

Moving beyond Stereotypes of Men's Foraging Goals

Reply to Hawkes, O'Connell, and Coxworth

Michael Gurven and Kim Hill

Department of Anthropology, University of California, Santa Barbara, California 93106, U.S.A. (gurven@anth.ucsb.edu)/School of Human Evolution and Social Change, Arizona State University, P.O. Box 872402, Tempe, Arizona 85287, U.S.A.

We concur with the title of the Hawkes, O'Connell, and Coxworth discussion, "Family provisioning is not the only reason men hunt" (Hawkes et al. 2010). We said so explicitly in our paper, and we advocated a similar conclusion to theirs, which is that "explaining the variability should be the task at hand." We repeat that the major point of our essay was to highlight the complexity of evaluating why men hunt and share food and to suggest that multiple motivations transcend the dichotomy of provisioning versus signaling. Hawkes, on the other hand, has consistently argued for 20 years that hunted prey are nondefendable public goods whose acquisition can only be explained by showing off or costly signaling and that the sexual division of labor arises not from intrafamilial cooperation but instead as a consequence of male mating strategies (Hawkes 1990, 1991, 1992, 1993; Hawkes and Bliege Bird 2002; Hawkes et al. 1997, 2001*a*). She and her colleagues have never proposed that any other factor can account for men's subsistence decisions. They have explicitly argued that human pair bonding has little to do with spousal cooperation, where "subsistence benefits . . . [may be] absent altogether" (Hawkes et al. 2001*b*:695).

We instead suggested that the benefits of within- and between-family cooperation are critical to understanding why men hunt and share their kills and why wives and children encourage rather than discourage husbands and fathers to hunt. We also suggested that human societies are more complex than Hawkes et al. (2010) assume, with food sharing and divisions of labor as cultural institutions driven by social norms that promote cooperation. These features of human social life are integrated with the evolved human life history. While we see hunting as designed mainly to promote extended kin group welfare (with signaling as a portion of the benefit to hunters), direct "provisioning" of wife and children is far too narrow an interpretation of our position. Food is a currency that is used for multiple purposes, and these extend beyond familial "provisioning" and "signaling."

Hawkes et al. (2010) identify three main problems with our essay: (1) they argue that our use of caloric return rates

is misleading because payoffs can only be the portions of game consumed by family members; (2) they suggest our analyses of (Hadza) sharing contingency are invalid; and (3) they assert that we contradict ourselves by having "two minds" concerning the relative importance of provisioning versus signaling on men's foraging behavior.

We argued at length that using total return rates (not just portions kept within the family) is important because doled-out shares may not be lost value, just as insurance premiums or social security payments are not "lost value" from a paycheck. We also pointed out the weakness of only counting calories given the overwhelming nutritional evidence that energy content alone is not the only goal of human foragers. Hawkes et al. (2010) consistently deny that macronutrient content is relevant to the sexual division of labor. Additionally, because Hawkes et al. dismiss any familial benefits to sharing, they equate any high-variance strategy as inconsistent with intrafamilial cooperation. Figure 4 in our paper illustrates many ways in which the value of food given away can be recovered and how men targeting widely shared game may provide benefits to their families. It is premature to argue that these benefits, rarely measured in most studies, are only by-products of men hunting for other reasons. Hawkes et al. ignore this point, figure 4, and its pathways and instead focus on a limited version of only one pathway: dyadic reciprocity of contingent food quantities.

In their critique, it is not clear whether Hawkes et al. disagree with just our Hadza analysis or with all estimates of contingency. Despite the admittedly imperfect way in which contingency has been measured, significant positive relationships between giving and receiving have been found in at least eight populations (Gurven 2004); most of these, to our knowledge, are immune from the problems that affected the small, biased Hadza sample. Do Hawkes et al. believe that all of these independent estimates are invalid measures of reciprocal exchange?

Hawkes et al. (2010) focus primarily on our analysis of Hadza contingency even though this result was only a minor element of the larger body of evidence that supports our argument. We agree that observation bias is important to consider in these kinds of analyses, but they may be overestimating the extent of this bias. Their example only permits hunters to receive shares when present in camp, but no information is given about the availability of other family members of the hunter who could receive shares when the hunter was away. Also, observation "bias" not only is something to control for but it also likely reflects the desire for certain people to preferentially coreside and, hence, share food, together (Gurven et al. 2004). Even taking the observation bias into account, however, Hawkes et al. find a significant contingency correlation. There is no methodological justification to dismiss 2 of the 15 data points (13% of the data!) as "outliers" only because inclusion of these would show that sharing is contingent. Those two "outliers" were the most

frequently sampled hunters, and so they should be most robust to measurement error. The Hadza sharing sample is small and, at best, inconclusive; we find it ironic that Hawkes et al. (2010) criticize our analysis of their small, biased data set—but don't explain how they used the same data to conclude that Hadza sharing was *not* contingent (without ever attempting the type of dyadic analysis that we presented and they now refute).

Regarding the third problem, it was our goal to highlight evidence that is inconsistent with the recent trend of viewing hunter-gatherer men as hunting primarily or exclusively in order to signal phenotypic quality. Critiquing the “signaling only” hypothesis for hunting is not the same as being an advocate of a “provisioning only” model. Half of our paper was devoted to (re)developing a framework for examining trade-offs to help explain variation in male behavior—to combine conflictive and cooperative interests!

We are disappointed that Hawkes et al. ignore many of our arguments and much of our evidence and instead focus their attention on a few points to support their view that provisioning is only a by-product of signal-seeking goals. While they claim to emphasize the goal of explaining “variation in men's hunting goals among and within ethnographic cases,” their strong statements against the possibility of provisioning clearly contradict this: “preferential attention is the selective incentive motivating hunters . . . men's work evolved and often continues to be shaped by showing off” (Hawkes and Bliege Bird 2002), “paternal provisioning is actually not practiced among the best-documented low-latitude foraging populations” (Bird and O'Connell 2006, cited in Hames's comment in Gurven and Hill 2009:62). Hawkes and colleagues continue to ignore the larger suite of psychological, behavioral, economic, nutritional, hormonal, and ethnographic evidence that supports the important roles of provisioning and familial and extrafamilial cooperation. It is also a disservice to not address published evidence among the Hadza that contradicts their position; for example, reestimates showing low caloric value of Hadza roots would underestimate women's economic contributions to the family (Schoeninger et al. 2001); experiments showing that men do not rank signaling their hunting prestige above their families eating well highlight preferential concern for familial welfare (Wood 2006); evidence of ample small-game hunting, even in the past (McDowell 1981) suggests that referring to the Hadza men as exclusively big-game hunters may be a misrepresentation; evidence from other researchers shows that paternal care is integral to Hadza marriages (Marlowe 1999, 2003, 2005).¹

The debates over men's foraging goals spawned an exciting and contentious literature over the past 2 decades that has the unfortunate consequence that many other interesting fea-

tures of food acquisition, social behavior, grouping patterns, and life history remain understudied. We outlined these at length in our paper and in our reply to the commentaries, including complex divisions of labor, strategic use of sharing as social insurance, and group augmentation. We also advocated for better studies of multicurrency contingency and for theoretical inquiry to explain how sharing norms, ownership rights, production schemes, and intergenerational nutrient flows might work in human populations (Gurven 2006; Hill 2002; Kaplan and Gurven 2005). Neither costly signaling nor dyadic reciprocity can adequately explain the appearance and character of these cultural traits. In our opinion, the evolutionary cultural ecology of human cooperation is still in its infancy.

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1. In the Mangola area from 1979 to 1980, McDowell (1981) reports 145 small game (birds, hyrax, small antelope) in a sample of 574 person-days. He writes: “Hadza do eat some meat about 3–6 days per week but it is usually from small animals, and only occasionally available in the huge quantities provided by big game kills” (p. 14).

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