period. The Ache case is rare in that all adult men always worked long hours on observed foraging trips. Thus we do not know whether those who failed to hunt would still receive shares. In many other societies (e.g., the Hiwi and the Yora) there is significant variability in time allocated to resource acquisition. Comparative data on food sharing under conditions of highly variable work effort should be especially productive. In any case, until we can establish criteria and methods for analyzing contingency in food sharing, this debate cannot be settled empirically.

Unfortunately, existing theories suggest that the ecological conditions favoring tolerated theft (Blurton Jones 1984, 1987) and risk-reduction reciprocity (Kaplan and Hill 1985a) are the same: nonsynchronized variation in food acquisition. Thus, research focusing on the ecological factors that predict food sharing may not usefully distinguish between the two theories. Tolerated theft does appear to make some unique predictions about the occurrence of food sharing: {1} foods should not be shared on the basis of foods given in the past or expected in the future; {2} other things being equal, differences in power (both physical and social) should predict shares received; {3} other things being equal, differences in the value of resources to competitors should predict the share each receives (although this may be true in reciprocal models as well); and {4} when differences in power and resource value are difficult to determine (e.g., same age, sex, and body size), we should find increased levels of display and increased probability of agonistic interactions. None of these propositions have been tested explicitly.

Hawkes's model and alternatives based on nepotism and reciprocity also have some clear implications for the study of food consumption patterns in children. First, we need to determine how children obtain the food they eat. In the Ache study, we examined only the relationship between the original acquirer of foods and the consumers of those foods. We did not analyze the initial and secondary distributions. We need to know who are the primary recipients of food when they are first shared out and how those primary distributions are associated with secondary distributions and, ultimately, food consumption. In what frequencies do children of different ages and sexes receive food from its primary acquirers, from their mothers, from their fathers, and from other nonacquirers? To what extent are these distribution patterns a function of parental production? According to Hawkes's model, children should receive most food directly from distributions to themselves or to their mothers. They should receive food passed through their fathers’ hands no more frequently than do the children of other men. If, in contrast, a central motivation of men’s food acquisition is the provisioning of children, we should see a strong bias in secondary distributions favoring own children of men. Second, Hawkes’s model predicts that the children of high-producers of widely shared foods should not be nutritionally better-off than those of low-producers. We need to focus research on age- and sex-specific food consumption, nutritional status, and health of children as a function of parental food production. Specifically, we need to determine whether the children of high-producers of widely shared foods eat more and are larger and healthier than those of low-producers. In addition, since individuals with more food can produce either higher-quality children or more children than those with less food, we will also have to focus on the reproductive consequences of food production. However, since Hawkes’s model also predicts that high-producers should experience reproductive rewards from the attention they receive, we will need methods that can distinguish between the reproductive consequences of increased energy and rewards based on shared food, on the one hand, and those that result from increased attention, on the other.

Available data on the nonfood rewards associated with food sharing are very weak. In the Ache case, we do not know if the higher reproductive success of good hunters is a spurious correlation due to individual characteristics that affect both hunting ability and reproductive performance, the result of trade of food for other rewards, or simply the result of increased attention from potential recipients of food. Nor do we know whether high-producing men can exert leverage over others by threatening to withhold food or move elsewhere (which is effectively the same). The correlations reported so far need to be replaced by tests of specific causal models.

In this article Hawkes performs the valuable service of reminding us that the final answers are not in regard to the causes and consequences of variable food acquisition and food sharing patterns. We hope that this critique of her discussion will serve to stimulate the empirical work necessary to move beyond scenario building to accumulated knowledge.

Reply

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My argument is this: Some food resources, notably large animals when they are unpredictably acquired, are too expensive to defend. Others can claim shares of them without investing in capture and without repaying shares of the same foods later. These resources are like public goods. Hunters (and their own families) will often have more to eat if they take shares from others while devoting their own foraging efforts to foods less open to outside claim. If so, self-interested foragers will supply the widely shared foods only when they get something in addition to their own shares and when they value that something more than feeding their own families. If other people pay special attention to those who supply public goods and this attention gives them social benefits, particularly mating advantages, then foragers who value these benefits enough will supply the public
goods. By this argument, the reason men often hunt large animals is not to feed their families but to gain the social benefits. They hunt more often than women do because for them these benefits more often outweigh the benefits of family provisioning.

Hill and Kaplan have made key contributions in developing and testing hypotheses about benefits other than family nutrition that hunters gain for the resources they supply [Kaplan and Hill 1985a, b]. Nevertheless, they are skeptical about this public-goods argument. They contend that reciprocal altruism [Trivers 1971], whereby pairs of individuals trade goods and services for mutual long-term benefit, may account for the sharing patterns, differences in sharing by resource type and gender differences in resource choice being better explained by other factors. The public-goods argument follows their lead in emphasizing the importance of mating incentives to hunters, but hunters get those benefits in different ways in the two models. A central difference is this: the trade argument assumes that the portions of widely shared foods that others claim are under the control of the hunter, who can negotiate repayment in some appropriate good or service with each recipient. The public-goods argument assumes that such control is too expensive; hunters get mating benefits not from a series of private deals but from the preferential attention others pay them as the source of food they expect to share in.

Two things distinguish reciprocal altruism from other ways in which individuals gain net benefits from social strategies. First, since there is a delay between the transfer of a benefit and its repayment, “cheating” is possible. Individuals can accept a benefit and then fail to reciprocate. Second, reciprocators can punish cheaters by refusing subsequent transfers. I argued that the absence of such retaliation against those who fail to provide widely shared foods showed that the sharing of those resources did not fit a model of reciprocal altruism. My emphasis on these diagnostic features is also the basis for disputing Hill and Kaplan’s observation that reciprocal altruism “is clearly the basis of modern economic phenomena.” If, as they note, state enforcement adjusts individual payoffs so that “cheaters” gain no short-term benefit, something besides reciprocal altruism provides incentives for cooperation. In addition, money is a standard that allows the immediate conversion of goods otherwise less readily commensurable and an immediate guarantee that prevents cheating. This obviates the danger of “defection” inherent in the delayed returns of reciprocal altruism and makes possible exchanges with strangers.

Noting the limited quantitative data on sharing aside from the Ache, Hill and Kaplan question the basis for my assertion that while Hadza hunters vary enormously in their contribution to collective meat consumption “there is no comparable, let alone correlated, variance in consumption.” I reported the wide range in daily acquisition rates for 13 Hadza hunters observed for at least 20 days during 130 observation days in the late dry season and “short rains” of 1985 and the wet season and early dry of 1986. [Four of the hunters got nothing; the range for the remaining 9 was from 0.008 large animals/day and 0.315 kg/day to 0.098 animals/day and 20.59 kg/day—about three orders of magnitude.] The most accurate measure we have of individual food consumption is the anthropometry [though it is an imperfect measure of meat consumption, because people eat more than the meat of large animals]. During this fieldwork we weighed subjects periodically, allowing calculation of weight changes over the days of observation—from late dry into the short rains [October to December] and from late wet to early dry [March to June]. My assertion rested on the absence of a relationship between the hunting success rates of the 13 hunters and changes in their body weights.

The smaller the sample, the louder the noise. Measurement error and “random variation”—a bout of malaria in one hunter, time spent visiting harvesting farmers by another—can easily swamp the effects of variables of interest. The absence of evidence of a relationship is not evidence of its absence. In response to Hill and Kaplan’s request for supporting data, I calculated in addition to the weight changes for each man the weight changes for the same periods for his wife and the means for the children in his household. A hunter’s weight change is not related to his mean daily prey capture rate [large animals/day: \( r = 0.035, p = 0.9287 \) or his mean daily meat acquisition rate [kg of meat/day: \( r = 0.304, p = 0.3931 \)]. However, his wife’s weight change [while hardly comparable in magnitude] is correlated with his prey capture rate [large animals/day: \( r = 0.67, p = 0.0485 \)]. [For kg of meat/day the correlation is not significant: \( r = 0.552, p = 0.1257 \).] His children’s mean weight change is even more strongly related to his prey capture rate \( (r = 0.869, p = 0.0023) \) and his meat acquisition rate as well \( (r = 0.834, p = 0.0052) \). These correlations make it less likely that the absence of a correlation for the men themselves is due simply to measurement error or small-sample effects.

The patterns falsify my more general inference that there is no differential consumption among group members. The most obvious interpretation is that even though most of these very large animals are eaten by others, hunters keep a special share for their families and may succeed in claiming larger shares from others [although the lack of a relationship between a hunter’s own weight changes and his success seems inconsistent with this]. Alternatively, the children of good hunters may be given better treatment by group members generally, as is assumed in the showoff model [Hawkes 1990], or the differential may be due to different amounts of time spent living in the same camp with better hunters—a possibility that seems inconsistent with the pattern for the men themselves. Because Hadza children forage for much of their own food [Blurton Jones, Hawkes, and O’Connell 1989, Hawkes, O’Connell, and Blurton Jones n.d.], it may also be that the children of better hunters tend to be better foragers themselves. Fuller exploration requires the analysis of additional data. The familiar lesson is the fallibility of ethnographers’
impressions—an endorsement of Hill and Kaplan’s emphasis on the importance of quantitative data.

The most important question the patterns raise is this: What becomes of the general argument about public goods if hunters’ families do keep a special share? Cases in which all those in the neighborhood get the same shares of a food whether they have hunted it or not provide the simplest illustration of how such resources are like public goods. When a forager and his family eat all he acquires, the resource is unambiguously a private good. If these are two extremes, what of all the cases in between? There are at least two possibilities. One is that each unit shared out is being exchanged for other consumables by the acquirer. This is the trade argument. It counts all resources, once harvested, as the private goods of the acquirer. Portions go to others for favors returned in an array of two-person private exchanges, and the acquirer turns all of his harvest into goods for his own [or his family’s] consumption—the units that he doesn’t eat are like Hill and Kaplan’s example of the color television sets that the manufacturer doesn’t watch. Another possibility is that acquirers cannot afford to exercise this control. The argument that large animals are like public goods rests on the proposition that shares are successfully claimed on grounds of current “need” rather than on grounds of past or future sharing [Blurton Jones 1984, 1987]. The cost to hunters of trying to keep “too much” or of trying to adjust the size and direction of shares for their own private exchanges is too high to pay. Hill and Kaplan dispute my argument that this is commonplace. They note that anecdotes about claims prosecuted on the basis of “need” can be readily matched with anecdotes about exclusions from distributions as a penalty for past stinginess. As they say, this is not an issue to be resolved by anecdotes, but the common talk about penalties for noncontributors often includes a denial of those very penalties. The comment from Lee that Hill and Kaplan cite is an example (“total or near total distribution of meat . . . draws the good feelings of his neighbors, feelings that later make him a welcome guest at the neighbors’ distributions”). If shares are distributed to all, then those who have not provided any in the past are likely recipients. Welcome or not, they still receive portions. Another comment from Lee about meat sharing is that “news of a large kill at any nearby waterhole is sufficient to bring up to 50 people for an overnight stay” (1979:261). This exemplifies the constraints assumed by the public-goods argument: the large size of big-game animals limits the control anyone can afford to impose on their distribution.

An even simpler version of the game I used to illustrate the public-goods problem will show that it arises even when acquirers keep larger shares if they do not control a substantial fraction of the distribution. When foragers choose their targets from two kinds of resources, some shared widely and others shared little, the choice that will maximize their [or their families’] consumption depends on which of the following alternatives is larger: [1] the share of the “public good” they could control if they acquired it or [2] the amount of the other foods they could get instead. A difference between what a forager can expect to acquire and what he can expect to control is of primary importance.

The magnitude of this difference is the central issue. Are the empirical patterns consistent with the hypothesis that hunters control all or most of their catches and so negotiate private exchanges for other goods and services? If so, trade could be a “simpler” explanation of why better hunters make disproportionate food contribution to others: they exchange each share of meat that they don’t keep for something else. The larger the prey, the more problematic this hypothesis. A hunter can’t get kudu meat to “trade” for sexual favors without taking the whole kudu. If, when resources come in large packages, many get portions whether they give sexual favors or not, the reason to trade for shares disappears. The trade hypothesis draws attention to benefits other than food that hunters get, but in addition to the problems just mentioned it leaves two other aspects of the larger pattern that are addressed in the public-goods model—the gender differences in resource choice and the differences in sharing by resource type—to be explained in other ways.

My argument is that there are some resources over which foragers can exercise little control, and they know it in advance of their foraging decisions. The knowledge affects resource choice. This can only be so if sharing varies predictably with resource type, a pattern noted qualitatively in many ethnographic accounts and demonstrated quantitatively for the Ache in the forest [Kaplan and Hill 1985a]. Among the Ache, game animals, taken only by men, are most widely shared. The sharing of nongame resources, taken by both men and women, varies considerably. The analysis in Hawkes [1991] shows that while the sharing of these nongame foods differs consistently with resource type, it does not differ with the sex of the acquirer. [It also shows that even for gathered resources men preferentially target those more widely shared.] The findings are consistent with the argument that something about the resource determines how much a forager can keep. These data do not include observations on sharing at the agricultural colony. Differences in sharing by context such as Hill and Kaplan note for the Yora and the Ache would not be inconsistent with my argument. Where spatial distances and permanent structures increase “privacy,” they limit challenges over resources and so reduce the cost of holding more than others have. That the Ache share more in the forest than at the mission agricultural settlement is consistent with the immediate proximity of family hearths and the lack of walls in the forest versus the greater spatial dispersion and permanent structures at the settlement.

Trivers [1971] suggested that the evolution of reciprocal altruism would require the same individuals’ interacting repeatedly for long periods of time with substantial gains to be had from cooperation. Small human communities in which people are intimately familiar with the neighbors they see daily fulfill these require-
ments. But Trivers is also responsible for focusing attention on conflicts of interest between mates [1972] and among the closest kin [1974]. The very contexts in which the requirements for reciprocal altruism are best met are also contexts in which struggles over short-term self-interest can be especially visible. Partly because the costs of noncooperation are high, the opportunity for manipulation is high as well.

We all expect to interact continuously “henceforth” with members of our own nuclear families. We may also talk to each other of the virtues of generosity and of debt and obligation and even threaten penalties for stinginess. Yet domestic activities can be under daily negotiation. To the extent that family contributions are not directly commensurable [making dinner every day, mowing the lawn once a week] trade may not capture the motivating payoffs. Even when possible long-term “gains from trade” are high, the allure of short-term benefits may interfere. If only parents and children would reciprocally “give a bit” and split the cost when their interests diverged, they could reap the gains from trade by obviating “wasteful conflict.” But even between parents and offspring we expect attempts at manipulation with signals adjusted to serve the interests of the signaler and signal reading adjusted to serve the receiver. These homely observations cannot pass for serious empirical evidence, but, as Hill and Kaplan note, we are moved to pursue the questions that we think need answers.

The public-goods argument links resource characteristics to sharing patterns and these to gender differences in foraging strategies. Thus it connects environmental characteristics and their variation with social strategies and their variation, both between and within communities. Whether there is such a forest must depend on the empirical status of the trees. In this critique, as in their work generally, Hill and Kaplan do exactly what is necessary to advance the inquiry. In the careful attention they give to formulating alternative hypotheses and collecting the data to test them they set the pace for us all.

References Cited


—n.d. Hadza children’s work. MS.


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**On Population, Foraging Efficiency, and Plant Domestication**

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In a recent paper on resource husbandry and the origins of agriculture, Layton, Foley, and Williams (CA 32:255–47) present a wide-ranging attempt to break free of models that presume an evolutionary progression. They focus on the material and political conditions that would have accompanied adaptive shifts among several subsistence strategies (especially pastoralism, foraging, and agriculture). They use the diet-breath model to make predictions about the transition from foraging to plant cultivation, and they suggest that if—for any of a variety of reasons—the hunter-gatherers’ preferred [high-ranking] foods diminish in abundance, then they should add foods of lower rank, foods previously not worth harvesting, to their diet. Ethnographic evidence on resource exploitation suggests that these typically are plant foods (e.g., seeds, tubers), which can be quite plentiful despite their low (net) return for the effort invested in harvesting and processing them.

Layton et al. suggest the possibility that increased mobility in search of lower-ranked resources added to the diet “should . . . lower the density of the hunter-gatherer population” [p. 256], but they focus on an alternative pathway in which “certain low-ranking food resources, if husbanded, may contribute to population increase” [p. 260]. They go on to suggest a mechanism for this predicted effect. In a favorable environment, stands of seed-bearing grasses and forbs might be sufficiently abundant and predictable to be defended [see Dyson-Hudson and Smith 1978]. The possibility of reliable yield and exclusive use would lead to sedentism, husbandry to increase yield, and a diet increasingly rich in starch. By analogy with changes observed among the !Kung San ([Lee 1980]), the reduced nomadism and altered diet would produce population growth. This causal sequence makes “population growth . . . a consequence rather than a cause of intensive husbandry” (p. 262).

In a reply, Hawkes and O’Connell [CA 33:63–66] endorse the application of foraging models to analyses of agricultural origins, but they question several of Layton et al.’s conclusions on technical grounds and develop several additional insights from the models [see also Winterhalder and Goland n.d.]. We will focus on one of their two points about population: “Layton et al. argue . . . that greater use of low-ranked but relatively abundant resources toward the end of the Pleistocene promoted population growth” (p. 64). According to Hawkes and O’Connell, this prediction is inconsistent with the assumptions and models of foraging theory: “Increases in diet breadth result from reduced foraging return rates and so lead to declines in population growth rates” (p. 64). In their follow-up, Layton and Foley (33:219) abandon their original hypothesis by noting their agreement with this assertion.

Layton et al. predict “population increase” and Hawkes and O’Connell “declines in population growth rates.” Although both sets of researchers treat these as necessarily contradictory possibilities, they of course are not: a population can grow even as its rate of increase diminishes. Only if the growth rate becomes negative will the population decrease in size. Further, because of the possibility of dispersal, out-migration, or increasing range size, neither growth nor growth rate implies increasing density, the measure of importance to the ecological models cited in the two papers. Because it seems apparent in context that both sets of researchers mean to refer to an increase or decrease in population density, we will use this measure in the discussion to follow.

We will use a population ecology model to demonstrate that both sets of predictions need amendment. First, contrary to Hawkes and O’Connell, we show that a decline in foraging efficiency associated with expanding diet breadth may produce either an increase or a decrease [or conceivably no change at all] in the density of the foraging population. Second, unlike Layton et al., we show that changes in hunter-gatherer density do not necessarily require changes in mobility or territoriality. Such changes in forager population density can result solely from the characteristics of the newly adopted resources.

The diet-breath model used by both sets of researchers has no population component. By contrast, our approach subsumes the diet-breath formula within a simulation that incorporates population models for foragers.