

It is interesting that the “emic” views of the Havasupai concerning reproduction (as reflected in Spier 1928, Nag 1962, Martin 1984, and Smithson 1971 [1959]) differ sharply from Martin’s “etic” observations about the underlying causes of changing reproductive performance in the politico-jural domain of Havasupai society. I wish that he had dwelt on the implications of “primary” and “secondary” consequences of a changing sex ratio for Havasupai social structure.

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## On the Magic Number 500: An Expostulation<sup>1</sup>

KEVIN M. KELLY

Medical City Dallas Hospital, Research Suite C740,  
7777 Forest Lane, Dallas, Tex. 75230, U.S.A. 21 III 94

Hunn’s recent report (CA 35:81–85) sent me searching for an old manuscript (Kelly 1980) on a subject I had been told over a decade ago was no longer an issue in anthropology. Hunn invokes the magic number 500, linking this purported demographic central tendency with his own toponymic findings to suggest that there is a “cognitive limitation” to individual human memory. The perception of 500 as a magic demographic number originates in the volume edited by Lee and DeVore (1968). However, as Hunn notes, the notion that 500 individuals constitute a self-defining limit to the size of the extended hunting-gathering social unit derives ultimately from the work of Birdsell (1953).

Using areas and distributions of Australian groups taken from a map constructed by Tindale (1940) and estimates of population size compiled by Krzywicki

(1934), Birdsell (1953) argued that (1) the area occupied by the Australian tribe was largely determined by local mean annual rainfall and (2) the size of the Australian tribe approximates a statistical constant of 500 persons. Building upon these conclusions, Birdsell (1958, 1968, 1973) later argued that this statistical tendency was indicative of equilibrium mechanisms operating among hunter-gatherers in general. These models and their inferences have had a substantive and far-reaching effect on anthropological thought, not only having been widely cited but also having often served as the stimuli or bases for other studies and models.<sup>2</sup>

The legacy of the 1953 analysis, invoked most recently in works by Hunn (1994), Aiello and Dunbar (1993), and Birdsell (1993), has been the theorem that the basic unit of hunter-gatherer organization is self-defining and its corollary that the size of that unit tends to be 500 individuals. I intend here to question the validity of these conclusions, arguing that the notion of a self-defining unit of hunter-gatherer social organization is built on error and that an accurate reading of the analysis and data reveals a set of interrelated social, environmental, and demographic parameters. My hope is that an appreciation of these relationships will encourage others to reexamine the bases for their models and stimulate renewed interest in the socio-ecology of hunting-gathering peoples.

A careful reading of Birdsell’s 1953 article, a rather imposing 37 pages of interlinked arguments concluding with a test of the statistical constant, reveals algebraic errors and circular reasoning. Unable to approach the question of population size directly because of a paucity of data, Birdsell quite correctly notes (p. 177) that the tribal area and density are inversely related *if tribal size is held constant*:

There remains the important problem of functionally relating the size of tribal area to density. There is no reason for rainfall to directly determine the area occupied by the Australian social unit known as the tribe. Unfortunately there are too few estimates of the population size of aboriginal tribes to work directly with density as a dependent variable. The bridging device is found in the definition of population density as the number of individuals per unit area. It follows that an exact inverse relationship is established if the population size of the tribe is held constant throughout the series.

The methodological problems become more apparent as the discussion proceeds and the analysis unfolds (p. 177, emphasis added):

1. This essay is drawn from Kelly (1980). Thanks are due to Cheryl Mills Kelly, Morley Herbert, Gayle Weger, Shirley Boots, and Gary Boots for their patient reading of and valuable comments on this manuscript. Additional thanks are owed to the editor, Richard G. Fox, and to an anonymous referee, whose suggestions have simplified and improved the presentation. Any errors or omissions are mine alone.

2. For example, these findings have been cited in models (Casteel 1972; Dumond 1972; Smith and Young 1972; Wilmsen 1973; Birdsell 1975b, 1993; Wobst 1974, 1976; Cohen 1977; Hayden 1981; Aiello and Dunbar 1993; Hunn 1994), in critical works (Yengoyan 1968, 1976; Birdsell 1970, 1977; Divale 1972; Smith 1972; Cohen 1977), in reviews (Hassan 1978, 1981), in textbooks (Birdsell 1975a, 1981; Hardesty 1977), and in analogous studies (Martin and Read 1981; Vorkapich 1981).

Thus it becomes crucial for the following analysis to make the intervening *assumption* that: (1) population densities are causally and *inversely* related to mean annual rainfall within the tribal territory; (2) in a statistical sense the population size of the Australian tribe may be considered a constant, in this case approximately 500 persons. If mean annual rainfall, which is the best simple measure available for biologically effective rainfall, does in fact show a reasonably high correlation with the size of the tribal area, it will tend to validate both of the foregoing assumptions.

The argument that if mean annual rainfall is highly correlated with tribal area, then the size of the tribal population tends to be constant is a conditional proposition (if  $p$ , then  $q$ ). A relationship is established between tribal area and mean annual rainfall, fitting the data to a curve described by an equation in the form  $Y = aX^b$ , where  $Y$  is tribal area,  $X$  is mean annual rainfall, and  $a$  and  $b$  are constants (p. 182). However, it is the tendency of these populations to number 500 persons that the analysis is attempting to establish. Once a correlation is established between mean annual rainfall and tribal area (i.e.,  $p$  is true), the conditional proposition is true *if and only if* tribal populations tend to be constant, in this case 500 (i.e.,  $q$  is true). The proposition that population size is a statistical constant does not follow from the correlation, and to assume so is a serious error in logic.<sup>3</sup> As a consequence, some proof of the second proposition must be presented.

The initial reaction might be to dismiss the premise of an inverse relationship between population density and mean annual rainfall as an editorial error. However, the attempt to validate the second premise—the proposition that the size of the Australian tribe may be considered a constant—suggests that these variables are viewed as “inversely related” for a reason. The second premise is tested indirectly by considering whether population size varies as predicted from the rainfall-area equation if one assumes that population size is not constant (p. 178):

If for any reason the average size of the tribal population varied as a function of rainfall, the distorting influence of this factor could not be detected. Thus for example, tribes might have consistently small sized populations in desert areas, and larger ones in regions of high rainfall without this being apparent in the original correlation. But there is little evidence to suggest that this type of variation is important.

The reason for asking whether population size varies as a function of rainfall is the need to explore the possibility that all of the independent variables (i.e., density, area, and population size) are functions of rainfall. The

answer to this question is important because “if other critical parameters vary as some systematic function of rainfall, it would not be apparent in this form of analysis” (p. 178).

A simple proof of the second proposition is to examine the relationships predicted between tribal size and tribal area assuming that a different parameter, population density, is constant—a proof by contradiction.<sup>4</sup> Birdsell’s narrative suggests that this is precisely what he did, substituting  $aX^b$  for  $Y$  in the density equation  $D = N/Y$ , where  $D$  is density,  $N$  is population size, and  $Y$  again is tribal area. Therefore  $D = N/aX^b$ . In other words, if population density ( $D$ ) is held constant, population size ( $N$ ) must increase as mean annual rainfall ( $X$ ) increases or “tribes might have consistently small sized populations in desert areas, and larger ones in regions of high rainfall” (p. 178). The fact that Birdsell’s proof leads to the empirically contradicted statement that “tribes might have consistently small sized population in desert areas” leads him to conclude that the original statement (that population size tends to be constant) must be true.

The logic which compels the prediction of smaller populations in the desert (and the reason I believe that the reference to “inversely related” was not an editorial mistake) rests with the exponential constant  $b$  from the rainfall-area equation. Although the equation is  $Y = aX^b$ , the exponent is in fact negative:  $b = -1.58451$  (p. 182). Therefore  $D = N/aX^{-1.58451}$ . However, because  $1/X^{-1.58451} = X^{1.58451}$ ,  $D = NX^{1.58451}/a$ .

The negative sign on the exponent fundamentally changes the relationships predicted among the demographic and ecological parameters. It now becomes evident that, when density ( $D$ ) is held constant, population size ( $N$ ) must decrease as mean annual rainfall ( $X$ ) increases. In other words, the rainfall-area equation predicts that populations are larger in areas of lower mean annual rainfall and smaller in areas of high rainfall. Given an accurate reading of the equations, a much more complex set of ecological and demographic relationships—free of constraints on size of the regional hunting-gathering social unit—begins to emerge. As Yengoyan (1976:123) has noted, “lower population densities, larger tribal sizes and areas, characterise the interior continent” of Australia.

Up to this point Birdsell’s argument has focused on the sorting out of the relationships among the critical parameters. Presuming to have resolved these entanglements, the narrative now turns to the apparent demonstration that the tribes tend to number 500 persons. It is this tendency which led Hayden (1981:522) to apply the rainfall-area correlation as an indicator of sedentism and others (e.g., Birdsell 1958:191; Yengoyan 1968:188; Hassan 1978:63; 1981:16; Martin and Read 1981:151;

3. The error of such reasoning is known as “asserting the conclusion” (Dolciani et al. 1964:24). A discussion of the principles of logic may be found in most mathematics textbooks (for example, Dolciani et al. 1964:19–25; Miller and Heeren 1986:40–98).

4. As Miller and Heeren (1986:97) explain, “a statement can be proved by contradiction by assuming that its negation is true. The assumption that the negation is true is used to produce some sort of contradiction, or absurdity. The fact that the negation of the original statement leads to a contradiction means that the original statement must be true.”

Vorkapich 1981:218) to extend the relationship to density.<sup>5</sup> Unfortunately, an examination of the analysis quickly reveals that preselection of the demographic data ensured the predicted outcome. Again, the reader is presented with two assumptions: that "(1) the size of the tribal populations approximate a statistical constant, and (2) that the size of the tribal area is the inverse express of tribal density" (p. 178). On the basis of these assumptions "an ecologically more homogeneous series is obtained by making certain *systematic* corrections" (p. 178, emphasis added) whereby "the categories of tribes subject to such exclusion may be chosen by two types of criteria: (1) systematic deviations from the expected densities due to variations in ecological factors; and (2) deviations in either direction due to variation from the assumed constant of 500 persons per tribe owing to actions of cultural variables."

The result of applying the second criterion should be obvious. Data are restricted by removing the portion of the sample that does not cluster around 500 and whose variance cannot be explained environmentally. This criterion eliminates 57 tribes from the initial series of 409. Tribes described in the first category are excluded when the observed density varies to an unspecified degree from the expected density. On ecological grounds, 224 tribes (54.76%) are eliminated from the original sample. Including 5 groups which are unaccounted for, the beginning sample is reduced by just under 70% (69.92%), from 409 to 123 tribes. From this select series, the widely cited correlation coefficient of 0.81 is calculated (p. 182); a correlation of 0.59 had been computed for the tribal series as a whole (p. 178).

The model is "tested" by verifying the fit between observed values of population size and estimates of population size derived from the revised rainfall-area equation. Estimates of population size are made using a coefficient called the "area ratio."<sup>6</sup> The area ratio is defined as the "measured area of tribal territory divided by the area predicted by the basic equation for its value of rainfall" (p. 184). However, "the method of estimating tribal populations from the area ratio determined from the basic equation *requires* that the size of the population approximates a statistical constant" (p. 198, emphasis added). Furthermore, the groups for which population estimates are made are the tribes in the "ecologically more homogeneous series" which remain following Birdsell's "systematic corrections." Consequently, the estimates should fit the observations, and according to Birdsell (p. 199) they do.<sup>7</sup>

Throughout the article Birdsell explicitly and repeat-

edly assumes constant population size. However, over time this stipulation has been forgotten (cf. Lee and DeVore 1968), allowing the emergence of the magic number 500. The missteps which led to the acceptance as fact of the equilibrium assumption can be understood historically in terms of the methodological and theoretical vacuum that they were ambitiously intended to fill. Birdsell's analysis is notable as an exception to what had been a general failure on the part of anthropologists to give ecological variables serious consideration (Bartholomew and Birdsell 1953). However, that analysis demonstrates neither self-defining population equilibrium among hunting-gathering groups nor population densities strongly tied to a single environmental variable. In fact, an accurate extension of Birdsell's rainfall-area equation to the other critical parameters (i.e., density and size) reveals a much more complex set of ecological and demographic relationships consistent with the ethnographic evidence (e.g., Yengoyan 1968, 1976) and free of self-defining constraints on population size.

Clearly, more significance has been attributed to the results of the 1953 analysis than can be justified. Many of the conclusions reached in the analysis are mathematically unattainable, and, in particular, the assumption that the sizes of these populations tend to be constant, even in a statistical sense, is not proven. This fact and the recent revival of the magic-number concept should remind all of us of the need for regular reexamination of the assumptions and data upon which our models are built.

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5. The original correlation cannot be directly extended to related parameters, particularly as all parameters appear to vary.

6. In Birdsell's 1973 article, in contrast, population predictions are reported to have been calculated from the density ratio, defined (1953:184) as the reciprocal of the area ratio (compare Birdsell 1953:198, fig. 9, with Birdsell 1973:433, fig. 1).

7. He reports a mean of 575 and a standard deviation of 300 (p. 199) for the series of tribes used in the final area-rainfall equation ( $r = 0.81$ ). Therefore, statistically, approximately 68% of the tribal populations fell in a range between 275 and 875 persons.

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## On Evolutionary Ecology and Cultural Realities

DANIEL STILES

U.N. Environment Programme, P.O. Box 47074,  
Nairobi, Kenya. 8 III 94

Hawkes (CA 34:341–61) tries to demonstrate that paternal family provisioning is not the main goal of male foraging strategy in past and present hunter-gatherer societies. She also challenges the widely accepted idea that, even if the consumption returns to a hunter and his family are less than the energy expended to kill the animal, it is the expectation of delayed reciprocity that explains why he does it. Ethnographic data on the calorie/day value of big-game kills are plugged into a payoff matrix contingency table to demonstrate that the successful hunter is a long-term loser in terms of food for him and his family as he continues to provide big animals to the community. The concept of big-game kills as a public good is an unnecessary complication (her conclusions would be no different if the kill were a private good distributed communally), although it does seem to have distracted many of the CA☆ commentators from the principal flaws of her model.

If food provisioning doesn't explain why hunters hunt big game, what does? Hawkes hypothesizes that it is social attention and with another contingency table demonstrates that she is right. By "social attention" she means potential allies in disputes and decision-making and access to mates who will increase his net fitness benefits by ensuring that he produces more offspring than had he not provided meat. As an evolutionary ecologist, she assumes that reproducing offspring is the ultimate goal of any socioeconomic action, the action dichotomy investigated here being food provisioning to family versus social attention. The definition of "family" is critical.

Games theory, contingency tables, and so on, are not, however, appropriate methods to apply to human social behavior. People in traditional societies are constrained in their decisions by cultural rules. In ecosystems with patchy and periodically scarce resources these rules tend to be particularly stringent. Their principal objective is the survival of the group, not the individual. In addition, individuals in a meat-sharing situation are not all equal. Proximity to the hunter is much less important in determining a share than the social category of the individual in relation to the hunter (Gibson 1988). Moreover, the hunter is usually a man in the prime of life, much