Bushman Birth Spacing: A Test for Optimal Interbirth Intervals

Nicholas Blurton Jones*
Graduate School of Education, and Departments of Anthropology and Psychiatry, University of California Los Angeles

Long interbirth intervals (IBIs) are reported by Lee and by Howell for !kung women, averaging around 4 years between births. Is this low rate of births maladaptive? An earlier analysis of the ecological constraints upon !kung women suggested that it might not be. Given the observed features of !kung ecology, shorter IBIs require a mother to carry much greater loads (backload) of baby and food on her foraging trips. Calculating this load for each IBI shows a sharp upturn in backload when IBIs fall below 4 years. It was suggested that sharply increased backload might lead to such a severe increase in mortality of offspring that the observed 4-year IBI actually maximized the number of offspring that the woman raised.

This proposition is tested in this article using reproductive histories of individual !kung women collected by Howell. Backload calculated in the previous study is the best predictor of the observed losses of children at each IBI. Furthermore, mortality of the children is indeed so sharply related to IBI that the 4-year IBI appears to be the interval that maximizes reproductive success for most women.

KEY WORDS: Birth spacing; Infant mortality; Optimization; Parental investment; Hunter-gatherers.

INTRODUCTION

The long interbirth intervals (IBIs), roughly four years, reported for the !kung by Howell (1979) and Lee (1972, 1979) might seem to be a clear example of people failing to maximize reproductive success. Reproductive success would seem to be maximized by frequent births, particularly if offspring mortality is not taken into account. Yet it is widely accepted that in many human populations shorter interbirth intervals lead to higher mortality of infants and children (e.g., Hobcraft et al.

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Address reprint requests to: Dr. Nicholas Blurton Jones, Graduate School of Education, University of California, Los Angeles, 405 Hilgard Avenue, Los Angeles, CA 90024.

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52 Vanderbilt Ave., New York, New York 10017
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1983). If mortality is high enough when there are frequent, closely spaced births, then fewer, more widely spaced births may leave more descendants. In this paper I report a test, using reproductive histories collected by Howell from !Kung women, to see whether the 4-year interbirth interval leaves more surviving offspring than does either a shorter or a longer interval.

The proximate mechanism by which long intervals can be achieved without modern birth control seems to be no longer a puzzle. Frequent suckling apparently has quite direct effects on the mother's endocrine system (Konner and Worthman 1980, Howie and McNeilly 1982). However, in this paper I am not concerned with the nature of the mechanisms, only with their adaptiveness. I expect the mechanisms to be capable of giving flexible but adaptive outcomes, being able to produce intervals that vary from individual to individual and culture to culture, depending on where circumstances place the optimal interbirth interval.

Ultimate explanations that have been offered for such long intervals include the following: (1) children are not valued highly in !kung society (this neglects to ask why these values are held and not others, and contradicts stated !kung values); (2) restraint on population size in order to conserve resources and a leisurely life-style; and (3) Lee's (1972) clearer and more plausible suggestion that the work entailed by shorter interbirth intervals is simply too much. This nonetheless neglects the question of why that much work is too much work.

Blurton Jones and Sibly (1978) attempted to look at the adaptiveness of the long interbirth intervals, elaborating on Lee's proposition by including the weight of food carried by the mother in their calculations of mother's work, using other data gathered and published by Lee. Like Lee (1972), they showed that shorter intervals lead to greater workloads for the mother, specifically the weight of baby plus the food to be carried on foraging excursions (reprinted here as Figure 1). We suggested that exceeding these loads in the hot dry season could lead to risks of death or injury for the mother, perhaps from heat stresses. If, as seemed more likely, she took care to avoid such risks by limiting her load and her choice of days on which to forage, there might be a risk of nutritional shortage for the family. Undernutrition is widely regarded as increasing susceptibility to infection, and thus might influence child mortality. In addition it might be reasonable to expect an impairment of the mother's lactation at high levels of work, heat or water stress, and lowered food intake. Though lactation seems to be a "protected" process, there is some evidence for such impairment presented by Jelliffe and Jelliffe (1978). Consequently unless mothers' work was somehow eased, shorter intervals might lead to much higher mortality of children or mothers, such that the reproductive success of a mother would actually be lower if she reproduced at short intervals than if she stayed in the range of 4 years. Much longer intervals would be less effective simply because fewer births could be fitted into the reproductive span. Thus we suggested that the work entailed

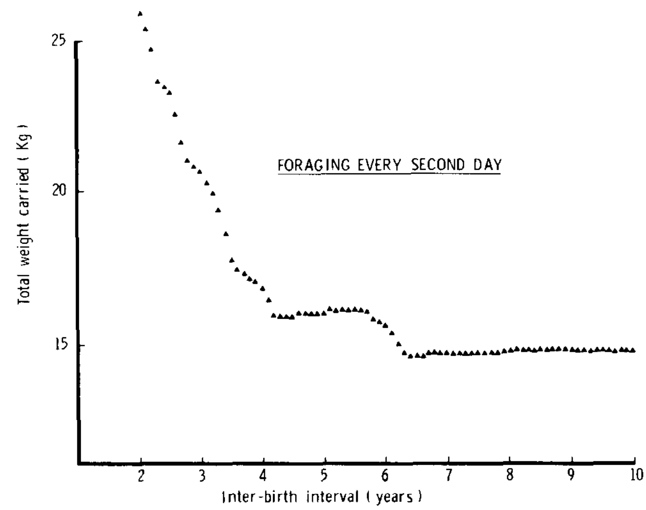

FIGURE 1. Graph of weight that the Blurton Jones and Sibly model calculates that a woman would carry if she maintained interbirth intervals shown [keeping other factors constant—described in Blurton Jones and Sibly (1978)], plotted against a range of possible IBI.

for the mother had consequences that seemed very likely to render the 4-year intervals optimal reproductive intervals.

The direct test of Blurton Jones and Sibly's suggestion proceeds through three steps: from Howell's reproductive histories of !kung women I (1) attempt to establish the relationship between mortality of children and interbirth interval and ask what curve of mortality against IBI is represented by the observations, (2) using this curve, I determine which intervals yield the greatest number of surviving offspring, and (3) I then match this to the observed number of women employing such intervals.

It should be clear that in this article I pursue the question of optimal birth spacing only from the mother's viewpoint. According to parent–offspring conflict theory optimal intervals for the baby would be rather longer.

Several studies of birth interval and mortality have indicated that mortality may cause short intervals as often as vice versa. There is much evidence that the death of a baby leads rapidly to a pregnancy in many populations. But my interest in this article is in the effects of interval length upon mortality. For this reason I exclude from this analysis all "replacement" births when the first child died before the next pregnancy. I then assume that in the intervals used in this study (which are intervals when the next birth occurred while the previous child was still alive) any relationship between mortality and interval is a result of an influence of interval upon mortality.

METHODS

The Sample of Women

I use data on !kung women whose reproductive lives were spent living predominantly in the bush and not at cattleposts. I chose this subsample because Lee (1972) showed that the long interbirth interval is primarily associated with dependence on bush foods, and because Blurton Jones and Sibly's model is entirely based on Lee's data on the bush food economy. Thus the total of 172 women were classified according to their subsistence economy as follows:

1. Lee provided me with the classifications used in his 1972 paper. He rated women on a scale of 1 to 4, from most exclusively dependent on bush foods to most dependent on cattlepost food sources, using a combination of his knowledge of the families during his fieldwork, the age of the women, and the place where they lived at the time of the observation. He emphasized that people changed their subsistence from time to time and so every woman may have used some of each kind of resource. Even the most cattlepost-dependent women will have often used bush foods.
2. Because of these changeable subsistence histories, with Howell's help I categorized people by the locations at which they gave birth. Approximate dates are known for the arrival of cattle and pastoralists at most locations. This information is used to check the classification of people ranked as "2" and any about whom Lee had expressed doubt. The two approaches to classifying the women agree well. Nonetheless some cases remain difficult to classify with any confidence. These 37 are left out of the analyses.

I then take the classified women and combine the 1s and the 2s into a predominantly bush food group of 65 women and the 3s and 4s into a predominantly cattlepost group of 70 women. Only the 65 bush food women are used in this paper.

The Sample of Interbirth Intervals

The methods by which the data were collected were described in detail by Howell (Howell 1979). This analysis was done from her data sheets. These contain a summary of each woman's reproductive history, and include reported season of birth, and calculated year of birth. From these I counted the length of interbirth interval with help, guidance, and some independent checks from Howell. These data sheets also show whether each child survived or died, and if it died when, and when was the last record of the child alive. From this I scored whether each child lived to 10 years of age or more (survivor), or died, and whether it died after the subsequent birth, or clearly before the next pregnancy (12 months or more before the next birth). Three cases were left out because they may have died just before or just after the next pregnancy began.

The outcome of an interval was categorized as "successful" if it added another surviving child to the family, or "failed" if either or both of the children died before reaching the age of 10. (I chose 10 years old because mortality was very low beyond that age, and taking a low age allows inclusion of more data, the births up to 10 years before Howell's last census.) The score that I refer to as "mortality" or "survivorship" is thus a slight underestimate of actual mortality because in 7 of the 96 intervals used in this paper, both of the children who delineate an interval died.

Each interval was designated as "replacement," "first," or "later." First interval in this case means the interval after the first child that a woman bears that survives at least until the next birth to that woman. Since the backload model predicts that first intervals will be under less constraint than later intervals (data that confirm this will be presented in another article), this article also only concerns intervals after the first interval.

"Replacement" intervals (when the first child dies before the next pregnancy) are also omitted from this analysis, because it seems clear that in these intervals mortality determines the interval and not vice versa. Thus I am examining a quite specific but major component of a set of adaptive stategies (the timing of about half the births).

The 65 bushfood-dependent women provided data on 272 births. These provided 193 interbirth intervals, of which 57 were first intervals and 40 were "replacement" intervals. This leaves the 96 "later" intervals of bush-living women that are the subject of this paper.

All the analyses have used an interbirth interval as the basic unit. I have ignored two aspects of their independence from other units. First, I ignored their adjacency to other intervals (except in the definition of "replacement" intervals). This adjacency would introduce additional variation in the findings: a short interval following a long interval might have more success than if it followed another short interval (as shown by Hobcraft et al. 1983). In ignoring this distinction (a distinction that would improve our chance of finding what we expect), I err in the conservative direction. I also ignore the fact that some women contribute more intervals to the data than others. Sixty-five women provide us with 96 usable intervals. There is thus the question of whether our findings represent effects of women or of intervals. If my approach was an inductive one this would be a serious problem. It would still be a problem even if I used only one interval per woman. But my concern has been to make predictions about the data and test them.

Questions about Howell's original methods are relevant insofar as they argue for *bias* in birth dates rather than for extensive random variation. Note that Howell's methods do *not* depend on either Howell or the mother stating birth intervals. Such would have led to bias on the basis "he survived, so he must have been born long after his big sister." Howell describes in detail

Table 1. The Raw Data (Grouped by IBI)

IBI (months)	Total (*N*)	Deaths	Survivors	Success %
24 (18–29)	6	5	1	16.7
36 (30–41)	23	14	9	39.1
48 (42–53)	29	13	16	55.2
60 (54–65)	16	7	9	56.2
72 (66–77)	6	0	6	100.0
84 (78–89)	7	0	7	100.0
96 (90–101)	2	0	2	100.0
108 (102–)	7	4	3	42.8
Totals	96	43	53	

the way that date of birth of dead children was ascertained. This would appear to introduce noise and not bias.

The most important methodological question relating to Howell's data seems to me to concern the very long intervals (6 years and more). These appear to give a quite remarkable degree of protection to both children. Is this because a dead baby between the two births has been forgotten? In the field Howell paid special attention to such intervals, asking repeatedly and in different ways whether there was another child, or an abortion, and asking relatives and neighbors what they knew of this. Some of the intervals arise when a woman changed husbands. The intervening period of single life or marital discord and dispute, with lowered risk of pregnancy might account for these intervals but does not account for their low mortality.

Deriving and Testing the Hypotheses

Three steps in testing optimality of IBI are discussed in the sections that follow.

1. What is the relationship between mortality of children and interbirth interval? What curve of mortality against interbirth interval best fits the observations? This is the most important step in the study, for the answer determines where the calculated optimal interval will be. It is known that interbirth interval is strongly related to mortality under third world conditions in general (e.g., Hobcraft et al. 1983) but we can bring more specific expectations from Blurton Jones and Sibly's model. The Blurton Jones and Sibly backload model presupposes that mortality is somehow due to mother's workload. Specifically the model (Fig. 1) suggests that there should be an inflection in the curve of mortality against IBI, at around 4 years. At longer intervals, mortality (following backload) should be fairly level. Below 4 years, mortality should climb steeply to very high levels. To test this suggestion we examined the fit of a straight line relationship between mortality and the backload implied by each IBI.

We might be skeptical about the data fitting every inflection of the calculated backload and therefore prefer to test the fit of a simple concave

Table 2. Comparison of Three Predictors of Success of an IBI: χ^2, Degrees of Freedom, and Probability for Four Tests[a]

	Backload	1/IBI	IBI
Improvement	15.47	15.76	10.23
df	1	1	1
p	<0.000	<0.000	0.001
Goodness of fit	33.22	35.03	38.85
df	28	32	31
p	0.228	0.326	0.157
Hosmer's	1.76	8.05	18.44
df	5	6	6
p	0.881	0.235	0.005
Brown's	1.05	0.75	9.59
df	5	6	6
p	0.592	0.687	−0.008

[a] High improvement chi-square with low *p* shows the independent variable contributes significantly to predicting the dependent variable. Low *p* for goodness of fit, and low *p* for Hosmer show poor fit of observed to predicted. Low *p* for Brown shows inappropriate model.

curve that would reach some asymptotic low level of mortality at long IBI. It seems unlikely that child and infant mortality ever gets very near zero under the conditions of !kung life. It is therefore more realistic to suppose that the data for long IBI would represent a steady, low mortality. An appropriate curve would be given by a reciprocal transformation, for example $Y = a + bx$ (1/IBI), or by a polynomial such as $Y = A + b'x + b''x^2$.

It is important to test whether these models really give any better account of the observations than would a simple straightline relationship of success to interbirth interval. Thus a third model is tested: success predicted linearly by interbirth interval.

The relationships between mortality and predictor variables were investigated in two ways. One was by logistic regression [with the BMDP program (Dixon 1981)], using the original *ungrouped* data, with success (survival) or failure (death of a child) as a dichotomous dependent variable, making fuller use of the available information (Table 2, Fig. 2).

The other was by linear regression, using as dependent variable the percentage of interbirth intervals that were successes, having *grouped* the data into 12-month blocks, centered on year points. The data tend to cluster about the year point because of the way that IBI were determined. Blocks were 24 months (18–29), 36 months (30–41), 48 months (42–53), 60 months (54–65), 72 months (66–77), 84 months (78–89), 96 months (90–101), and 108 months (102–) (Tables 1, 3, and 4).

2. Which interbirth intervals yield most surviving offspring? If we know the mortality associated with each interval we can calculate the number of surviving offspring that its repeated use would give rise to. This can be calculated from the number of births (reproductive span/IBI) multiplied by the probability of survival at that IBI. I call this figure "Yield." It is the number

Table 3. Statistical Tests of Each Predictor of Mortality in Grouped Data

	Backload	1/IBI	IBI + (IBI)^2	IBI
Overall F	7.61	7.08	7.40	3.37
P	0.033	0.037	0.032	>0.05
r^2	0.559	0.541	0.747	0.359

of offspring expected to reach 10 years of age in a reproductive career that keeps to the IBI specified. The optimal IBI will be the one with the highest yield (Table 5, Figs. 3, 4). I assumed a constant reproductive span of 20 years, believing its duration to be independent of IBI, and determined by quite other factors.

3. How does the performance of !kung women match up to the optimal performance? Given the premise that natural selection will have designed a reproductive system that is efficient, the simplest expectation is that the observed interbirth intervals will be normally distributed about the optimum interval. I seek to explain what most individuals in the population do by testing whether it is the course of action that appears to maximize reproductive success. Neglected factors in addition to mortality may render the apparent optimum misleading. These cannot be investigated until, like first intervals and "replacement" intervals, their importance has been proposed.

To match up the actual performance of the women against the "ideal" performance the yield of each IBI is plotted in Figure 3 (for the ungrouped data) and Figure 4 for the grouped data, together with the number of times each IBI occurred.

The degree to which the women match the optimal interval can also be tested by the method used by Smith (1981). Using the *grouped* data we can correlate the yield of an IBI with the number of times that interval occurs, arguing that higher yielding intervals should be more often employed. A high positive correlation will indicate a close match (Table 5).

Another method would be to see what proportion of the intervals are intervals that yield within, say, 80% of the maximum yield (Table 6). This would be particularly useful if several intervals have very similar yields, so that suboptimal intervals nonetheless yield almost as high as the optimum interval.

RESULTS

Relationship Between Mortality and Interbirth Interval

Ungrouped data. We tested, using logistic regression, the goodness of fit of the data to curves predicted by 1/IBI, calculated backload, and IBI. Results are shown in Table 2 and Figure 2.

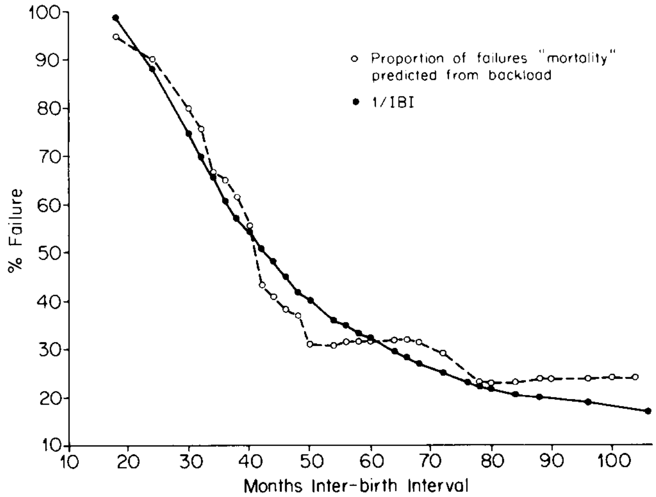

FIGURE 2. Fitted curves of failure ("mortality") predicted by backload and by 1/IBI, shown plotted against IBI, for ungrouped data. Results obtained by logistic regression.

It can be seen that the best fit is obtained from Load and from 1/IBI. In Table 2 we see that not only does IBI give us a substantially lower chi-square to enter the model but it fails the tests for the significance and reliability of predictor variables.

Grouped data. Linear regression using the grouped data gave similar results (Tables 3, 4). IBI as the only predictor failed to pass any statistical tests and did not give a significant model (*F* test). Calculated backload, 1/IBI, and the polynomial all gave significant models, predicting curved relationships between IBI and success.

Thus as hypothesized, a concave curve seems to be the best fit to the observations, with mortality never declining to zero.

Table 4. Predicted Percentage Success for Grouped Intervals

IBI (months)	Backload	1/IBI	IBI + $(IBI)^2$	Observed
24 (18–29)	11.9	14.7	7.3	16.7
36 (30–41)	43.9	44.8	39.9	39.1
48 (42–53)	67.1	59.9	64.3	55.2
60 (54–65)	72.1	68.8	80.4	56.2
72 (66–77)	74.5	74.9	88.2	100.0
84 (78–89)	80.5	79.2	88.2	100.0
96 (90–101)	80.0	82.4	79.7	100.0
108 (102–)	79.8	85.0	63.1	42.8

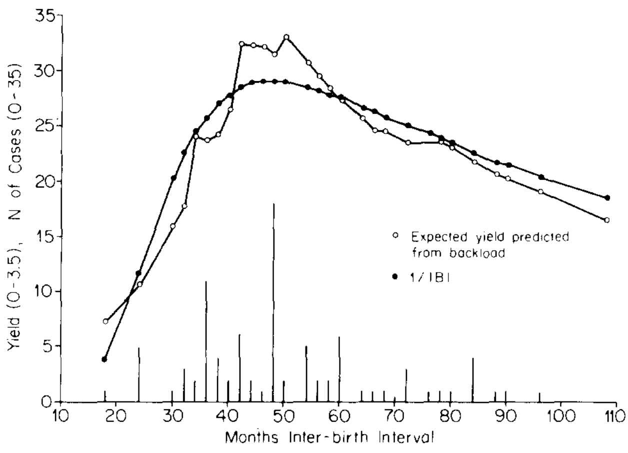

FIGURE 3. Graph of yield of surviving teenagers predicted by the best predictors (backload and 1/IBI), and bar chart of frequency of occurrence of each interbirth interval. Ungrouped data.

Optimal Interbirth Intervals

Ungrouped data. The yield of each IBI predicted by the best two predictors (backload and 1/IBI) is shown in Figure 3. This indicates that maximum yields derive from the intervals around 50 months.

Grouped data. For the grouped data, Table 5 and Figure 4 show that the best predictors (backload and 1/IBI again) give the highest yield from the 36-, 48-, and 60-month groups. The observed raw data (column six in Table 5) suggests that yet longer intervals yield better. This is discussed below.

Is Performance Optimal?

Ungrouped data. The match with optima predicted from concave curves (1/IBI and backload) is remarkably close (see Fig. 3). For yield predicted by backload the optimum IBI was 50 months. For yield predicted by 1/IBI equal yields arose from 46, 48, and 50 months. Interbirth intervals from 42 to 54 months yielded almost as highly. Intervals of 20 and 30 months are predicted to yield poorly, and intervals of 80, 90 and 100 months yield poorly. These very short intervals are markedly less abundant than the 40 to 60 range.

The frequency distribution of IBI has a mean value of 55 months. The mode and median is 48 months.

Grouped data. The highest yield predicted by backload and 1/IBI is from the 48-month group: prediction from the polynomial gives 60 months, closely followed by 48 months (Table 5, Fig. 4).

The block with most cases is the 48 (42–53) month block, followed by

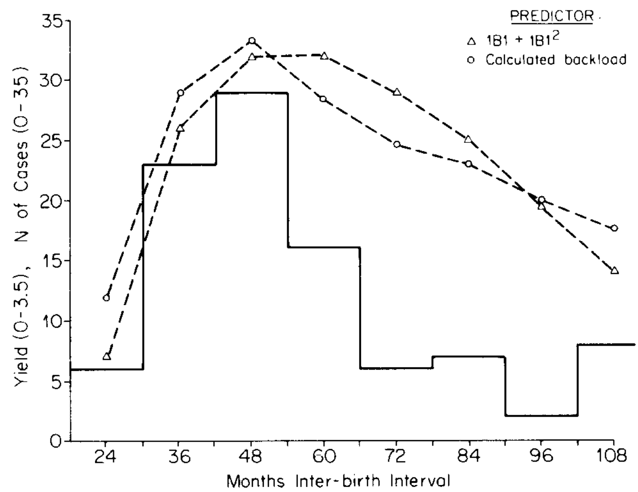

FIGURE 4. Graph of yield of surviving teenagers predicted by the best predictors (backload and 1/IBI), and histogram of frequency of occurrence of each interbirth interval. Grouped data.

the 36 (30–41) month block, and then the 60 (54–65) month block. All other blocks contain only a handful of cases.

The correlation method, applicable to the grouped data, suggests that the performance of the women fits very closely with the optimum when yield is calculated from the predictions of backload, 1/IBI, and the polynomial (r = 0.8102, 0.8065, and 0.5817, respectively. Yield predicted by the raw data gives a surprisingly low correlation r = 0.2088 (Table 5).

Table 6 shows what proportion of intervals yield at 80% or more of the maximum yield. This again suggests that performance matches optimal yield rather well.

Are Even Longer Intervals Really Better?

If any of the fitted curves (1/IBI, backload, two-term regression) is the right summary of the data, then the !kung optimize with remarkable precision.

Table 5. Yield of Surviving Offspring from Each Grouped Interval According to Each Predictor

IBI	N	Backload	1/IBI	IBI + (IBI)^2	Observed
24	6	1.187	1.468	0.729	1.67
36	23	2.921	2.982	2.656	2.60
48	29	3.352	2.996	3.213	2.76
60	16	2.883	2.752	3.217	2.25
72	6	2.482	2.493	2.944	3.33
84	7	2.302	2.265	2.522	2.86
96	2	2.000	2.060	1.993	2.50
108	7	1.772	1.888	1.400	0.95
Correlation between N and yield:					
R		0.8102	0.8065	0.5817	0.2088

Table 6. Percentage of Intervals That Yield More Than 80% or 70% of Maximum Yield[a]

Percent of Maximum Attained	Yield Predicted by		
	Backload	1/IBI	IBI + (IBI)^2
Predictions from Grouped Data			
80%	71%	77%	77%
70%	74%	84%	84%
Predictions from Ungrouped Data			
80%	46%	73%	
70%	70%	85%	

[a] Example: the top leftmost figure, 71%, means that 71% of all the 96 intervals were of a length that yields more than 80% of the highest yield of any interval. The highest yield was 3.35: 80% of this is 2.68. Intervals in the following groups yield higher than 2.68: 36, 48, and 60. These groups contain 68 of the 96 intervals, which is 71% of the 96 intervals.

But the raw observations suggest that the 72-month interval is optimal, and IBIs of this length are very rare. If 72 months is optimal, the !kung fail to optimize. They would fail in an unexpected direction: most of the time they do not wait long enough between births. They cluster around the second best region, 48 months. Do the six cases at 66–77 month intervals give a reliable estimate of mortality?

Several arguments enable us to decide the meaningfulness of the 72-month observation:

1. The 72-month observation is not significantly different from the 60-month observation. This is perhaps an unfairly harsh test, for if 72 and 84 are combined they are significantly different from 48 and 60 months (Fisher's exact test, $p = 0.006$).
2. It is hard to believe that mortality ever falls to zero, or even near it, under the circumstances of !kung life.
3. Deaths at the 72-month interval occurred in cattlepost people and in first intervals (to be reported elsewhere). Overall mortality would be expected to be lower in cattlepost people and yet we have a death at 72-month IBI.
4. Mortality would have to fall below 17% at 72 months (less than 17% failing to survive from birth to 10 years of age) for this interval to yield more descendants than 48 months.
5. An observation of just one death at 72-month interval would give a mortality of 16.7%, which renders 72 months no better than 48 months.

Thus perhaps we should not take 72 months too seriously as a candidate for optimal IBI.

DISCUSSION

The common-sense hypothesis about !kung birth spacing—reproductive success will be maximized by short interbirth intervals, therefore long intervals are not optimal and the !kung do not maximize reproductive success—is not supported by the data. Each analysis shows that short intervals leave fewer descendants than do long intervals. It is clear that the !kung do not show maladaptively long interbirth intervals, nor do they show a random scatter of intervals. The !kung interbirth intervals have a slightly skewed but nearly normal distribution about the most likely optimum interval.

All the potential errors and problems in my analysis combine to suggest that if the performance is not actually optimal then it errs in the direction of too many short intervals and not enough long ones. For example, in all models the 60-month intervals yield as well as do the 36-month intervals. Yet there are more intervals in the 36-month group than in the 60-month group. If we discard all the very long intervals (of greater than 90 months) than the calculated optimum is 72 months. There are very few intervals of this length. I have already discussed reasons for regarding the observed zero mortality at this interval as unrepresentative. But that the errors go in this direction strengthens our disproof of the "common-sense" hypothesis. Again, if the bush-living !kung women fail to optimize it is because their interbirth intervals are too short, not because they are too long.

In this analysis I have ignored variation in length of reproductive career. Yet Howell's data show that this is clearly a major determinant of lifetime reproductive success. This in turn should make us ask why anyone stops reproducing earlier than anyone else. A number of questions and predictions can be derived from the backload model and from the proposition that investment in grandchildren may rapidly outweigh the value of investment in further children. But at present the issue of variation in length of reproductive career is better explained by ideas such as "target fertility" and "replacement," which postulate that reproduction is aimed not at the maximum possible but at a target family size, or at attaining at least some adult descendants.

Several other topics for discussion arise from this article but can only receive brief mention here:

1. How good a measure of fitness is the number of children raised to 10 years of age? I was unable to examine reproductive success of the children of the intervals, or the risks of maternal death associated with different interbirth intervals.
2. Does theory really lead us to expect performance to be normally distributed about the optimum? Are the individuals who produce intervals that are far from the optimum failing to be adaptive or are they doing the best that they can under circumstances that actually differ from those of the majority?
3. Only if mortality reaches very high levels at short IBIs, the graph of mortality against IBI having at some point a very steep slope, will selection favor lengthening IBI. A comment in Hobcraft et al. (1983, p. 606) suggests that this was not the case for the societies from which their data came.

4. The "gee whiz" aspect of testing optimization is probably less important than the original modeling exercise of Blurton Jones and Sibly. The direct reproductive data show it was not entirely fanciful to suggest that 4-year interbirth intervals were optimal for the !kung. But this is mainly important for the encouragement that it gives to efforts to find out whether the !kung could somehow rearrange their lives to leave more descendants, and if not, why not. These efforts could lead us to a detailed and precise knowledge of the practical constraints on !kung life.
5. Why does the backload model make such good predictions? The backload model succeeds in producing a remarkably close fit to the curve of mortality against IBI (and in making several other correct predictions to be reported elsewhere). Backload as a predictor accounts for an amazing 55% of the variance in mortality of children under 10 years old born of intervals selected for this sample. It may thus be that mother's load really is an important influence. But we may wonder by what causal route it influences mortality, and time to next conception, and what are the significant correlates of backload? Is some other measure of mother's work really the crucial variable, such as the amount of time or energy her work leaves for other fitness enhancing activities? Can other costs of parenting substitute for backload? The crucial issue may be the shape and dimension of the curve of costs of parenting against age of child, rather than the particular nature of the parental behavior and its particular costs. Although weight of food seems to be an important component of the Blurton Jones and Sibly model, considering a wider taxonomic spectrum accentuates this question. Chimpanzees have longer IBIs than do humans (Tutin 1980). Since chimpanzees do not provision their families, mother's work as a provider cannot be the explanation of the long IBI, although her work at carrying her offspring might be. However, to try to cover too wide a taxonomic sample might lead us to ignore the extreme variation in IBI within and between human populations. This seems to be an extremely labile characteristic, and clearly of great potential adaptive significance.

One way to approach these questions is to examine mother's work and IBI in other populations. It is often claimed that all hunger-gatherers have such long IBIs. The Blurton Jones and Sibly backload model is very specific to !kung ecology and geography, but if it applies at all to other hunger-gatherer cultures it predicts shorter IBIs whenever women do not have to carry so much food for so far as do the !kung women. At present there are no quantitative data published on the IBI of other hunter-gatherers.

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