is quite surprising given the passage of some 20 years between the studies and the apparent reduction in abundance of large animals, and several apparent differences in method and circumstances. Even if small game have to be shared at the level W&M report, the figures from Hawkes small game hunting experiment still show that paternal investors should desert the hunting and sharing lifestyle to specialize in the pursuit of small game. Their children would eat more meat and eat meat more often. But Hadza do not do this, men continue to hunt large game, sometimes pass by small game, and if they shoot a small animal often eat it all for themselves out of camp. Only seldom are they seen bringing a small animal home. Wood & Marlowe 2013 Table S1 show 183 small game animals brought to camp in 2297 hunter-days of observation, one animal every 12.6 days. Hawkes' show-off proposition is an attempt to fill the gap, an attempt to suggest why Hadza persist in hunting very large animals. She suggests some social outcome is a place to look. Such social factors need not be nebulous, non-material, non-sociobiological factors.

Many evolutionary anthropologists appear to take reciprocal altruism (RA) (i.e. not generalized reciprocity) for granted, perhaps in a wider sense than that proposed by Trivers and extensively tested by biologists, economists and modelers. The adherent to broad RA would argue that the more successful hunter gets more frequent return gifts which can benefit his family, and thus the more he hunts the better his children fare. (This would not follow from generalized reciprocity). But the adherent supposes that reciprocal altruism can be maintained, and that there are not alternative strategies which would benefit the hunter's fitness more, even by giving greater benefit to his wife and children. The core of Hawkes et al. (1991 and 2001) argument is not the assumptions about tolerated theft. The core is the pay-off matrix (Hawkes et al.1991 table 4) that contrasts the food that reaches children of a big game hunter (BGH) in a camp of BGHs with the supply of food reaching a defaulter who intensively pursued small game. Given the data from follows of hunters, and experimental data on men paid to pursue small game, men who wish to maximize the flow of meat to their children should default from the BGH group and pursue small game.

SI 22.3. Hunter-gatherer "leisure".

The question of hunter-gatherer "leisure" is still with us. Bogin (2011) suggested that !Kung foraged so sparingly because they were too poorly nourished to raise the energy required to forage longer hours and more frequently. Had he had the privilege of visiting the !Kung in their foraging days he would have noticed the endurance and speed of both men and women, available at any moment (such as when a child goes missing, or when a long-tracked animal is finally close by). Hadza, though perhaps heavy enough to escape the worst labels of under-nutrition are small but likewise able to cover significant distances rather fast. Being small and slender may not be simply a signal of under-nutrition but of a compromise between numbers and size of offspring (Walker et al. 2008, Walker & Hamilton 2008). It can be advantageous for endurance in a hot climate.

Alternative reasons for !Kung sitting at camp have been suggested in the literature. The disincentive of widespread sharing has been considered at length (Sahlins 1974). The biological payoffs of the social tasks of "leisure" perhaps should not be underestimated. We have suggested that the Hadza "men's place" may not only be a mild signal of a wish for male dominance over women (Lars Smith's "feeble attempt at a men's resistance movement") but a mechanism for "mate guarding", by keeping observation and hearsay on the location of other men, potential rivals all. The Hadza men's place and associated ceremony and closely guarded secrets invites comparison with Rodseth's 2009 paper on social control of women and his 2012 paper on the "bachelor threat". The women's midday cook ups in the bush, and sewing groups in camp may perform not entirely dissimilar functions. Both could usefully be explored by a capable eavesdropping linguist.

SI 22.4. Postscript 1. Lewis et al (2014) on mobility and sharing.

At the time I submitted the manuscript for this book, 2nd December 2014, I had not seen Lewis et al. (2014), published two weeks later. Their paper suggests that mobility may have accounted for the wide-spread, non- reciprocated sharing commonly reported in hunter-gatherers. In my chapter 22 I strongly, though in the vaguest terms, also support this view. But it should be clear that I have in mind a different process from the one that Lewis et al model. I have not been able to see a model such as theirs, or my (Blurton Jones 1991: 181) model of a mixed population of sharers and scroungers, accounting for all the features of the Hadza sharing ethic.

SI 22.5. Postscript 2. Lusekelo (2014).

Lusekelo (2014) suggests that contrary to Marlowe (2002) and my (2015) claims, Hadza do slowly adopt agriculture. But the details of his account indicate that even in the 2010s (he does not give the date of his visit but it is before his conference presentation in February 2014) the actual number of Hadza farming, at Yaeda, Mongo wa Mono, and now Domanga are very few: "I managed to count only 6 families with 25 typical

Hadzabe at Yaeda Chini village" (Lusekelo 2014:90). While the numbers of Hadza growing food may vary with year to year variation in climate, it is evident that the majority of Hadza still display the resistance to settlement that Marlowe (2002) and I (2015, covering only the 1985-200 period) discussed. Lusekelo also cites an estimate of the size of the Hadza population that is not based on systematic census and far exceeds my figures and the recent thorough census by Brian Wood.

SI 22.6. Postscript 3: Skaanes 2015 Notes on Hadza cosmology. Epeme, objects and rituals. Hunter Gatherer Research 1:247-267.

In chapter 22 I exhort human behavioral ecologists to try to investigate cultural "superstructure" as a set of adaptations or adaptive propensities. Skaanes (2015) shows us just how difficult this may be!

During most of my fieldwork I felt there were topics that Hadza would not willingly or reliably discuss with us. I thought perhaps one day, when Hadza had become completely accustomed to researchers and their curiosity, or formed lasting personal relationships with a researcher, they might be prepared to talk about their conscious beliefs and cultural rules. Woodburn may have already accomplished this, Marlowe might in due course. This day appears to have arrived with Skaanes. As evident in her paper her informants appeared willing to ensure she understood properly and got things right. Quite an accomplishment for the fieldworker.

We should await completion of Skaanes' reports before we attempt to begin an adaptationist interpretation or offer questions and predictions. To my mind two descriptive possibilities stand out in her paper. First, that there appears to be an extensive religious or spiritual life of women that may differ from that of men. Power (2015) and previously has written on this topic. The evolutionary interests of men and women differ in many aspects of life, why not also in their religious lives? Second, that men's epeme feast and epeme status may fall squarely into the patterns of men's secret societies described for many cultures, and even to suggest a weak form of male gerontocracy, reportedly a clear feature of Australian aboriginal societies but not often discussed for African hunter-gatherers.