
Inuit Sex-Ratio Variation

Population Control, Ethnographic Error, or Parental Manipulation?¹

by Eric Alden Smith
and S. Abigail Smith

Historical censuses of Inuit (Canadian and North Alaskan Eskimos) often contain highly male-biased juvenile sex ratios that have been interpreted as evidence of female infanticide. We use model life tables to estimate elements of historic Inuit population structure missing from the censuses themselves and use these estimates to examine the major explanations for Inuit sex-ratio bias found in the literature. The argument that sex-ratio bias is primarily an artifact of incorrect age assessment due to early marriage of females is not upheld, although this factor appears to account for some proportion of the bias. Psychological explanations based on male dominance fail to explain variation among Inuit populations. Functional explanations that portray female infanticide as a form of population regulation or as a device for balancing the numbers of adult men and women are inconsistent with empirical evidence as well as ecological and evolutionary theory. We conclude that estimated rates of female infanticide (ranging from 0 to 40% and averaging 21% for the ten populations analyzed) are best explained as consequences of parental efforts to match the number of sons with locally prevailing but regionally variable rates of sex-specific mortality and economic productivity. We argue that these findings have broad significance for the analysis of sex-specific parental investment in many human populations.

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The most glaring consequence of the struggle for existence is manifested in the way in which [the Netsilik] try to breed the greatest possible number of boys and the fewest possible girls. For it is solely economy that lies behind the custom that girls are killed at birth. . . . The reasoning that lies behind infanticide is as follows: A female infant is only a trouble and an expense to the household as long as she cannot make herself useful. But the moment she is able to help she is married and leaves her family; for it is the rule that the woman goes with the family into which she has married. For this reason they try to regulate births in order to get as many boys as possible.

KNUD RASMUSSEN, *The Netsilik Eskimos*, 1931

In the literature on human demography, Inuit occupy a special place.² This is due primarily to their selective infanticide and senilicide, which have often been attributed to the extraordinary ecological pressures Inuit are alleged to have faced prior to the coming of European mercantile and governmental institutions to the Arctic. Some investigators have presented evidence of extremely high rates of infanticide, directed mostly at female offspring, and have seen in this a form of adaptation to scarce subsistence resources. This in turn has inspired claims that infanticide as a means of population regulation may have been widespread since Paleolithic times. Others have interpreted female infanticide as a means of sex-ratio manipulation for personal or social ends of one kind or another. Finally, a small but growing number of investigators has questioned whether the rates of infanticide estimated for various historic Inuit groups were anything but an aberration or even an artifact of inadequate census techniques.

This paper has several aims. We will summarize the earliest available census data for various Inuit groups and discuss the general evidence for ascribing the biased sex ratios recorded in most of these censuses to infanticide. We will go on to examine the census data through the lens of model life tables, in order to reconstruct the age-sex structure of historic Inuit populations more fully, and then use these results to estimate the amount of infanticide and its relative importance in comparison with differential age at marriage. Finally, we will examine several alternative explanations for biased sex ratios: population regulation, balancing the adult sex ratio, differences in cost of male versus female offspring, and differences in benefit. To the extent possible, hypotheses derived from these theories will be tested against the Inuit census data.

2. Following recent scholarly practice and indigenous preference, we use the term "Inuit" to refer to any of the various peoples whose native language is Inupiaq/Inuttit and whose aboriginal homeland extends from northwestern Alaska across Arctic Canada through Greenland (Damas 1984a:7). Although "Inuit" is a plural noun meaning (roughly) "the people," in conformity with standard practice we also use it as an adjective, reserving the singular form ("Inuk") for referring to individuals.

Inuit Population Structure

INTERPRETING THE CENSUS SEX RATIOS

The existence of female infanticide among Inuit groups at the time of first Euro-American contact is postulated on the basis of rather limited direct evidence. It is clear that female infanticide, to the extent that it occurred, has disappeared, and therefore any discussion of the practice depends on data gathered between about 1880 and 1930. There are two major sets of such data: the reports of the Fifth Thule Expedition of 1921–24 (Birket-Smith 1929; Rasmussen 1931, 1932), including a study of infanticide among the Netsilik by Rasmussen (1931), and a compilation by Weyer (1932) of early censuses which also included the results of his own field study (see appendix for sources).

The Fifth Thule Expedition reported female infanticide as customary among the Netsilik (Rasmussen 1931: 139–42), their inland neighbors the Utkuhikalik (Rasmussen 1931:489), and various subdivisions of the so-called Caribou Inuit (Birket-Smith 1929:294). Rasmussen deduced a high rate of female infanticide (about 66%) from the reproductive histories of 18 Netsilik women (1931:141) and recorded several anecdotes about infanticide from informants. This study has been the primary source for much subsequent discussion of Inuit female infanticide (for example, Balikci 1970, Freeman 1971), although there are numerous other anecdotal reports from various early explorers and travelers (see especially Weyer 1932:146–49 and Oswalt 1967:193–94 for references). According to Schrire and Steiger (1974), there is only one firsthand published observation of an instance of female infanticide, reported by Diamond Jenness (1922).

More influential but less direct evidence for female infanticide has been found in the high ratio of boys to girls in the published census data collected by several early explorers and ethnographers and summarized in Weyer (1932). In many of these censuses, boys significantly outnumber girls. This evidence appears so compelling that later ethnographers (e.g., Weyer 1932; Balikci 1967, 1970; Arima 1984:455) have taken female infanticide as demonstrated.

Recently, however, some investigators have questioned the widespread existence of infanticide. Many argue that the available data cannot be trusted to produce meaningful analytical results. The small sizes of the groups sampled, the difficulties of accurately judging ages under field conditions, and the prevalence of migration in most of the groups are said to make the census data suspect as a basis for standard demographic analyses. A stronger and more general criticism is that male-biased child sex ratios and the female infanticide that purportedly underlies them are essentially illusions induced by the way in which censuses were taken. In particular, the tendency for early census takers to define those not yet married as “children,” coupled with the younger marriage age of females, is held responsible for the bulk of the sex-ratio bias. The most prominent advocates of this view have been Schrire and Steiger (1974,

1981; Schrire 1984), but Damas (1975:412; 1984b:392n) and Remie (1985) have offered similar suggestions. This particular position, which we term the “differential labeling hypothesis,” is discussed at length below.

The earliest detailed census data that exist for Inuit groups consist primarily of secondhand compilations from surveys of population (and sometimes household) composition by various explorers and travelers (Boas 1888, 1901–7). More detailed data begin to appear in the 1920s in the reports of various Arctic expeditions: the Canadian Arctic Expedition of 1913–18 (Jenness 1922), the Fifth Thule Expedition of 1921–24 (Birket-Smith 1929; Rasmussen 1931, 1932), and the Stoll-McCracken Arctic Expedition of 1928 (Weyer 1932). The figures in table 1 are drawn from these and other sources, with cross-checking against unaggregated data in the original sources whenever possible.³ They consist of census data on ten Inuit populations recorded during the period from about 1880 to 1930 (see fig. 1 and appendix). The childhood sex ratios are skewed toward a preponderance of boys in every case, yielding a mean ratio of 173 boys/100 girls.⁴ By contrast, the mean adult sex ratio is female-biased (92 men/100 women), and only 3 of the cases exhibit a (moderate) preponderance of men over women (fig. 2). This indicates a radical alteration of sex ratio from childhood to adulthood in most of these censuses. This sex-ratio reversal has been interpreted in two ways: as a result of higher adult male mortality and as a reflection of differential age at marriage. Here we intend to consider these two causes *simultaneously*.

3. Rasmussen's (1931) data on infanticide among the Netsilik are widely cited, but the accuracy and representativeness of these data have been questioned (Remie 1985, 1988; Schrire and Steiger 1974; Schrire 1984) for three reasons: (1) Rasmussen made errors in arithmetic and left inconsistent records. (2) At the time of his census, Netsilik had been subject to recent population disturbances, particularly extensive migration to the Hudson Bay coast in response to trade opportunities with whalers and fur traders. (3) The environmental and social conditions of the western Netsilik with whom Rasmussen worked were particularly harsh. While we accept these caveats, the analysis presented here is not compromised by these particular defects. First, we have corrected Rasmussen's data with reference to his house lists (Rasmussen 1931:84–90; see appendix). Second, our analysis also includes the census conducted by Comer some 20 years earlier, covering a larger proportion of the Netsilik population and one located in a different area—in fact, the emigrant Hudson Bay population Remie refers to. It is worth noting how closely Rasmussen's and Comer's censuses agree on child and adult sex ratios (table 1) despite the differences in census date, location, and size; this increases our confidence in the general accuracy of these data sets. While it is likely that many of the census figures in our sample do contain errors and distortions of the sort noted by Remie et al., we do not consider these sufficient to justify discarding such data. Rather, they constitute an argument in favor of using a broad sample of populations in order to explore the general outlines and possible causes of regional variation in Inuit sex ratios. In other words, we are not *generalizing inductively* from the Netsilik data (a weak approach, as the criticisms cited show) but testing a set of hypotheses against it *and other data*.

4. Following standard demographic convention (though in contrast to the presentations in Weyer [1932], Schrire and Steiger [1974], and Irwin [1989]), we report sex ratios as males per 100 females. Thus, an even sex ratio is 100, a male-biased (also termed “high”) one is >100, and a female-biased (low) sex ratio is <100.

TABLE I
Census Data for Ten Inuit Populations, 1880–1930

Population	Raw Census Figures					Children/Adult		Sex Ratios	
	Total	Boys	Girls	Men	Women	Boys/ 100 Men	Girls/ 100 Women	Boys/ 100 Girls	Men/ 100 Women
Aivilik	102	27	15	26	34	104	44	180 ^a	76
Baffin Land	288	41	39	101	107	38	39	105	94
Cape Smyth	138	27	14	45	52	60	27	193 ^a	87
Caribou 1900	324	79	60	81	104	98	58	132	78
Caribou 1923	222	61	36	55	70	111	51	169 ^a	79
Copper	501	123	55	165	158	75	35	224 ^a	104
Netsilik 1902	446	138	66	119	123	116	54	209 ^a	97
Netsilik 1923	261	74	37	77	73	96	51	200 ^a	105
Padlimiut	212	59	47	47	59	126	80	126	80
Utkuhikalik	164	52	27	46	39	113	69	193 ^a	118
Mean	266					94	50	173	92

SOURCES: See appendix.

^aSignificantly male-biased (> 100). Bias is measured by z-score for binomial distribution, with $\alpha = 0.5$, one-tailed. Padlimiut ratio is just short of significant. None of the Men/100 Women ratios are significantly different from unity ($\alpha = 0.5$, two-tailed test).

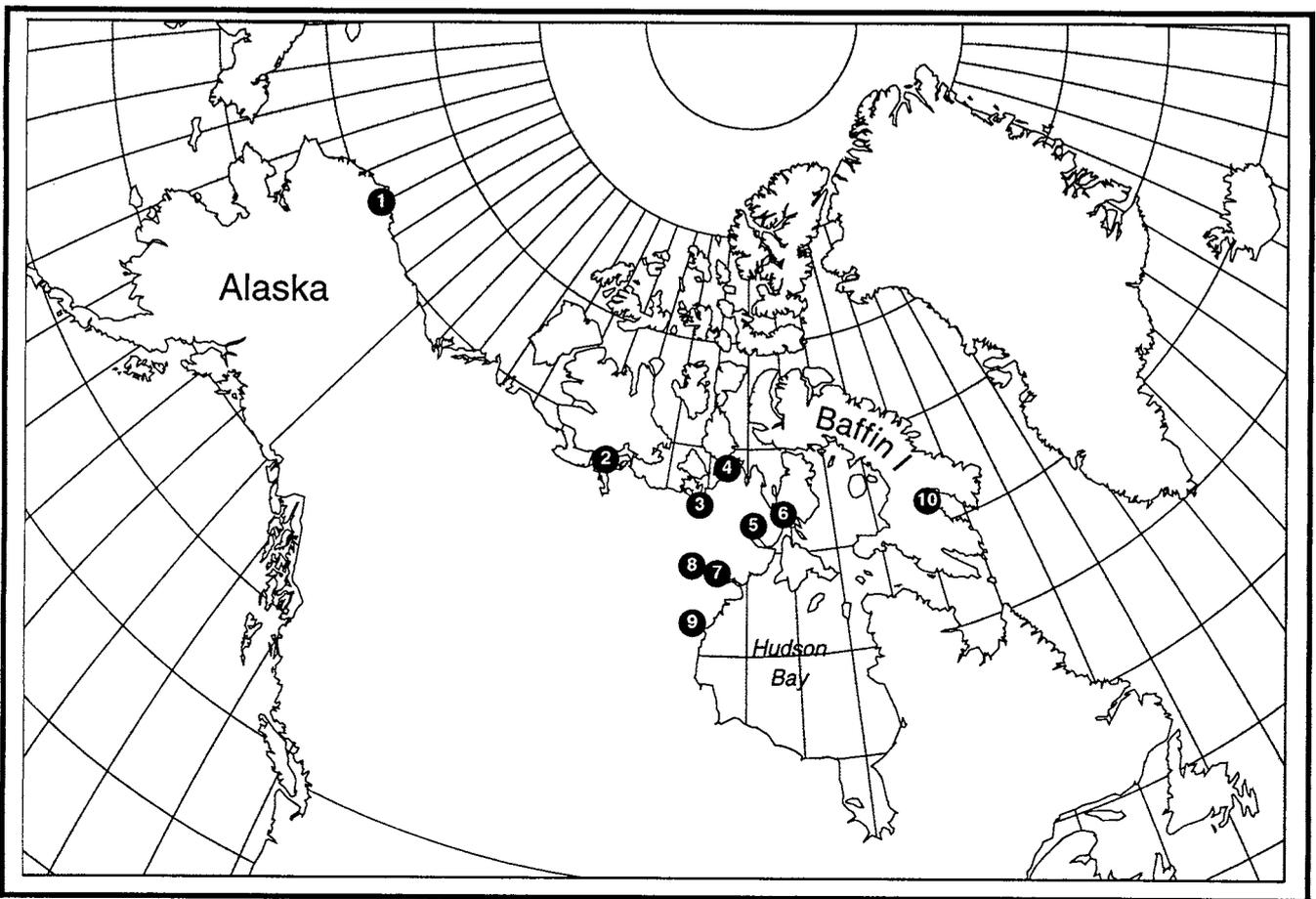


FIG. 1. Approximate locations of census populations. 1, Cape Smyth; 2, Copper; 3, Utkuhikalik; 4, Netsilik 1923; 5, Netsilik 1902; 6, Aivilik; 7, Caribou 1900; 8, Caribou 1923; 9, Padlimiut; 10, Baffin Land.

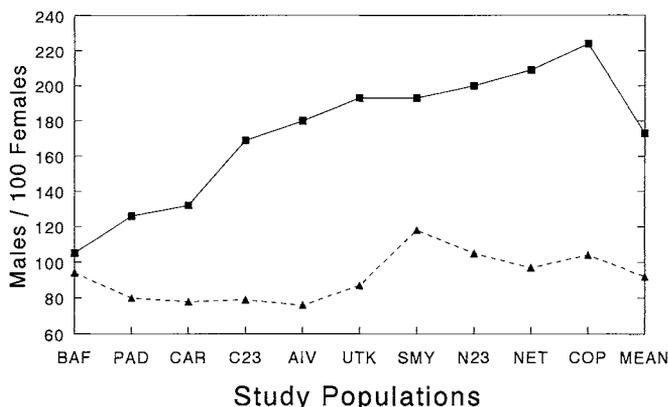


FIG. 2. Census sex ratios. ■, boys/100 girls; ▲, men/100 women. AIV, Aivilik; BAF, Baffin Land; CAR, Caribou 1900; C23, Caribou 1923; COP, Copper; NET, Netsilik 1902; N23, Netsilik 1923; PAD, Padlimiut; SMY, Cape Smyth; UTK, Utkuhikalik.

What evidence is there in these data, sparse as they are, that female infanticide was the cause of the observed high child sex ratios? Without explicit behavioral evidence, we cannot pinpoint infanticide per se as the cause of unbalanced child sex ratios; various other mechanisms for creating systematic differentials in the ratio of boys to girls would also be consistent with the census data. These might include unusually male-biased primary (conception) or secondary (birth) sex ratios, elevated mortality in girls due to unidentified natural causes, or various parental actions causing elevated mortality of female offspring short of direct infanticide (so-called selective neglect). For a number of reasons, we view parental action (including but not necessarily limited to infanticide) as the most likely candidate.⁵ How-

5. The "normal" secondary sex ratio (birth ratio) for the great majority of well-enumerated human populations falls around 105:100, although there is some variation (Chahnazarian 1988, Sieff 1990). Given the lack of age-specific data and the potential biases caused by infanticide or sex-biased mortality as well as differential age at marriage, we cannot use the census data themselves to verify the appropriateness of assuming a 105:100 secondary sex ratio. The best evidence on this point that we have located is in Birket-Smith (1928:23), who cites a large composite age-specific census for Greenland Inuit in 1921 ($n = 13,927$) to show that "the birth rate, as in Europe, amounts to 100 girls as against about 106 boys." Greenlanders at this time were still subsisting primarily by hunting and fishing but because of Christian influence and Danish legal control did not engage in infanticide to any known degree. The wide variation in Inuit child sex ratios revealed in table 1, the recency of Inuit dispersal throughout the Arctic region (ca. 1,000 years or less before the period of census taking) from a single North Alaskan source, and the existence of some intermarriage between areas argue against the view that genetically fixed differences in secondary sex ratios can account for the skewed child sex ratios found among some Inuit populations (Irwin 1989). As for differential mortality from natural causes, again the evidence from a number of human populations indicates that this falls more heavily on males from conception on; as a consequence, the 105:100 secondary sex ratio usually approaches unity by the time of sexual maturation in most human populations for which data are available (Fisher 1930).

ever, at this point our primary concern is to determine the likely age-sex structure of the various Inuit populations. Thus, we rephrase the question of interest as follows: What evidence do the census data provide that reported variations in sex ratios between groups and (by age-class) within groups are systematic rather than random or spurious? We feel that the evidence is considerable.

First, the difference between the mean child sex ratio (173 boys/100 girls) and the mean adult sex ratio (92 men/100 women) is very large and statistically highly significant.⁶ Second, the child sex ratio is significantly male-biased in 70% of the censuses, whereas the man/woman ratio is not significantly different from unity in any (table 1). Third, if high child sex ratios were occurring by chance or were a result of high sex ratios at birth, we would expect that on average groups with high child sex ratios would also have high adult sex ratios. In fact, there is no significant correlation between census child and adult sex ratios ($r_s = 0.526, p > 0.5$), suggesting that whatever factors acted to produce high child sex ratios in some Inuit groups, they (or something else) also acted to produce more even sex ratios among adults.⁷

In light of the weaknesses in the early census data, much of the recent discussion of Inuit female infanticide has involved analysis of models of hypothetical populations with various assumed vital rates and infanticide levels rather than of actual census data. Schrire and Steiger (1974) developed a stochastic simulation from which they concluded that populations practicing systematic female infanticide at rates higher than about 8% could not sustain themselves for more than a few generations. Chapman (1980) criticized this study, pointing out that it assumed constant fertility levels regardless of the incidence of infanticide. He argued instead that women are likely to shorten the interval to the next birth in response to death of an infant, thus at least partially counteracting the higher mortality associated with the practice of infanticide. Chapman offered his own simulation incorporating these assumptions, from which he concluded that female infanticide could persist at levels higher than 30% without driving a small population to extinction.

6. A paired t -test of the difference between census boy/girl and man/woman sex ratios indicates that these two measures have very different distributions across this set of populations ($t = 7.421, p << 0.001$, two-tailed t -test with 9 degrees of freedom). Mean sex ratios here and throughout are weighted by populations rather than by individuals.

7. Schrire and Steiger's (1974) differential-labeling hypothesis would seem to predict a *negative* correlation between census child and adult sex ratios: all else being equal, labeling females as "women" at an earlier age than males will produce a male-biased ratio among children and a female-biased one among adults. Just such a pattern is found in the Greenlandic data summarized by Weyer (1932), who accounts for this as the result of the "rarity of infanticide" due to "Christian influence" (pp. 135-37). In any case, the weak (and statistically nonsignificant) positive correlation noted here suggests that the differential-labeling hypothesis is at best only partially correct.

In models such as those used by Schrire and Steiger and by Chapman, the assumptions made about mortality and fertility rates determine a unique equilibrium point. Chapman's results are significant in that they demonstrate that there exists at least one set of demographic assumptions that includes female infanticide at rates of up to one-third of girls born and allows maintenance of small populations for long time periods. But the value of simulation models for determining infanticide rates in the absence of accurate mortality and fertility data is questionable (as Chapman himself recognizes); this is because simulations using constant rates over long time periods are extremely sensitive to small changes in mortality and fertility assumptions, as Chapman essentially showed.⁸ Acker and Townsend (1975) suggest instead analyzing a deterministic model using different mortality and fertility functions to discover the range of assumptions (including different levels of female infanticide) that produce equilibrium populations.⁹ This suggestion is taken up in a later section, where we employ model life tables to evaluate a number of claims about historic Inuit censuses.

THE DIFFERENTIAL-LABELING HYPOTHESIS

Having concluded that high rates of infanticide could not be sustained, Schrire and Steiger (1974) suggested that most of the sex-ratio bias reported in historic Inuit censuses was the result of differential labeling of adoles-

cent males and females, married females being counted as women while unmarried males of the same age were counted as boys.¹⁰ In other words, what appeared to the census takers as fewer girls than boys and has been interpreted by most subsequent analysts as evidence of female infanticide is alleged to be an artifact of the tendency for Inuit females to marry several years earlier than males. To our knowledge, this hypothesis (plausible and appealing enough on the face of it) has not been subjected to any sustained test, a task we take up here.¹¹

We first examine whether differential labeling as a function of marriage age is sufficient to account for the observed childhood sex ratios. We then use the census data to determine the age-sex structure of each census population and employ model life tables to explore the demographic parameters necessary to produce these structures. This analysis serves to answer several pertinent questions: What model life tables provide the best fit to the population structures indicated by the census data? Does the inclusion of female infanticide improve the fit of the analysis to the data, and if so, at what rates? Is differential age at marriage (as hypothesized by Schrire and Steiger) a likely determinant as well?

If we correct for the hypothesized distortion of females' systematically being labeled adults earlier than males by shifting enough women into the category of girls to make the child sex ratio even (table 2), we observe several converging lines of evidence indicating that the differential-labeling hypothesis is not a plausible or complete explanation for the high child sex ratios (and low adult sex ratios) found in the census data. First, there is very wide variation in the proportion of the females in each Inuit group that must be shifted in order to even the child sex ratio, from 1% (Baffin Land) to 38% (Netsilik 1902 and Utkuhikalik). Using life-table analyses, we estimate the required marriage-age differential to range from 0 to 12 years. However, this would require some implausible assumptions about mortality rates (see below). The general cultural similarity of historic Inuit groups and available ethnographic information on age at marriage¹² make it unlikely that this pa-

8. In response to Chapman's analysis, Schrire and Steiger (1981) have argued that his correction for increased fertility from infanticide (due to shortened interval between births) is unwarranted for a number of reasons and in any case it is "misleading" to term the differential in fertility rates between their model and Chapman's "small," since "the large differences in model performance are directly due to large differences in parameter values" (pp. 113-14). We fail to see the logic of Schrire and Steiger's objection. Chapman's results depend on two differences from theirs: higher age-specific fertility rates (yielding 7.66 live births for birth intervals not shortened by infanticide, compared with Schrire and Steiger's 7.2) and a correction for interbirth intervals when infanticide interrupts the suppression of ovulation through nursing. The former is clearly a small difference, and, as Chapman (1980:321) notes, his figure is closer to the limited empirical data on total female fertility available for Inuit (see n. 18). The latter obviously varies in its effect depending on the rate of infanticide assumed, but Chapman calculated 8.64 live births at a 30% infanticide rate and a maximum of 15.46 if all infants were killed. Finally, Schrire and Steiger (p. 115) charge Chapman with translating greater "stress" in the form of "a prevailing shortage of food" into higher fertility, whereas all he argues is that among Inuit—as is known to be the case among other human populations—infant death, including infanticide (which need not be motivated by resource scarcity), decreases the interval to the next birth.

9. As Acker and Townsend (1975) point out, that equilibrium would be the mean or expected outcome of many trials of the stochastic model and can be determined analytically from the mortality and fertility functions specified. The stochasticity incorporated in Schrire and Steiger's and Chapman's models is needed to determine what the chances of extinction might be, given these vital rates and certain assumptions about the form and magnitude of their stochastic variation. Stochastic simulation is not needed, however, to answer other questions, such as the relative contributions of sex-biased mortality and aging errors addressed here.

10. As noted above, Damas (1975) independently presented the same argument.

11. Initially Schrire and Steiger (1974) proposed the differential-labeling hypothesis as a plausible resolution of the data for 8 of the 12 Inuit groups they discussed, those with census child sex ratios ranging from 96 to 193 boys/100 girls (mean = 137), while 4 other groups with more biased ratios (203-268, mean = 223) were accepted as likely having significant amounts of female infanticide (Schrire and Steiger 1974:table 1). However, they directly tested the differential-labeling hypothesis only with reference to a Baffin Island population never reported to have engaged in female infanticide and with a child sex ratio of 120. By the time of their reply to Chapman, they go so far as to refer to their own study with the claim that "it is rare to be able to rebuff popular myths with such elegance" (Schrire and Steiger 1981:115), despite the fact that their simulation model does not even incorporate differential marriage age.

12. There is some ethnographic evidence to support our assumptions about age at first marriage and hence the division points between childhood and adulthood. Rasmussen's Netsilik monograph includes two photographs with the caption "Two girls of

TABLE 2
Census Figures Adjusted to Equalize Child Sex Ratios

Population	Females Shifted ^a	Percentage Shifted ^b	Adjusted Men/100 Women	Census Men/100 Women
Aivilik	12	24	118	76
Baffin Land	2	2	96	94
Cape Smyth	13	20	115	87
Caribou 1900	19	12	95	78
Caribou 1923	25	24	122	79
Copper	68	32	183	104
Netsilik 1902	72	38	233	97
Netsilik 1923	37	34	214	105
Padlimiut	12	11	100	80
Utkuhikalik	25	38	329	118
Mean		23.4	160.6	91.8
Standard deviation		11.8	73.7	13.6

^aNumber of census "women" that must be moved into "girls" category to create an even child sex ratio (100 boys/100 girls).

^bPercentage of females shifted into "girls" category (i.e., [females shifted × 100]/total census females).

parameter varied sufficiently to produce such a wide range of census sex ratios.

Second, when the child sex ratios are evened by shifting women into the "girl" category, the resulting adult sex ratio is male-biased (and generally highly so) in all but two cases. This would require that adult female mortality be higher than that of adult males. Such a pattern is contrary to all the available ethnographic evidence, which indicates instead that mortality rates of men were higher than those of women because of the frequency of hunting accidents and homicides (e.g., Arima 1984:455; Balikci 1970:152-53; Birket-Smith 1928:23-26; Damas 1984a:392; Weyer 1932:127-28).

Finally, if differential labeling were the sole or primary explanation for high childhood sex ratios, we would expect the number of females of marriageable age to be somewhat greater than the number of men. To the

extent that men suffer higher mortality through hunting accidents and homicides, there should in fact be an excess of marriageable females (assuming roughly equal mortality of males and females during childhood and low levels of polygyny). The ethnographic reports indicate that exactly the opposite is the case: in many groups, females of marriageable age are scarce, and males fight bitterly over women (e.g., Weyer 1932, Steenhoven 1959, Balikci 1970). Even more telling, competition for females appears most intense precisely in those groups in which the child sex ratios are highly male-biased—the opposite of what one would predict from the differential-labeling hypothesis but precisely as expected if female infanticide (or some other factor reducing the relative number of females graduating through the population) were taking place.

We conclude that even if some differential labeling did occur (which is likely in any census lacking precise age categories), this cannot entirely, or even primarily, account for the skewed childhood sex ratios we see in this set of data. Instead, we are led to suggest that females in most of these populations must have been subject to significantly higher mortality than males at birth or during childhood (or, alternatively, significantly male-biased birth ratios [but see n. 5]). In other words, we find the data inconsistent with the idea that differential labeling alone is responsible for the pattern of sex-ratio bias in the census data.

In sum, the census figures seem to indicate a systematic male bias in the child sex ratios *greater than could result from differential age at marriage* (at least in most groups) and a systematic pressure towards unity in the adult sex ratios *even in cases with highly skewed child sex ratios*. We do not dismiss the possibility that differential age at marriage and differential labeling in cen-

about fourteen, the marriageable age" (1931:472). According to Ma-laurie (1956:97), writing about Northern Greenland Eskimos, "only at about twenty or twenty-four years, when the young hunter is capable of keeping a family, does marriage take place." Arima (1984:455) states that marriage among Caribou Eskimo usually occurred "when the girl reached puberty," but he does not indicate the typical age of the groom. Citing various sources, Gilberg (1984: 586-87) notes that among the Polar Eskimo "girls often married between the ages of 12 and 16" while "most men did not marry until their midtwenties," and he attributes this differential in part to "the excess of males in the population." Damas (1975:412) states that "girls married quite young in the areas where the sexes were skewed, even before puberty in some cases" but that young men were often unmarried at eighteen or twenty. Similar (if sometimes vague) statements occur elsewhere in the literature on Inuit. In addition, several of the censuses take the form of household surveys, which typically include one man, one woman, and a few children, providing further reason to assume that marriage was equivalent to "adulthood" as defined in these censuses.

suses resulting therefrom played some role in biasing the sex ratios recorded in historic Inuit censuses. But in order to assess the likely contribution of this factor relative to such factors as parental sex-ratio manipulation and adult mortality differentials, we need to estimate demographic details not revealed directly by the censuses.

LIFE-TABLE ANALYSES

To explore the possible causes of Inuit sex-ratio variation, we devised a method of fitting the census data to model life tables. We began from the fact that the censuses, incomplete as they are, nevertheless provide five measures that describe the age-sex structure of each population: child sex ratio, adult sex ratio, boy/man ratio, girl/woman ratio, and (lumping the sexes) child/adult ratio. We assumed a standard human birth ratio of 105:100 (see n. 5). Following Schrire and Steiger (1974), we assumed that children "graduated" to the adult category in the censuses when they married. Age at marriage was set initially at 15 for girls, 20 for boys, and adjusted only when doing so produced a better fit of model to data (see below). For each population listed in table 1 we identified the mortality schedule from Weiss's (1973) model life tables for anthropological populations that produced the closest match to the observed (census) ratio of boys to men. We then selected the combination of juvenile and adult mortality schedules for females that produced a ratio of girls to women closest to the observed ratio.¹³

At this point we found that in most of the model populations the ratio of children to adults (the two sexes combined) matched the census figures closely but the child and adult sex ratios were too low—in other words, there was an overall excess of females, both girls and women. Increasing the model mortality rates for girls could not correct this, as it had the effect of reducing the number of girls relative to the number of women, thus failing to match the census girl/woman ratio. The only adjustment that would preserve the ratio of girls to women while reducing the overall number of females in the population was to lower the number of girls entering the youngest age-class (the analytical equivalent of female infanticide).

The results of this analysis are shown in table 3. There we list, for each of the ten populations, the model mortality schedules for each sex, ages at marriage, and the computed female infant reduction necessary to produce the best fit to census figures. Also listed are the five ratios computed from the raw census data (see table 1) and the ratios produced by the model life tables. For each population, the sum of the absolute difference between

census data and model life table for these five ratios ranges from 1 to 5 and averages 2.7 (i.e., ≈ 0.5 point difference for each ratio), indicating a very close fit.

The most notable feature of these calculations is that the female infant reduction or infanticide rate needed to match the census ratios is much lower than the rates often inferred from the census child sex ratios (fig. 3). The female infant reduction estimated with model life tables ranges from 0 to 40%, with an average of 21%. It has been assumed by many previous researchers (e.g., Balikci 1967, Birdsell 1968, Weyer 1932) that the female infanticide rate must be the difference between the number of girls required to create a balanced child sex ratio and the number recorded in the census. To take one extreme case, that of Cape Smyth, the rate extrapolated from the census would be 48%, in contrast to the estimated value of 5%. By contrast, the Utkuhikalik have the identical census child sex ratio, but the best-fit model life table requires a 40% reduction in female infants (still about one-sixth lower than the 48% rate derived via direct extrapolation from the census data). And in the famous case of Rasmussen's 1923 Netsilik census, the traditional figure of 50% can be lowered to 29%.

On average, the model-life-table method yields estimated female infanticide rates about half the size of those inferred from the raw census data (averaging 21% vs. 39% [fig. 3]). This striking difference between the two sets of estimates is due to the inclusion of differential age at marriage in fitting the model life tables. In this respect, then, Schrire and Steiger's (1974) suggestion that researchers must consider the role of differential age at marriage in interpreting Inuit census data is given considerable quantitative support here; at the same time, the results are consistent with our argument that marriage age alone cannot account for biased sex ratios in the majority of these cases. In our analysis, only one population (Baffin Land) has a best-fit model life table with a marriage-age differential but no female infant reduction, no model population has female infant reduction but no marriage-age differential. The remaining nine populations require *both* differential age at marriage and female infant reduction to achieve a best fit. It is possible to get some idea of the relative importance of differential age at marriage and female infant reduction by fitting model life tables without allowing one or the other factor to vary. In the case of the Aivilik population, which has a female infant reduction estimate (21%) matching the ten-population average of 20.7%, eliminating this factor (i.e., using the standard 105:100 child sex ratio) in the model-life-table analysis reduces the fit to census data substantially, while eliminating the differential age at marriage makes the fit even worse; eliminating both factors dramatically reduces the ability to fit a model to the census data (table 4).¹⁴

13. Weiss (1973) provides 72 life tables, which represent the combinations possible from 9 age-specific mortality schedules for juveniles (ages 0–14) and 8 schedules for adults. All analyses were conducted using Lotus 1-2-3 and Quattro spreadsheets to match census data with model life tables through iteration, selecting the juvenile and adult schedules with the closest fit to the census data for each population.

14. Not only does eliminating differential age at marriage and/or female infant reduction greatly impair the fit of a model life table to the Aivilik census data, but the best fits possible are less *realistic*, given that they require a female infant reduction of 64% and male mortality roughly twice that of females at all ages (if no differ-

TABLE 3
Summary of Model-Life-Table Analyses

Group	Boys/ 100 Men ^a	Male Survivor- ship ^b		Girls/ 100 Women ^a	Female Survivor- ship ^b		Estimated Age at Marriage		Female Infant Reduction (%)	Boys/ 100 Girls	Men/ 100 Women	Children/ 100 Adults ^c
		JS	LE(15)		JS	LE(15)	Boys	Girls				
Aivilik												
Observed	104			44						180	76	70
Computed	104	70	25	44	70	35	20	14	21	181	75	70
Baffin Land												
Observed	41			36						105	94	38
Computed	40	65	35	36	70	35	13	12	0	107	95	38
Cape Smyth												
Observed	60			27						193	87	42
Computed	61	70	35	28	70	35	18	10	5	193	88	43
Caribou 1900												
Observed	98			58						132	78	75
Computed	96	40	30	58	45	32.5	20	15	8	131	79	75
Caribou 1923												
Observed	111			51						169	79	78
Computed	112	50	25	52	50	35	20	15	22	169	79	78
Copper												
Observed	75			35						224	104	55
Computed	75	55	35	36	70	35	20	12	39	224	107	56
Netsilik 1902												
Observed	116			54						209	97	84
Computed	115	45	25	54	45	35	20	15	37	208	97	84
Netsilik 1923												
Observed	96			51						200	105	74
Computed	96	60	25	51	55	35	20	15	29	200	106	74
Padlimiut												
Observed	126			80						126	80	100
Computed	127	30	25	81	35	25	20	15	8	126	80	101
Utkuhikalik												
Observed	113			69						193	118	93
Computed	112	50	25	69	60	25	20	15	40	193	119	92

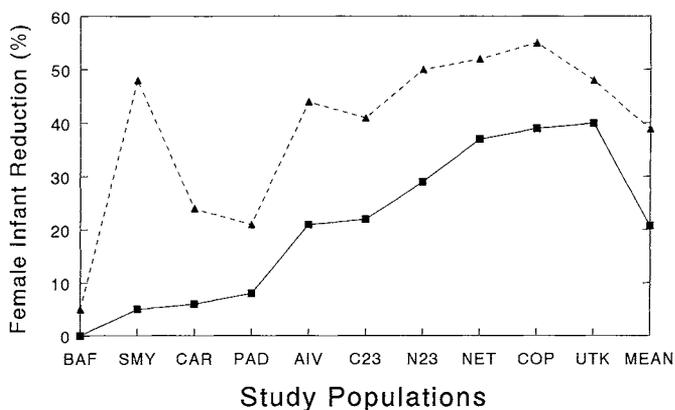
^aAge-classes defined by age at marriage.^bFirst number in each pair represents juvenile survivorship (% born surviving at age 15), second number the life expectancy at age 15 of everyone who lives to age 15, in other words, the *additional* years such an individual is expected to survive. (Higher numbers indicate lower mortality rates.) For each of these numbers Weiss (1973) has derived a corresponding mortality schedule; the two numbers together identify a complete life table that can be located in Weiss's appendix.^cReduction in female infants (from standard 105:100 secondary sex ratio) needed to match the census child and adult sex ratios given the best-fit mortality functions and ages at marriage.

FIG. 3. Female infant reduction (estimated percentage of female infants below 105:100 birth sex ratio). ▲, census; ■, model estimate.

From this analysis, then, we get a picture of Inuit sex-ratio manipulation that is much less extreme than the one traditionally painted. It is clear that a model without female infanticide (or some other factor biasing actual child sex ratios) will not fit the data for most historic Inuit populations, but it is equally clear that female infanticide alone does not fully explain the extremely high sex ratios reported. A combination of factors, some of which may remain forever unknown because of the scarcity of contextual data for the early censuses, is required.

entail age at marriage is allowed) or a differential of 11 years (if female infant reduction is held to zero). We stress again that even with these adjustments the "best" fit possible is not nearly as close as when both factors are allowed to vary.

TABLE 4
Effects of Eliminating Differential Age at Marriage and/or Female Infant Reduction from Model-Life-Table Analysis of Aivilik Population

Source of Estimates	Male Survivorship ^a		Female Survivorship ^a		Estimated Age at Marriage ^b		Female Infant Reduction ^c (%)	Boys/100 Girls	Men/100 Women	Children/100 Adults	Boys/100 Men	Girls/100 Women	Fit ^d
	JS	LE(15)	JS	LE(15)	Boys	Girls							
Census data	—	—	—	—	—	—	—	180	76	70	104	44	—
Model with both factors ^e	70	25	70	35	20	14	21	181	75	70	104	44	2
Model w/o infanticide ^f	70	35	70	35	25	14	0	173	75	68	100	44	14
Model w/o differential age at marriage ^g	35	20	70	35	15	15	64	177	84	72	101	48	19
Model w/o either factor ^h	40	20	30	35	15	15	0	128	80	77	97	61	87

^aSex-specific survivorship rates used in best-fit model life table (see table 3).

^bAges at marriage (boys, girls) used in best-fit model life table (see table 3).

^cEstimated percentage of female infant reduction from 100:105 birth ratio in best-fit life table compared with standard 105:100 birth ratio (see table 3).

^dSum of the deviations between census ratios and computed ratios (the smaller the number, the closer the fit).

^eBest fit presented in table 3, allowing both age at marriage and infant sex bias to vary.

^fBest fit achievable assuming 105:100 birth sex ratio and no sex-biased infant or juvenile mortality.

^gBest fit achievable assuming same age at marriage.

^hBest fit achievable assuming same age at marriage and no sex-biased infant or juvenile mortality.

Sex-Ratio Manipulation: Theory and Analysis

Having established that biased child sex ratios are unlikely to be mere artifacts of census methods in the majority of the Inuit populations discussed, we can turn to the question of why this phenomenon occurred. Various distinct and often competing explanations have in fact been proposed. Aside from the differential-labeling hypothesis discussed above, most explanations of Inuit sex-ratio bias emphasize parental psychology, population regulation, and/or adaptive sex-ratio manipulation.¹⁵

PSYCHOLOGICAL ACCOUNTS

A few researchers have proposed specific psychological processes to account for recurrent female infanticide among Inuit. Thus, Freeman (1971) argued that the immediate cause of this practice was the desire of an Inuk father to "reassert his dominance over his spouse," given his desire for sons as hunting companions, his "envy" of his wife if she should have a daughter as help-

mate and protégé, and the likelihood that "he may feel threatened in his position of dominance" if his spouse comes out ahead (p. 1015). In a commentary on Freeman's piece, Hippler (1972) argued that it was not so much male dominance that was asserted through infanticidal decisions as ambivalence towards women and envy of infants—a psychological complex he considered a by-product of certain aspects of Inuit child rearing.

We certainly do not deny that specific psychological processes or motivational states are causally involved in Inuit infanticide. It may well be that Freeman and/or Hippler have identified important ones. However, neither of these psychological accounts offers much hope of explaining variation in infanticide rates *between* Inuit groups. Since Freeman's analysis was addressed specifically to the Netsilik practice, he might be excused from attempting to explain Inuit infanticide in general. However, a successful explanation of Netsilik infanticide should identify not only putative causes of this practice present in the Netsilik case but also the absence or diminution of these causes when female infanticide is reduced or absent. Yet Freeman's explanation and Hippler's variation on it rely on features shared with other Inuit groups to explain something found only in some of them, and to varying degrees. Given that the specified features of sexual division of labor and child rearing are common to all Inuit, why did some Inuit populations apparently respond with rates of female infanticide as high as 40% while others abjured the practice? Thus, at best, these psychological accounts are insufficient

15. One distinct explanation not treated here is the proposal that female infanticide was aimed at denying female mates to nonkin competitors (Riches 1974; 1982:60–61; Remie 1985). Whatever the merit of this argument, it logically applies only to those relatively few Inuit societies with high rates of (not just preference for) kin endogamy (e.g., cousin marriage) and child betrothal.

or incomplete explanations of the phenomenon in question.

Furthermore, these explanations presume that infanticide is a decision made by the father and is dominated by motivations of gender identity and power, but ethnographic data indicate that this was not always the case. For example, Rasmussen (1931:140) quotes the Netsilik woman Nalungiaq as follows:

Life is short. We all want to be as prosperous as we can in the time we are alive. Therefore parents often consider that they cannot "afford" to waste several years nursing a girl. We get old so quickly, and so we must be quick and get a son. That is what we parents think, and in the same way we think for our children. If my daughter Quertilik had a girl child I would strangle it at once. If I did not, I think I would be a bad mother.

Not only does Nalungiaq suggest that women may be involved in the decision and that economic rather than power relations are of primary concern but she indicates that *grandparents* (and specifically *grandmothers*) may make the decision (Balicki 1967:619-20; Irwin 1989:251-52). These considerations lead us to believe that Freeman's and Hippler's accounts are not just incomplete but incorrect.

POPULATION REGULATION

Population regulation as a rationale for biased sex ratios is based on the inference that population growth rates will be limited much more readily by the supply of women than by that of men (Scrimshaw 1984:454). That is, given the physiological facts of female gestation and nursing and the social facts that polygyny, remarriage of widows, and extramarital reproduction are practiced to at least some degree, it follows that even extensive male mortality may have little impact on population growth rates, whereas limiting the number of females reared almost certainly will affect them. Hence, a population whose members wish to regulate its growth rate but lack effective birth control might turn to female infanticide in order to achieve this end. This argument has been presented numerous times, perhaps most prominently by Harris (1974; Divale and Harris 1976) and Birdsell (1968), and has been proposed specifically as an explanation for Inuit male-biased child sex ratios (e.g., Abernathy 1979:75-76; Weyer 1932:130-31).

Despite its popularity, we believe there are two major reasons to doubt the population regulation explanation. First, it presumes that historic Inuit populations were straining the limits of their resources, making overpopulation a chronic problem or imminent possibility. But given the frequent extreme and unpredictable fluctuations in resource availability documented for the Arctic (e.g., Burch 1972; Freeman 1984:36; Savelle and McCartney 1988:35-36; Smith 1991:chap. 3), such a close balance between resources and population is unlikely. Population regulation in a long-lived, slowly reproducing

species such as ours would involve a considerable time lag between the onset of population/resource imbalance and the relief of reduced population growth. If resource supplies are subject to rapid and unpredictable fluctuations (e.g., due to short-term changes in animal movements, weather conditions, ice cover, etc.), it is hard to envision the regulation of population/resource balance through fertility controls. Population regulation of this sort would require accurate estimates of long-term average conditions in order to adjust fertility (and the supply of fecund women) accordingly.

Instead of regulating themselves near carrying capacity, the scanty available evidence seems to indicate that historic Inuit populations were subject to periodic crashes caused by localized prey scarcity or inclement weather. This type of population dynamics would result in a "sawtooth" growth curve, with the population spending the majority of the time recovering from crashes under conditions relatively favorable for population growth (Harpending and Bertram 1975). Under such a regime, we expect to find opportunistic reproductive strategies aimed at achieving rapid population growth whenever possible, as well as crisis responses to short-term ecological catastrophes, rather than long-term strategies to limit growth.¹⁶

Regardless of whether this objection to the population regulation argument holds, a second one presents serious difficulties for both evolutionary (selectionist) and economic (rational-choice) versions of population regulation. This is what has been called the "free-rider problem." Even if resources are in chronically short supply (or will soon be so), such that members of a population would benefit from population regulation, it does not necessarily follow that such regulation would either evolve by natural selection or be chosen by rational self-interested actors. This is because individual parents who restrict their reproductive output in order to lower overall population growth rates may face a very altruistic reward structure: the gain from their reproductive restraint is divided among the entire population, while the costs fall only on them. If n is Ego's lifetime reproduction and N is the size of the population of which she/he is a member, Ego's act of infanticide costs her/him $1/n$, but Ego's gain in improved resources is only $1/N$. Under reasonable conditions, where n is typically ten or fewer offspring while N is on the order of several hundred or more, the ratio of cost to benefit is clearly unfavorable. Even if we assume a starting point of universal reproductive restraint, such a system of regulation is subject to unraveling by selfish defection (Grafen 1984, Bates and Lees 1979).

16. Discussion of periodic population crashes for Inuit populations can be found in Arima (1984:460), Birket-Smith (1929:68), Burch (1979), Gilberg (1984:585), Jenness (1922:39-42), Mathiassen (1928:21), and Saladin d'Anglure (1984:480), as well as in the review by Rowley (1985). Several of these sources note that infanticide was one response to resource crises and that it was also employed in other contexts such as infant deformity, but none suggest that such context-sensitive infanticide was intended to limit future population growth.

To overcome this free-rider problem, one of three factors would need to be invoked: (1) strong *genetic* group selection favoring population limitation at the expense of individual fitness, (2) strong *cultural* group selection (Boyd and Richerson 1982) favoring cultural evolution of norms with the same effect, or (3) social or economic conditions making it in the rational or adaptive interest of individual parents to limit their long-term reproduction. It is hard to see how any of these factors could have been effective among historic Inuit. High individual and family mobility between local groups documented for Inuit in the early contact period would mean that groups composed of reproductively profligate individuals would expand in the regional population at the expense of groups that restrained their growth rate via female infanticide or other means, while any gene flow that accompanied migration (including marriage between members of different local groups) would make genetic group selection weak and ineffective relative to individual selection. A history of cultural group selection is belied by the lack of sanctions or ideology promoting female infanticide as a social good; rather, infanticide among Inuit is reported to have been purely a family decision. Finally, it would be in the interest of individual Inuit families to limit long-term reproductive rates if they were tied to specific resource areas, but the historical and ethnohistorical data indicate that residential relocation of individuals, families, or even larger units was quite common: Inuit were not stuck with (the foraging equivalent of) the family farm.¹⁷

While the cost of raising offspring can favor fertility limitation in the short run, it is not at all clear to us how it can favor sex-biased limitations—assuming that boys eat just as much and are just as hard to care for as girls. Female infanticide as *population* regulation is necessarily a long-term strategy and given the social, economic, and demographic structure of historic Inuit would fail to overcome the free-rider problem. Rather than long-term family planning and low, stable fertility

17. Evidence of high Inuit mobility, including movement of entire “bands” (local groups), is scattered throughout the ethnographic literature on Inuit and has recently been reviewed by Rowley (1985). Some of the most detailed evidence comes from research on Copper Inuit: Damas used extensive ethnohistorical evidence to determine that the numbers of band-exogamous and band-endogamous marriages were “approximately equal” and “the composition of the band was continuously changing (only about two-thirds of the members of a band would be together from one year to the next [Damas 1969:126])” (Damas 1975:413). Jenness (1922:32) notes that “each [Copper Inuit] group has its local name, a name derived from the district it habitually frequents in summer; but the individual members are constantly changing from one group to another, not merely temporarily for some special purpose . . . but permanently also, whenever the new district offers greater advantages, especially in the matter of game.” Jenness goes on to recount life histories to support this point in detail. For another population in our sample, Rasmussen (1931:177) collected sketch maps of areas known through personal experience and concluded: “Considering that the area the Eskimos require for caribou hunting and trout fishing is comparatively small, it is astonishing to see the tremendous sketches of country that [individual] Utkuhikjalingmiut are familiar with.”

levels, the historic Inuit demographic strategy more typically seems to have involved variable but often high fertility and widely fluctuating mortality levels. Although informants report occasional infanticide during resource crises, the more usual way of relieving food shortages was the involuntary one of death through starvation. In addition, at least some of the populations subject to ecological crises seem traditionally to have had high levels of fertility.¹⁸ In sum, neither Inuit demographic and ecological history (as we currently understand it) nor evolutionary or economic theory offers support for viewing female infanticide, or sex-ratio bias in general, as an element of a population-regulation strategy.

BALANCING ADULT SEX RATIOS

In most early Inuit censuses, male-biased child sex ratios coexisted with roughly balanced adult sex ratios in the same populations. A number of researchers have interpreted this pattern as due to the combined action of female infanticide and high adult male mortality from hunting accidents and homicide. Some (beginning with Weyer 1932) have gone on to explain the occurrence of female infanticide as a device for avoiding (or at least ameliorating) an excess of adult females in the face of relatively high adult male mortality. The analyses presented above do seem to be consistent with this “balance” hypothesis: the census boy/girl ratio is male-biased in all ten populations and significantly male-biased in seven of them, the man/woman ratio is not significantly biased in any of them, and the two ratios are statistically unrelated. Furthermore, the estimated female infant reduction averages 27.3 for the seven significantly male-biased populations and only 4.7 for the remaining three populations (table 3). All of this suggests a skewing of the sex ratios in childhood via female infanticide, coupled with some process pushing them back towards equality in adulthood. We have also shown that it is unlikely that this pattern is due exclusively to differential age at marriage, although this factor seems to play an important role.

But two aspects of the adult sex-ratio balance hypothesis deserve closer examination. First, does the model-

18. For example, Freeman (1971:1012) cites Rasmussen’s (1931:141) report of 8.3 live births for a sample of 10 postmenopausal Netsilik women. The figure for Caribou Inuit, another regional population with a highly skewed child sex ratio, is 7.6 live births for 43 postmenopausal women (Acker and Townsend 1975:469), although these date to a somewhat later period than the censuses in table 1. Jenness (1922:164) provides some fertility data on 9 Copper Inuit women, but all of these either are lacking data on deceased offspring or are for women too young to have no risk of further conception. Remie (1985) cites unpublished data collected by Van de Velde for 22 Netsilik women, covering the period 1880–1940 and only partially overlapping with Rasmussen’s data. The live births per woman equal 5.0 for this sample, but it is not clear from Remie’s discussion if all the women were postmenopausal; the female infanticide rate for this sample is either 23% or 32% depending on inclusion of one rather problematic reproductive history.

TABLE 5
Census and Computed Sex Ratios and Adult Mortality Schedules

Population	CSR ^a	Boys/ 100 Girls ^b	ASR ^c	Men/ 100 Women ^d	Male LE(15) ^e	Female LE(15) ^e	AMD ^f	Female Infant Reduction ^g
Baffin Land	99	105	105	94	35.0	35	0	0
Padlimiut	103	126	98	80	25.0	25	0	8
Caribou 1900	105	132	94	78	27.5	30	2.5	6
Cape Smyth	111	193	111	87	35.0	35	0	5
Aivilik	133	180	95	76	25.0	35	10.0	21
Caribou 1923	135	169	96	79	25.0	35	10.0	22
Copper	144	224	135	104	35.0	35	0	39
Utkuhikalik	153	193	146	118	25.0	25	0	40
Netsilik 1923	158	200	127	105	25.0	35	10.0	29
Netsilik 1902	167	209	119	97	25.0	35	10.0	37
Mean	131	168	113	94	29	33	4.3	20.7

^aComputed child sex ratio (males/100 females, ages 0-14) derived from model-life-table analyses.

^bCensus ratio of boys/100 girls.

^cComputed adult sex ratio (males/100 females, ages 15+) derived from model-life-table analyses.

^dCensus ratio of men/100 women.

^eLife expectancy (additional years) at age 15.

^fAdult male mortality differential, defined as female LE(15) minus male LE(15).

^gPercentage female infant reduction (from assumed birth ratio of 105:100) computed from model life tables.

life-table analysis support the postulated link between biased child sex ratios and differential adult mortality? Second, what is the theoretical basis for supposing that infanticide is a strategy designed to equalize adult sex ratios in the face of high adult male mortality?

On the first question, the balance hypothesis proposes that child sex ratios were adjusted through female infanticide or some other form of parental manipulation so as to compensate for relatively higher male mortality in adulthood, thereby leaving each group with approximately equal proportions of men and women. Two testable predictions follow from this: (1) adult male mortality should exceed adult female mortality, and (2) the differential in adult male mortality should be positively correlated with child sex ratio (i.e., the greater the excess male mortality, the more male-biased the child sex ratio).

To test the first prediction, we need some measure of age- and sex-specific mortality rates. Our model-life-table analysis (table 3) provides the best available estimates of these for the populations in question. The balance hypothesis is concerned not with the absolute level of adult male mortality but with the *difference* between it and adult female mortality. We term this difference the adult mortality differential and define it as female life expectancy at age 15 minus male life expectancy at 15. As is shown in table 5, the adult mortality differential ranges from 0 (five cases) to 10 (four cases), with a mean value of 4.3. Thus, the model mortality rates for adult males exceed those for women in half of the cases and are equal in the remaining cases, a difference that

is unlikely to be due to chance.¹⁹ These findings are consistent with the first prediction.

The second prediction is that parents engaged in female infanticide (or some other means of increasing the number of male offspring) when they could anticipate that sons would die at a higher rate than daughters. This should result in a positive correlation between degree of male bias in child sex ratio and rate of adult male mortality *relative to mortality for adult females*. Since the census boy/girl ratios confound actual age-sex structure with the effects of differential age at marriage, we will test this hypothesis by examining the correlation between computed child sex ratio and relative adult mortality. If the ten populations are ranked by increasing computed child sex ratio (as they are in table 5), a rough but noticeable trend of increasing adult mortality differential is revealed.²⁰ It would appear that some (but not all) of the variation in child sex ratios can be predicted from differential adult mortality, indicating moderate support for the balance hypothesis.

Less favorable for the balance hypothesis is the finding that there is no statistically significant tendency for

19. Since model mortality rates for men and women are not derived from independent samples, we again use the *t*-test for paired variates, testing here for a significant difference between male and female life expectancy at age 15 (see table 3) across populations. The result ($t = -2.68, p < 0.05$) is for a two-tailed *t*-test with 9 degrees of freedom.

20. A rank-order test indicates a moderate but nonsignificant correlation ($r_s = 0.489$, corrected for ties; $p < 0.20$, one-tailed *t*-test with 8 degrees of freedom).

populations in the sample with relatively high adult male mortality to respond with high amounts of female infanticide.²¹ Put in more qualitative terms, it is hard for the balance hypothesis to account for populations such as Copper Inuit and Utkuhikalik, which exhibit highly male-biased computed child sex ratios and high degrees of female infant reduction but no differential in mortality rates of men versus women.

While the empirical tests for the balance hypothesis just summarized are equivocal, we are more concerned with its theoretical adequacy. We are aware of no explicit theoretical justification for explaining female infanticide as a means of equalizing adult sex ratios. Rather, it is simply assumed that the populationwide benefits of sex-ratio balance, such as providing equal numbers of marriageable men and women or sufficient hunters to provision the remainder of the population, are an adequate explanation for the origin or maintenance of female infanticide by parents.²² This is a classic example of functionalist explanation, subject to the standard critiques of this form of social theory (e.g., Elster 1983, Smith and Winterhalder 1992).

We have not come across any ethnographic evidence that Inuit themselves viewed populationwide balance of the adult sex ratio as the reason for practicing female infanticide. To the contrary, Balikci (1970:152) suggests that if Inuit were aware of the population-level consequences of recurrent female infanticide, they were seen as an unfortunate by-product rather than the goal of their actions:

The Netsilik apparently remained unconscious of the dangers resulting from female infanticide, namely the scarcity of women. Van de Velde (1954:6) describes a group of little girls accompanied by a boy playing in his house at Pelly Bay. The father of the boy remarked regretfully: "If only all these children were males!" Van de Velde added: "Your boy is not even engaged and you would like all these girls to be boys? Where is he going to find a wife then? Boys will have to kill each other in order to get one!" The Eskimo replied with a stupid smile: "Izagoralluar" (It is true, despite that . . .).

Since evidence of conscious motivation to balance the adult sex ratios appears to be lacking, the balance hypothesis must appeal to evolutionary forces (either genetic or cultural) to provide a causal dynamic. Although it is difficult to test evolutionary explanations in cases lacking sufficient historical evidence, there are two cri-

21. The Spearman rank correlation coefficient between adult mortality differential and female infant reduction, corrected for ties, is only 0.194 and nonsignificant ($t = 0.56, p > 0.50$, one-tailed t -test with 8 degrees of freedom).

22. For example, Williamson (1978:67) states: "Female infanticide is common in societies where a high death rate among men would otherwise create an imbalance in adult sex ratios. . . . An Eskimo hunter's life is dangerous and many die young in hunting accidents. Without female infanticide the adult sex ratio would become unbalanced, and there would be too few hunters to provide for the women and children."

teria which can be used: logical (consistency with evolutionary theory) and empirical (consistency with comparative evidence on variation). The two are of course related, as the theory tells us what sort of empirical variation we should expect to find. Theories concerning the action of natural selection on sex ratio (see Charnov 1982) predict the adult sex ratio of a population not directly but via predictions concerning the allocation of parental effort to raising offspring of one sex or another. To view the population sex ratio itself as an adaptation is to commit essentially the same group-selection fallacy involved in explaining family planning by its effects in regulating population.

But let us set this objection aside for the moment. Does the evidence at hand support the balance hypothesis in the form of the prediction that males and females are equally abundant during their reproductive years? If we compute the reproductive years of men and women using the model life tables referenced in table 3, we obtain a ratio of 1.22 man-years per woman-year, with a wide range of variation (0.95 to 1.49, s.d. = 0.19). Furthermore, this ratio is significantly different from unity in a majority of the populations.²³ A similar result is obtained if the balance hypothesis is stated in terms of total adult-years. We conclude that the balance hypothesis, even though consistent with some of the data and estimates available, fails to provide a convincing explanation for sex-ratio variation among historic Inuit populations.

The pattern of systematic child sex-ratio bias among many historic Inuit populations, coupled with a generalized tendency towards more equal sex ratios among adults, does not seem to be well explained by conventional anthropological arguments concerning population regulation or balancing adult sex ratio. The Darwinian theory of sex allocation (Charnov 1982) suggests two alternative explanations: Fisher's (1930) theory of equal parental investment and one applicable when fitness gains from offspring of one sex are higher because of their interactions with kin.

EQUALIZING PARENTAL INVESTMENT

Fisher's theory is based on four premises: (1) The *total* fitness of males and females in a given population at a given point in time must be equal (since every offspring

23. Reproductive years are defined as age at marriage to 45 for females, age at marriage to 60 for males, and calculated using the mortality functions derived from Weiss (1973) that produce the best fit to each population (table 3). A z -score binomial test indicates that the ratio of male:female reproductive years is significantly different from 1:1 ($p < 0.05$, two-tailed) in six out of ten populations, including those four with the highest estimated female infant reduction. If a standard onset of reproductive age of 15 for females, 20 for males is used for all populations instead of the ages at marriage listed in table 3, the mean ratio of male:female reproductive years is nearly unchanged at 1.18 (s.d. = 0.17), and the same six cases differ significantly from unity. Finally, if total adult years (from age at marriage to death) are used, the number of populations that deviate significantly from unity increases to eight.

in a species with two sexes has exactly one father and one mother). (2) The rarer sex will have higher *average* fitness (total offspring/total number of parents of each sex). (3) Parents have a finite amount of parental investment (time and resources to devote to raising offspring). (4) There is heritable variation in parents' characteristics which determines their progeny's sex ratio. Given these premises, it follows that natural selection will favor strategies of parental investment that maximize parental fitness, which in turn will favor an equilibrium (populationwide) sex ratio that *equalizes total parental investment allocated to male versus female offspring*.

Assuming that male and female offspring are equally costly to raise (see below), Fisher's theory predicts an even sex ratio at the end of the period of parental care. Imagine a population that has a biased sex ratio at the onset of reproductive age. Since every child produced has one mother and one father, this means that individuals of the rarer sex have a larger *average* number of offspring. It follows that a parent who raises more offspring of the rarer sex will have more grandchildren (and thus higher fitness) than one who raises fewer or equal numbers of that sex. Thus, to the extent that parental investment allocations are heritable, selection will favor increased parental investment in the rarer sex until equal numbers of males and females in the total population are raised. This is an example of what evolutionary biologists term frequency-dependent selection: in this case, the optimal sex ratio (i.e., the ratio that maximizes parental fitness) depends on the frequency of each sex in the total population. If males and females differ in what they cost the parent to raise, however, selection favors a biased sex ratio. Put in simplest terms, Fisher's theory predicts that selection will favor greater investment in the cheaper sex until the point at which total parental investment in offspring of each sex is the same. Thus, if males (for example) are twice as costly to raise, the optimal strategy is to raise two females for every male.

In its original form, Fisher's theory is concerned with the evolution of genetic variation controlling sex ratio. Some might question its relevance in cases where sex ratio is adjusted (postconception) facultatively, as in sex-biased infanticide, or when sex ratio is affected by culturally inherited beliefs and preferences (as seems likely in the Inuit case). We do not consider these sufficient grounds for avoiding an empirical test of Fisher's theory with Inuit data. For one thing, Fisher's model has the widest (cross-species) predictive power to date, including its explanation for the slightly male-biased secondary sex ratio found in most human populations. Second, even facultative and culturally variable modifications of offspring sex ratio could be subject to optimization in rough agreement with Fisher's logic. Facultative adjustments depend ultimately on cognitive mechanisms that are themselves genetically evolved, and values and beliefs that are transmitted *culturally* to *biological* offspring will be subject to natural selection, which may favor an outcome closely matching what would evolve genetically. Although these are only possibilities, they seem to us to justify an empirical test.

How might Fisher's theory be tested in the Inuit case? One direct test would be to show that for each population $mC_m = fC_f$, where m = number of male offspring raised, f = number of female offspring raised, and C_m and C_f represent the mean cost (in parental investment) of raising one male and one female offspring, respectively. This equation is just a restatement of Fisher's central prediction that the optimal parental strategy equalizes parental investment in offspring of each sex. An alternative way of stating this relationship is $mC_m/fC_f = 1.0$. We will term the left-hand side of this equation the parental investment ratio. Obviously, if male and female offspring are equally costly to raise, then $C_m = C_f$ and the first equation simplifies to $m = f$ (i.e., raise equal numbers of males and females). Conversely, in cases where parents raise more boys than girls, consistency with Fisher's theory requires that $C_m < C_f$ (i.e., boys are less costly to raise than girls).

There are various complexities involved with measuring "cost." Trivers (1972) initially defined parental investment as any action by a parent that (1) increases the fitness of a particular offspring while (2) decreasing the parent's ability to invest in other offspring. While it has become customary to define parental investment somewhat more broadly, as "any characteristics or actions of parents that increase the fitness of their offspring at a cost to any component of the parent's fitness" (Clutton-Brock 1991:9), the basic notion that raising a given offspring entails an *opportunity cost* in ability to raise additional offspring remains central to the concept. In practice, one must use some operational measure of cost, such as average number of years of dependency per child of a given sex. In demographic terms, this equals child-years in a same-sex cohort/number in that cohort surviving to marriage age (assuming that marriage marks the end of significant parental investment and the beginning of potential offspring reproduction). This measure includes parental investment in offspring who die in childhood as well as those who survive to adulthood (as Fisher's theory says it should).

We can use the marriage ages, mortality schedules, and cohort sizes calculated earlier with model life tables to derive the average cost of raising boys and girls in each Inuit population (table 6, fig. 4). Boys cost more than girls in every population (averaging 25 years of parental effort per boy raised versus 16 years per girl); this is because of both the longer span of parental investment in male offspring (since they first marry an average of five years later than females) and their generally higher mortality rates. Yet roughly equal numbers of the two sexes are raised to adulthood (defined as computed age at marriage). Thus, the parental investment ratio that should equal unity according to Fisher's theory deviates significantly from it in all but one of these populations and averages about 1.7.

Not only does the analysis indicate that parental investment in most of these populations was skewed towards sons but also it reveals that those groups with the highest estimated female infant reduction rates are also the ones with the most male-biased survivorship ratios

TABLE 6
Parental Investment Analysis

Population (Model)	Child-years ^a		No. Raised ^b		Average Cost ^c		Parental Investment Ratio ^d	Survivorship Ratio ^e
	Boys	Girls	Boys	Girls	Boys	Girls		
Aivilik	1,562	864	63	56	25	15	1.8*	113
Baffin	1,016	951	69	71	15	13	1.1	97
Cape Smyth	1,436	765	69	70	21	11	1.9*	99
Caribou 1900	1,291	975	50	56	26	17	1.3*	89
Caribou 1923	1,198	709	45	39	27	18	1.7*	115
Copper	1,297	580	53	44	25	13	2.2*	120
Netsilik 1902	1,106	532	40	28	27	19	2.1*	143
Netsilik 1923	1,383	691	55	39	25	18	2.0*	141
Padlimiut	817	650	27	32	30	20	1.3*	84
Utkuhikalik	1,198	622	45	36	27	17	1.9*	125
Mean			51.6	47.1	24.8	16.1	1.73	113
Standard deviation							0.37	19

^aYears of dependency (birth to marriage), assuming initial cohort sizes, marriage ages, and mortality schedules specified in model populations (table 3).

^bNumber of each sex in cohort surviving to mean age of first marriage in model populations.

^cChild-years divided by number raised to marriage age.

^dParental investment in raising boys relative to girls (boy-years ÷ girl-years). Asterisks indicate a deviation from unity (i.e., equal investment) significant at $\alpha = 0.05$ (z-score binomial test, two-tailed).

^eSex ratio at marriage age ($100 \times$ boys raised ÷ girls raised). In no model population does this ratio deviate significantly from equal survivorship (z-score binomial test, two-tailed, $\alpha = 0.05$).

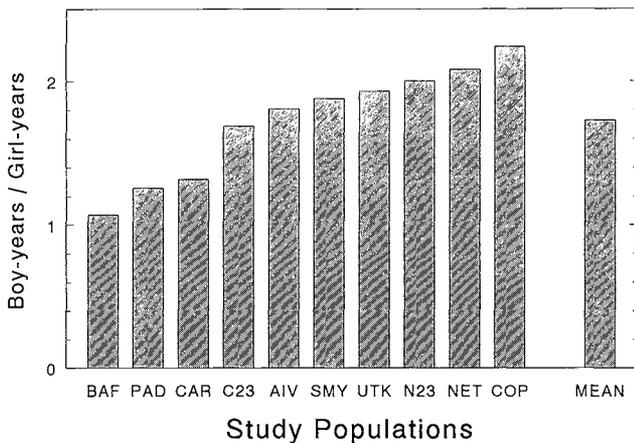


FIG. 4. Parental investment ratios (boy-years/girl-years invested in raising offspring to marriage age). Populations as in figure 1.

(fig. 5).²⁴ This supports the idea that the biasing of child sex ratios (by infanticide or equivalent means of reducing daughters raised) is part of the means by which parental investment was differentially directed toward sons. The model-life-table analyses indicate that immature sons not only died at somewhat higher rates (table

24. From table 5, we see that there are six groups with female infant reduction > 20 , while the remaining four groups all have female infant reduction < 9 . The six high-value groups are exactly the same as those with a survivorship ratio > 100 .

3) but were dependent on parental care longer because of their later age at first marriage, both of which increased the cost per son raised to maturity. As a result, most Inuit populations in our sample appear to have allocated 1.5 to 2 times as much parental investment per male raised to maturity as per female. This suggests that for some reason male offspring were (perceived as) worth more than females. This rather obvious deduction leads to our final hypothesis.

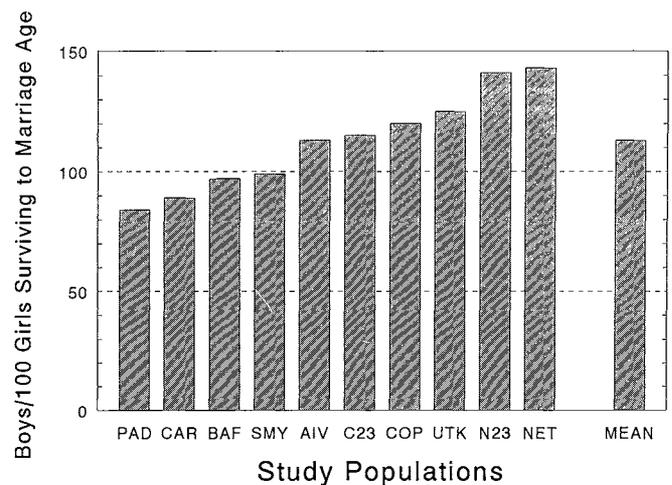


FIG. 5. Computed offspring survivorship ratios (boys/100 girls surviving to marriage age).

DIFFERENTIAL PAYBACK

Current Darwinian theory of sex allocation is considerably more complex than the standard version of Fisher's model outlined above (Charnov 1982, Clutton-Brock 1991). Put simply, it proposes that fitness returns from offspring are a function of three variables: the net cost of raising offspring, the effect of offspring interactions with parents or other relatives on the parents' inclusive fitness, and offspring mating success. Fisher's prediction of equal parental investment in male and female offspring assumes that the net cost of offspring of each sex is not affected by the number of same-sex siblings (i.e., that there is a linear trade-off between raising sons and raising daughters), that offspring interactions are limited to passively receiving parental care, and that offspring mating success is determined purely by the population sex ratio. Evolutionary biologists in recent years have devoted considerable attention to the effect of relaxing each of these simplifying assumptions.

For example, if there is a considerable amount of inbreeding, the Fisherian assumption that offspring fitness depends on the sex ratio of the larger population no longer holds, for it is the sex ratio of the inbred group that is evolutionarily relevant; in this case, fitness through male offspring exhibits diminishing returns, and parents should produce just enough sons to fertilize the females in the inbred group or subpopulation (Hamilton 1967). More relevant to the Inuit case, however, there may be disproportionate fitness returns from one sex or the other, as for example when offspring of one sex contribute more to the fitness of their siblings or parents than do opposite-sex offspring. If so, the Fisherian sex ratio will not be evolutionarily stable; instead, the stable sex ratio will be biased towards the more productive sex (Clutton-Brock and Albon 1982, Hughes 1981, Seger and Charnov 1988, Taylor 1981).

In the few recorded instances in which Inuit themselves offered explanations for the practice of female infanticide, they stated that boys were valued because they would eventually contribute to the family food supply, while daughters would not. This suggests that male offspring had higher value for parents *not* because they produced a larger average number of grandchildren (in a population whose sex ratio was at equilibrium, they could not) but because they increased the survival and well-being of their siblings and/or parents. It is tempting to take this statement as authoritative and grant Inuit the final say on the reasons for their own actions. Yet we should guard against accepting beliefs at face value; we know enough about the ideology of gender antagonism in other cultures to realize that there may be gaps between what people say, what they think, and what they actually do. Furthermore, a greater economic value for sons (real or perceived) does not entail female infanticide. After all, many people believe that male children return greater economic contributions to the family or kin group than females without practicing female infanticide.

In any case, a distinction can be made between the

beliefs and motivations that structure action and the forces that caused those beliefs and motivations to evolve and persist.²⁵ The question that ultimately concerns us here is why sex-ratio manipulation evolved (by cultural or genetic means) in traditional Inuit society. The limited ethnographic evidence suggests that the conscious reason Inuit had for practicing female infanticide was the greater "worth" of boys, who would grow up to be hunters and thus could provide food and assistance in hunting to their parents or other close relatives (such as siblings). Clearly, food is not the same as fitness. What we are proposing, however, is somewhat more subtle. First, the psychological mechanisms that motivate Inuit (in common with other humans) to value fitness-correlated outcomes such as increased food availability, increased survivorship of offspring, and the like, are plausibly due to genetic evolution in hominid (and pre-hominid) history. Second, the culturally specific beliefs held by Inuit concerning the frequency and contextual appropriateness of female infanticide could have evolved culturally because of a selective advantage these beliefs conferred on their bearers. That is, the culturally transmitted rationale for female infanticide might persist and spread if it led its bearers to raise a greater total number of offspring (who themselves passed on the rationale) than bearers of an alternative rationale that ignored sex ratio of offspring, *even if Inuit themselves were not aware of this evolutionary process.*

Were male offspring more "productive" (in the specific Darwinian sense of enhancing parental fitness) than female offspring in historic Inuit populations with female infanticide? The traditional division of labor gave women the primary role in child rearing but also a crucial economic role. It is true that males did the great bulk of the hunting and fishing and gathering was of minor importance or even insignificant, but women processed the raw materials into food, clothing, fuel (seal oil for light and cooking), and other goods (e.g., skins for tents and boats) without which survival, let alone hunting, would have been impossible. The most reasonable position is that males and females were equally important in the traditional Inuit division of labor, a view supported by the rarity of polygyny and Inuit emphasis on the economic necessity of marriage.

But the crux of the Darwinian hypothesis of differential benefits from sons versus daughters is not their contributions to the regional economy or to their own conjugal households; rather, it is their marginal contributions to *parental* inclusive fitness, either direct (i.e., grandchildren raised by offspring themselves) or indirect (e.g., contributions to the survival and reproductive success of parents, siblings, or other close relatives). The indirect contribution of sons took two main forms: sharing of the catch with parents and siblings and cooperative labor (e.g., in group hunting with fathers, uncles,

25. This distinction is central to a variety of theoretical positions in anthropology and cognate fields. Among these, views closest to our own can be found in Boyd and Richerson (1985), Durham (1991), and Smith and Winterhalder (1992).

siblings). In contrast, daughters had much less opportunity to contribute to their natal households or to siblings after marriage: females engaged in little food production, child-care constraints were minimal (because women and children generally remained in camp most days), and most female labor was individual (e.g., cooking, sewing, scraping skins) rather than cooperative. Women did participate in a few group foraging activities, such as caribou drives and weir fishing, but these involved men as well and thus did not affect the *relative* contributions of the two sexes. In manufacturing, sewing skins for a large boat (*umiq*) or tent (*tupiq*) is the only real case of exclusively female labor cooperation that comes to mind, but these were fairly rare activities and not necessarily done with or on behalf of consanguineal kin. Finally, daughters might be said to channel contributions of both goods and labor to their kin via the husbands (sons-in-law) they attracted; assuming that males were more likely to share or cooperate with consanguineal than with affinal kin, this would reduce but not eliminate the differential payback of sons hypothesized here.

Thus, a plausible case can be made that sons yielded a higher marginal fitness return than daughters in the social and ecological conditions experienced by historic Inuit. While this differential-payback hypothesis can be stated formally (Hughes 1981), putting it into testable form would be difficult and finding data sufficient to test it more difficult still. One important point to keep in mind is that any successful explanation must account for the fact that Inuit populations apparently differed tremendously in the amount of sex-ratio bias (and presumably rates of female infanticide). In contrast, the relatively greater contribution of males to kin through cooperative labor and food sharing seems fairly constant. How can the variation in sex-ratio bias be explained given the relatively homogeneous Inuit division of labor? Two possibilities come to mind.

The first has to do with proximity of males to members of their natal households. While boys do seem to have remained at home some five years longer than females, the ethnographic evidence makes it seem unlikely that they made significant economic contributions to their households prior to age 15. $W\frac{1}{2}\&7\frac{5}{8}\%:410$). The postmarital residence patterns of other populations included in our analysis are not well described but are most often labeled "flexible." More fine-grained data would be needed to determine the opportunities for interaction with natal kin and how these varied by gender and between different populations.

The second possible explanation for variation in Inuit sex-ratio bias concerns differential subsistence pressure. As several investigators have pointed out, male-biased child sex ratios among Inuit correlate with subsistence "marginality," as indexed by lower mean temperature and higher latitude (Irwin 1989), game scarcity (Damas 1975), and smaller dog teams (Riches 1974).²⁶ There are

26. Irwin (1989) has shown that census boy/girl ratios are positively correlated with latitude and negatively correlated with tempera-

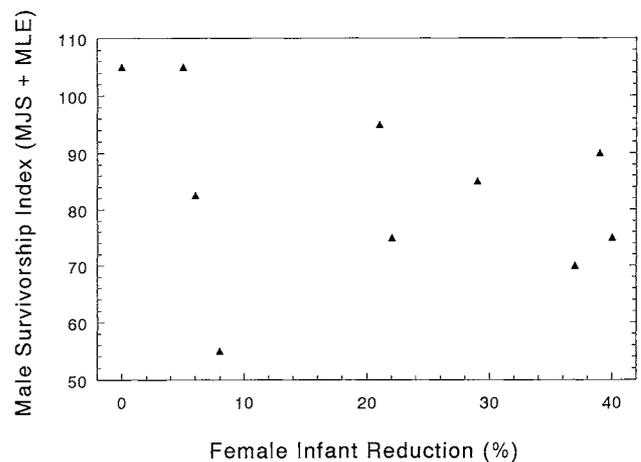


FIG. 6. Male survivorship versus female infant reduction, each data point representing one census population.

at least two ways this correlation could be explained by the differential-payback hypothesis. First, the greater male contribution to parental and/or sibling fitness may have been especially pronounced in areas with severe hunting conditions: the scarcer the game, the more important the contributions of labor and food shares from kin presumably would become. Second, it may be that severe conditions led men to take greater risks while hunting, thus increasing adult male mortality and favoring greater female infanticide to increase the numbers of adult male offspring. (This differs from the adult balance theory in that the optimum number of sons surviving to adulthood would not necessarily even the sex ratio of the entire adult population.) The assumption here is that female infanticide would increase not only the relative number of sons raised but also the absolute number by shortening the period from birth of a daughter to next conception, which had at least a 50% probability of being a son (Chapman 1980; see also n. 8 and Nalungiaq's view that parents cannot afford "to waste time nursing a girl").

This last suggestion finds some support in the demographic estimates presented earlier. If we sum juvenile and adult male survivorship for each population, we find that this "male survivorship index" is negatively correlated with our estimate of female infanticide rate (fig. 6).²⁷ In other words, the higher the male mortality,

ture, both of which he takes as proxies for greater risk of adult male mortality (i.e., darker and colder winter hunting conditions). As for dog-team size, Riches (1974) argues that this indexes the availability of surplus food; dog teams were used for transport and in the fur trade era (ca. 1900–1940) for checking traplines.

27. The male survivorship index (MSI) used here is defined as male juvenile survivorship rate (MJS) plus adult male life expectancy (MLE), using the rates derived from fitting Weiss's schedules to the census data for each population (see table 3). These two rates are in different units (juvenile survivorship = proportion surviving to age 15; adult life expectancy = mean number of years of life beyond age 15), so the resulting index is a dimensionless or "dummy"

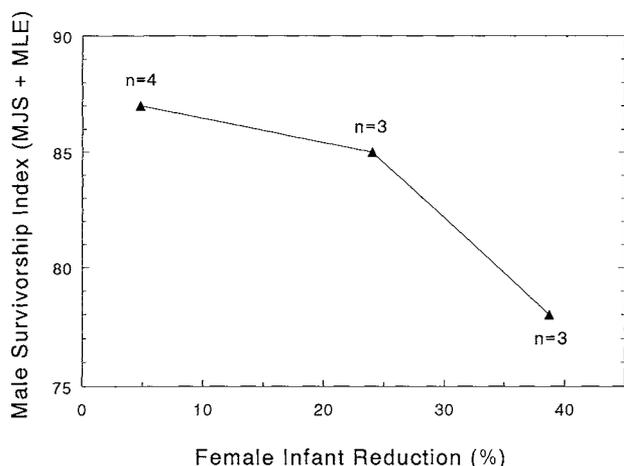


FIG. 7. Male survivorship versus low (0-8, \bar{x} = 4.8), medium (21-29, \bar{x} = 24.0), and high (37-40, \bar{x} = 38.7) female infant reduction.

the more male-biased are the child sex ratios. This association is especially apparent when we aggregate the data by degree of female infant reduction (fig. 7). The logic of this association as a test of the differential-payback hypothesis is that payback from sons increases the value of adult sons, making the optimal sex ratio more male-biased. Where ecological conditions elevate male mortality, parents who produce more male-biased child sex ratios via higher rates of female infanticide will better compensate for deaths of some of their sons and thus be more likely to approach the optimum. It is worth stressing that this empirical correlation of sex-ratio bias with offspring mortality *after* termination of parental investment contradicts the standard Fisher model (cf. Hewlett 1991, Smith 1993). Instead, the available data are consistent with the differential-payback hypothesis, though the small sample size and variable data do not yield statistical significance.²⁸

Although the data at hand are not sufficient to provide a definitive test of the payback hypothesis, it is noteworthy that they match the pattern in a worldwide sample

variable but reflects the computed overall mortality rate for males in each population. Since both components of MSI (MJS and MLE) refer to survivorship rates, higher values of each (and hence of MSI) indicate *lower* mortality rates. Thus, for example, Aivilik MSI = 70 (MJS) + 25 (MLE) = 95, while that for Baffin Land = 70 + 35 = 105; according to these estimates, Aivilik males had juvenile mortality rates equal to those of Baffin Land males but had higher adult mortality (hence lower MLE and a lower MSI). We use MSI rather than the relative adult mortality differential used in testing the balance hypothesis because the differential-payback hypothesis concerns the absolute numbers of surviving sons, not the balance of adult males and females.

28. The correlation between male survivorship and female infant reduction was tested via Spearman's rank correlation coefficient ($r_s = -0.472$, $t = -1.516$, $p < 0.40$, one-tailed t -test with 8 degrees of freedom). Since the sample size equals the number of Inuit populations in this study ($n = 10$), a correlation coefficient of ± 0.564 or greater would be required for statistical significance at $\alpha = 0.05$ (Thomas 1986:510, table A.13).

of hunter-gatherer censuses reported by Hewlett (1991), who demonstrated a strong and significant positive correlation between reliance on male foraging and male-biased juvenile sex ratios. Krupnik (1985) has surveyed demographic data for Siberian groups and argued that the sex-ratio biases found among Inuit groups are not characteristic of most of these reindeer-herders and foragers. At the same time, Helm (1980) has presented evidence for high rates of female infanticide among various historic Dene (Northern Athapaskan Indian) groups in the Western Subarctic. Although Helm does not discuss explanations for this practice, the epigraph to her article consist of statements by two Dene women that explicitly pinpoint the desire to raise more hunters. In a broader sense, the Inuit results add to widespread evidence for parental manipulation of sex ratio in accordance with the economic value of male versus female offspring documented for many other human populations (reviews in Cronk 1991a, Dickemann 1979, Hrdy 1992, Johansson 1984, Sieff 1990, and Volland 1989; for a contrary case, see Schiefenhövel 1989).

Summary

In the literature on Inuit demography, much has been made of the skewed sex ratios apparent in many early censuses. Some have seen evidence for population regulation or male dominance in these data, while others have questioned the empirical validity or adaptive significance of female infanticide. This paper has asked two questions concerning this material. First, what kinds of demographic parameters best characterize the early, rather crude censuses, especially with regard to sex ratios and sex-specific mortality? Second, to the extent that demographic analyses suggest actual skewing of sex ratios, what explanations of this skewing have greatest theoretical and empirical validity?

The first question was addressed using model life tables developed by Weiss (1973) to reconstruct the age-sex structure, mortality schedules, and ages at marriage most closely matching the early census data. Two main findings resulted. First, there is considerable variation in the model mortality and child sex ratios characteristic of different Inuit groups. Second, the hypothesis of Schrire and Steiger (1974) that most of the reported skew in child sex ratio is due to differential age at marriage is called into question; marriage age is shown to be a likely contributor to biased sex ratios in the early censuses and thus to lower the model child sex ratios (and hypothesized infanticide rates) considerably but by itself is insufficient to explain variation in Inuit sex ratios in nine of the ten populations analyzed.

Alternative explanations for Inuit sex-ratio manipulation include population regulation, prospective balancing of adult sex ratios in the face of heavy adult male mortality, and selection for optimal expenditure of parental effort. The population regulation hypothesis is not directly tested here but discounted as inconsistent with the ecological realities of Inuit populations as well

as contrary to evolutionary and economic theory. We have argued that it is an example of functionalist thinking, conflating the benefits to society or population with the interests of individual members. The adult sex-ratio balance hypothesis is given mixed empirical support but seems to lack any convincing theoretical basis; it is at best a correlation in search of an explanation.

Two distinct Darwinian models of sex allocation have been examined: Fisher's model and a model of differential payback. Fisher's model predicts equal parental investment in daughters and sons and hence can explain male-biased sex ratios only if males are cheaper to raise; Inuit demographic data indicate the opposite and suggest a parental investment of 1.5 to 2 times more per son raised to age at marriage than per daughter. The differential-payback hypothesis argues that such biased investment could be adaptive (at the family level) if males contributed disproportionately to the fitness of their parents or other close relatives (e.g., siblings). We have discussed reasons this might be so in the hunting-dominated economy of the Far North and shown that varying rates of adult male mortality may help account for variation in estimated female infanticide rates in a manner consistent with this hypothesis. In sum, although a conclusive explanation of Inuit sex-ratio variation is still not at hand, the hypothesis that sons "re-paid" their higher cost through material and labor contributions to parents and other close relatives seems to be the most plausible candidate.

Appendix: Sources

In the following list of sources for the raw census data, the date immediately following each population name is the year of data collection; publication dates are given in parentheses. In selecting and tabulating the census data, we made every effort to cross-check all figures for accuracy and consistency. When possible, we combined censuses from adjacent groups that appear to have been members of a single breeding population or endogamous unit. The sample includes a majority of the populations of Canadian Inuit, exclusive of the Quebec-Labrador Peninsula. It does not include Greenlandic groups, and only one North Alaskan population is included. It extensively samples the domain where female infanticide has been alleged to be frequent, however, as well as some adjoining groups.

We depart from the practice of others (e.g., Schrire and Steiger 1974, Irwin 1989) of relying primarily on the census compilation published in Weyer (1932:134-35) because that compilation omits several censuses from the relevant period and area, contains several errors in totals or ratios, lists many subpopulations separately, increasing the effect of stochastic variation in the data, and contains some cases of dubious relevance. On the last point, four Greenland censuses summarized in Weyer are excluded here because they are the only early Inuit censuses in which the census takers specifically recorded age-classes, in this case 0-10 and above 10,

which both precludes tests of the differential-marriage hypothesis and complicates comparison with other groups using the model-life-table approach; because Greenlandic groups have a long history of Danish colonial administration and thus a demographic history rather different from that of the Canadian and North Alaskan Inuit; and because their even or female-biased sex ratios may reflect Danish (and Christian) sanctions against parental manipulation of sex ratio (Weyer 1932: 135-36).

1. Aivilik (Aivilingmiut), 1898. Collected by Capt. George Comer in northwestern Hudson Bay, published in Boas (1901-7) in the form of household lists (numbers of men, women, boys, and girls in each household censused).

2. Baffin Land, 1883. Combined data from nine local groups on southern Baffin Island, collected or compiled by Boas (1888). We have eliminated one group in which sex was not specified for children, so our totals are different from those in Weyer (1932) and Schrire and Steiger (1974).

3. Cape Smyth, 1881-83. Collected by Middleton Smith (1902) at Point Barrow, Alaska, on the 1881-83 Polar Expedition sponsored by the U.S. government as part of the Hamburg International Polar Conference.

4. Caribou Inuit, 1898-1902. Collected by Comer, published in Boas (1901-7). We have combined the figures for two local groupings: "Sauniktumiut" (Hauneqtormiut, $n = 178$) and "Kinipetu" (Qaernermiut, $n = 146$). These two groups, along with other Caribou Inuit, were recensused two decades later by Birket-Smith.

5. Caribou Inuit, 1922-23. Collected by Birket-Smith (1929). We have combined three local groupings: Hauneqtormiut ($n = 54$), Harvaqtormiut ($n = 78$), and Qaernermiut ($n = 90$). Although these may once have constituted larger and demographically distinct populations, it is likely that they formed a single intermarrying population by the time of Birket-Smith's census. Weyer's (1932) adult sex ratio for Hauneqtormiut is incorrect, while Birket-Smith's (1929:68) published total for Harvaqtormiut does not agree with his household lists, underenumerating by one man and one woman, and has been corrected here.

6. Copper Inuit, 1915 and 1923. Collected by Jenness (1922) and Rasmussen (1929). We have combined figures for three regional groupings: Bernard Harbor ($n = 127$), Tree River ($n = 25$), and the eastern bands of Copper Inuit ($n = 374$), the first two censuses collected by Jenness on the Canadian Arctic Expedition of 1913-18 and the third collected by Rasmussen seven years later. The eastern Copper apparently did not overlap geographically with the groups counted by Jenness although they did intermarry with them. This is the only instance in which we have lumped data from different time periods and collected by different observers, but we feel it is justified here by the lack of geographical overlap and the closeness in time. Rasmussen's (1932) census is by household, with each individual named and households assigned to one of eight local groups.

7. Netsilik (Netsilingmiut), 1902. Collected by Comer, published in Boas (1901-7) in the form of household lists. It is likely that these Netsilik represent recent immigrants to Repulse Bay from the previous Netsilik homeland to the northwest (see Balikci 1984 and Remie 1985).

8. Netsilik, 1923. Collected during the Fifth Thule Expedition and published by Rasmussen (1931) in the form of household lists giving the names of each household member and household (camp) location. Totaling these (rather than taking Rasmussen's totals, which Remie [1985] as well as Schrire and Steiger [1974] have noted contain errors), and counting six unmarried males whom Rasmussen terms "young men" as adults yields the figures listed in table 1.

9. Padlimiut, 1922-23. Collected by Birket-Smith on the Fifth Thule Expedition. Birket-Smith's (1929) total for girls is one less than the numbers entered in his household enumerations. Although not certain of the source of the error, we have elected to change the total. On the assumption that they are part of a single deme, we have included Birket-Smith's figures for interior Padlimiut collected at the same time. Although Padlimiut are included under the rubric of "Caribou Eskimo" in ethnographic surveys, we separate them here on the grounds that they were sufficiently distant and demographically distinct from the Caribou Inuit identified above to be analyzed separately.

10. Utkuhikalik (Utkuhikalingmiut), 1923. Data collected by Rasmussen (1931) at the same time as his census of the neighboring Netsilik, to whom the Utkuhikalik are closely related. While an argument could be made for combining these two groups into a single population, the inland orientation of Utkuhikalik seems to have made them culturally and demographically distinct from the Netsilik, and they have been so treated by Schrire and Steiger (1974). Again, we have computed our own totals from Rasmussen's household lists rather than relying on his totals and have designated six unmarried individuals he listed as "young men" as men rather than boys (contrary to Schrire and Steiger's tabulation).

Comments

JUDITH ANDERSON

*Department of Psychology, Simon Fraser University,
Burnaby, B.C., Canada V5A 1S6. 24 VI 94*

This paper helps to clarify several issues relevant to female infanticide among the Inuit. Smith and Smith present useful evidence concerning the relative importance of infanticide and differential age at marriage in accounting for "missing" female children. In addition, they provide a much-needed critical evaluation of several hypotheses for explaining female infanticide. I hope that this valuable emphasis on rigour will eventually produce an explicit quantitative model of the differential-

payback hypothesis. However promising, this hypothesis cannot be evaluated until it has been formalized, because, in general, infanticide is a costly way for parents to achieve fitness goals. Infanticide can be favoured by natural selection only in situations where benefits actually outweigh those costs. In order to estimate costs and benefits under the differential-payback hypothesis, it will be necessary to model discounting processes, since a decision about the fate of a newborn daughter depends upon probabilistic expectations for the future. Thus, the comparison of expected economic contributions by sons versus daughters to the parental household ("economic value") needs to be formulated in a fashion analogous to reproductive value (Fisher 1930): we need to calculate the present value of the future economic contributions of an offspring, given its current sex and age, mortality schedule, and age-specific ability to contribute. The parents' goal is, presumably, to increase the economic value of the entire sibship.

Without appropriate data, it is not clear exactly what the sex-specific "economic value" curves will look like as a function of child's age, but they will undoubtedly share many of the characteristics of reproductive value curves, and these characteristics suggest that sacrificing a daughter in hope of producing a replacement son may not always increase the economic value of the sibship. For example, while daughters may not contribute economically as much as sons after marriage, their contributions probably begin earlier than sons', increasing the present value of their future contributions. In the case of a newborn son hoped for in the future, his economic value as part of the sibship will have to be discounted by (1) the probability that the next child will be female and (2) the probability that no further children will be produced because of either loss of fertility or the substantial risk of maternal mortality in preindustrialized societies (Hewlett 1991, Royston and Lopez 1987).

If the differential-payback hypothesis is stated in evolutionary terms, as Smith and Smith have done, a further set of functional relationships is required to compare the inclusive biological fitness of the parents for sibships in which the daughter is reared versus those in which she is killed. To estimate parental fitness as expected numbers of grandchildren (Trivers and Willard 1973), we sum current reproductive values for the sibship, each appropriately modified by the economic well-being of the family, which is in turn a function of the composition of the sibship. The nature of reproductive value curves also limits the conditions in which total reproductive value of a sibship can be increased by sacrificing a daughter in hope of replacing her with a son. In many nonindustrialized populations, boys have greater natural mortality than girls and/or begin to reproduce at a later age, so the reproductive value of a son will not equal that of a daughter the same age until they are young adults (Anderson and Crawford 1993). In addition, because reproductive value for both sexes increases sharply with age during childhood, any future child will always have a smaller reproductive value than one already in existence, who will of course be older. Sensi-

tivity analysis of a model of sex allocation based on offspring reproductive values (Anderson and Crawford 1993) demonstrated that infanticide intended to alter tertiary sex ratio did not increase expected numbers of grandchildren under most conditions, especially if a daughter was killed. Cost-benefit interactions under the differential-payback hypothesis are probably more complex, so it is crucial to identify what set of assumptions, if any, results in a net benefit to the parents and whether those assumptions are met in Inuit populations.

On a technical note, the prediction from the differential-payback hypothesis that female infant reduction rates should be negatively correlated with male survivorship seems reasonable. However, the test provided in figures 6 and 7 is problematic because the female infant reduction estimates are not independent of male survivorship. Female infant reduction was estimated to correct for an excess of females expected from model life tables. This excess was greatest when there were relatively few males, implying low male survivorship. Therefore we would expect female infant reduction to be negatively correlated with male survivorship simply from the way it was estimated. Testing this prediction requires an independent estimate of female infant reduction.

MONIQUE BORGERHOFF MULDER

Department of Anthropology, University of California at Davis, Davis, Calif. 95616, U.S.A. 27 VI 94

Smith and Smith offer a penetrating, critical, and imaginative analysis of an old anthropological chestnut—why Inuit kill their baby daughters. They convincingly show that any likely systematic misassignment of the age of males and females is insufficient to explain observed sex-ratio biases and make creative use of model life tables to estimate the extent of female-biased infanticide in ten different sample populations. Then, turning to Darwinian explanations for this pattern, they find yet further evidence (Sieff 1990, Hewlett 1991) of a departure from the simple Fisherian prediction that parents should produce more of the sex that is cheaper to raise. Using a perhaps oversimplified (see below) but theoretically sound measure of the operational costs of raising children of each sex to independence, Smith and Smith show that Inuit parents are indeed raising greater numbers of the more expensive sex (males). Then, appropriately, they turn to what they call the differential-payback hypothesis.

It is at this stage of the argument that I have some suggestions, constructive rather than critical. First, to test the hypothesis that “paybacks” from sons will be particularly valued (and hence female infant reduction rates highest) in populations with the harshest subsistence conditions, Smith and Smith predict a positive association between female infant reduction and male mortality. The explicit assumption here is that severe conditions foster greater risk taking and hence greater mortality amongst males in hunting and associated sub-

sistence pursuits. Technically, female infant reduction should be plotted on *adult* male survival, not the summation of juvenile and adult survival (as in figs. 6 and 7), since hunting accidents presumably occur primarily to adults. Conducting this analysis, I find no association between female infant reduction and adult survivorship ($r^2 = 0.15$), or, parenthetically, between female infant reduction and juvenile survivorship ($r^2 = 0.01$; survivorship figures taken from table 3). Since it is only the *sum* of male mortality that correlates with female infant reduction, the differential-payback hypothesis, formulated specifically to deal only with postindependence investment, is not technically supported. However, since many hunting accidents may occur prior to independence among inexperienced juvenile males (E. A. Smith, personal communication), the methodological upshot of all this is that the measure of the costs of raising boys and girls up to the age of independence should include a term for differential paybacks during their juvenile years.

Second, somewhat surprisingly, Smith and Smith do not report the strong positive correlation between female infant reduction and the relative costs of raising boys to girls (parental investment ratio; $r^2 = 0.67$), a finding that demonstrates (more convincingly than they appreciate) that female infanticide occurs predominantly in situations in which males are particularly costly. This correlation provides direct evidence for the suggestion, made only tentatively in the paper, that female infanticide serves to increase the *absolute* number of sons via the curtailment of the period of lactational amenorrhea following the birth of daughters. This is a radical and, I would argue, distinct form of the differential-payback hypothesis, at least insofar as the hypothesis has been used in the human literature to date. In short, daughters’ risks of infanticide are not simply proportional to their relative costs and contributions (as compared with those of sons) but also affected by the absolute number of sons required to ensure subsistence.

Inevitably, the differential-payback hypothesis remains somewhat tentative because of the lack of relevant ethnographic detail. While these gaps can no longer be filled for the historic Inuit populations under discussion, they bear mentioning if only because sex-ratio manipulation is so commonly studied in contemporary populations (e.g., Sieff 1990). First, patterns of postmarital residence and how these are influenced by age, sex, and the sex distribution within the sibling set must be determined, because residence most likely constrains and/or promotes the extent to which offspring influence their parents’ fitness. Second, when considering sex differences in contributions to parental fitness it becomes critical to consider not only sex differences in returns to economies of scale (as Smith and Smith indicate) but also whether, for example, four daughters and two sons can be as productive as two daughters and four sons. In the Inuit case it is clear that even if economies of scale for males and females are the same, the latter configuration will be more productive, since women can do little economically gainful work if no carcasses are brought

back to camp. In horticultural communities, where women are often the primary providers of raw material and foodstuffs, the situation might be quite different. It is therefore critical to look not just at the amount of work that men and women do but also at the interdependencies between the sexes in their output.

ERNEST S. BURCH JR.

*Department of Sociology and Anthropology,
Elizabethtown University, Elizabethtown, Pa.
17022-2298, U.S.A. 22 VI 94*

Smith and Smith accepted an interesting challenge when they decided to investigate the skewed sex ratios reported for early historic Inuit populations. I applaud the thoroughness of their survey of the relevant issues and, in general, agree with their conclusions. My comments consist primarily of addenda. I begin by focusing attention on substantive matters, particularly those concerning the two Inuit populations in their sample with which I am most familiar—the North Alaskan Inuit and the Caribou Inuit.

Information I have acquired over the years indicates that most people in both populations wanted to have as many children as possible, for a number of different reasons. Infanticide occurred primarily, perhaps only, in families experiencing extreme privation. The “belief that conferred selective advantage on its bearers,” to paraphrase Smith and Smith, was that parental survival was more important than infant survival. In particular, infanticide increased the survival chances of the mother, some of whose energy would be lost to the baby in the form of milk and much of whose energy would be drained by having to carry and generally look after it. The final decision to “throw” a baby was made by the mother, although others ordinarily were consulted. Infanticide was much more likely before a baby had been named. Another key consideration was the prospect of a journey, in which the mother would have to carry both the baby and baggage. Keeping one eye on the future even in a time of crisis, mothers more readily abandoned baby girls than baby boys for precisely the reasons encompassed in the differential-payback hypothesis.

Smith and Smith’s information from North Alaska comes from Cape Smyth. They quote Smith (1902:114) as their source, but the unaggregated data were published earlier by Ray (1885:49). The basic point here is that this entire population was under duress at the time. If we accept Simpson’s estimate (in Bockstoe 1988:507) that, at the end of 1853, the population of Cape Smyth was three-quarters that of Point Barrow, which was 309, then the population of Cape Smyth would have been about 230 at the time. The combined total would have been about 540. Thirty years later, when the census cited by Smith and Smith was made, the population of Cape Smyth had declined to 138, and that of the two villages combined was only 180 (Smith 1902:113). There are very good reasons to believe that this decline was

TABLE I
*Population Totals for Caribou Inuit Subpopulations,
1902–22*

Subpopulation	1902	1917–18	1922
Harvaqturmiut	—	186	78 ^a
Hauniqturmiut	178	96	54
Paatlirmiut (Coast)	—	304	108
Qairnirmiut (Kinipetu)	incomplete	214	90
Total		800	330

SOURCES: For 1902, Boas (1901-7:377-78); for 1917-18, Ford (1918); for 1922, Birket-Smith (1929:67-68).

^a Rasmussen’s (1930:22-23) list of Harvaqturmiut totals 74.

due not to emigration but to newly imported European diseases (Fortuine 1989:209) and famine.

Turning to the Caribou Inuit, I treat the different subpopulations separately rather than lumping them. The relevant data (see my table 1) show the horrendous declines due to famine that occurred in each of them between 1917-18 and 1922. The inland Paatlirmiut are not included because their historical relationship to the other subpopulations is problematic. Boas’s (1901-7) 1898 figures for the Qairnirmiut (Kinipetu) are almost certainly incomplete and are therefore excluded as well; they also happen to refer to the particular segment of the Qairnirmiut beginning to be affected by disease. As in Alaska, the data on sex ratios among the Caribou Inuit come from populations under extreme stress, which are the ones in which infanticide is most likely to occur. Smith and Smith state that Inuit population dynamics were probably characterized by a sawtooth growth curve. In at least these parts of their sample, the “growth curve” was sharply negative. Thus, caution is in order when extrapolating from early historic Inuit data to foragers in general unless a condition of duress is being assumed.

Finally, I commend Smith and Smith for their use of models in attempting to understand the Inuit material. As far as I can tell, there is not a single early historic Inuit population for which the information is good enough to stand on its own in anything but the most rudimentary demographic analysis. However, in addition to model life tables, we need to apply to the Inuit data models of the effects on birth rates and infant sex ratios of (1) prolonged lactation, (2) periodic infertility due to excessive leanness, (3) spontaneous abortion due to extreme physical exertion, particularly heavy lifting, and (4) a harsh birthing regime required by magical beliefs. It also would be useful to develop models of accidental death rates among adult males in foraging populations, models in which hunters ages are taken into account. Until we have all of these, we cannot really understand the demography of early historic Inuit populations.

DAVID DAMAS

*Department of Anthropology, McMaster University,
Hamilton, Ontario, Canada L8S 4L9. 24 VI 94*

I am placed in an awkward, rather bizarre, and unfamiliar position in having to state that statements of mine, taken out of context, have been used here to suggest that my interpretation of data is largely adversarial to the writers' own. In fact I support and have, together with others, earlier represented the position of Smith and Smith in this article. In two places they group me with those who suggest that differential age of males and females at marriage rather than infanticide accounts for skewing of sex ratios in censuses of Inuit. I did indeed suggest that censuses of Central Eskimo had probably classed all married females as adults regardless of their ages, but I did not think that this factor accounted for the imbalance of sexes with respect, specifically, to Copper and Netsilik groups (Damas 1975:412n):

In support of my view that the sex imbalance existed in all age groups among Copper and Netsilik Eskimo are certain census data from later periods. For instance, for the Copper Eskimo area as a whole, Abrahamson et al. (1984:23) show a clear predominance of males over females for everyone born before 1928. In that census there is a ratio of 1:48 to 1:00, which agrees very closely with Rasmussen's (1932:69-71, 78-85) figures for 1923-24.

The other citation regarding my assumed position occurs in a section devoted to the Caribou Eskimo, where a census by Birket-Smith (1929:67-68) indicated a preponderance of males among children and a preponderance of females among adults. I challenged the interpretation of these ratios in terms of female infanticide and high mortality among adult males related to hazardous occupations. It was my view that if indeed Birket-Smith had classed all females as adults regardless of age if married, the actual male/female ratio would be obscured. Further, that possibility, together with an overall proportion of only 221 males to 211 females, made me suspect that these figures were "casting some doubt on the widespread occurrence of selective female infanticide among the Caribou Eskimo" (Damas 1984b:302n). To summarize, my view is that while the population figures for the Copper and Netsilik tribes support the occurrence of infanticide, the picture is less clear for the Caribou Eskimo. I wish also to cite my concluding statement regarding infanticide (Damas 1975:417n) to be matched with conclusions of Smith and Smith: "My position is that female infanticide is a direct response to marginal conditions. This thesis supports Rasmussen (1931) and Balikci (1967)."

Beyond this, I would like to comment on the debate regarding infanticide among the Inuit of the Central Arctic which caused Smith and Smith to defend the thesis previously offered by Rasmussen and supported by Balikci and myself. The issue as I see it is an expression of an unfortunate tendency toward parricide which is

found in anthropology today. It would not have been necessary for Smith and Smith to defend our common position if reasonable credence had been accorded the ethnography of the members of the Fifth Thule Expedition, especially that of Knud Rasmussen. Having worked with the census material in connection with a genealogical study and having interviewed informants who had survived the infanticide era, I found very little in the way of omissions or inaccuracies in the censuses and agreement among informants as to the existence of infanticide in earlier times in those groups which I have identified here.

NELSON H. H. GRABURN

*Department of Anthropology, University of California,
Berkeley, Calif. 94720, U.S.A. 20 VI 94*

I am impressed with Smith and Smith's comprehensive reexamination of the problem of Inuit sex-ratio variation. Their final hypothesis, that "sons 'repaid' their higher cost through material and labor contributions to parents and other close relatives," seems to be common sense. However, without better ethnographic data, even perfect quantitative modeling of this case would not amount to proof. We know that Inuit occasionally practiced infanticide, but there have been no direct counts of its occurrence or of the preeminence of the female over the male form. We also know that adult male Inuit suffered higher rates of mortality, from murder (Graburn 1969, 1970) as well as from accidents, than adult women. What we do not know is whether the few facts about infanticide are ethnographically linked to the more firmly established record of adult sex ratios. Here I am proposing another ethnographic scenario also based on the Inuit desire to raise sons, the "symbolic infanticide" of daughters. While my hypothesis accords with the well-known Inuit love of their children and accounts as easily for the apparent anomalies in the sex ratio, like Smith and Smith's hypothesis it suffers from a lack of quantified data.

I am suggesting that the majority of the ethnographers miscounted the numbers of male and female children in favor of the former. This in turn necessitated their hypothesis that there must have been great numbers of female infanticides. Most ethnographers recorded that Inuit loved babies in general and took pride in young sons in particular, but this did not have to lead to the murder of girls—only to their "symbolic murder." We have to consider the probability that the male-biased child sex ratio may have been caused in part by the fairly well-known Inuit institution of *sipiniq*, whereby some male neonates "turn into" girls but are treated socially as boys (Dufour 1975, Saladin d'Anglure 1977). As the child is being born, the midwife or sponsor is supposed to see if the baby is a boy or a girl, and if it is a boy she is supposed to grab and hold or pull on its penis. On occasion, she is not quick enough, and the penis slips inside or becomes rudimentary (as a clitoris) so that su-

perfidiously the anatomy remains female even though the parents know it is really a boy "inside." Of course, as Inuit names are not sex-linked, the "change of sex" does not change the name, and *from then on the family treats, addresses, and dresses the child like a boy*. The child may learn hunting with its father until the age of puberty, which brings changes too obvious to deny. At this point the child withdraws from the boys' social world, wears female clothes, and soon marries but *may always remain a good hunter or superior craftsman* (Saladin d'Anglure 1984:492).

Under such circumstances, a visiting ethnographer would be told "We have a son" or "Sallujualuk is a boy," and, of course, Sallujualuk is addressed as *irnira* (my son) and is dressed and plays like a boy. The fact that the institution of *sipiniq* was not recorded until the 1970s (see Saladin d'Anglure 1973, 1977 [originally noted in a manuscript written for him by Mitiarkjuk in 1966, according to Dufour (1975)]) shows that generations of ethnographers, male and female, were ignorant of it and could have got their census data wrong. I myself did not know about *sipiniq* until 1967, on my fourth field trip to live among the Inuit in the Canadian Arctic. One day when visiting a friend's household, I saw the six-year-old son pull down "his" pants and pee sitting down. I asked about this and was told that the child was one among a number of *sipiniit* in the community. Also, the central character of the autobiographical *The Land of the Good Shadows* (Washburne and Anauta 1940) was almost undoubtedly a *sipiniq*, a fact that I and dozens of others missed.

My suggestion that female babies are "socially murdered" and "revived" at puberty does not really solve the problem that Smith and Smith set out to explore, particularly the matter of the great geographic variability in the "sex ratio." If I am correct that *sipiniit* may account for the excess of boy children, then we still have to explain it. From my own knowledge and from the writings of Saladin d'Anglure and his team I know of *sipiniit* only in Nouveau Quebec, Baffin Island, and among the Aivilingmiut/Iglulingmiut, areas which overlap with those in Smith and Smith's study. The institution was most likely much more widespread and was missed by the ethnographers elsewhere as it had been until recently in the Eastern Arctic.

My suggestion is at least as likely as mass infanticide among the child-loving Inuit. This brings us a strong warning that we should not treat our "subject peoples" merely as numerical figures in some biologically derived ecological matrix, nor should we rely only on data provided by short-term census-taking explorers, whalers, and anthropologists.

CORNELIUS H. W. REMIE

*Instituut voor Culturele en Sociale Anthropologie,
Universit  Catholique de Nimegue, Pb 9108, 6500 HK
Nijmegen, The Netherlands. 13 VII 94*

Smith and Smith's article demonstrates once again how difficult it is to get a firm grip on early Inuit demography

in general and the phenomenon of female infanticide in particular. Sophisticated as their methodology is, it is built on a very weak basis. The census data they use come from societies that had sustained population crashes (Caribou Inuit, western Netsilingmiut, Copper Inuit) or were socially disrupted as a consequence of contacts with outside agents (as in the case of Uthuhik-jalingmiut, the Netsilingmiut, the Aivilik, and the Baffin Island Inuit). These factors, although briefly mentioned, are not or are insufficiently taken into account in explaining female infanticide. Smith and Smith's explanation focuses on the differential payback of female offspring, males contributing more to parental and/or sibling fitness than females. The (extended) family thus becomes the locus of female infanticide, the decision being based on parental assessment of fitness for survival. I agreed that the extended family is the locus of female infanticide but for different reasons.

Elsewhere (Remie 1985) I have argued that female infanticide is the result of preferential kin endogamy and child betrothal. My argument is based on empirical data on the Arviliguarmiut (eastern Netsilik territory). In a sample of 22 postmenopausal women a female infanticide rate of 31.5% was observed as well as one case of male infanticide. Of the 19 reported infanticide cases, 57.9% occurred in one extended family (hereafter family A), to which woman 12, woman 16, and woman 22 belonged (3 cases of female infanticide reported for woman 12 had occurred in earlier marriages outside that extended family). Three other cases occurred in a second extended family (hereafter family B, woman 19 and woman 20). Thus almost three-quarters of the reported cases occurred in two extended families. Even if one excludes the rather problematic reproductive history of woman 22, families A and B still account for more than 60% of the infanticide cases found.

According to Smith and Smith, relations of economy rather than power underlie the infanticide decision. Though this may very well be the case, power relations should certainly not be ruled out. Family A was headed by the famous Okpigalik, a rather dominant character who had a strong influence over members of his family and was feared by other men (Rasmussen 1931:13; Van de Velde 1979-84, vol. 2:161). Okpigalik not only ordered his wife (woman 22) to do away with a considerable number of girls (with one exception [see Steenhoven 1959:50; Van de Velde 1979-84, vol. 2:163]) but also ordered the killing of a male infant born to woman 12 (see Irqugaqtuq and Van de Velde 1979:18-19), a female infant born to the same woman (Van de Velde 1979-84, vol. 2:176), and a female infant born to woman 16 (Van de Velde 1979-84, vol. 3:282). Power relations are definitely present here. Okpigalik "ruled" his family with an iron hand. As a leader of the Ermalingmiut faction (see Remie 1993) he saw to it that marriages were endogamous. Of family B fewer details are known, but there too endogamic trends are clearly visible, as well as power relations (see Van de Velde 1979-84, vol. 4:469).

Smith and Smith (n.15) argue that my explanation, an elaboration of that of Riches (1974), applies only to those

relatively few Inuit societies with high rates of kin endogamy (e.g., cousin marriage) and child betrothal. However, both child betrothal and endogamic tendencies have also been reported for the Copper Inuit (Damas 1984:401), the Uthuhikjalingmiut (Rasmussen 1931:489), and the Caribou Inuit (Arima 1984:455)—almost all the groups for which census materials have been used here. I therefore see no reason that the explanation of Netsilik female infanticide could not apply to these groups. Differences in female infanticide rates between Inuit groups would then result from the degree of endogamy, which is related to subsistence marginality and/or sociocultural disintegration.

ERIC ABELLA ROTH

*Department of Anthropology, P.O. Box 3050,
University of Victoria, Victoria, B.C., Canada V8W
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Smith and Smith are correct in asserting that Inuit occupy a special place in human demography as a result of practising senilicide and infanticide. While both are highly emotional topics, the latter has repeatedly been the subject of anthropological analyses from psychological (Freeman 1971), demographic (Schrire and Steiger 1974, Chapman 1980), cultural ecological (Balikci 1967), and sociobiological (Irwin 1989) perspectives. Smith and Smith now bring the tenets and methodological approaches of evolutionary ecology (cf. Cronk 1991b, Smith and Winterhalder 1992) to the long-standing debate about the historical incidence and prevalence of Inuit infanticide. In doing so they provide us with an important test of the utility and power of evolutionary ecology. I would like to address my comments, therefore, not only to the specific analysis presented here but also to the theoretical framework from which it is derived.

At the core of evolutionary ecology's approach is adherence to the hypothetico-deductive method, featuring the creation of models followed by their testing against empirical data (Winterhalder and Smith 1992:11). In this case the relationship is complicated in that the only empirical evidence consists of historical aggregate data paralleling the types of data that demographers commonly confront (cf. Fetter 1990). Exacerbating these difficulties is the so-called differential-labeling hypothesis, which claims that the data are seriously flawed because of sex-specific age misclassification. Smith and Smith are to be complimented for beginning their analysis by employing modern demographic techniques to test the data's internal consistency and validity rather than accepting or rejecting them at face value or in light of ethnographic details which are debatable in themselves. The careful interpretation of the results of this first analysis, recognizing a combination of some differential age classification, lower infanticide rates than previously suggested, and other possible factors which may remain unknown

forever because of a lack of contextual data, sets the tone for the rest of the paper.

Smith and Smith go on to test a number of models dealing with sex-ratio manipulation, ranging from cultural ecology's long-standing theme of population regulation to Darwinian explanations based on concepts of parental investment and inclusive fitness. Repeatedly their interpretation of results is consistent with the hypothetico-deductive perspective that "hypotheses cannot be strictly proven, only soundly disproved" (Winterhalder and Smith 1992:12). It is clear, however, that Darwinian models of sex-ratio bias fare better than others, largely because of their predictive value. Yet, while potentially lending themselves to potential testing, evolutionary models have a major drawback with regard to their operationalization. To me this is best exemplified in Fisher's theory of parental investment, which, while intuitively very appealing in its simplicity, often immediately breaks down because the "costs" incurred in raising children are too complicated to model and/or compute. Again, I compliment Smith and Smith for choosing a demographic variable—average number of years of dependency per child of a given sex—to operationalize sex-specific costs in their analysis. Alas, the differential-payback model of Hughes (1981), reflecting the more ambitious and realistic current model of Fisher's theory, remains untested because of this lack of operationalization.

Despite this, evolutionary ecology offers two other basic advantages reflected in Smith and Smith's analysis. The first is an inherent concern with the origin and evolution of biosocial phenomena, exemplified here by infanticide. More consideration of the why as well as the how of social behaviour—what Irwin (1989:235) termed the "ultimate" versus the "proximate" mechanisms of behaviour—is certainly to be welcomed. Such consideration need not be limited to evolutionary ecology, for demographers (Bongaarts 1978) make the same distinction when they discuss "distal" versus "proximate" demographic variables. Secondly, evolutionary ecology's concern with the individual rather than the group offers a different perspective on behaviour for anthropologists trained to think in terms of culture groups. In the present example, evolutionary ecology's "methodological individualism" (Smith 1991:11) shifts the analytical focus for decision making about infanticide from a cultural group to individuals within a family unit. Placing these families within ecosystems experiencing differential subsistence pressures (see Irwin 1989) helps to explain the variation in female infanticide levels found within the historical sample, provides the linkage from individual- to group-level data, and puts the ecology into evolutionary ecology. In summary, evolutionary ecology comes off looking very strong in the hands of these highly capable practitioners. While not placing myself among the converted, I am highly impressed by what evolutionary ecology can contribute to the historic debate over Inuit infanticide and conclude by congratulating Smith and Smith on an enviable piece of scholarly research.

GEORGE W. WENZEL

Department of Geography, McGill University,
Montreal, Quebec, Canada H3A 2K6. 28 VI 94

Inuit selective female infanticide, especially its prevalence and importance to the ecological and social health of Central Canadian Arctic regional bands, is a topic long argued about but still unresolved among students of Eskimo culture and society. As Smith and Smith point out, the range of explanation regarding this matter reaches from the miscalculation by early census takers, due to differential marriage ages, of the numbers of "boys" and "girls" to be found in local populations to psychosocial evaluations of Inuit male-female relations. In between these extremes one also finds various ecological/environmental/socio-demographic rationales for or rejections of the entire matter.

What Smith and Smith have done here is considerably more than toss a highly general perspective into the discussion to see how it plays out. Rather, they perform an original, precise, and in many ways elegant analysis of whether selective female infanticide (or, as they put it, female infant reduction) ever actually occurred in various Inuit societies and why it may have been practiced. In so doing, they provide insight into another side of the question, one for which there are virtually no data, namely, that the activities carried out by adult Inuit males were as dangerous as various clichés about the harshness of the arctic environment lead one to believe. Whether the differential-payback perspective accounts for female infant reduction among Inuit is less important than the way in which the analysis has been undertaken; the whole discussion is the better for it.

Smith and Smith's work here, especially as expressed in their various tables, opens up several other questions (properly) not addressed here but nonetheless of considerable interest. The first is why the Caribou Inuit (as that term is used here), notorious for the precariousness of their ecological situation from the 1930s through the 1950s (see Tester and Kulchyski 1994), appear *not* to rely particularly heavily on female infant reduction (likewise the Padlimiut) and why there is so much difference between the 1902 data and those for this group 21 years later. A second question concerns why the data for southern Baffin-Cumberland Sound Inuit differ so markedly from those for the Netsilik and Copper Inuit (or, for that matter, Aivilingmiut, a group generally perceived as relatively secure ecologically). Does the ecological variability under which these societies lived affect decisions regarding female infant reduction? Does part of the difference relate to the hunting role played by young women, at least until the birth of a first child, among southern and eastern Baffin Inuit? Comprehensive recall data from surviving elders in these groups may contribute to an answer to this last question. In any case, Smith and Smith have here made a notable contribution to an old Northern question and to the way in which Inuit sociocultural questions can be analytically approached.

Reply

ERIC A. SMITH

Seattle, Wash., U.S.A. 22 VII 94

We thank the commentators for their thoughtful contributions, including the demographic and ethnographic data therein. I will respond to the comments in approximately the (alphabetical) order in which they appear above.

Anderson argues that explicit models of the differential-payback hypothesis are needed. I agree that that would be useful, though I doubt that data to test them will ever be forthcoming with regard to historic Inuit (as Borderhoff Mulder notes, such data are more likely to be found among some other human populations). Anderson particularly argues that such models should include age-specific value functions time-discounted to allow a computation of net present value, and again I agree. Clearly, the relative economic gain from sons versus daughters would have to be quite high to counter the longer period of dependency and higher mortality characteristic of historic Inuit sons (we calculated an average cost ratio of 1.7:1 for raising boys versus girls). The kind of modeling that Anderson suggests could make explicit just how high the payback must be in order to make infanticide adaptive, and she is right to point out that we have not arrived at that stage here.

Anderson suggests that male survivorship estimates determined our estimates of female infant reduction, creating a circularity in our test of the differential-payback hypothesis. But we estimated survivorship by fitting Weiss's life tables to census boy/man and girl/woman ratios, while female infant reduction (infanticide or its demographic equivalent) was estimated by fitting sex ratios to the census data *given the previously estimated survivorship schedules*. Furthermore, high rates of adult male mortality could not produce male-biased child sex ratios. Circularity *would* be a problem if we had used *relative* survivorship rates of males versus females; this correlates very highly with female infant reduction, but having realized the interdependence of these measures we discarded that particular test.

Borderhoff Mulder notes that either juvenile males contributed payback (in which case the analysis should subtract this from the cost of raising them) or female infant reduction should correlate only with adult male mortality (which it doesn't). I accept her point and expect that historic Inuit boys in their mid to late teens did participate actively in foraging, particularly in situations of scarcity; we noted as much in the article, and my own ethnographic observations among Inujjuamiut (Smith 1991) indicate that even today Inuit boys are fairly active hunters when not in school. I thank her for pointing out the correlation between parental investment ratios and female infant reduction estimates and its implications for the differential-payback hypothesis, as well as the complications introduced by interdependencies of male and female labor.

I am grateful to Burch for the additional information on the population history of Cape Smyth and Caribou Inuit, matters on which he is a recognized authority. His table presents some data of which we were unaware (and which may help answer the question Wenzel poses about differences between the two censuses of Caribou Inuit used in our study). Burch is likely to be correct that these populations were experiencing declines immediately prior to the censuses used in our study. However, one must consider the possibility that these early censuses are incomplete and hence that, because of sampling error, repeated censuses of named groups are not necessarily direct evidence of population decline. It is also important to make a distinction between decline due to epidemic disease and that due to famine (though admittedly they may be linked in many cases); if epidemics reduce the number of fellow humans competing for the food resources of an area, they might even have a positive effect on fertility (e.g., Netting 1981:130–31, 245 n. 7). Hence it may be misleading to lump the two factors together under the rubric of “duress.”

I thank Damas for restating his views on the infanticide controversy. Although he feels we have misrepresented those views, our intent was merely to credit him with suggesting the differential-marriage-age hypothesis independently of Schrire and Steiger, not to portray him as denying any significant role to infanticide in Inuit demography. I agree with his comments concerning the “unfortunate tendency toward parricide” fashionable in anthropology today and am pleased with his defense of Rasmussen’s ethnographic accuracy, but I feel that some skeptical reexamination of conventional wisdom is conducive to the health of the discipline. Finally, while Damas agrees with us (indeed, along with Balikci, Weyer, and others, precedes us by decades) that female infanticide is a “direct response to marginal conditions,” I wish he had clarified whether this agreement extends to the differential-payback hypothesis and parental optimization of sex ratio or whether he favors the alternative (and more common) view that infanticide was a form of population control.

Graburn’s account of the *sipiniq* phenomenon is fascinating not only for students of Inuit culture but for anyone interested in gender definition. It provides one good answer to the question posed by Schrire and Steiger (1974:179–80): Why didn’t Inuit just raise some of their daughters to be hunters? (As Wenzel notes, some of them in fact did!) It may also account for some component of juvenile sex-ratio biases in historic Inuit censuses. I doubt that it provides a general answer, however, given that at puberty *sipiniit* assume a (modified) female gender role. Against the unknown but apparently low frequency of *sipiniit* we must consider the extraordinarily male-biased juvenile sex ratios of some of the populations discussed here and the specific instances of female infanticide documented by Rasmussen and Jenness. For these reasons, I doubt Graburn’s suggestion that his hypothesis “is at least as likely as mass infanticide among the child-loving Inuit.” His own research on Inuit child abuse (Graburn 1987) and that of his col-

league Scheper-Hughes (1992) on Brazilian mothers certainly convince me that child loving and child harming are contextual and can coexist within communities and even within individuals. In any case, the cultural construction of *sipiniit*, as well as the less radical modification of the gender division of labor reported by Wenzel for some Baffin Land Inuit, reinforces the general thesis that the hunting-dominated economy of historic Inuit placed a premium on male-linked pursuits.

Remie suggests that we do not adequately consider the effects on infanticide of social disruption and demographic instability. Certainly the early censuses are snapshots of Inuit societies undergoing rapid demographic and social change, but how much demographic change is unclear. Damas (1975:411) concludes that Central Eskimo populations “were fairly stable during the nineteenth century and the first two decades of the twentieth.” To some unknown extent, our analysis underestimates demographic noise frozen in these data, but this is a rather vague criticism and one that applies to any analysis of them yet published. The question is whether the existence of migration, epidemics, and the like weakens our analysis, and Remie presents no argument that it does.

I am not as convinced as he that most Inuit expressed a strong preference for kindred endogamy. Even where such preferences clearly existed, the frequency of cousin marriages was a different matter. In the passage from Arima (1984:455) cited by Remie we read that “about one-tenth of marriages among the Qairnirmiut and Harvaqtuurmiut in 1921–1922 involved cousins,” while Damas (1975:414) states, “Although the Netsilik stated a preference for first-cousin marriage, they seldom practiced it, probably because the normal staggering by age, the low survival rate of all infants, and the unequal ratio of the sexes meant that their chances of eventually marrying a first cousin would be slim.” In any case, the Remie-Riches argument is that female infanticide was practiced to deny mates to (nonkindred) competitors. This is a rather high price to pay for spite, and I remain extremely skeptical. I am unaware of any statements by Inuit themselves that reveal such a motivation for female infanticide. In groups with highly male-biased sex ratios (and presumably female infanticide), marriageable females were reported to be scarce and eagerly sought. Daughters were thus bargaining chips that could be used to extract favors from suitors or their parents, particularly those who were not kin (since Inuit norms would have made it more difficult to demand these of kin). Indeed, the causal connection may have been the reverse of what Remie supposes: female infanticide created scarcity of brides, which led to child betrothals, and these (being long-term agreements) were most reliably contracted amongst close kin. Balikci (1984:424) says as much for the Netsilik: “In order to secure wives for their male children, mothers betrothed them to girls from their own kinship circle. First cousins were preferred. . . . The distrust of strangers was given as a reason for the preferred cousin marriage pattern.”

That a large percentage of infanticide cases in Remie’s

Arvilngjuarmiut sample occurred within just two extended families is interesting but hardly conclusive evidence for or against his explanation or mine. He would need to show that these families lacked opportunities to find mates for their daughters within their kindreds to have evidence relevant to this hypothesis.

I am pleased that Roth and Wenzel have understood and applauded what we are up to in this article both strategically (i.e., hypothetico-deductive research and comparison of multiple alternative hypotheses rather than the usual anthropological practice of ethnographic induction and theoretical advocacy) and tactically (use of model life tables, operational measures of parental investment, etc.). It is certainly true, as Roth and Borgerhoff Mulder caution, that figuring out how to measure parental investment is a thorny matter; it comes down to the fact that social interactions, especially those stretching over lifetimes, are very complicated. As we stressed, this is not the final word on Inuit sex ratios; we hope that it is a useful interim assessment.

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Documentation

The Managing Committee of the British School at Athens has approved a program to preserve the 65,000 photographs in the school's archives and make them accessible to archaeologists, art historians, ethnographers, geologists, and other interested parties through an innovative computer imaging system. These documents, dating to 1886, document the school's extended involvement in the history and civilization of Greece, the Balkans, and Asia Minor. More than just archaeological records, these photographs include records of cultural ceremonies, events, regional costumes, and customs which characterized life 80-100 years ago but have since passed into obscurity.

Of particular interest are photographs documenting lost monuments and civilizations. The Byzantine Fund collection, for example, contains photographs, drawings, and observations on monuments in Salonika (many destroyed in the great fire of 1917), Cyprus (damaged or destroyed in 1974), Yugoslavia (imperiled since 1992),

and many others in Asia Minor and the Middle East. The John Pendlebury Collection is the personal archive of the scholar and war hero, whose knowledge of the archaeology of Crete remains unparalleled. The Knossos excavation records document all the work done after Sir Arthur Evans on the Palace of King Minos and the home of the well-known Minotaur of Greek mythology.

These images and the many others in the collection will be scanned at high resolution (1200 dpi and upward) into a computer database with all the accompanying information. All 65,000 images will be simultaneously available. Additionally, the entire collection will be accessible by modem and telephone lines, thus providing unprecedented access to the collection while preserving the original images. Funding is being sought for this five-year program. For further information, write: British School at Athens, 52 Odos Souedias, GR-106 76 Athens, Greece.

thoroughly to their husbands' cultures that they failed to transmit any of their own distinctive cultures to their children. Such a view, effectively removing females from cultural transmission, is in direct opposition to the ethnographic evidence.

Perhaps this is why Soltis et al. shift from exogamous clans to endogamous "communities" and "tribes" when discussing "cultural variation among groups" (pp. 479–81). Although these "entities" may be more culturally distinct, they fare no better than clans in terms of being enduring units. Chagnon (1992:185) has pointed out that villages "change in composition so rapidly" that the idea of "intervillage" warfare is misleading, and Fried (1975) has argued against the validity of the concept of "tribe" as either a political or a cultural unit. Despite conformist cultural transmission biases, even cultural group selection requires a type of enduring group that simply has not existed during human evolution. The ethnographic reality is that "even on the simplest cultural levels social groups are task-oriented and do not constantly reassemble the same standard membership to achieve their purposes" (p. 84).

This critique of the notion of cultural group selection should not be taken as a critique of the more general idea that culture may literally "evolve" in a way analogous to biological evolution. Indeed, the fact that so much of human culture has been traditional, passed from parents to offspring, suggests that models based on natural (individual) selection may greatly increase our knowledge of human culture. The further fact that human social organization is largely dependent not on enduring and bounded groups but on traditions that encourage the identification of, and situational cooperation with, co-descendants of common ancestors is also consistent with the idea that human culture evolved through natural (individual) selection instead of group selection.

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Inuit Sex Ratio: A Correction

ERIC ALDEN SMITH

Department of Anthropology, University of Washington, Seattle, Wash. 98195, U.S.A.
(easmith@u.washington.edu). 20 III 95

Monique Borgerhoff Mulder (personal communication) has alerted me to the fact that the article on Inuit sex ratio I coauthored (CA 35:595–624) contains an erroneous test of Fisher's (1930) theory of sex ratio evolution. Fisher's theory predicts that selection favors parents who allocate equal amounts of parental investment (PI) to male and female offspring; it follows that if the sexes differ in PI, then parents will bias their sex ratio so as to raise more offspring of the cheaper sex. Our error has two parts: (1) we computed PI as the cost per child (of each sex) *surviving* to adulthood, whereas it should be computed per child (of each sex) *ever born*, and (2) we tested Fisher's model by seeing if PI expended per son equaled that expended on average per daughter, whereas it should be tested by seeing if the *total* PI expended on sons equals that expended on daughters. Thus, our statement that higher juvenile mortality makes boys more expensive than girls is exactly contrary to Fisherian logic: it makes them *less* expensive. (However, the longer period of parental dependence due to later marriage does increase child-years per boy born and thus does increase PI as defined by Fisher.) Furthermore, since our "PI ratio" employs PI per surviving child, it is a doubly incorrect index of Fisherian optimization.

As it turns out, these errors do not affect our conclusions that Fisher's model cannot explain the male-biased juvenile sex ratios found in most of the ten Inuit populations examined. The data to verify this appeared in our table 6. We operationalized PI as "child-years" of dependency and defined this (in footnote *a* of table 6) as "years of dependency (birth to marriage), assuming initial cohort sizes, marriage ages, and mortality schedules specified in model populations (table 3)." We also pointed out that "this measure includes parental investment in offspring who die in childhood as well as those who survive to adulthood (as Fisher's theory says it should)" (p. 608). Hence, given our assumptions about how to measure PI, Fisher's model would explain the Inuit data only if child-years for offspring of each sex were equal. But our analysis indicates that male child-years exceed those of females in every population (table 6). A new analysis using these data indicates that this deviation from equality is statistically significant in all but one of the ten populations (*Z*-score binomial, $p < .05$). Since

the exception, Baffin, is the only population *without* a male-biased juvenile sex ratio, I continue to reject Fisher's model as an explanation for Inuit sex-ratio bias.

In reconsidering this matter, it also occurred to me to examine Maynard Smith's (1980) model of PI. This model can be viewed as a variant of Fisher's theory, with altered assumptions about the constraints facing parents. It seems particularly relevant to the Inuit case (or any case of biased sex ratios involving infanticide) in that it explicitly assumes that there is a time lag between conception and parental identification of the sex of offspring (e.g., at birth). Thus there is a trade-off between PI in individual offspring before sex identification and the additional PI needed to raise offspring of the more costly sex. Selection can favor cessation of investment in a proportion of offspring of the more costly sex only if the former cost is lower and the latter cost is high. While the first condition might be met in humans (since the approximately one year required to gestate an offspring and return to ovulation if that infant is not nursed is short relative to total PI per offspring), the second condition is not met in the Inuit populations. In all of them, according to our calculations, boys cost more

than girls. Hence, given our assumptions about how to measure PI, Maynard Smith's reformulation of Fisherian logic also fails to explain the biased sex ratios of Inuit populations.

In sum, I continue to uphold what we called the "pay-back hypothesis" (termed "local resource enhancement" in the evolutionary ecology literature) as the most likely explanation for this social and demographic pattern. In other words, even though sons did not cost less to raise (and indeed likely cost more), once grown they were better positioned to provide goods and labor to their natal households than were daughters, particularly in those Inuit populations at greatest risk of food shortage.

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Errata

Through a printer's error, the biographical sketches were omitted from Smith and Smith's article in the December 1994 issue. They should have appeared as follows:

ERIC A. SMITH is Professor of Anthropology at the University of Washington (Seattle, Wash. 98195, U.S.A.). Born in 1949, he was educated at the University of California, Santa Barbara (B.A., 1971) and Cornell University (M.A., 1976; Ph.D., 1980). His research interests are ecological and evolutionary analyses of human social behavior and Inuit ecology, demography, and history. Among his publications are *Inujjamiut Foraging Strategies: Evolutionary Ecology of an Arctic Hunting Economy* (Hawthorne, N.Y.: Aldine de Gruyter, 1991), "Anthropological Applications of Optimal Foraging Theory: A Critical Review" (*CURRENT ANTHROPOLOGY* 24:625–51), and, with Bruce Winterhalder, the collections *Hunter-Gatherer Foraging Strategies: Ethnographic and Archaeological Analyses* (Chicago: University of Chicago Press, 1981) and *Evolutionary Ecology and Human Behavior* (Hawthorne, N.Y.: Aldine de Gruyter, 1992).

S. ABIGAIL SMITH received her B.S. from Colorado State University in 1975, an M.Ed. degree from the University of Washington in 1983, and an M.A. from the latter university in 1986. Her M.A. research was on Inuit demography.

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A misunderstanding by the copy editor produced an error in the final sentence of Martyn Hammersley's comment on Robert Auinger's article in the February 1995 issue (p. 120). The sentence should have read "However, what he proposes is not convincing as a scientific basis for ethnographic work: it is questionable in its own terms, and hardly begins to take account of the fundamental methodological, philosophical, and political disagreements which currently plague anthropology and the other social sciences."