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Optimal Foraging Models and the Case of the !Kung

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We concur with Sih and Milton that optimal foraging models "promise to be capable of providing valuable insights into the study of human foraging." Two important questions are raised by their comments: (1) How are optimal foraging models most usefully employed; and (2) Did we make a valid point about the !Kung? We address the general point first.

About Foraging Models

In our view, contrary to that of Sih and Milton, foraging models are not suited to describe the interaction of all, or even a large number of the variables that might affect subsistence-related behavior. They are intended to simplify the complexity of the real world for purposes of analysis. They help one specify a set of hypotheses about the factors that most significantly shape subsistence behavior. These hypotheses lead to predictions that can be tested against real data. If the data are inconsistent with predictions, one or more of the hypotheses is wrong and must be revised. The nature of the mismatch between predicted and observed behavior should guide this revision. To interpret it efficiently, one must keep the number of variables under simultaneous consideration to a minimum.

This is one of the reasons we used energy as the measure of benefit. As Sih and Milton point out, other qualities of food are also important. Nevertheless, attempting to incorporate such potentially complicating factors at the beginning of analysis invites confusion, not clarification. Almost all successful (that is, informative) applications of optimal foraging models, including those cited favorably by Sih and Milton (e.g., Belovsky 1978, 1980) employ the "piecemeal" approach (Krebs, Stephens, and Sutherland 1983), initially identifying a few variables as critical, and adding others only as necessary to improve the fit between predicted and observed behavior. The alternate approach, beginning with large arrays of nutrients, involves complex models, which demand unrealistically precise estimates of nutritional requirements (e.g., Keene 1982). Predictions are very sensitive to small changes in these estimates, so that many fragile hypotheses come under simultaneous test, making it virtually impossible to tell which are at fault (Smith 1983; O'Connell and Hawkes 1984).

Sih and Milton are mistaken when they say that it is a "major prediction" of optimal foraging theory that food value be measured in calories (Charnov and Orians 1973; Krebs et al. 1983; Smith 1983). Still, there are good reasons for preferring energy in the early stages of analysis (Smith 1979, 1983; Schoener 1971; Winterhalder 1981a), especially over more complex measures of food value, which reduce the comparability of different applications and narrow the generality of any conclusions. Because nutritional needs are so poorly understood, decisions about which nutrients to include in an analysis and assessments of the implications of acquiring various fractions of each often lack empirical or theoretical justification. Moreover, complex measures require complex models that have additional drawbacks (see Krebs et al. 1983; O'Connell and Hawkes 1984; Smith 1983 for further discussion).

Sih and Milton also find unrealistic the assumption that human foragers know or can accurately estimate returns available from alternative resources, though they are apparently prepared to accept it for other organisms. We find this analytic simplification *least* unrealistic for human foragers, whose intimate familiarity with their local environments has long been noted. Modeling the cost of acquiring this information is, as Sih and Milton say, an area of current interest. Constraints other than the need to acquire information, including predator avoidance (e.g., Milinski and Heller 1978; Sih 1980), territorial defense (e.g., Davies and Houston 1981; Martindale 1982), and minimizing the risk of starvation (e.g., Caraco, Martindale, and Whittam 1980; Stephens 1981; Stephens and Charnov 1982) may also be important. Nevertheless, keeping the analysis as simple as possible by studying only a few variables at a time allows the clearest assessment of results.

About the !Kung

The note (Hawkes and O'Connell 1981) that prompted Sih and Milton's comments was stimulated by the results of our analysis of the recent elimination of tree and grass seeds from the diet of the Alyawara of central Australia (O'Connell and Hawkes 1981, 1984). That analysis was guided by the optimal diet model (Charnov and Orians 1973; Charnov 1976), which drew our attention to the high processing costs of these resources relative to the returns they provide in energy and nutrients.

This led us to make two general points in our note:

1. The relative abundance and high nutrient content of many resources on which hunters rely has prompted the inference, now the conventional wisdom, that the costs of subsistence in hunting economies are very low. This inference fails to take account of the time required to process these resources. It takes five hours of cracking and pounding to produce 1 kg of mongongo nutmeats (Lee 1979:145), 4–6 hours to pick, winnow, and grind 1 kg of grass or tree seeds (O'Connell and Hawkes 1981; Simms 1984). In spite of their abundance and high nutrient content, these and other similar resources yield comparatively low returns on encounter—for example, approximately 1,300 kcal/hr for mongongo nuts (Lee 1979), 100–1,300 kcal/hr for certain species of central Australian and western North American grass seeds (O'Connell and Hawkes 1981; Simms 1984). In contrast, the available data suggest that returns on encounter with game animals in tropical and subtropical environments are in the range of 2500–>15,000 kcal/hr (e.g., Hawkes, Hill, and O'Connell 1982; Hill and Hawkes 1983; Hill, Hawkes, Hurtado, and Kaplan 1984; Yost and Kelley 1983; Lee 1979; see Jones 1984 for a summary). The more heavily foragers rely on high cost/low return resources, the greater their subsistence costs, and the less legitimately they can be characterized as "affluent" (Sahlins 1968).

2. This observation has an important, but initially counterintuitive implication. If hunters seek to maximize their rate of food acqui-

sition, then relatively high cost/low return resources like mongongo nuts and grass seeds may be eliminated from local diets, *regardless of their quantitative importance*, given a sufficient increase in the availability of resources that produce a better return relative to time invested in collecting and processing.

Sih and Milton disagree with this inference, arguing that "energy based rankings do not appear relevant for the !Kung." They suggest that protein may be the more critical measure of food value, although they cautiously observe that "the data are insufficient to evaluate the appropriateness" of these alternatives. The data are indeed limited, but suggest that Sih and Milton are wrong: energy is apparently a more critical resource than protein for the !Kung. Lee (1968), for example, reports a very high protein but just sufficient calorie intake at Dobe. He also documents a pattern of seasonal weight loss (Lee 1979; see also Wilmsen 1982). The extensive medical assessment of Truswell and Hansen (1976:194) leads them to conclude that "the only nutritional weakness in the San's diet is a shortage of energy (calories) usually in the spring dry season."

There is a more basic issue. Sih and Milton argue that "If protein rankings are indeed appropriate, then contrary to Hawkes and O'Connell's (1981) assertion that mongongos should be near the bottom of the list, they should be strongly preferred (see Lee's 1979 appendix D and table D.1)." The point of our note was to underline the importance of counting the *costs* of resources. We were concerned to show the difference between the value of a resource as measured only by its nutritional constituents, and its *net* value, counting processing costs. Mongongo nuts are a rich source of both calories and protein, but they also have high processing costs. These costs remain high regardless of whether energy or protein is chosen as the measure of food value. Mongongo nuts are *expensive* resources.

The table to which Sih and Milton refer reports the nutritional constituents of an array of !Kung plant resources. Surprisingly, the correlation between protein and calories in these resources is extremely high ($r = 0.93$). If game animals were added to the list, the correlation would be even higher. *This means that the ranking of !Kung resources would be very similar using calories or protein.* Either way, mongongo nuts are low ranked because of their processing requirements, and are thus likely to be eliminated given a sufficient increase in the availability of higher-ranked resources.

A final point about resource distribution: an assumption of the optimal diet model is that resources are encountered at random. This is often untrue in the real world, and certainly untrue for the !Kung. Sih and Milton suggest that the patch choice model may be more appropriate, especially because its predictions may differ radically from those of the optimal diet model. We definitely agree that the patch choice model is a useful tool in anthropological analysis (see O'Connell and Hawkes 1981, 1984; Hawkes et al. 1982; Winterhalder 1981b; Smith 1980 for examples of its application). Like the diet breadth model, it entails an unrealistic, but useful simplifying assumption: that resources are encountered in perfectly discrete patches separated by unproductive space. Picking one or the other of these models depends on the question of interest. Even if we consider mongongo groves as a patch, their value as a resource still depends on processing costs.

Summary

The major issue here is not the utility of foraging models, but how they are most productively employed. Sih and Milton advocate an approach that incorporates many complicating variables from the beginning of an analysis. This makes it very difficult to formulate quantitatively testable predictions about behavior or to tell which, if any, of the multiple hypotheses are *not* at fault when predicted and observed behavior fail to match. The alternative approach, in which the number and complexity of hypotheses simultaneously under consideration is kept to a minimum, has served students of foraging well in the past and should continue to do so in the future.

Sih and Milton conclude with the comment that foraging models have clarified the importance of quantifying not just energy, but also its cost. This is the point that prompted our note on affluent hunters.

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Agricultural Intensification and Women's Work

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Ember's analysis in "The Relative Decline in Women's Contribution to Agriculture with Intensification" (*AA* 85:285–304, 1983) of changing women's roles in agricultural labor organization is unconvincing. She fails to adequately define concepts, which leads to inappropriate comparisons, and she takes an ahistorical, synchronic approach which leads to viewing the process of change in isolation from the larger world. I offer the following four comments in an attempt to further discussion of this important issue.

1. The lack of a critical, comparative evaluation of the definitions, methods, and time frames used in the various studies she uses in her analysis makes the tables, each with different numbers of different cases from different surveys, very confusing. The only evidence offered in support of her central argument, that there is a relative decline in women's labor contribution to agriculture and an increase in their domestic work with intensification, is given in Table I. This table consists of a sample of 13 groups taken from a previous review (Minge-Klevana 1980). In subsequent sections she supports proposed causes for this change in women's roles by drawing on three additional cross-cultural surveys: the HRAF Probability Sample, the Ethnographic Atlas (Murdock 1967), and Nag's (1962) survey of factors affecting fertility. Mixing data from these surveys together, often in the same table, and using different sets of groups in each table (none of the five subsequent tables uses the same groups used in Table I to establish the condition to be explained) makes the argument unconvincing.

2. Although the concept of agricultural intensification is central to Ember's argument, it is never explicitly defined. She does imply, however, that intensification is to be equated with "having the plow or irrigation" (p. 287). This is not the usual definition of intensification, which centers on the increasingly frequent use of land (Boserup 1965:43), although it can include increased technical and labor inputs (Netting, Cleveland, and Stier 1980; Cox and Atkins 1979:139–140; Ruthenberg 1980:15–16). Thus, while the use of the plow or irrigation may be associated with intensification, it does not define it. Ember also states, in explanation of her assignment to categories in Table I, that people practicing hoe agriculture are probably nonintensive, although she "cannot be sure" (p. 287). While "hoe agriculture" is often equated in the anthropological literature with "slash and burn" techniques, which of course are relatively nonintensive, many people using the hoe as the major tool of cultivation have quite intensive systems. Ember does not, however, even follow her own definition. For example, the Tallensi and Ashanti are both classified as intensive agriculturalists in Tables II and IV even though neither use irrigation and neither depend on the plow. Although the plow was introduced to the Tallensi in the 1930s, it has not replaced the hoe as the primary tool in their intensive system of cultivation centered on continuous cropping of manured fields. Puzzlingly, in Table VI the Ashanti have become horticulturalists. They are in fact nonintensive, cropping a plot for three years and then fallowing from two to ten times as long (Allan