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Author(s): Carmel Schrire and William Lee Steiger

Source: *Man*, Jun., 1974, New Series, Vol. 9, No. 2 (Jun., 1974), pp. 161-184

Published by: Royal Anthropological Institute of Great Britain and Ireland

Stable URL: <https://www.jstor.org/stable/2800072>

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A MATTER OF LIFE AND DEATH: AN INVESTIGATION INTO THE PRACTICE OF FEMALE INFANTICIDE IN THE ARCTIC

CARMEL SCHRIRE

State University of New York at Binghamton

WILLIAM LEE STEIGER

Université de Montréal

I

Introduction

Traditional Eskimo societies are depicted as existing in a harsh and hostile environment. People relied heavily upon mobile food resources such as seal and caribou, and despite their well-documented hunting skills, suffered intensely when bad weather denied them access to their prey (Boas 1964: 19–20), or forced them to migrate in search of food. Ethnographic reports suggest that infanticide was a direct result of these privations, since infants born in times of stress were immediately killed to ensure the survival of the rest of the group.

The sex of the infant was of no consequence under these circumstances, for the lack of food, widespread debilitation and enforced moving allowed no bias towards males or females. It was the immediate solution to the problem of survival, and as such can be considered adaptive behaviour on the part of the group, to free the mother and themselves from the incessant demands of a newborn child. The same can be said of the long-term effects of this practice, which served effectively to increase the proportion of older individuals in the population, thus promoting greater stability of the group as a whole (Freeman 1971).

There is, however, another aspect of Arctic infanticide that is not the direct outcome of environmental pressures. Ethnographers report the practice of systematic female infanticide among many peoples (Birdsell 1968; 1972: 354–63; Panizo 1965). The rationale behind this practice was essentially one of economics. Although Eskimos were said to appreciate the role of women in their society, they regarded girls as unproductive consumers who did no hunting (Balıkcı 1968: 81; 1970: 150–2; Hiatt 1970; Jenness 1922: 166–7) and who left their homes to marry as soon as they became useful in other ways (Rasmussen 1931: 139). Since babies had to be breastfed for several years females were often killed at birth in order to save time between successive births and in the hope of producing a male next time (Rasmussen 1931: 140). On top of all this, male hunters were subject to greater risks than other members of Arctic groups, to such an extent that the presence of two or even three sons did not automatically ensure the food supply of a family in the years ahead. People desired many sons, and were said to practise female infanticide in a systematic manner rather than in response to immediate

Man (N.S.) 9, 161–184.

stress, with an eye to the future, in the recognition and anticipation of the needs to come (Balıkcı 1970: 153; Dunn 1968: 224).

The frequency with which systematic female infanticide occurred may have varied but for the Arctic people, the basic effects of this policy upon the structure of the population are clear. Among juveniles, males predominate over females. The relatively high mortality rate of males through hunting accidents, results in a predominance of female over male adults. In conjunction the two processes produce a balance between males and females in the overall count of the population (Weyer 1962: 134-7).

Up to this point the process appears smooth and regulatory, in that economically unproductive members are eliminated from the group at infancy, without creating repercussions among the more mature ranks. However, if an attempt is made to discover the amount of female infanticide needed to achieve this harmony, the picture becomes considerably less straightforward. Birdsell hazarded a guess that the answer lay in the upper range of between 15 per cent. and 50 per cent. of all female births (1968: 236-7). The most direct evidence for the Arctic, however, is found in a series of reproductive histories recorded among the Netsilik by Knud Rasmussen in 1922-3 (Rasmussen 1931: 139-42).

He recorded '96 births for 18 marriages and 38 girls killed' (1931: 141). Assuming that the initial sex ratios at birth were roughly equal for this sample, the figures suggest that some 80 per cent. of girls were killed at birth. This astonishingly high figure, together with 'the fact that of the 259 souls in the Netsilik tribe only 109 are female, whereas 150 are males', led the author to predict that 'Despite the high birth-rate the tribe is moving towards extinction if girl children are to be consistently suppressed' (Rasmussen 1931: 141).

The purpose of this article is to examine Rasmussen's prediction in a more objective way, in order to establish to what extent any hunting and gathering group may practise female infanticide and yet remain viable. Our analysis covers two main fields. First, the ethnographic data are reviewed in order to discover just how systematically female infanticide was practised in the Arctic. Secondly, we have constructed a computer simulation model of a typical Arctic hunting group. By varying the degree of female infanticide we are able to estimate the extent to which the recorded method of population control may be practised before the group first becomes unable to maintain a normal growth rate and becomes extinct.

II

Ethnography

The demography of hunter-gatherers is a complex and difficult field of study. By the time scientists were able to view such people with more than a dilettante's interest, many groups had become extinct. For the past fifty years, the territories of hunter-gatherers have been considered 'refuge areas', land rejected by other human populations into which hunters were forced to retreat in order to survive. It is thus postulated that the density of modern hunters reflects their recent retreat from more attractive environments, rather than presenting a true picture of traditional hunter-gatherer adaptation (see Hiatt 1970; also discussion in Lee & DeVore 1968: 210-12). This is an issue to be faced when analysing almost any

aspect of modern hunter-gatherer behaviour, including methods of population control.

To infer the number of girls killed at birth one needs several facts. First, the sex ratio at birth must be known. Next, one needs the mortality curves for both males and females. Finally, with a knowledge of other factors such as the degree of migration, it is possible to form the age distribution of males and females in the population and from it, estimate the extent to which newborn females have been suppressed. Naturally, the estimates are more reliable for larger populations.

Given that most hunter-gatherers have no written records, their census data are necessarily a short-term affair. An ethnographer who documents the composition of a group for a year or two cannot be sure that his findings reflect the true long-term picture (Lee 1972). Clearly, for a practice such as infanticide, this restriction means that its systematic nature can only be guessed at by analysing the sex ratios of the population at a given point in time. In addition, it is very difficult to assess the ages of preliterate individuals accurately, and undoubtedly risky to rely upon the memories of people concerning their reproductive histories. As a result, demographers may prefer to impose normal population curves upon their subjects, rather than expose the entire sample to the biases of unreliable data (see Howell 1973).

In addition to the methodological hazards outlined above, the incidence and importance of infanticide in Arctic societies may have been influenced by increasing contact with the outside world. The arrival of traders, whalers, missionaries and policemen affected the life-styles of the hunters: their diet and hunting patterns changed in response to the demands of whalers, traders and the introduction of guns and whaleboats. Settlement patterns were altered in accord with the presence of trade posts or whaling ships whose staff provided food all year round, and indigenous morals were assaulted and then re-adapted to the standards of Christianity and the Royal Canadian Mounted Police.¹ Whilst these changes might not have spelled an immediate end to the practice of infanticide they probably were accompanied by a considerable drop in its publicity if not in its actual popularity.

We have tried therefore to draw our quantitative data on female infanticide from the reports of people who visited the areas before the full impact of European penetration was felt there. Our earliest source, Boas, noted that whalers and traders were in the areas under discussion but he documented only the traditional methods of hunting, cooking, dress and travel (Boas 1901; 1907; 1964). Diamond Jenness studied the Copper Eskimo for two years (1914–16) as a member of the Canadian Arctic Expedition and observed that they obtained guns, ammunition and knives from the neighbouring Akilinnik people in exchange for caribou skins, fox and musk-ox pelts (Jenness 1922: 48).

The members of the Fifth Thule Expedition of 1921–1924 discovered varying degrees of acculturation among the groups that they studied. At the relatively isolated end of the scale were the Utkuhikhalingmiut of Back's River, whom Rasmussen visited in 1923, and assessed as follows:

... They were ... not altogether unacquainted with white men and white customs, though the nearest trading station was so far distant that it sometimes took a half a year to get there and back. The journey was only made by the younger men, so that none of the older ones had ever seen a white man before (Rasmussen 1927: 192).

The Netsilik were less isolated, though they still subsisted chiefly by hunting, and migrated annually between the interior and the coast. They moved inland from mid-July to December, hunting caribou with guns and fishing with traditional spears and traps (Rasmussen 1927: 206) and the rest of the year was spent on the sea ice, harpooning seals (1927: 228–30). They used dogs for transport and hunting (1927: 204). Their heavy reliance on these sources of food was exemplified by the fact that 25 people out of a group of some 260 souls died of starvation on two separate occasions in the three years prior to Rasmussen's visit (1927: 223).

The Caribou Eskimos also showed signs of contact. Rasmussen noted that the Quarnermiut, who had been dealing with whalers for a long time, had lost some of their 'original character' (Rasmussen 1927: 54), but became more specific when he observed that each hunter among the Caribou people owned a rifle (1927: 73). The effects of this introduction were profound among the Copper Eskimos, and Rasmussen noted that since the establishment of a trading post at Kent, where the Umingmaktôrmiut gained easy access to guns, the caribou had virtually disappeared. He was certain that they had not been exterminated, but rather that they were terrified by the noise of the guns and had gone elsewhere, and he expressed some foreboding about the economic future of the Copper Eskimos in that region (Rasmussen 1932: 13, 72).

All these changes were minor compared to the situation found among the MacKenzie Eskimos, who had '... changed their ways in most essential respects', as follows:

... skin boats had given place to schooners, sealing to trapping and fur trading on modern lines; earth-and-stone huts lined with driftwood were now replaced by something approaching modern bungalows or villas; and in addition to all these external changes, their ancient faith had given place to Christianity (Rasmussen 1927: 302).

These impressions do not, of course, allow us to make any numerical adjustments in the population figures recorded among the various groups (see table 1), but the varying degrees of contact and their possible implications should be borne in mind when drawing conclusions from the data.

In theory, the evidence of female infanticide may range from direct observation over a prolonged period to a series of legends and myths. In our study of the ethnographic literature, we find only one report that might be classified as 'direct' evidence. Diamond Jenness recorded the birth of a girl on January 22, 1915, in the vicinity of his station, as follows:

Neither of the parents wanted to have any children at the time, for their companions were intending to return to Tree River the following day. They were both young still, they said, and in all probability would have at least one more child later, a boy perhaps who could take care of them when they grew old. The woman therefore suffocated her child and laid it out on the ground a few yards from the camp where it was soon covered by the drifting snow. Only a year or two previously this couple had similarly exposed another little baby girl (Jenness 1922: 166).

Less direct evidence may be induced from a series of reproductive histories that Rasmussen made among the Netsilik in 1922, for the explicit purpose of illustrating the extent to which female infanticide was practised there (Rasmussen 1931: 140–1). The material was obtained by asking 'all of the women at Maler-ualik how many children they had borne, and how many girls they had 'put out

TABLE 1. Population distribution for Arctic groups.

Category	Group	Numbers						Grand Total	F/M ratio		
		Females			Males				Juv.	Ad.	Overall
		Juv.	Ad.	Tot.	Juv.	Ad.	Tot.				
A	Utkuhikhalingmiut ¹	27	39	66	57	40	97	163	47.4	97.5	68.0
	Netsilik ²	66	123	189	138	119	257	446	47.8	103.4	73.5
	Netsilik ³	37	74	111	78	72	150	261	47.4	102.8	74.0
	Copper ⁴	38	115	153	102	116	218	371	37.3	99.1	70.2
B	Copper ⁵	24	50	74	23	55	78	152	104.4	90.9	94.9
	Caribou ⁶	83	129	212	120	102	222	434	69.2	126.5	95.5
	Kinipetu ⁷	27	46	73	38	35	73	146	71.1	131.4	100.0
	Sauniktumiut ⁸	33	58	91	41	46	87	178	80.5	126.1	104.6
	Aivilik ⁹	15	34	49	27	26	53	102	55.6	130.8	92.5
	Central ¹⁰	39	119	158	41	111	152	310	95.1	107.2	104.0
	Alaskan ¹¹	46	29	75	50	30	80	155	92.0	96.7	93.8
	Alaskan ¹²	14	52	66	27	45	72	138	51.9	115.6	91.7

KEY TO TABLE 1

¹ Rasmussen 1931: 473–7. The household listings used here were made in spring 1923. Rasmussen claimed a total of 164 people, made up of 97 males and 67 females. Our count contains one less female.

² Boas 1907: 377–8. These figures are drawn from a census made by Captain George Comer in 1902.

³ Rasmussen 1931: 84–90. The household listings used here were made in spring 1923. Ten groups were counted, eight by Rasmussen himself and two by other people (84–5). Rasmussen claimed a total of 259 people present, made up of 150 males and 109 females. A summary (84) lists the membership of five groups, but these are not cross-referenced in the household lists. In addition to this, certain places, such as Satoq (said to be the Simpson Peninsula 86), are incorrectly located on the accompanying map (facing 9). Consequently, our attempts to cross-match his figures and places are hampered. In all, however, our counts located two extra females, giving an overall total of 261 persons.

⁴ Rasmussen 1932: 78–85. These household listings apply to the Eastern group of Copper Eskimos, living between Ogden Bay and Bathurst Inlet at an unspecified date between 22 November 1923 and 15 February 1924. The only overall total cited is ‘377 persons’ (69–7). Our counts located 373 individuals, though two infants of unspecified sex were omitted from the analysis in table 1.

⁵ Jenness 1922: 42. These counts record the composition of two groups. One was located at Bernard Harbour in November 1915 and totalled 127 persons; the other was found at Tree River in February 1916 and contained 25 individuals. This is the only breakdown given by Jenness for the Copper Eskimo, whom he estimated to total some 700–800 souls (p. 42). Curiously enough, he cited the figures we use to indicate the preponderance of males over females in this society, but noted that the Tree River group was atypical in that it contained six girls and only two boys. The disparity between these figures and Rasmussen’s count, made nine years later, will be discussed in the concluding section of the article.

⁶ Birket-Smith 1929, part I: 65 sqq. These figures were compiled by the author and Knud Rasmussen between 1922 and 1923. They represent the totals of household lists compiled and added by the author. Several errors in his additions have been corrected (the numbers of juvenile males, adult females and juvenile females) but the overall difference between his totals and ours, is only 2 out of 434 souls.

⁷ Boas 1901: 7. These figures were compiled by Captain George Comer in March 1898. Boas described the group as a tribe on the west coast of Hudson Bay living at Chesterfield Inlet. Birket-Smith included them with the other Barren Ground Caribou people as ‘Quarnermiut’ (1929: 68). The figures should therefore be compared with his total series in 6.

⁸ Boas 1907: 377–8. These figures were collected by Captain George Comer in 1902. They are the same group as the Hauneqtôrmiut that Birket-Smith lists with other Caribou Eskimos (1929, part I: 67–8).

⁹ Boas 1901: 7. The counts were made by Captain George Comer in March, 1898. Boas described their territory as that lying between Repulse Bay and Chesterfield Inlet and Mathiasen included them with his Iglulik people (1928: 5; 15–17).

¹⁰ Boas 1964: 18. These figures represent a census from an unspecified source, made in December, 1883, of settlements on Baffin Land, Davis Strait and Padli. We exclude eighteen individuals whose sex was unspecified.

¹¹ Weyer 1962: 134. The figures are attributed to his own fieldnotes.

¹² Smith 1902: 113–14. The count was made at Cape Smyth where an observation station was occupied for two years from the autumn of 1881.

of the way' (1931: 140). He then presented the testimony of eighteen individuals ranging in age from 20 to 65 years, and concluded that there were '96 births for 18 marriages and 38 girls killed' (1931: 141). Unfortunately there is no mention of the sex of every child born, but assuming that the same number of boys and girls were born, we may infer from Rasmussen's figures that 48 girls were born, 80 per cent. of whom were killed at birth, leaving 20.8 infant females per 100 males.

These figures have been widely quoted (Balikci 1967: 616; 1970: 148; Hiatt 1970:7) and Balikci goes as far as to say that the sex ratio found in the reproductive histories 'is in harmony with his [Rasmussen's] 1923 census as a whole' (1967: 616). This is not so. Our analysis of the census reveals an overall ratio of 74 females per 100 males, and among juveniles, 48 girls per 100 boys. This disparity led us to check the original count. We find that Rasmussen has added the cited figures incorrectly. He records 116 live births, not 96, and as a result, the percentage of girls killed, assuming an equal distribution of male and female births, is only 67. This gives us a ratio of 34.5 infant girls per 100 boys, a corrected figure that now harmonises better with the rest of his data.²

The next level of evidence for female infanticide in the Arctic occurs in the census data from a number of groups. The quality of the material we use here varies, and it may be divided into four main series. First, there are those populations that are divided into males and females with no regard to the age groupings (Rasmussen 1932: 69–71; Stefánsson 1914: 131). Second, there are the groups where married people are differentiated according to sex but the children are not (Mathiassen 1928: 15 sqq.; Parry 1824: 492). The third series is sub-divided as 'Men', 'Women', 'Boys' and 'Girls' with no further elucidation of these terms (Boas 1964: 18; 1901: 7; 1907: 377–8; Birket-Smith 1929 Part I: 65–67; Jenness 1922: 42; Smith 1902: 113–14; Weyer 1962: 134, 136). The final group contains a head count of males and females as well as household lists which record the name, sex and biological role of every individual in each family unit present (Rasmussen 1931: 84–90, 473–7; 1932: 78–85). The lists were collected by 'writing down the names of everyone in the group at one particular time' (Rasmussen 1932: 71).

The sex ratios found in different age groups in a population must be known in order to infer the degree to which female infanticide was practised. We have not found any census made before 1950 that provides the age of every member of the group. The best we have is a rough division of groups into 'adult' and 'juvenile' components, which is found in the last two series mentioned above. It is quite explicitly contained in the first, though the authors never explain quite how they determined their groupings. It is implicit in the household lists of the latter group, and we analysed these by counting as adults all married people as well as those described as 'old', 'full-grown' or 'widowed', and as juveniles, those unmarried people listed as 'children' or 'half-grown'.

We feel that these two series are comparable, not because there is any direct evidence that the ethnographers categorised adults and juveniles as we have, but because it seems that marriage was the most likely indicator of maturity among Arctic people. The Eskimos were often swaddled in heavy outdoor clothing. Some sources report that they were very modest and refused to undress in front of strangers (Birket-Smith 1924: 410–11). In fact, Amundsen observed that '... these people [Netsilik], of either sex, never disrobe in the presence of strangers,

except in the greatest emergency' (1908, vol. 11: 13). Whether this was a widespread feature or not, the Arctic ethnographer must have been disadvantaged compared with his tropical counterpart whose trained eye was free to wander at will over his subjects. It seems inevitable that the Arctic worker relied more heavily on a cultural criterion of maturity even though he might originally have intended to use physiological ones.

This is not to say that we are satisfied with the nature of the age assessment data. Rather, we have some confidence that the rough assessments of ethnographers and our figures derived from analyses of household lists, are comparable. The result of these findings are listed in table 1. The only group excluded here is the Sinamiut (Boas 1907: 377-8) because the sample of forty-five individuals is too small to be used for our purposes.

Weyer (1962: 133-7) claimed that all these groups practised female infanticide, and this is confirmed by the primary sources for the Utkuhikhalingmiut (Rasmussen 1931: 489), Netsilik (Rasmussen 1931: 139), Copper (Jenness 1922: 165-7) all the Central Eskimo (Boas 1964: 172) and the Alaskans (Weyer 1962: fn. 134). A secondary source (Oswalt 1967: 193-4) confirms this practice among all Alaskans. The primary source for the Caribou figures, Kaj Birket-Smith, disclaimed any knowledge of the current status of female infanticide and attributed the biased sex ratios to unspecified 'special circumstances' (1929, Part I: 294, 66). His followers are far less hesitant (Mathiassen 1928: 212; Weyer 1962: 134-7) and Birdsell uses the figures in a very specific way to illustrate the 'success of their population planning' using female infanticide (1972: 362).

The groups in table 1 fall into two distinct categories, called A and B. Category A includes the first four groups (Utkuhikhalingmiut, Netsilik (2), and Copper). The sex ratios for these groups range from 37.3 to 48.4 girls per 100 boys in the juvenile count and from 68 to 74 females per 100 males overall. The remaining groups comprise Category B, in which the juvenile ratios range from 55.6 to 104.4 girls per 100 boys whilst the overall ratios cover the more restricted range from 91.7 to 104.6 females per 100 males. Because the sex ratios for juveniles and overall never overlap between the two categories, they really are distinct.

The data listed in table 1 suggest the presence of two somewhat different behaviour patterns:

1. The preponderance of males over females in Category A is very marked. This is particularly noteworthy when viewing the ratios found in the overall population count, since the effects of a higher male mortality rate through hunting accidents has already registered. Assuming that roughly the same number of boys and girls are born, these figures might well reflect a high degree of female infanticide: the juvenile sex ratios suggest that between 52 and 63 per cent., or one in two girls born were killed, and a more conservative estimate based on the overall counts, suggests that at least 30 per cent., or about one in three girls were suppressed at birth.
2. The pattern evidenced in the groups of Category B is different. The initial juvenile sexual disparity is counterbalanced by the adult ratios to produce a situation that approaches parity in the overall population count.

Various authors suggest that the latter result reflects female infanticide being

counterbalanced by a high male accidental death rate. Birdsell reviewed Birket-Smith's figures for the Caribou Eskimo in this light, and concluded:

The interesting point about this Caribou Eskimo census is that it provides *definite evidence* for population planning from generation to generation. There is *no other* explanation for the high sex ratio among the immature other than that infanticide was responsible for it. The low sex ratio among adults . . . testifies to the success of their population planning. For these values represent a balanced state in which a minority of superior hunters could take over widows and children of the men who died (Birdsell 1972: 362, our emphasis).

Leaving aside for the moment the question of exactly how many males succumb to hunting accidents, it should be clearly understood that the census data do not, in themselves, support Birdsell's contention since the figures could equally well reflect a systematic error by the ethnographer in ageing males and females, as follows. It seems that marriage was one, if not *the* most important criterion used by Arctic workers to distinguish border-line juveniles from adults. Most sources agree that alliances took place at an early age, with girls being married when they were younger than boys. 'Marriage', says Weyer, 'occurs at a youthful age, especially in the case of girls' (1962: 139). Thus, a girl might be wed before puberty whilst a boy had to wait until he was able to provide food and shelter for his new family (Balicki 1970: 102; Birket-Smith 1929, Part I: 292; 1935: 143; Jenness 1922: 158; Mathiassen 1928: 210; Rasmussen 1931: facing 472). The precise age differences are seldom quoted, but where it is attempted (Balicki 1970: 102) there is a gap of some five to six years between the marriage age of males and females.

If marriage were a criterion used to assess maturity, one might expect to find girls of 12 to 18 years classified as women, and boys of the same age grouped with the juveniles. Such an error, if repeated systematically would reduce the incidence of juvenile females, and inflate the number of adult ones. In the overall population count, however, we might expect to find roughly the same number of males and females and this is precisely the case in Category B of table 1.

An illustration of the errors implicit in using marriage as a criterion of maturity in Arctic groups is seen in an analysis of a census made by Reverend A. L. Fleming of 469 people living in 23 settlements in the Lake Harbour and Dorset areas in 1913.³ The findings of his census are tabulated in table 2a to show that only half (51.7 per cent.) of the people are classified according to age, whilst nearly all (91.7 per cent.) of the rest are listed according to their marital status only.

Table 2b shows the sex ratios of juveniles and adults in the population, and the figures are comparable to those found in table 1 in that all married and widowed people are automatically classified as 'Adult', though our definition of 'Juvenile' is more precise. The ratios are very similar to those found among Category B of table 1. In addition to this, a detailed breakdown of the census data shown in table 2a shows that the proportion of females declines steadily in the age groupings from 0 to 19 years—a process that might be interpreted as reflecting the steady decline in the incidence of female infanticide over the two decades prior to the census taking. However, the same table 2a also shows that there are far more females than males over 20 years of age and that some of these (29) are widows.

In all, these data reflect a population where wives are more easily gained than husbands, where women marry earlier than men and where there is a higher male mortality rate for individuals over 20 years of age. The interplay of these features

TABLE 2. Fleming census of 23 settlements in Lake Harbour and Dorset areas (1913)

2a. Age distribution

Age	Sex			Total
	Male	Female	None given	
0-4	27	24		
5-9	53	45		
10-14	29	22		
15-19	14	9		
20-24	1	4 (1)		
25-29	1	2 (2)		
30-34		3		
35-39		4 (2)		
40-44		3 (3)		
45-49		1 (1)		
				242
Married	98	102*		
Widowed/Old		20		
No Age	3	2		
Other			2	
Total	226	241	2	269

The bracketed figures in the 'Female' column indicate the number of widows in the group.

*Four cases of men married to two wives are recorded.

2b. Sex ratios derived from table 2a (467 individuals)

Assuming Juveniles age (yrs.)	F/M Ratio		
	juv.	adults	overall
0-4	88.9	109.0	106.6
0-9	86.3	117.8	106.6
0-14	83.5	128.2	106.6
0-19	81.3	136.9	106.7

2c. Sex ratios derived from table 2a (223 individuals)

Age (yrs)	Numbers		F/M ratio
	females	males	
0-4	24	27	88.9
5-9	45	53	84.9
10-14	22	29	75.8
15-19	9	14	64.3

alone might well produce the situation found here, as well as in all the other groups in Category B, without any female infanticide being practised at all.

These observations do not necessarily deny that female infanticide was practised in the Eskimo groups under discussion, but they do suggest plausible alternative explanations for the sexual disparities found in the groups of Category B. Although we have volunteered an estimate of the extent to which female infanticide might have been practised in the groups of Category A, this should be regarded as a very

rough estimate because the same potential mis-ageing of females probably occurred here as in Category B, and in addition to this, we do not know the extent of the male accidental death rate. Given all these limitations on the data we have used, we are forced to reject the kinds of conclusion reached by Birdsell, who used Birket-Smith's figures for the Caribou Eskimo to conclude that they suppressed:

'... at least one out of every three girl babies over and above the children of both sexes who were killed to provide the necessary spacing' (Birdsell 1972: 362).

These errors and limitations of the Arctic ethnographic data on female infanticide led us to try and devise a more independent way to assess to what extent a small Arctic group might suppress certain members at birth and yet remain viable. The problem should now be phrased in more biological terms: whatever the social restrictions that prevent women from hunting and induce the Eskimo economic categories of 'useful' and 'unproductive' people, a stark and immutable biological fact remains. Women bear children and the future of the society is inevitably invested in this potential. Severe culling of female ranks in infancy, especially in small, endogamous groups, will surely jeopardise the future of the whole group. We turn now to a discussion of this matter using a computer simulation model to predict the effects of the practice of varying degrees of female infanticide on small Arctic groups.

III

The simulation model and its consequences

Three basic processes combine to govern the dynamics of all populations. These are the processes of birth, death, and migration. Often these processes may be easily described mathematically, as certain random functions, and together they determine the future size and age composition of the population.

The age-specific *mortality function* gives the instantaneous death rate at each moment of time, for both females and males of given age within the population. The age-specific *fertility function* describes the instantaneous birth rate at any time, for women of given age, with respect to both male and female offspring. Finally, the *migration function* specifies the instantaneous rate of adding or subtracting males and females of given ages to the population, by means of emigration or immigration.

Theoretically, there is nothing further that need be known in order to describe the future size and composition of the population. In fact, a particular specification of these three functions (and the way they interact) would constitute a model for the dynamics of the population 'determined' by the functions. Yet, a proviso must be made concerning the use of 'determine'.

This is necessary because of the random component in population dynamics. The birth, death, and migration processes are subject to chance fluctuations. For example, whatever death rate governs the future of the ten 80-year-olds living in a group, there is always the chance that 0, 1, 2, ... or all 10 of them will live yet another year. The rate given by the mortality function is interpreted as being 'on the average' with respect to a probability distribution that prescribes the likelihood of values around this average.

In any sensible model for these random components, the larger the group size, the more overwhelming the odds that the actual death rates will agree with their averages as given by the mortality function. However, in small groups, the precise nature of the random component would be important. We shall keep these remarks in mind and shall always use 'determine' in that sense.

While it is easy to describe the three functions that determine the interesting features of the future course of a population, it is far more difficult to describe that future course of events. Except in the most elementary cases the mathematics is simply too intractable. Indeed, without gross simplifying assumptions that generally render the resulting model useless, mathematical analysis is quite impotent for the task of depicting stochastic population dynamics.

Computer simulations, however, are often exactly what is needed. They can take a particular specification for the three functions and, by tracing developments at *many* nearby time points, produce a time trajectory for the population that contains all the size and composition data of interest. By repeating this process many times, and by averaging the behaviour over all trajectories, the random components have decreasing effect and the resulting description can often be quite exact.

In what follows, we describe a particular model for population dynamics within small Eskimo groups. The nature of the three determining functions is discussed. The surprising fact is that the course of events seems fairly insensitive to small changes in these functions. Finally, we present our findings about how systematic female infanticide affects population dynamics. It will be seen that only a very small degree of this policy can be tolerated. In conjunction with the preceding remark, this powerful simulation technique would seem to disprove the ethnographic contention that female infanticide was systematically used as a population control mechanism (Birdsell 1968: 237).

1. *The model*

The three functions described above may not vary with time. When this is the case the population process is called *homogeneous*, and the same rate mechanisms prevail throughout the course of development of the population.

In our simulations we assume that the fertility, mortality, and migration functions are homogeneous. Partly, this creates simplicity. However, our major consideration was ignorance as to exactly how these functions might reasonably change in time.

Furthermore, we assume that the migration function is identically zero. This assumption clearly does not hold for some of the actual Eskimo groups that we examined. Neighbouring groups may exchange people through marriage, for example. Nevertheless, we wanted to get a feel for the degree of systematic female infanticide that such groups could tolerate, based only on the processes governing birth and death. Later, given this relationship, the role of migration might be investigated.

To be specific then, we used zero for the migration function. There was no movement of people, either in or out of the group, to affect the population size and composition.

The mortality function is given in table 3. The entries are the conditional probabilities of dying in a particular year of life, given survival up to the beginning

TABLE 3. Age specific mortality function.
(Age i means at least i but younger than $i+1$)

Age	Conditional Probability of dying		Age	Conditional Probability of dying		Age	Conditional Probability of dying	
	Male	Female		Male	Female		Male	Female
0	·108	·104	24	·043	·021	48	·038	·036
1	·021	·020	25	·043	·022	49	·039	·038
2	·011	·011	26	·043	·022	50	·040	·039
3	·012	·012	27	·042	·023	51	·042	·040
4	·012	·012	28	·042	·023	52	·044	·042
5	·012	·012	29	·041	·024	53	·045	·044
6	·013	·013	30	·040	·024	54	·047	·046
7	·013	·013	31	·038	·025	55	·049	·047
8	·014	·014	32	·037	·025	56	·051	·049
9	·014	·014	33	·035	·026	57	·053	·052
10	·015	·014	34	·034	·026	58	·056	·054
11	·015	·015	35	·033	·027	59	·058	·056
12	·016	·015	36	·031	·027	60	·060	·058
13	·016	·016	37	·031	·027	61	·063	·061
14	·021	·016	38	·030	·028	62	·065	·063
15	·025	·017	39	·030	·028	63	·068	·066
16	·029	·017	40	·030	·029	64	·071	·069
17	·032	·018	41	·031	·030	65	·074	·071
18	·035	·018	42	·031	·030	66	·077	·074
19	·037	·019	43	·032	·031	67	·080	·077
20	·039	·019	44	·033	·032	68	·083	·080
21	·041	·020	45	·034	·033	69	·086	·084
22	·042	·020	46	·035	·034			
23	·043	·021	47	·036	·035			

of that year. As is ordinarily the case, females have slightly lower values at all ages. We feel that the approximation of the instantaneous rate function by such a function (that governs the rates of death in intervals a year long) does not constitute a serious distortion.

The general 'U' shape of such conditional probability curves seems to be the same for all populations. Variations occur in the relationships between the probabilities for differing ages, say 0-2 years compared to 15-18 years. Our figures were chosen to accord with all the death data derived from death certificates from Baffin Island; there were 786 reported deaths from 1928 to 1967 (Kemp, field notes 1968).

The fertility function that we used is shown in table 4. The entries are the probabilities of a live birth within a calendar year, for a woman of each given age. We deny the small possibility of multiple births, aiming more at the correct statistics of population dynamics than at the correct mechanisms producing the changes.

Apparently all fertility functions share a common property; as age increases, they increase to a peak, but at a decreasing rate, and then decrease, but at an increasing rate. They also seem to be skewed to the left in that the average age at which a woman has children is less than the most probable age. Like the mortality function, the remark about approximating instantaneous rate functions pertains here as well.

We now describe the operation of our model, and how the fertility and mortality functions, together with a stochastic component, generate a population

TABLE 4. Age specific fertility function.
(Age i means at least i but less than $i+1$)

<i>Age</i>	<i>Probability of offspring</i>	<i>Age</i>
15	·10	46
16	·12	45
17	·15	44
18	·17	43
19	·19	42
20	·20	41
21	·22	40
22	·24	39
23	·25	38
24	·26	37
25	·27	36
26	·28	35
27	·29	34
28	·29	33
29	·29	32
30	·29	31

trajectory. First, we specify some initial population by giving the starting number of males and females of each age. The programme then generates deaths according to the mortality function. Suppose, there are M_j males and F_j females at least $j-1$ years old, but less than j years old, $j=1, 2, \dots, 69$. Let p_j and q_j respectively denote the male and female death rates for such individuals as given by the mortality function. Using a uniform random number generator, M_j Bernoulli trials (see Feller 1968: ch. 6) with success probability p_j , and F_j Bernoulli trials with success probability q_j are performed. If M'_j and F'_j are the (random) numbers of successes (deaths) for j -year-old males and females respectively there will be

$$M_{j+1} = M_j - M'_j$$

$$F_{j+1} = F_j - F'_j$$

males and females of age at least j but less than $j+1$, who survive the year and continue on to the next. This process is repeated successively for $j=1, 2, \dots, 69$ after which all surviving individuals are supposed to die.

The foregoing procedure leaves no individuals less than a year of age, and this is where the fertility function is used. Suppose, for example, there are F_j women at least $j-1$ years old, but less than j , that r_j is the relevant value given by the fertility function, and that R is the birth ratio for the population (we chose 106/206 as the fraction of male births). Then F_j Bernoulli trials with success probability r_j were performed using the previously mentioned uniform random number generator. Finally, supposing that C_j is the (random) number of successes (births), C_j trials with success probability R gives $M_1(j)$ and $F_1(j) = C_j - M_1(j)$ respectively the (random) number of male and female births that arose from women aged less than j but at least $j-1$. Repeating this process for $j=16, \dots, 47$ and adding, we obtain $M_1 = M_1(16) + \dots + M_1(47)$ and $F^*_1 = F_1(16) + \dots + F_1(47)$, the numbers of male and female newborns entering the group for the coming year.

Infanticide, if any, is now accounted for. If I denotes the population's systematic female infanticide rate, as a fraction of all live births that are killed, F^*_1 Bernoulli

trials with success probability I are performed. The (random) number of successes (infanticides), K , are deleted and $F_1 = F^*_1 - K$ remain to see what the next year will bring.

The process continues in the same manner. After each year the survivors are determined and age one year. The surviving females between 15 and 46 then generate offspring which, after infanticide, complete the group that proceeds into the next year, and a repetition of the whole process.

What significance can the trajectory of such a model have? A trajectory is a typical history of a group governed by the three functions. Features that occur in most trajectories may be considered likely events in store for populations governed by these functions. If the functions are reasonable specifications for a certain actual group, and if infanticide causes group size to diminish towards zero, we can deduce the impossibility of such an infanticide rate for any living population. In the next section we shall argue these points further.

2. *Some consequences*

We use the preceding population model chiefly to investigate the effects of infanticide on the size composition of small groups. It turns out that systematic female infanticide of any appreciable degree reduces total size inexorably to zero.

We enhance the chance of survival for the model group as follows. The fertility function we use appears to ascribe greater fertility than is typical for Eskimos.⁴ The mortality function is similarly conservative, giving lower mortality than recent Baffin Island data⁵ would suggest. The fact that our model populations nevertheless become extinct when they practise female infanticide, combines with these two remarks strongly to suggest that female infanticide could not have been practised systematically among real groups destined for survival.

Specifically, the model was run for several different, but fixed, infanticide rates,

TABLE 5. Results of various infanticide rates on a hypothetical population.

		Time (years)						
		0	100	200	300	400	500	
Rate	10	B	79	90	77	60	39	43
		G	79	68	54	40	37	39
		M	204	224	167	136	112	91
		F	204	201	153	117	91	92
	15	B	79	62	50	53	35	30
		G	79	57	40	44	29	25
		M	204	196	148	119	81	82
		F	204	165	125	113	88	77
	20	B	79	48	29	20	10	12
		G	79	33	16	11	9	17
		M	204	134	70	42	26	33
		F	204	89	54	33	31	37
B=number of boys (0-14 years) G=number of girls (0-14 years) M=number of males F=number of females.								

and population trajectories were generated. For each rate, table 5 shows the population sizes for juveniles (aged 0–14) and adults of each sex, at various points in time, for typical trajectories. Even at 10 per cent. female infanticide, the population seems to be heading slowly towards extinction, with half-life of something less than 400 years. This tendency towards extinction grows as the infanticide rate increases, and at 20 per cent., the half-life seems to be a little more than 100 years. Graphs 1, 2 and 3 of these trajectories appear in the Appendix.

The 10 per cent. trajectory shown in table 5 and Graph 1, actually has an increase in population size between the initial population and the one 100 years later. This may be attributed to two phenomena. First, chance fluctuations that are superimposed upon a slowly decreasing straight line (representing the slow, steady decline to zero of the population size) are not unlikely to combine and net a population trajectory with local increases. This is explainable by the apparent 'cycles' characteristic of random walks (Feller 1968) and a slow enough linear decrease. Of course it becomes increasingly unlikely to be above the initial size, because of random fluctuations, no matter how slight the downward trend, by the law of large numbers (see Feller 1968). A more intuitive explanation appears when one realises that fewer than expected deaths, and greater than expected births (especially female), produce enduring bulges in the population pyramid.

Secondly, analysis of the detailed age distribution within the trajectories reveals the not unexpected fact that after a certain time, say eighty years, age distributions become stable. That is, the relative frequencies for individuals of each age remain nearly constant over time. The form of the stable age distribution is determined only by the fertility and mortality functions. The eighty years is necessary for this form to emerge as the effects of the initial age distribution gradually diminish.

The initial increase in size of the 10 per cent. trajectory is probably a combination of these two effects. The increased size of the 20 per cent. trajectory between years 400 and 500 is due solely to random fluctuations.

The conclusion we draw is entirely an obvious one: there is some rate of female infanticide which, if enforced systematically, would ensure the ultimate extinction of the population. What is surprising is that this threshold rate seems to be so low. Because our birth and death curves were both conservative with respect to the possibility of extinction, the threshold of tolerable female infanticide is probably less than 8 per cent. In terms of constituting a meaningful cultural practice, this is virtually negligible. It constitutes our first main theoretical statement.

Having refuted systematic female infanticide as a possible mechanism for controlling population sizes, we now discuss a more subtle use of the model. If a group were practising infanticide in order to achieve some prescribed goal concerning size or composition, the rate of infanticide would probably change in response to the success or failure of the policy, and to chance fluctuations. For example, if infanticide were intended to control the growth rate, it would increase in response to increased growth rate, and decrease in the opposite case. We were thus led to consider policies allowing variable female infanticide rates, as follows:

There seem to be at least two hypotheses concerning the goal of an infanticide policy. It may be used to control the sex ratio of the group or, as a supplement to natural mortality, to control size or growth rate. The latter possibility is not really

interesting because, by the previous result, only a negligible amount of preferentially female infanticide is consistent with positive growth.

To investigate the feasibility of an infanticide policy directed at controlling the sex ratio, we suppose that a population maintains a target sex ratio and gradually modifies the actual infanticide rate to achieve the target value. This is a reasonable assumption. Suppose that 80 females per 100 males (F/M)⁶ is a comfortable sex ratio as regards the ability of male hunters to bring in sufficient food for the whole group. If there are in fact 110 F/M for example, and women do not hunt, there will be great stress on the hunters and indeed, on the entire group. This would translate into a real need to select male offspring beyond the selection offered by nature, and female infanticide satisfies this need. As the infanticide rate increases, F/M decreases towards the target. Conversely 60 F/M would imply an easy time for the hunters and a surplus of food. Accordingly the group could easily tolerate more individuals, especially those who process skins, make clothes, etc. and a relaxation of the actual infanticide rate would allow the F/M ratio to increase towards the target value.

Since infanticide has an immediate effect on the sex ratio, it is a useful and satisfying policy. It also has a cumulative effect as the present age-group grows older. Thus if the target is attained through a gradual increase in the infanticide rate, it is likely to be overshot while the rate begins to slowly decline.

Table 6 and Graphs 4, 5 and 6 present the results of our experiments on population control policies using a variable rate of female infanticide. The experiments were performed as follows. For the Copper, Netsilik and Caribou groups, we considered the overall F/M sex ratio figures from table 1 as the target values for

TABLE 6. The effects of maintaining a target sex ratio through variable female infanticide.

Model group	F/M target	Time (years)					
		0	100	200	300	400	500
Copper	70.2	B	79	35	11		
		G	79	16	5		
		M	204	96	21	X	X
		F	204	56	19		
		R	39	43	52		
Netsilik	73.5	B	79	26	18	2	
		G	79	23	7	1	
		M	204	92	37	5	X
		F	204	69	31	9	
		R	35	30	50	59	
Caribou	95.5	B	79	62	80	51	34
		G	79	65	57	42	31
		M	204	161	148	130	81
		F	204	164	154	122	89
		R	12	13	17	12	14

B=number of boys (0-14 years)

G=number of girls (0-14 years)

M=number of males

F=number of females

R=infanticide rate (%)

X=less than 10 extant individuals.

these populations. The first two are in Category A and had extremely low ratios. We were interested in the future course of history for populations who tried to maintain these sex ratios through selective female infanticide.

Proceeding as in the earlier section, an initial population and initial infanticide rate were specified for each group and its target sex ratio. The model advanced the trajectory forward in time as has been already described, except that whenever the F/M ratio rose above the target value, the infanticide rate was increased. The amount depended on how far from the target value the actual F/M was, but it was constrained to be less than 2 per cent. per year. Similarly, if the actual F/M value was below the target, the infanticide rate was decreased by an amount depending on the departure from the target, but also less than 2 per cent. per year.

The graphs indicate that in all cases the populations tend towards extinction. The values of F/M in table 1 entailed infanticide rates which were on the average much higher than the permissible threshold. Indeed, to maintain a target ratio of 73 F/M, the Netsilik would have needed an average infanticide rate of more than 40 per cent!

It is surprising that the Caribou Eskimo, with a target sex-ratio of 95 F/M, should appear to be headed towards extinction. The explanation is simple however. An average infanticide rate of about 15 per cent. is necessary to maintain the target. The results as reported in table 5 have already shown such a practice to be genocidal.

The Caribou example contains another apparent anomaly. Why should 15 per cent. female infanticide be necessary to maintain an overall sex ratio that is nearly 1? The answer is that the infanticide policy, in seeking to maintain a target sex ratio of 95 F/M, must compensate for the automatic tendency of a higher male mortality rate, especially in the hunting years of 15 to 40, to boost the ratio. Given the level of differential male mortality used in table 3, a target ratio of around 105 F/M could probably be maintained without female infanticide.

The foregoing experimental results on variable infanticide rates do not prove anything about the three groups from which the target sex ratio parameters were taken. Indeed, the real populations may not have been trying to maintain any target ratio. Even if they were, they might not have used female infanticide to achieve the target. If, as is likely for example, the Caribou practised very little infanticide, the 95 F/M reported in table 1 would probably vary from year to year. Successive observations would show the ratio fluctuating within a range of, say, 10 per cent. but the *actual* range would of course depend on the differential male mortality. For example, a higher male mortality might give a range of 88–98 F/M while no differential mortality would imply the range 95–105. The experiments only allow us to assert the following: if a group with mortality and fertility functions close to those of tables 3 and 4 were to try to keep their sex ratio at 95 F/M by a *policy of systematic female infanticide*, then the average infanticide rate would be about 15 per cent. and the group would be headed towards extinction.

Something like this conclusion is true in general. When there is a differential male mortality, the infanticide policy, in trying to maintain a target sex ratio, must also compensate for the extra male deaths. These tend to push the infanticide rate above the tolerable threshold, and on the road to extinction. Also, a significant change in the juvenile sex ratio must occur before a small one appears in the

overall sex ratio. This would suggest that the target should involve only adults, to whom the sex ratio is really more culturally relevant, since they are the hunters. In this case, however, the cogency of infanticide as a means of controlling the adult sex ratio would appear to be minimal, as well as risky, since there would be a lag of at least fifteen years before any change in policy could register.

IV

Conclusions

A review of the ethnographic literature on female infanticide in the Arctic reveals that most authors consider it to have been an efficient way of coping with the stresses that the environment imposed on the hunters living there. There can be no questions of the rigours of Polar life, and the current, slick rendition of all hunter-gatherer groups as the original 'affluent society' (Sahlins 1968) bypasses the innumerable, tragic sagas of Arctic privations. Unquestionably, affluence lies in the mind of the anthropologist rather than the belly of the hunter. Given the harsh surroundings, the main justification for infanticide is an economic one based on a strong sexual division where men hunt and women are regarded in some senses as unproductive consumers. Such culling as is needed therefore, is inevitably directed towards the potentially unproductive elements in the group, that is, female infants.

It is difficult to assess the degree to which this practice *actually* occurred. We have used a series of reproductive histories as well as census data from a number of different groups. None of these sources provides reliable information about the extent to which female infanticide was practised because they generally reflect a short-term picture and use non-biological criteria to age people. In addition, they contain almost no information about differential death rate of hunters, infant mortality and precise biological ages of every member of the group. We eventually distinguished two main categories of data, termed 'A' and 'B', and conclude that female infanticide seems evident in 'A' only; possibly one in every two or three girls born, were suppressed at birth.

In the groups of Category B we were unable to discern any female infanticide. Because the overall sex ratios were nearly 1, the preponderance of male juveniles and female adults might as easily be explained by the ethnographic mis-ageing of adolescents as by the combination of female infanticide and higher male mortality, respectively. Thus, we had a certain reluctance to accept the ethnographic evidence at face value. We devised a computer simulation model to test the effect of varying degrees of female infanticide in a small Arctic group over time. All the variables used were biased in favour of Eskimo survival: overall mortality and differential male accidental death rates were conservatively low; the level of fertility was high in comparison to reported values. We treated the hypothetical group of 408 people as a closed system or an endogamous unit, and found that they proceed inexorably towards extinction if more than 8 per cent. of girls born are systematically killed at birth.

A more subtle use of the simulation model involved an experiment on varying rates of infanticide. We studied the effects of policies that sought target sex ratios through the use of female infanticide, and concluded that even for targets near

100F/M, the groups died out because the requisite rate of infanticide was on the average above 8 per cent. This generalises to our main conclusion: in small hunter-gatherer groups, population control mechanisms using female infanticide have low threshold values (about 8 per cent.) and when the average infanticide rate is above this threshold, the policy is genocidal.

Several questions now arise. The pattern evidenced in the groups comprising Category A is consistent and suggests a high degree of female infanticide. Is it possible that the people under discussion were, in Rasmussen's terms, headed straight for extinction—a course from which they were diverted only by the timely arrival of European law, medicine and welfare? This is surely an unlikely conclusion.

It is of course true that the full impact of female infanticide is not registered for some time. Balikci claims that the Netsilik remained unconscious of the inherent dangers of this practice and quotes a story to illustrate his point, a tale that ends with the Eskimo who is discussing the issue smiling stupidly as he shrugs the matter off (Balikci 1970: 152). We find this fatalistic attitude hard to reconcile with the patent desire of Eskimos to remain in business by achieving some degree of harmony between their resources and technology. It seems highly improbable that people who could see the effect of an infanticide policy within fifteen years—the scarcity of wives for their sons and a certain diminution in the number of grandchildren—would have continued doggedly to suppress infant girls.

All in all, it seems far more likely that the data found in some groups do not reflect the normal population distribution, but are rather the result of an emergency situation whose effect would be ironed out and normalised over the course of time. Some concrete evidence is found for this view in the two very different sets of figures that we have presented for the Copper Eskimos (see table 1). The censuses were done by two different people, Rasmussen and Jenness, nine years apart. The groups sampled were not inhabiting overlapping areas though they were both classified as Copper. The sex ratios found in the two groups are very different and we have placed them in different categories. Rasmussen's group belong to A with a juvenile F/M ratio of 37 and an overall one of 70, whilst Jenness's group has a juvenile ratio of 104 and an overall one of 95. Jenness considered part of his sample to be 'exceptional' because of the preponderance of girls, but the overview of these counts does stress the likelihood that the intensity with which females were suppressed, varied considerably, even within the same major group, over time and space. All that is sure however, is that whatever the extent of infanticide, it must be below 8 per cent. on the average to ensure the survival of the group.

In short therefore, hunter-gatherers such as the Eskimo groups we have reviewed could only tolerate a small degree of female infanticide and must therefore have practised it sporadically, in response to short-lived crises and periods of stress (cf. Birdsell 1968).

Although the practice is no longer current, we are bound to suggest a far more efficient way of culling these populations, by simply modifying the traditional sexual division of labour. Instead of insisting that only men hunt, women might have been trained for the task. This type of role exchange did occur traditionally under extraordinary conditions (Kemp, pers. comm.) but normally only males were regarded as potential providers. Dropping this restriction would allow

Eskimos to cull both sexes at times of emergency and thereby recover from the effects of this policy more quickly than they would by using female infanticide. The moral of the situation is quite simple: you can always teach a woman to hunt, but you'll never teach a man to bear a child.

NOTES

We are most grateful to Dr Nancy Howell (University of Toronto) for her perceptive and helpful comments, to Mr W. B. Kemp (McGill University) for his advice and for allowing us access to his unpublished field notes, and to Wendy McKay for assistance with computations and graphs. We dedicate this paper to our parents, whose ignorance of the matters discussed here, enabled them jointly to produce offspring with a F/M ratio of 700.

¹ A fine account of the effects of the whaling industry on the indigenous inhabitants of Hudson Bay is found in Ross (1971).

² In this context, it is amusing to note that Rasmussen never shone in mathematical fields. Freuchen (1958: 35-6) records that the young Knud failed his Danish school entrance exams because of his inability to master arithmetic:

Knud told me that it was physical torture for him to go to school on the mornings that he was to have mathematics class . . . Later in life (he) retained an irrational ill-will toward mathematical formulas. It wasn't that he couldn't master them, but he set up a sort of resistance every time something had to be figured out . . .

³ We are grateful to W. B. Kemp for drawing our attention to this census, which is housed in the National Archives, Ottawa.

⁴ The ethnographic sources we consulted all agree that Arctic women had a low average number of births. Rasmussen (1931: 140-1) recorded 116 live births for a sample of 18 Netsilik women whose mean age was 41.5 years, giving an average of 6.4 live births per woman. For the Copper Eskimos, Jenness noted that 7 was the largest number of children recorded for any woman, and said that they usually bore 4 to 5 offspring of whom only 3 survived infancy (Jenness 1922: 163-4). Damas (1972: 42) supported this statement. Parry reported a similar situation among the Iglulik (1824: 527). Murdoch (1892: 38-9) discussing the situation at Cape Smyth, Alaska, noted that it was rare to find a woman with as many as 7 children. Malaurie (1956: 99) observed that 4.65 was the average number of children per woman in Greenland. Our fertility curve, which overstates the groups' chance of survival, implies a figure of 7.2 live births per female in the population.

⁵ Kemp (unpublished field notes) recorded all 786 deaths reported between 1928 and 1967 on Baffin Island. Our mortality function ascribes lower death rates even for the males aged 15-40 years than an analysis of his data suggests were true for that area.

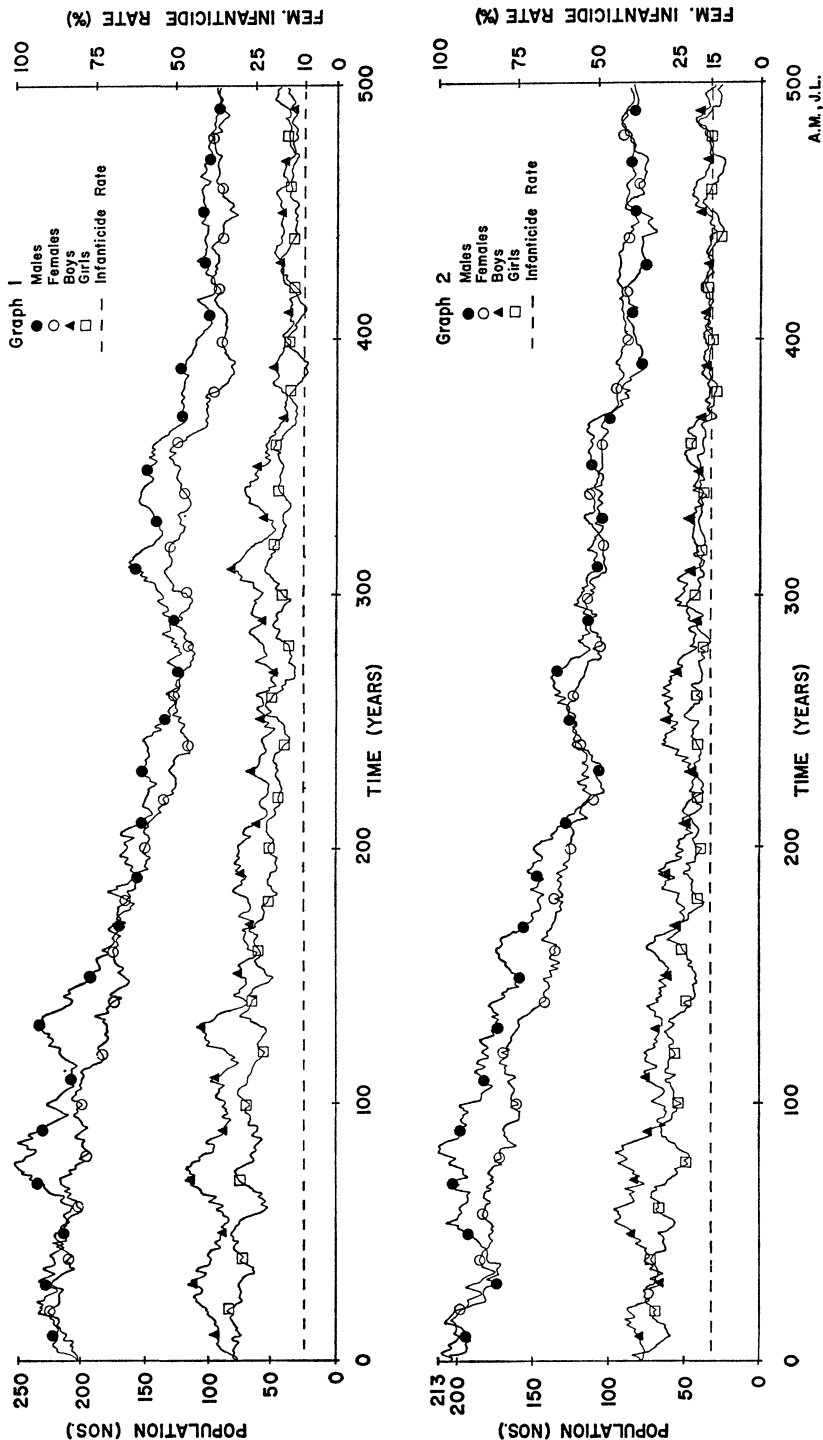
⁶ Throughout this paper, F/M denotes the proportion of females (F) per hundred males (M).

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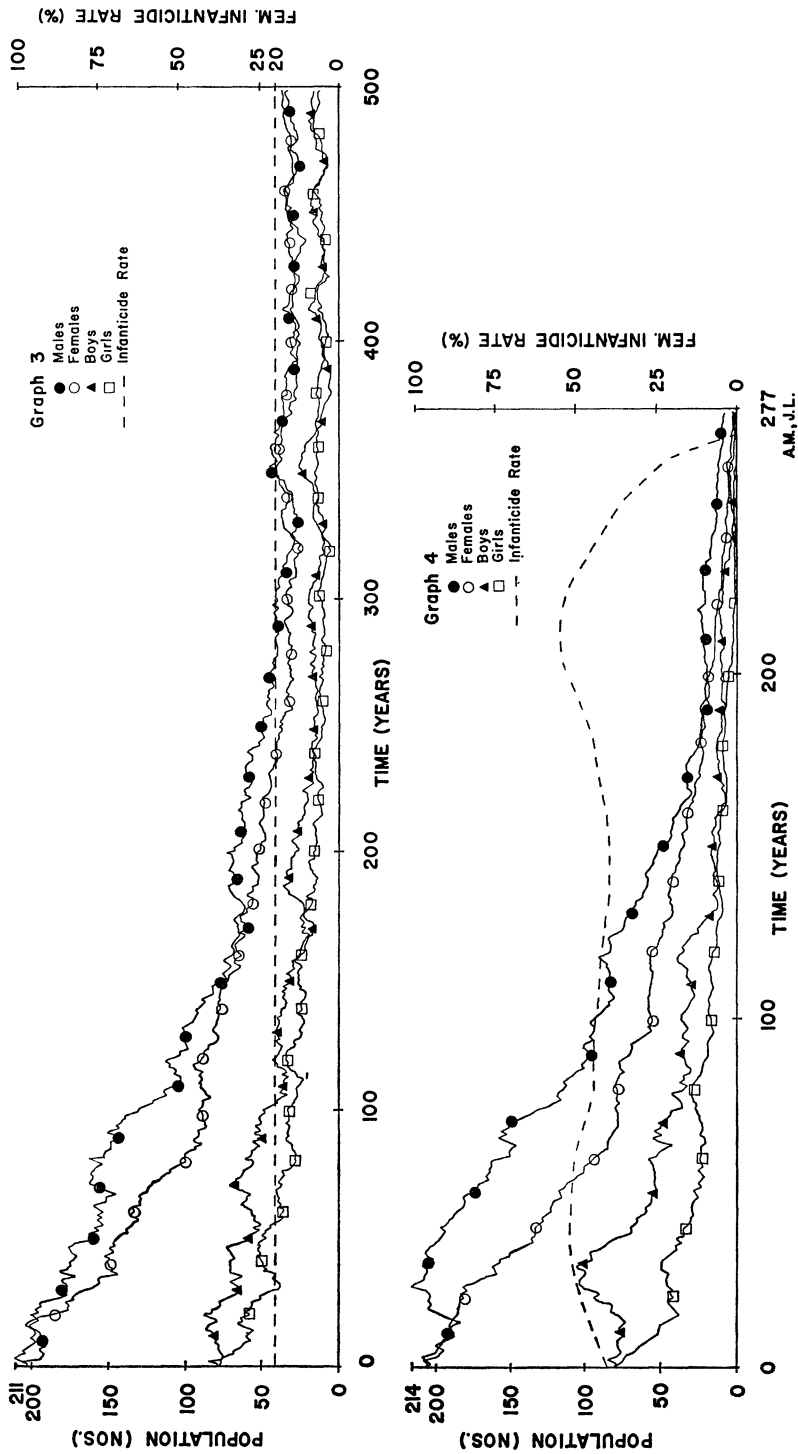
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APPENDIX



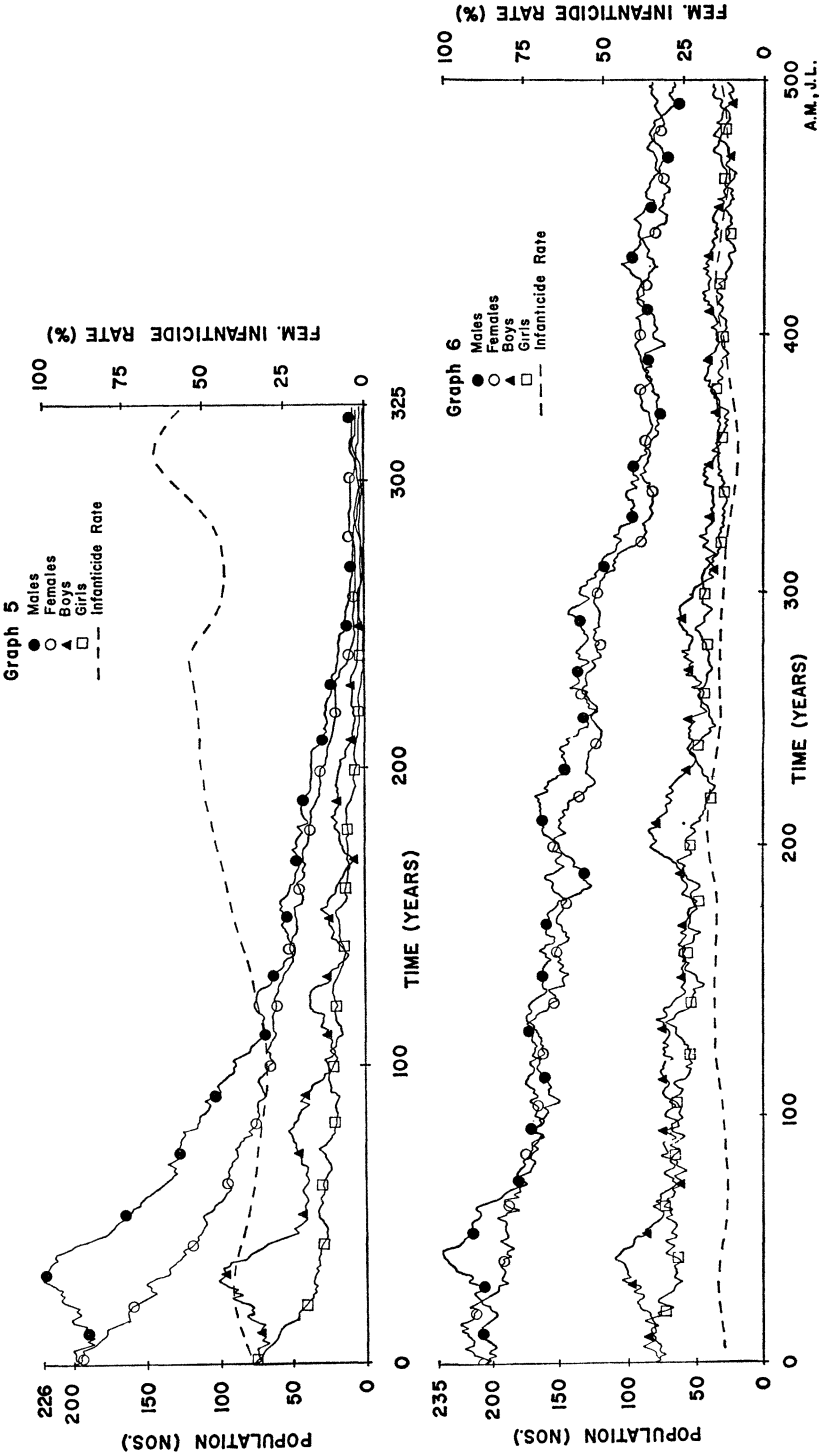
Graph 1. Typical trajectory under constant 10% infanticide.

Graph 2. Typical trajectory under constant 15% infanticide.



Graph 3. Typical trajectory under constant 20% infanticide.

Graph 4. Typical trajectory under an infanticide policy seeking a target sex ratio of 70:2 F/M (Copper⁴). (Superscripts are as in table 1)



Graph 5. Typical trajectory under an infanticide policy seeking a target sex ratio of 73:5 F/M (Netsilik²).

Graph 6. Typical trajectory under an infanticide policy seeking a target sex ratio of 95:5 F/M (Caribou⁶).