



Archeology and Optimal Foraging Theory: Appropriate Analytical Units

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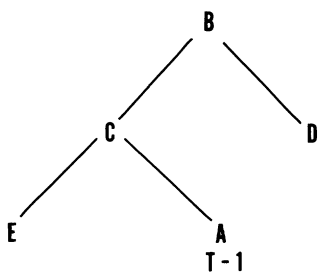


Figure 3

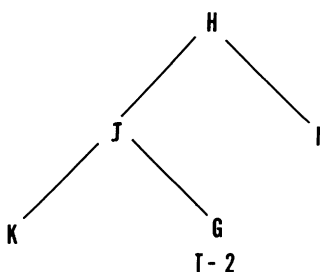


Figure 4

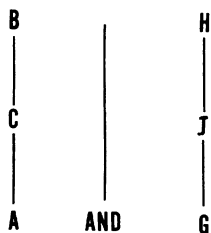


Figure 5

their respective places in their trees (see Figure 5).

Contrary, then, to Lancy and Strathern, I assume that each label in a pair has fit in a definition structure that contains a category beneath which the label from the pair is attached. This assumption would not be necessary if twos were idioms; in such a case their meanings would not be inferable from their constituent parts. However, if the meanings of twos are inferable from their constituent parts, they need not be listed separately in a Melpa lexicon. It is plausible to assume that entries in a lexicon are basic or atomic meanings that figure in the derived meanings of sequences constructed from lexical entries and rules for their combination. Either a pair is a formative

or it is a sequence of formatives. We find in some language samples in the Lancy and Strathern article the word for pig. In fact, the word occurs in more than one sequence and the sequences it occurs in are not the same except for the word 'pig.' Therefore, the word 'pig' is a constituent of a pair. If the pair has constituents, it is not itself a minimal meaning formative. We must conclude that the pair is not a formative; it is, at least in some cases, a sequence of formatives. If it is a sequence of formatives, it has a compositional structure formed by its constituent formatives and the rules for their combination.

Further, whereas pairs may literally be used in communicative interactions, taxonomies are rarely so used. Taxonomies are meaning schemata. Pairs appear to be terms or combinations of terms. Lexical entries in a Melpa lexicon would be fed by the definitions representable in taxonomic schemata in which Melpa terms (pairs) have fit.

Thus, pairing and taxonomizing presuppose nonintersecting relationships. But in no sense are they alternatives for one another. Taxonomies are basic and pairings are derived and compositional in nature. Specifically, they are neither functionally equivalent and substitutable for each other, nor are they substitutable for each other and not functionally equivalent.

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Archeology and Optimal Foraging Theory: Appropriate Analytical Units

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In a recent review of *Hunter-gatherer Foraging Strategies* by B. Winterhalder and E. Smith, John Martin (AA 85:612-624, 1983) has criticized the application of optimal foraging theory in anthropology on methodological and substantive grounds. Since Martin deals with my article in that book ("Archaeological Applications of Optimal Foraging Theory") in

some detail, I would like to respond to some of his criticisms, focusing on issues that are particularly of relevance to archeology. In particular, I would like to consider the role of time dynamics in the study of foraging strategies, and the selection of appropriate units for analyzing the time frame within which foraging decisions are made.

It is true, as I pointed out in my paper and have expanded on elsewhere (Yesner 1983a,b), that most applications of optimal foraging theory (in biology or anthropology) have paid insufficient attention to the time frame within which foraging strategies operate, particularly the frequency of resource fluctuations. Such fluctuations may be the result of stochastic variation (e.g., variation in numbers of animals in a given area, in constraints on hunting imposed by weather conditions, or in numbers of available hunters), or of deterministic processes (e.g., animal population cycles). It is difficult to generalize on this issue because the best strategy under one set of circumstances may be to "track" resource fluctuations with frequent changes in prey choices, while in other situations it may be best to maintain the same dietary breadth but alter length of patch use or distribution of foragers (settlement patterns), depending on the frequency, amplitude, and predictability of resource fluctuations. However, this does not deny that *the impact of such changes can be studied by using predictions generated by optimal foraging theory* (since this is what the theory was designed to test), nor that foragers develop non-instantaneous behavior models ("foraging strategies") over varying periods of time, in accordance with expected values of net energy intake ("mean rates" of return, in Martin's terms). Martin apparently recognizes this when he notes that foraging decisions are made "over a period when costs and returns remain constant." It then becomes an empirical question as to over how long a period such "mean rates" apply, and how these are integrated into an overall foraging strategy.

An example of this is the test of the "proportional hunting" hypothesis presented in my article. This test depends on a demonstration that rankings of resources (based on net energy intake) are similar in the natural environment and in archaeological biomass data, based on the assumption that faunal remains accurately sample the prehistoric diet and that faunal densities remain fundamentally unchanged, conditions that I argue were met by the prehistoric Aleutian data. The result was a largely successful test of the proportional hunting hypothesis for both sea mammals and birds. Martin, however, argues that the test

would have been more successful (meaningful?) if a more restricted time unit, such as a season, were utilized to produce four pair-wise comparisons. There are three major objections, however, to the use of such an analytical unit.

1. One assumes that Martin has suggested the use of a season as an analytical unit in order to reduce the use of "mean rates," or to calculate them over as short a period as possible. However, there is no *empirical* reason for using a season as a unit of analysis. Sites were not exclusively used during a single season, nor were resources present only during a single season. Furthermore, there is no information to suggest that a season was the unit for foraging decisions. If a time unit is to be selected at all, it should be appropriate to each prey type and its period of availability (e.g., mid-summer to early fall for anadromous fish, spring and fall for migratory fur seals or waterfowl, and so on).

2. Seasonality data are *already* integrated into the model, since environmental abundances were calculated by averaging over abundances during each season of the year for each species.

3. In suggesting only 4 pair-wise comparisons, Martin seems to be suggesting that natural biomass ranking should be adjusted seasonally and then compared to the average archaeological biomass ranks from each site, which are *not* seasonally adjusted. The result of such an analysis would be spurious, and would reduce the goodness-of-fit for all four comparisons. If, however, Martin means that 16 pair-wise comparisons should be made between seasonal rankings for the natural environment and for appropriate seasonal fauna found at each site, then the data are simply not adequate for such comparisons. Midden strata that form the units for faunal analysis are not generally specific to a given season, and even if the results of deposition of a single season's activity could be identified, the sample size of faunal remains from such a small unit would create a spurious test of the hypothesis.

A similar problem would also affect Martin's suggestion that the data be broken down by patch type. If, for example, the habitats of bird species included in the test of proportional hunting were broken down into patch types such as cliffs and rocky ledges, lakes and streams, bays and reefs, interisland passes, and open ocean, one would find that very few species of birds were confined to or even consistently hunted in any one patch type. In addition, the Aleut ethnographic data suggest that three or four different hunting methods

may have been used for a single bird species! Furthermore, it is impossible from the archaeological data to judge in which patch type a given prey item was exploited whose remains are found in a particular occupation layer of a site. (Even if this could be done, however, the results would not differ from those of the original analysis, since it would merely break up the results from each site into component structural units, within which the ranks would stay the same.) A similar argument could also be raised about Martin's objection to the use of qualitative codes to produce biomass estimates. In all three cases, an attempt was made to select units that would not result in sample size problems. Perhaps as our data or methodologies become more refined, we will be able to achieve still more refined results, but even our current methodologies do not "require data and computations beyond the analyst's means" if they are used carefully.

A similar point could be raised in regard to the role of food storage among hunter-gatherers. Some hunter-gatherers use such techniques as part of a "logistic" approach to resource exploitation (Binford 1980). The Aleuts are a good example of such a group, because many types of food (particularly anadromous fish and sea-mammal flesh) were stored. One would perhaps expect that optimal foraging theory could not be applied to the analysis of such societies. Clearly, how-

ever, the result of such food storage practices would be to *stabilize* the ranking of resources by *reducing* seasonal fluctuations. This should lead to even greater confidence in the use of "mean rates" of net energy intake in applying optimal foraging theory to the prehistoric Aleuts or other such groups.

In sum, while there may still be some problems in operationalizing the application of optimal foraging theory to prehistoric data, with judicious selection of analytical units and appropriate tests of hypotheses, this theory has potential as a robust analytical tool for decoding the basis of human foraging strategies.

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