POPULATION AND PREHISTORY: THE LATE PHASE ON ANEITYUM

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Except for the assistance that is expressly acknowledged, this work is entirely the product of my own researches.

Norma McArthur.

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References
"Toilsome was our journeying together; not without offence; but it is done". Literally, Winifred Mumford shared one of the toughest parts of the journey - on Aneityum in 1973 the isolation, the rain, the mud and the ticks of Anumej. It was she who proposed and made the topofil and compass traverses of the rivers, and who has transformed my maps and diagrams so superbly. She made the journey less toilsome and the hardships rewarding.

Less literally because he made only part of that journey to Anumej, but by no means less emphatically, Jack Golson was there at every step too. Without his confidence and support there would have been no beginning and no end - even if the end here accomplished is not what he hoped for or expected.

There are others who have contributed to this study: Wally Wasserman of Canberra College of Advanced Education and John van de Graaff of Canberra Technical College who prepared the contour map from the aerial photographs of the Anumej area; Les Groube who introduced me to the art of surveying and taught me more than he perhaps realised about the selection of sites; Erica Fisher and Russell Darroch who organized the computer simulations; Gennesse Winch and Peggy Cole who typed the manuscript; Dragi Markovitch who printed the photographs; and throughout it all, Lois White who coped.

In the New Hebrides, there was the Chief Surveyor of the Condominium who helped secure the aerial photographs and provided additional material to assist the cartographers; there were the people of Aneityum who worked with us in many and various ways; and there was Artie - Arthur Krafft - who was daunted by nothing and was a tower of strength both in crises and out of them.

I thank them all.
The documentary evidence available about Aneityum - the southernmost island of the New Hebrides - and its people soon after continuing European contact provides a unique opportunity for an exploration of the likely relationships between archaeological evidence and the populations which created that evidence. This case study examines the documentary and archaeological records for the Aneityumese in the late phase of their prehistory; and the demographic record - such as it is - for the ensuing half-century highlights the problems of inference which might be encountered in attempts to translate an inanimate archaeological record into people long dead. As the title suggests, the emphasis is on population, partly because the archaeological evidence is limited (though no more so than on many islands of the Pacific), partly because this is one of the few well-attested examples of a diminishing population. The probable causes of its diminution are discussed, as are its archaeological implications. The concluding chapter reflects more broadly on the difficulties inherent in the interpretation of both archaeological and some historical evidence in terms of populations, and outlines the kind of procedure that must be followed if the estimation of population is to be an objective of an archaeological investigation.
For the world beyond its immediate neighbourhood Aneityum's history began when Capt. James Cook sighted the island in August 1774 during his second voyage to the Pacific. After two weeks at Tanna collecting wood and water and repairing the tiller, Resolution "stretched to SW in order to get to the Southward of Tanna and to have a nearer View of the Island Annattom, which at Noon extended from S^E to S^W distant about 10 Leagues." It was the southernmost island of the archipelago Cook had named New Hebrides, and it "seem'd to be only a small island of a moderate height and hilly surface" then, but later Cook changed this to "a good height and hilly Surface", adding its latitude and longitude "and more I must not say of it" (Cook/Beaglehole, 1961 pp.508-9, 524).

The first Europeans on record as having landed on Aneityum (or Keamu as its inhabitants called it and a map of which is in the pocket on the back cover) and survived are Lieut. George Ward Cole, R.N. and Capt. Lawler of the brig Alpha, but the people of two different villages on the east and south coasts each reported subsequently that they had seen a ship with "white men" on board (Shineberg, 1967 p.22; Geddie, J 2 Nov 1854). In each account a small boat was sent ashore, in one to collect the food that the natives had heaped on the beach to placate the strange gods on this island that moved, in the other to land a man who was "allowed ... to live a short time, and then killed and cooked" (A.W.Murray, 1863 p.22).

It is not known whether these were two different ships or both incidents concerned only one, or whether one of the incidents concerned the Alpha before she arrived at Port Patrick on the north coast on 17 March 1830. Here a party of Rotumans and Tahitians was put ashore to cut sandalwood, and for "several days" the Aneityumese were friendly and helped cut and carry the wood in return for trade goods. When some sugar-cane was stolen from one of their plantations, allegedly by one of the visitors, the Aneityumese retaliated by attacking the unsuspecting foreigners at breakfast, killing two of them and wounding several others (Shineberg, 1967 p.23). Five Aneityumese were also killed in the affray (A.W.Murray, 1863 p.22) and the Alpha left hurriedly.

Eleven years later, the London Missionary Society settled two Samoan "teachers" or catechists near Port Patrick. When they
were visited in June/July 1842, those on board the mission vessel *Camden* sighted three sandalwood ships trading on the west coast of Aneityum, and in January 1844 Capt. James Paddon arrived to establish a sandalwood station and trading depot at Inyeug, the small islet in the main harbour at Anelgauhat, which he acquired cheaply because the Aneityumese believed it haunted. Paddon's company in this venture probably consisted of 39 people altogether. At least 57 "colonists" had set out with him from New Zealand -- five Europeans whose names are known, 30 "sawyers and woodcutters" (who may or may not have been Europeans), 16 Chinese from Macao and six Maoris -- but 17 of the group were killed in a massacre at Mare (one of the Loyalty Islands) and two who were wounded in this attack were left at Norfolk Island (Turner, 1861 p.367; Shineberg, 1967 pp.61, 98-100).

Once Paddon's station -- with its "jetty, flagstaff, weather-boarded houses, piles of sandal-wood, a rusty swivel mounted here and there" -- was built and functioning, the numbers of foreigners on Inyeug varied from time to time. In April 1845 Rev. George Turner (1861 p.367) of the London Missionary Society landed there from the *John Williams* and was "conducted to the store" by "a Mr. Murphy", one of the original group of settlers. Paddon was then absent in Australia and two of his ships were away collecting sandalwood from other islands, so there was "only one white man there, in addition to Mr. Murphy, and five Chinese".

Paddon left Sydney to return to Aneityum on 3 May 1845 in his cutter *Rover's Bride* with a crew of eight, eight male passengers, 25 head of cattle and miscellaneous provisions including one ton of hay. Eleven days later a schooner *Castlereagh* sailed from Sydney for Aneityum with 25 cows and 2 ¹⁄₂ tons of hay, two cases of plants, miscellaneous hardware and one male passenger. In August Paddon was back in Sydney for stores, to sail again on 17 September with 18 passengers (including two married couples, one of whom had two children, and nine "natives of the South Sea Islands") ironmongery, hardware, stores and three horses (Shipping Gazettes, 10 May, 17 May, 30 Aug, 20 Sep 1845).

The only passengers reported as bound for Aneityum from Australia in 1846 were 23 who travelled steerage; and in 1847, eleven at most were reported to have sailed from Sydney, but perhaps as many as 25 left the island in the course of that year.
(Shipping Gazettes, 1 Aug 1846; 17 Apr, 15 May, 3 Jul, 21 Aug, 11 Sept 1847). However, not all who took passages to or from Aneityum were necessarily attached to Paddon's station on Inyeug. As early as 1846 Paddon had a party of men cutting wood on the Isle of Pines (New Caledonia) and with ships calling at Aneityum for water and provisions as well as for sandalwood, there were more destinations than Sydney open to anyone who wished to leave the settlement at Inyeug. At least four of the vessels known to have been engaged in the sandalwood trade called there in 1844, three in 1845, six in 1846 and eight in 1847, and Paddon's own vessels moved about the islands of the New Hebrides and the Loyalty Islands (Shineberg, 1967 pp.100, 220-31).

In February 1848 the station on Inyeug was destroyed in a hurricane, in which one of Paddon's ships was wrecked and her crew of nine drowned. Nohoat, the chief of the district around Anelgauhat, invited Paddon to build a new station on the mainland and so he was at Anelgauhat when a party of French Marists arrived to settle in May 1848 (Shineberg, 1967 pp. 102-3). The two Samoan catechists who had been left at the harbour under Nohoat's protection in April 1845 had built themselves a house towards the eastern end of the bay, and with Paddon's help the Marists acquired land to the westward on which to erect a large two-storey iron house to accommodate the eight priests and eight laymen who formed their party (Turner, 1861 pp. 433-4). Undeterred by their presence and their numerical strength, in July 1848 Rev. John Geddie of the Presbyterian Church of Nova Scotia elected to start his mission at Anelgauhat: for him, "Popery" was as great a challenge as "Paganism" (Patterson, 1882 p.156) and he built his house and a chapel (the frame of which was donated by Paddon) near where the Samoans had their house and garden.

And so, within six months of Paddon's move to the mainland, "the south-west harbour presents all the appearances of a rising foreign settlement. The land is being laid under cultivation, roads made, cattle introduced, and houses built" (Turner & Nisbet, 1850 p.53). Apart from "the natives [who] removed inland to get away from" the Presbyterian mission when it first started (Mrs Geddie, MR 1854 p.153), this incursion of foreigners did not greatly disturb the traditional pattern of settlement on Aneityum, but this will be elaborated further in Chapter 2.
Many of the foreigners suffered severely from "fever and ague" in the early months of 1849 and again in 1850 (Geddie, J 1 Mar, 15 Mar, 1 Apr 1849; 15 Jan, 1 Mar 1850). Despite their attempts to drain a marsh that lay behind their iron house (Anon. MS pp. 16-7), the French Marists abandoned their mission sometime between September 1849 when there were only two Priests on the island (Geddie, MR 1850 pp. 123, 164) and 21 March 1850 when Mrs Geddie (MR 1850 p.153) reported to her daughter (then at a mission school in England) "The Roman Catholic Priests have left the island. They did not make any converts: indeed they did not attempt to teach the natives", and the large iron house was then occupied by one of Paddon's employees.

In January 1852 Paddon himself left the island "to escape the sickly season" (Geddie, J 29 Jan 1852) and on 17 September Geddie (MR 1853 p.66) wrote

The sandal-wood establishment is fast breaking up. There has been a great destruction of property. Those things that cannot be conveniently removed are committed to the flames. In a few months, it will be impossible to tell from external indications, that a foreign establishment existed on Aneityum. I wish that other memorials of their residence on this island could be as easily obliterated. However, its final removal took several months more because Mrs Geddie (MR 1853 p.166) reported in mid-February that there were then two vessels in the harbour "assisting to remove property" belonging to "the foreign establishment". By then Rev. John Inglis, of the Reformed Presbyterian Church of Scotland, and his wife had been on Aneityum for more than seven months and had established a second mission station near Port Patrick on the northern coast.

For more than four years then Aneityum had no European residents other than the missionaries and their families, and it was during this time that the mission had its greatest success. Both missionaries remained on the island for 24 years (Geddie until 1872, Inglis until 1876) with only occasional periods of leave. Although in the early years of their association Geddie maintained frequently that he "labour[ed] in harmony with [his] esteemed colleague, Mr Inglis", his forbearance was strained by Inglis's acquiescence in the punitive shelling of Tanna and Erromanga by H.M.S. Curacoa in August 1865 as a reprisal for the destruction of mission property (HFR 1866 pp.31-5). When this "outrage disgraceful
to all concerned was committed, Geddie was in Nova Scotia on his first and only home leave, and Inglis was the senior missionary in the New Hebrides Synod.

Geddie's biographer, Rev. George Patterson D.D., described him as a "shrinking, timid little creature" as a child, and there is abundant evidence in his journals that although a crusader, he was a sensitive, kindly man, very much aware of his own inadequacies and his responsibilities towards the people of Aneityum. Early in 1851 the wife of one of the more humble converts died and contrary to custom, she was buried. On 1 March Geddie wrote in his journal: "It is a serious matter to break through customs established from time immemorial, and which derive much of their stability from being interwoven with the superstitions of the people". A year later, he was immensely saddened because the chief Nohoat, who had been a good friend to Geddie and his family and other members of the mission, was "backsliding". "His present situation is a peculiar and anomalous one. If he renounces christianity he can scarcely go back to heathenism" because he had lost his influence amongst his people by breaking most of the ancient tabus and agreeing to the destruction of his sacred grove.

Inglis never doubted or questioned his authority to impose a new set of standards on others, and his biographical sketch of Geddie, written perhaps many years before its publication, should be read in the light of the differences between the two men in relation to the Curacoa incident which Geddie so greatly deplored. To Inglis (1887 pp.259-60) Geddie was

- sometimes the victim of strong prejudices, both as regarded men and things .... He was frequently disposed to draw important conclusions from very shaky premises. On subjects which he thoroughly understood, or matters that had come under his own observation, no man could have had more correct views, .... but on subjects requiring extensive investigation his conclusions were the result of intuition rather than of induction; and hence, on all questions resting on inferential evidence, or requiring a large induction of facts, his opinions were of little value. He was often prone to look on the dark side of things ....

Nevertheless, he remains our principal witness to the late phase of prehistory on Aneityum.

Supplementing the documentary evidence from Geddie, Inglis and other later missionaries is the information gleaned from modern sources. The four principal informants were Daniel Kauyapae and
his father Yarowili, and David Yautaoa and his father Thamaona.

None knows when he was born, but Yarowili married in 1923 and recalls that he started to shave in 1917, so he was probably born around 1900. Daniel was the second child born to the marriage, so he is now about 47 years old. Thamaona is probably about 10 years older than Yarowili; he remembers seeing the missionary who was stationed on Aneityum from 1877-86 when he was a small child, but this was probably on the occasion of a meeting of the Synod because he was 13 or 14 years old when Dr Gunn (who was on Futuna from 1883 to 1903) was first stationed on Aneityum. Yautaoa, the fourth of his seven children and the only son to survive beyond childhood, is perhaps slightly younger than Daniel.

Neither Yarowili nor Thamaona remembers his grandparents, but Thamaona's father lived to be a very old man. Both men acknowledged that what they knew about the past had been told them by their fathers or more rarely, by others of their father's generation, and given their probable ages, it is unlikely that their fathers had been born when Geddie first arrived in 1848 or even seven years later when practically the whole population was at least nominally Christian. However, both men knew at least their own part of the island extremely well, and though Thamaona was very deaf and almost blind by 1973, Yarowili (who had lost the sight of one eye the year before) pored avidly and eagerly over the aerial photographs we had with us. He was better able to interpret them than the Australian-born timber lessee who had been on the island for 20 years and for whom Yarowili had worked until quite recently, seeking out stands of kauri and helping to plan access roads.

The significant interviews with Thamaona were held in 1972 when his son was not present and Daniel acted as interpreter. Daniel was present at all but one interview with his father and mother Guthenugra, but Yarowili understood English well enough to need no translation of the questions and though he may have replied in "language" for his son to interpret, occasionally he corrected Daniel's translations in pidgin. Guthenugra's marriage to Yarowili was her second and she is almost certainly older than Yarowili. Her first husband was a nephew of the former chief and the only surviving son of this marriage now holds the title. In her own right she is entitled to land in the Anelgauhat area;
Yarowili's land is in Anau-unse, one of the traditional districts into which the island was divided; and Thamaona's is near Umej on the south coast east of Anelgauhat.

Not surprisingly, the information given by the two generations was usually identical, whether it was given in response to questioning or volunteered, and Yautaoa was especially good in offering odd pieces of information casually. Where any of their stories could be checked against facts as stated by Geddie in his writings, not all of them published, they were invariably accurate, and hence there is good reason to believe that items that cannot be checked are also correct. Collusion cannot be ruled out, because conversation and courtesy are still important to the Aneityumese and my questioning of any one person about the past inevitably stimulated talk between him and others.

And this talk would be in "language", the language of Aneityum which is not spoken generally on any of the islands nearby. Geddie reduced it to writing and he and Inglis translated the Bible into it in a form that satisfied the British and Foreign Bible Society, and this version of the Bible is still in use on the island by both the Presbyterians and the handful of Roman Catholics. Nowadays there are probably no more than 300 speakers of "language", and most of them speak pidgin as well, but for the most part ordinary, everyday conversations between themselves are in "language". Daniel, who was the head man of our small work force, invariably relayed the orders for the day in it; and when asked to ask Yautaoa for permission to make camp at Umej on our way from Anumej through to Anelgauhat, there followed a long and dignified discussion back and forth between the two men which ended finally with Daniel tearing his eyes away from the mountain tops and saying "He say yes".

As will be apparent already, the Harvard system of referencing is used throughout, and the only references listed are those cited directly in the text. A complete bibliography on the subject, even the inclusion of all the works that have been read in the course of this study, is redundant in view of Parsons's (1972) compilation for his review article. Since that was prepared there have been more studies for parts of the Pacific that incorporate some population aspects (e.g. Bellwood, 1972; Groube, 1970; Irwin, 1973; Kellum-Ottino, 1971) but except for Green's (1973) study of Tonga which will be discussed elsewhere their consideration of population is
largely incidental to the factual reporting of the pattern of settlement.

Although all might be criticised for their assumptions and methods -- the latter often extraordinarily reminiscent of Forster's (1778 pp.217-21) rigmarole of the round and square breadfruit trees to support a number that Cook reckoned for Tahiti in 1774 from the number of canoes drawn up on the beach (McArthur, 1970 p.1098) -- it is unfair to single them out from the untold number open to rebuke, and this would make a dreary catalogue. With justification we might all lament with Robert Burton (1621), "It is a most difficult thing (I confess) to be able to discern these causes whence they are, and in such variety to say what the beginning was. He is happy that can perform it aright".
CHAPTER I

THEORETICAL CONSIDERATIONS AND POPULATION MODELS.

Desirable though it might be to proceed directly with the evidence for Aneityum, there are some misunderstandings, even misconceptions that underlie many of the studies concerning prehistoric populations -- some of them of a statistical nature, some demographic, some neither -- which are best dealt with at the outset. If at times my critique appears harsh, I can only plead that by not fumbling at fallacies, not ignoring past failures, the way will be clearer for other methods, other research strategies that may produce better results.

For Aneityum, one is in a privileged position. With the documentary records that exist for the early years of European contact and which may be considered in large measure a description of the state of Aneityumese society immediately preceding that contact, at least some of the potential ambiguities of the prehistorian are removed; the flaws in analogies are more easily detected, and one is less prone to import "contemporary standards and scales of social and moral values" into inferences and interpretations (Piggott, 1960 pp.92-3, 96). The problems of demographic inference which will be discussed here are not a digression from the study of prehistory, but merely another example of the difficulties of indirect inference which beset the whole of prehistory, to a much greater extent than was the case with old-fashioned archaeologists more interested in the characteristics and taxonomy of artifacts than in the people who made them.

An immediate example is the fact that, by its very nature, archaeological evidence -- be it tools, weapons or pottery, house platforms or post holes, agricultural systems or earthworks, even most food debris -- relates to the activities of the adult members of the population concerned, and only rarely (if at all) to the activities of the young. Hence, if there is any relationship between the quantifiable remains of some prehistoric population and the size of that population, it can only be an indirect one because there is no direct or unique relationship between the size of a population and its composition or structure with respect to age and sex; and it
is this latter which determines the numbers of adults of each sex relative to the total population.

The structure of a population which experiences no migration is determined by its past history of mortality and fertility, and although there are general patterns for both the risks of dying before each age, and the likelihood for women of reproductive age to conceive, the magnitude of these risks is not the same for all populations. The chances of dying before attaining some specified age will depend on the presence or absence of diseases of one kind or another, as well as on some cultural characteristics of the society itself and the risks attaching to behavioural traits. The pattern of fertility is likewise constrained, either by biological factors or by social and cultural factors such as the age at which females marry and whom they might marry, the acceptable interval between successive births and the acceptability of infanticide. In small population isolates where each individual counts for relatively more than he would in a large population, all of the factors contributing to or detracting from the risks of mortality and fertility are magnified.

In general, the numbers of females at each age are not likely to be very different from the numbers of males at the same ages unless females run much higher or lower mortality risks than males, or there is selective infanticide, or both. Some examples of variations in the age and sex composition of populations of the same size are illustrated in the population pyramids of Fig. 1.1. All relate to 10,000 persons, and all except the first two are 'models' designed specifically to simulate the distorted sex ratio and the comparative scarcity of children which were noted in the first count of Aneityum's population. Infanticide and the strangling of widows were accounted responsible for both departures from the norm, but these simulation models concentrate on infanticide alone.

It is irrelevant whether the infanticide should be regarded as a change in mortality or one in fertility, because once a child was born and allowed to survive, it ran the same risks of mortality and fertility as others of the same sex; but for simplicity of programming the computer simulations, the children not to survive were removed from the births in the input data, so that the simulations were run on 'effective' fertility schedules which retained the pattern of the original age-specific fertility rates but not their level. The
"MODERN" POPULATION
10,000 persons

STABLE POPULATION
60-30 years r<1%p.a.
no infanticide
10,000 persons

INFANTICIDE SIMULATION
- 5% male, 10% female births -
10,000 persons

INFANTICIDE SIMULATION
- 5% male, 20% female births -
10,000 persons

INFANTICIDE SIMULATION
- 5% male, 25% female births -
10,000 persons

INFANTICIDE SIMULATION
- 5% male, 30% female births -
10,000 persons

Fig. 1.1 Population pyramids for six hypothetical populations, each of 10,000 persons.
computer programme was essentially the Keyfitz & Flieger (1971 pp.158-73) programme for population projections, and each simulation was run for 1,000 years under identical mortality conditions from the same initial population. The age and sex structure attained by each after that period of time was then re-scaled to produce a total of 10,000 persons.

The population represented by the pyramid A in Fig.1.1 might be termed a 'modern' population, and it is typical of populations where the mortality for both males and females at each age is relatively low and fertility is high so that the population is increasing at a rate of about $3\frac{1}{2}$ percent per year. At this rate of increase the population would double in 20 years, and the proportion of the total that could be described as 'adult' is approximately 50 per cent. For our present purposes it is convenient to choose the age of 15 years as that which separates adults from children, even though there may be biological differences between populations which either retard or accelerate puberty.

The population pyramid B depicts what demographers call a 'stable' population, and its structure derives solely from the particular set of age-specific mortality rates for each sex combined with the set of age-specific fertility rates and sex ratio at birth which were chosen to define it. As long as these rates continued unchanged, the population would increase at less than 1 per cent per year, which would lead to a doubling in size in about 80 years, with just over 60 per cent of its total aged 15 years or more. Different sets of mortality and fertility rates would produce different populations, and those used here are modifications of patterns common to several island groups in the Pacific.

This stable population was the base population for all of the simulation models, and as they were all subjected to the same schedules of mortality (i.e. the sets of age-specific mortality rates for each sex) as were used to construct the stable population, the differences in their demographic structures are essentially in the amount of female infanticide permitted, because the level for males is the same throughout. In the model represented in the population pyramid C 5 per cent of males and 10 per cent of females were not allowed to survive; in D the rate for females was increased to 20 per cent, and thence to 25 per cent in E and
TABLE 1.1: The numbers of males & females aged 15 years and over, and the numbers between 15 and 59 years, depicted in the population pyramids of Fig. 1.1

<table>
<thead>
<tr>
<th>Population</th>
<th>Numbers aged 15 years and over</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Males</td>
<td>Females</td>
<td>Persons</td>
</tr>
<tr>
<td>A</td>
<td>2487</td>
<td>2445</td>
<td>4932</td>
</tr>
<tr>
<td>B</td>
<td>3022</td>
<td>3018</td>
<td>6040</td>
</tr>
<tr>
<td>C</td>
<td>3249</td>
<td>3085</td>
<td>6334</td>
</tr>
<tr>
<td>D</td>
<td>3572</td>
<td>3026</td>
<td>6598</td>
</tr>
<tr>
<td>E</td>
<td>3752</td>
<td>2988</td>
<td>6740</td>
</tr>
<tr>
<td>F</td>
<td>3945</td>
<td>2944</td>
<td>6889</td>
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</table>

<table>
<thead>
<tr>
<th>Population</th>
<th>Numbers aged 15 to 59 years</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>A</td>
<td>2359</td>
<td>2326</td>
<td>4685</td>
</tr>
<tr>
<td>B</td>
<td>2946</td>
<td>2887</td>
<td>5833</td>
</tr>
<tr>
<td>C</td>
<td>3155</td>
<td>2930</td>
<td>6085</td>
</tr>
<tr>
<td>D</td>
<td>3454</td>
<td>2854</td>
<td>6308</td>
</tr>
<tr>
<td>E</td>
<td>3620</td>
<td>2806</td>
<td>6426</td>
</tr>
<tr>
<td>F</td>
<td>3796</td>
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30 per cent in \( F \). In total, these amount to infanticide rates of 7.5 per cent of all births irrespective of sex for \( C \), 12 per cent for \( D \), 15 per cent for \( E \) and 17 per cent for \( F \), so that even at its maximum only one child in six born is not allowed to survive.

With the particular schedules of mortality and fertility used in the simulations, more than one child in five could have been disposed of at birth without causing any decrease in the size of the population provided there was no discrimination by sex. Where there is discrimination against female infants such that relatively fewer of them are allowed to survive, the critical level -- given the initial assumptions about mortality and fertility -- lies somewhere between 20 and 25 per cent of all female births. With four out of five males and females allowed to survive, the population doubled in about 880 years, and such doubling would have been achieved in little more than 700 years had there been no male infanticide. If all males born, but only 3 out of 4 females born, are allowed to survive the population halved in just under 600 years, and the halving would have happened sooner if there had been any infanticide of males.

"Critical levels" such as these are not universally applicable, and the extent of infanticide that might be tolerated by any population without decreasing its size depends on the combination of the mortality and fertility experienced by it. Schrire & Steiger (1973) for example, estimated that the small populations of the Arctic could tolerate no more than 8 per cent of female infanticide if they were not to become extinct within 500 years, but this fraction would have been increased if the mortality rates assumed in their simulations had been lower. In this present series, the fertility rates assumed were conservative; had they been higher, and the mortality remained unchanged, more infanticide could have been tolerated without diminution in the size of the population.

The numbers of males and females aged 15 years and over in each of the populations depicted in Fig. 1.1 are summarized in Table 1.1, together with the numbers aged between 15 and 59 years in each. As the amount of infanticide increases, the proportion of adults increases from 60 per cent to nearly 70 per cent, and the change in the numbers of males is even more dramatic -- from 25 per cent
of the total of 10,000 in A to nearly 40 per cent in F. On the other hand, there is remarkable uniformity in the proportion of the total contributed by females aged 15 years or more in all of the populations except A, the more remarkable when it is remembered that 90 per cent of female infants born in population C were allowed to survive, and this proportion decreased with each model to only 70 per cent in F.

For archaeologists -- and others -- the nature of the evidence will determine which segment of the population is or was relevant. The two kinds of evidence for which the structure of the population is perhaps most significant are those relating to the fundamental needs of people for shelter and food. For simplicity, it will be assumed that the archaeological record for the first consists of clusters of clearly identifiable house sites within some prescribed area, and for the second, undeniable evidence that the people who inhabited those houses practised some form of agriculture.

Given no limit to the amount of building material available, the number of houses occupied at any one point of time will depend largely on the number of households to be accommodated. Though the definition of what constitutes a 'household' will vary from society to society, for many of them a household will centre on a married woman, and such households might be expected to constitute the majority of households in a community. There may of course need to be some modification of this simple definition if other evidence warrants it, and the equation of households with married women might well represent the least number of dwellings required to shelter a particular community.

If the sex ratio in the adult sector of the population is such that it might be assumed that all females marry as soon as they are nubile, and each occupies a separate house immediately on marriage, the number of occupied dwellings is likely to approximate the number of females aged 15 years and over. If husbands lived apart from wives and children this estimate based on women alone would be too low, but to establish the upper end of the range, some further assumptions are needed about the patterns of male behaviour. Though these must necessarily be arbitrary, they should nevertheless be recognized explicitly because the size and composition of such households would rarely resemble those of households which centre
on a married woman.

In the various populations of Fig. 1.1 and Table 1.1, there are sufficiently more males than females in populations D, E and F for it to be reasonable to assume that any female aged 15 years or over would be married. For A and B, and perhaps C, where the numbers of males and females are more nearly equal, there can be no such certainty of marriage for a female as soon as she attains the age of 15 unless one postulates either polygyny or an equally young age at marriage for males. Hence, in these three populations, estimates for the number of houses that might be expected which rely on the numbers of adult females would be less conservative than comparable estimates for the other three populations.

However, assuming that this criterion is equally valid for all six populations, the number of households expected in each ranges from 2,445 to 3,085, and as each population consists of 10,000 persons, the multiplying factors connecting households and population size range from 4.1 down to 3.2. If one transforms these households into house sites surviving at some later period, the inflation of these numbers by some common factor to estimate the number of people who inhabited the houses would clearly lead to errors in the estimates, and the magnitude of the errors would depend on the choice of the inflation factor. If perchance one hit upon 4 as the average number of people per house, this would exaggerate the size of five of the six populations by about 20 per cent; if the smaller factor of 3 were used, the size of one population would be under-estimated by about 25 per cent, but there would be only negligible errors in the other five estimates. Any inflation factor larger than 4 would exaggerate the size of all of the populations.

Nor does one fare better with estimates based on the amount of land cultivated, or needed to be cultivated, to provide food. Again as a first approximation to the size of the work force available for cultivation, one might take the numbers of males and/or females aged 15 years and over, or alternatively limit the numbers to those aged between 15 and 59 years. The first is probably the more realistic because some of the younger members of the population might participate in this activity while some of the old people might not, so there would tend to be some cancelling out. However, a decision might need to be made as to the propriety of the participation of
women in cultivation, because their exclusion considerably reduces
the size of the work force available, but changing roles of males
and females in agriculture -- as a result of the introduction of
some new crop, for example -- might greatly increase the potential
for food production.

However, if each adult male in each of our population models
cultivates the same amount of land, all of equal productivity,
population $F$ would have more than $1\frac{1}{3}$ times as much land under
cultivation as population $A$. If both males and females are
involved in food production, uniformly throughout all populations,
the work force in $F$ could -- and may have needed to -- cultivate
40 per cent more land than $A$. Yet both areas of cultivation are
supporting the same total population. The converse argument of
some arbitrary acreage required to provide adequate sustenance per
head of population, irrespective of the structure of the population,
is also simplistic and becomes more so when the estimates of acreage
or quantities required derive from contemporary land use and modern
varieties of food crops, and take no account of what Lea (1969)
described as the "non-nutritive functions of food".

A further illustration of erroneous inference or comparison
concerning land use is given in Chapter 4, but at this point it
might be stressed that the population models which have been used
here (and will be used again) for illustrative purposes are not
the only models that might be devised, nor are they exceptional in
terms of the proportions of adults in the total population. In a
set of 304 stable populations that were constructed by Carrier &
Hobcraft (1971 pp. 94-112) the proportion of persons aged 15 years
and over ranges from 89 per cent down to 46. The range in Weiss's
(1973 pp.115-86) set of models for stationary populations is smaller
than theirs, but his upper limit for the proportion of the total
contributed by persons aged 15 years or more is 68 per cent, which
is virtually the same as in population $F$ and unlike $F$, it is
stationary and includes no infanticide. Any of these models is
equally plausible when nothing is known about the population except
what has been left behind in or on the ground, but inferences from
there to the size of the population that created what remains can
no longer ignore the range of possibilities likely for the
demographic structure of such a population.

In the Pacific region, almost any population of 10,000 persons
Fig. 1.2 Hypothetical frequency distributions of 100 villages by size, each with a mean of 24 households per village.
would be distributed over the inhabited area in discrete clusters or groups of varying sizes. These population groups would not necessarily be homogeneous in respect of their age and sex composition, nor can one generalise about the likelihood of any one of them having precisely the same demographic structure as the total population. A scaling-down of the total into the small components which would represent the populations of villages of different sizes, for example, will be a first approximation to their population structure, but no more than that. In general, one might expect more variability in the age/sex structure of small units of the same size than in the larger ones, simply because each individual in a small group carries relatively more weight in that group's total than does an individual in a larger aggregate. However, the relative weight of the smaller units or villages as compared to the larger in the total population is not invariable: instead, it is directly related to the frequency distribution of the villages by size.

Again for simplicity, let us assume that our population of 10,000 is dispersed throughout 100 villages ranging in size from fewer than 10 households to 80 or more. The number of villages in each size-class between these limits is not known, but as the average population per village is 100, the average number of households per village can be estimated by equating 'households' with some group within this population of 100 people. However, the mean alone does not determine the shape of a frequency distribution, and the same average number of households per village might be derived from several different distributions of villages by size. Three such distributions are shown in Fig. 1.2 but, as with the population models presented above, these are only examples of how 100 villages might be distributed according to size and still retain the same average number of households per village.

For each of the frequency distributions shown here the average number of households per village is 24, but the relative contributions of the populations in villages around this average to the total vary quite markedly. In the most skewed distribution Z, villages in the two size-classes of 20-24 and 25-29 households constitute only 7 per cent of all villages, as against 42 per cent in the quasi-symmetrical distribution Y and 31 per cent in X, and these percentages represent the likelihood of selecting an 'average village' for investigation. If villages smaller than these leave
little trace of their former occupancy, the proportion of villages excluded from observation in a survey of the area would be respectively 63, 36 and 47 per cent.

Although one would not be tempted to generalise from the few really big villages (say, 50 households or more) in X and Y, the frequency with which villages of this size or larger occur in Z could mislead one into assuming that the average village size was much larger than the 24 households which is the mean of the complete distribution. If these three distributions are truncated so as to exclude villages with fewer than 20 households, the average numbers of households per village in the remainders are 32 in X, 30 in Y and 49 in Z. Inflation from one or more such villages to the total of 100 villages would therefore over-estimate the number of households in the population by at least 25 per cent (Y) and perhaps by more than 100 per cent (Z).

Even if there is no such exaggeration of the average size of villages in terms of households, the number of persons per household, and therefore the number of people in villages of different sizes, depends on the population model chosen to represent the age structure of the population. In general, the smaller the proportion of the total contained in that segment used to identify 'households', the larger the average number of persons per household must be. Going back to Table 1.1, an 'average village' of 24 households in population A would contain about 100 people, but in populations B and C this number would fall to about 80. In the other three models the number of females aged 15 years and over is less likely to represent the number of households because of the surplus of males at these ages, but if the criterion is bent a little to allow for them by averaging the numbers of males and females, the 'average village' of 24 households would decrease progressively in size from about 75 people in population D to about 70 in population E.

In effect this means a difference of one person in the average size of households -- from 4 in population A to 3 in population E -- and the logical question at this point is how much difference would such changes in average size of households make in the dimensions of the dwellings needed for their accommodation?

There seem to have been only two published studies relating population and floor area of dwellings. The first was a short paper by Naroll (1962), the other a lengthy study by S.F. Cook & Heizer.
(1965, 1968) of which there are two versions, and although these were published three years apart, they are best seen as one study written in 1964 (Cook & Heizer, 1968 p.79).

Naroll's exercise covered the "largest settlements" in each of 18 diverse societies, six in North America, six in Oceania, three in South America, two in Africa and one in Eurasia. Combining the literature, he was able to find more or less direct estimates of populations in these settlements in times past, and could also estimate corresponding floor areas indirectly from reports of the dimensions of a 'typical' dwelling and the number of dwellings. Cook & Heizer used 27 ethnographic "regions" of aboriginal California, but they were forced to rely on indirect methods to estimate both populations and floor areas; these were constructed by multiplying numbers of dwellings reported by 'typical' household size to derive populations, and by 'typical' dwelling size to derive floor areas. All were shown as of a 'typical' village in each region, because "we rarely know how many villages were occupied at a particular time" (Cook & Heizer, 1965 p.33). All three authors had previously taken part with others in studies relating population and settlement size (as distinct from floor area) -- Naroll, 1956; Naroll & Bertalanffy, 1956; Cook & Treganza, 1950; Cook & Heizer, 1953; Heizer & Baumhoff, 1956 -- but these are not of immediate relevance to the present question.

A short answer to that question was given by Naroll (1962 p.588): "the population of a prehistoric settlement can be very roughly estimated by archaeologists as of the order of one-tenth of the floor area in square meters occupied by its dwellings", i.e. 10 square metres per person. Cook & Heizer (1968 p.114) summed up much more equivocally: for a household of six the basic floor requirement is 20 square feet per person (i.e. 1.86 square metres), but it approaches "a limit of 100 square feet" (i.e. 9.29 square metres) per person for households larger than six. Willey (1968 pp.217, 223) mistakenly, but excusably, interpreted Cook & Heizer to have given the simple answer "one person per 20 square feet ... stable for aboriginal California", but himself asked "Does 20 square feet of floor space per person correspond to some human biologically or psychologically determined mean?" He speculated that the ratio might hold "for hunter-gathering societies and for farming societies up to a certain level of development", but fall below
20 square feet "at perhaps the threshold of urban life" -- this last suggestion in curious contrast with the actual conclusions of Naroll and Cook & Heizer. However, we shall now see that those conclusions are not validly supported even by the data and analyses presented by their authors.

Naroll and Cook & Heizer fitted least-squares log-linear regression lines of the form \( \log y = a + b \log x \) i.e. \( y = e^{a+b} \) (where \( e \) is the base of natural logarithms) to the data supplied by \( n \) pairs of observations \( \{P,F\} \) where \( P \) is the population and \( F \) the floor area (in square metres) of each of \( n \) settlements or villages, both estimated in the manner described above. From Naroll's data we can calculate the two regression equations:

\[
\begin{align*}
(1) \quad \log F &= 3.07 + 0.84 \log P \quad \text{i.e. } F = 21.62 P^{0.84} \\
(2) \quad \log P &= -1.25 + 0.91 \log F \quad \text{i.e. } P = 0.29 F^{0.91}
\end{align*}
\]

For these equations, \( n = 18 \), the correlation coefficient \( r = 0.878 \).

From Cook & Heizer similarly -

\[
\begin{align*}
(1) \quad \log F &= -1.14 + 1.54 \log P \quad \text{i.e. } F = 0.32 P^{1.54} \\
(2) \quad \log P &= 0.88 + 0.62 \log F \quad \text{i.e. } P = 2.42 F^{0.62}
\end{align*}
\]

Here \( n = 27 \), \( r = 0.978 \), with \( F \) in square metres as in Naroll.

The equations (1) and (2) are not mere reflections of each other such as might be obtained simply by solving (1) for \( P \) and (2) for \( F \). By (1) error is minimized in the direction of \( F \), by (2) in the direction of \( P \). Both Naroll and Cook & Heizer published only (1), because they regarded population as "causal" or "independent" and floor area as "dependent" (although statistically neither regression implies anything about cause or effect). At once we have a paradox. The archaeologist in this exercise is supposed to be "predicting" populations from what he can now measure of dwelling sites, and should presumably give priority to the regression (2) which looks towards population, not to (1) which looks towards floor area. "That the chain of reasoning is thus reversed is important to bear in mind, since we shall seem at times to be arguing backwards" (Whiting & Ayres, 1968 p.117). Accordingly, the authors may be given the benefit, contrary to their own practice,
of "predicting" population from (2). The inherent difference of meaning between (1) and (2) already indicates a certain quantitative conflict; its recognition would only heighten and complicate the critique which follows.

Naroll's rule-of-thumb -- one tenth of the floor area in square metres equals population -- gives a reasonable approximation to his regression equation only for those settlements where the floor area exceeds about 50,000 square metres. In the range of 400 to 16,000 square metres, which includes 13 of his 18 societies, the rule-of-thumb falls grossly short of the regression population estimates. Naroll (Fig.1, p.588) has perhaps been misled by the eye-catching compression of numbers in a logarithmic grid. The scatter of points about the regression itself is similarly deceptive: the 18 observations comprise one in which the error of population estimate is ten-fold, another three in which it is more than 100 per cent, 11 between 40 and 100 per cent, and three about 10 per cent. With Naroll's data, floor area is a poor and misleading guide to settlement population.

Cook & Heizer get better results. In only one of their 27 observations is the error of "prediction" from the regression equation more than 30 per cent, in 17 it is about 10 per cent or less, and in the remaining nine between 20 and 30 per cent. It is tempting to infer that this much-improved record reflects the homogeneity of aboriginal California by comparison with Naroll's far-flung settlements, and/or the superiority of the data analyzed by Cook & Heizer.

Nothing could be further from the truth. "Village" population in each "region" is the product \( P = h \cdot m \), where \( h \) is the number of occupants per dwelling and \( m \) the number of dwellings in a 'typical' village. This is being plotted against \( F = q \cdot m \), where \( q \) is the floor area per dwelling and \( m \) the same number of dwellings in the same 'typical' village in each region. Cook & Heizer (1968 p.92) early admitted that they could find no useable correlation between \( h \) and \( q \) at the household level, because of "the identity of values for so many regions" (p.95). The "identity" arises because the number of persons per family "is almost universally taken as 6.0 in aboriginal California", and "the number of families per house, with a few exceptions is 1.0" (p.89), so that "the conventional number of 6 occupants per house" is "assigned" in 18 out of 27
regions (p.95). But "the mean house floor space varies widely from region to region" (p.92). In fact a family of six is assumed in all but four regions, where it is either 7 or 7.5. The exceptions to an "assigned" household size of six are almost entirely due to the few "multifamily" regions in which 2.0, 2.5 and 3.0 families are assumed to share one dwelling. These household numbers are not supported by any empirical evidence that can be measured.

Thus in 18 of the 27 regions the best "prediction" of population per village is obtained by the formula \( P = 6m \), which results in a perfect correlation between population and number of houses, correct to the nearest person and with a probability range of zero. This is independent of any errors in the counting of houses in a 'typical' village, and presumably it is easier to count houses than to measure the total floor area. The introduction of floor area per house produces 'noise' around the signal. The success of "predictions" in 27 regions from the regression equation between population and floor area per village reveals nothing about "some biologically or psychologically determined mean" as Willey had hoped. It discovers for us, and re-discovers for Cook & Heizer, the precise methodology underlying their so-called observations.

Thus the question asked above on p.18 remains unanswered - by Naroll because his statistics behave too badly (possibly as the result of errors in the original estimates), by Cook & Heizer because theirs behave too well in their controlled environment. All of these writers (and other besides) seem to have been misled by high correlation coefficients obtained from dubious, spurious and logarithmically-transformed data; perhaps also by the lure of "allometry" -- for the uninitiated, the study of log-log statistical relationships -- as a new vision of quantitative generalizations underlying cultures (Naroll, 1962 p.587; Cook & Heizer, 1968 p.98), in which the mathematical form of the generalization becomes more important than the facts on which it is based. Cook & Heizer would certainly have been better advised to heed their own laborious proof (1968 pp.105-10) that -- in terms of site area rather than floor space -- aboriginal California is best considered as "three discrete entities", with no generalization spanning the whole.

But there are more fundamental misgivings. The occasion for
asking our question in the first place was the demonstration by means of simulation models that a population of a given total size might be made up of families or households differing in both number and average size, depending primarily on the age and sex structure of the population, and perhaps other factors as well. To insist that a population is just a number -- as, for example, do Cook & Heizer (1968 pp.84-5) when they postulate that "population may be taken simply as the aggregate number of inhabitants without consideration of age, sex, or economic or social status" -- is to close one's eyes to the facts of life which inevitably intrude upon the interpretation of archaeological evidence. Many others are just as blinkered, although perhaps less ready to make the explicit admission.

Again, the alleged "universal" acceptance of 6.0 (or some other number) as the average family size in the society being studied betrays a simplistic lack of discrimination between babies and bath water. It would seem that the people who created and lived at archaeological sites were somehow different from populations alive today, and that every husband and wife then had some average number of children, gathered around the household hearth or oven, irrespective of the wife's age or the ages of the older children. Though Braidwood & Reed (1957 p.24) and Birdsell (1957 p.55) are more conservative in their estimates of this number than were Cook & Heizer (1965, 1968) and S.F. Cook (1972 p.13), all seem to have confused average completed family size with the average family size in a population at any particular time. They appear to forget that women in prehistoric societies also needed the whole of their reproductive span to achieve some ostensibly appropriate number of children, and that while some were doing that, women younger than they of necessity had fewer children, so that the average family size in any population at any time was smaller than the average completed family.

The magnitude of the differences between the two averages might be less dramatic in some population groups than it was for the Indian population in Fiji in 1956, but this was a population with early marriage for females, high fertility and low mortality, even among infants. At that time the average number of children surviving to women at or near the end of their reproductive period was 6; but the average for all women of reproductive age was only 3 living children
and the average family unit in this population consisted of 5.1 people (McArthur, 1958 pp.28, 160, 219). Hence the assumption that in primitive societies, households or biological families invariably consisted of two parents and either 3 or 4 offspring presupposes a whole set of conditions either not dissimilar to, or even better than those enjoyed by the Indian population in Fiji in 1956.

The absurdity of extending generalizations such as these to small population groups should need no further emphasis. The kind of situation that might arise in a group of 50 households as a result of chance variations in the sex of children born has already been described (McArthur, 1966 pp. 108-9; supporting document in back cover) and as the number of households decreases, the likely variability in both the age and sex composition of the community increases, so that the likelihood of any one village or community being a microcosm of the total population, with each household reflecting the characteristics of the whole in an equally determinate fashion, is exceedingly remote. Of necessity, the reverse process of generalizing from a single sample to the whole is still more hazardous, because it is impossible to assess whether the various errors that might arise are cumulative, or whether some may tend to cancel out others.

At this point it might be useful to recapitulate the salient problems of inference covered so far. First, there is no unique relationship between the size of a population and its demographic structure, nor is there any necessary uniformity of structure between units of the same size within any population, so that there is therefore no direct and simple relationship between the archaeological evidence and the size of the population that created that evidence. The selection of sites for investigation (especially in tropical areas) is largely dependent on the physical quantity of the remains, and their representativeness or otherwise rests on the shape of the frequency distribution which described the totality of villages according to their size. The number of persons who might have been accommodated per house within villages will vary according to the demographic structure of the population, and if there is any simple mathematical relationship between the number of occupants and floor area, it has yet to be demonstrated.

Even with a time perspective of thousands of years, the people
who created and lived in or on archaeological sites were living populations subject to exactly the same demographic processes as populations of the present day. Their size increased if the accessions to the population, either through births or immigration, exceeded the losses through death and emigration. Migration (in either direction) is a process intrinsically different from the essentially continuous birth and death processes, and the numbers of births and deaths which are likely to occur depend fundamentally on the age and sex structure of the population, although both might also be modified or constrained by social or cultural or environmental factors.

Within the last two centuries there have been several attempts to devise general theories of population growth, and though most of them have been discredited by the course of events, vestiges of at least two still remain, still sufficiently entrenched in the literature for S.F.Cook (1972 p.25) to write, "The relation between carrying capacity and population always rests, at least implicitly, on a demographic assumption: the human aggregate will always multiply until it reaches the upper limit of size imposed by the carrying capacity of the environment". The first "always" in this sentence properly extends the generalization beyond the field of prehistory, but it is the generalization itself that needs to be examined.

Its simplicity is both appealing and has the ring of truth. Its evident applicability at all times to all peoples and all areas of settlement offers to the investigator important inferences about population, merely from the evidence of physical surroundings, with extraordinary economy of hypotheses. Its free association with names such as Robert Malthus and Raymond Pearl -- their "limits of subsistence", "potential resources for the support of growth", etc. -- commands respect, both in the widest social context of mankind and in the scientific laboratory. Yet if one looks harder at the meaning of its words, and at the words of Malthus and Pearl themselves, any scholar will realise that he has been "maffled" into delusory confidence.

Of all the men of talents whose writings I have tried to read up to this hour, Mr Malthus has the most perplexed understanding. He is not only confused in himself, but is the cause that confusion is in other men. Logical perplexity is shockingly contagious; and he who takes Mr Malthus for his guide through any tangled question ought to be able to box the compass very well; or
before he has read ten pages he will find himself (as the Westmoreland guides express it) "maffled", and disposed to sit down and fall a-crying with his guide at the sad bewilderment into which they have both strayed (De Quincey 1823 in Masson, 1897 p.34).

De Quincey is neither the first nor the last to suffer this indignity. Malthus in his first Essay on the Principle of Population did indeed assert (as a natural corollary of incontrovertible postulates of reason and experience) that "the principle, according to which the population increases, ... keeps the inhabitants of the earth always fully up to the level of the means of subsistence" (Malthus, 1798 pp.364-5), which is very much the "demographic assumption". But this is the strongest form in which Malthus ever stated that proposition. In the five editions of the Essay that followed, from 1803 to 1826, no such bald statement appears. Unfortunately, it was never expressly retracted, and words resembling it, or seeming to invoke it, can be found in each of these five editions (e.g. Malthus, 1803 p.16; 1826 I p.29). Similarly, the curious rigmarole contrasting the respective geometric and arithmetic rates of increase for population and subsistence, by which Malthus had illustrated and confirmed his first proposition, is displayed in all editions (e.g. Malthus, 1798 p.14; 1803 pp.5-7; 1826 I pp.6-10).

But the principle itself was already undermined in the first edition (Malthus, 1798 p.364) by the admission that its operation might be halted "for a long period" among peoples whom Malthus regarded as "savages". In the second edition, it was in effect abandoned by the replacement at various points of "means of subsistence" by the words "actual produce", which is a far cry from "carrying capacity" and taints inferences from physical surroundings to population with circularity. Thus: "Europe is by no means as fully peopled as it might be"; "There is probably no island yet known, the produce of which could not be further increased. This is all that can be said of the whole earth. Both are peopled up to their actual produce. And the whole earth is in this respect like an island" (Malthus, 1803 pp.6, 46; 1826 I p.67).

These confusions were perhaps not altogether the result of "logical perplexity". The first Essay was a vigorous pamphlet directed against Godwin, Condorcet and other Utopians, with "no deep learning or parade of learning" (Bonar, 1926 p.xiii Notes).
By 1823, when De Quincey wrote of his mafflement he may have read five editions (up to that of 1817) in which Malthus progressively modified his stand, partly because of his indefatigable study of the latest statistical and other information then becoming available about all parts of the globe, and partly because he tried to combine his pamphleteering impulses (against changing targets - e.g. the Poor Laws rather than the Utopians) with a growing sense of scholarship. The resulting perplexities, contagious still in our own times, were multiplied by his habit in successive editions (except the second) of "omitting and inserting instead of re-writing in full. His chapters cease to be old without becoming new" (Bonar, 1885 p.51).

It is common to suppose that the jump from the first to the second edition lay mainly in the listing of "moral restraint" as one of a trinity of checks to population, the others being "vice" and "misery". This was essentially a semantic and propagandist change of emphasis. At least from our point of view, the great jump was from a deterministic view of population governed at all times by available resources, to a looser, more historical and more descriptive scheme. The only further major textual change, apart from the assiduous accumulation and reporting of "facts", came in the fifth edition of 1817, which stressed still more the role of potential means of subsistence as "the extreme practical limit to the progress of population, which no nation has ever yet reached, nor indeed ever will. ... When we refer therefore to the practical limits to population, it is of great importance to recollect that they must be always very far short of the utmost power of the earth to produce food" (Malthus, 1817 pp.135-8; 1826 II pp.153-5)."But this acknowledged truth obviously affects only the actual quantity of food and the actual number of people, and has not the most distant relation to the question respecting the natural tendency of population to increase beyond the powers of the earth to produce food for it" (Malthus, 1817 p.312; 1826 p.489).

The logical formulation of this profound change in methodology, in so far as it exists, was given in the third edition, and deserves to be quoted in full.

The ultimate check to population appears then to be a want of food arising necessarily from the different ratios according to which population and food increase. But this ultimate check
is never the immediate check, except in cases of actual famine. The immediate check may be stated to consist in all those customs, and all those diseases which seem to be generated by a scarcity of means of subsistence; and all those causes, independent of this scarcity, whether of a moral or physical nature, which tend prematurely to weaken and destroy the human frame.

These checks to population, which are constantly operating with more or less force in every society, and keep down the number to the level of the means of subsistence, may be classed under two general heads; the preventive, and the positive checks (Malthus, 1806 I p.15; 1826 I p.12; my italics).

With minor changes in punctuation, this was the opening passage of the second chapter of every edition from the third to the sixth, and was the prelude to Malthus's statement of his three central propositions as enunciated in the second and all later editions (Malthus, 1803 p.16; 1806 I p.15; 1826 I pp.23-4). It is surprising that posterity over about 170 years seems to have ignored the crucial words "independent of this scarcity" which broke the "impregnable fortress" of supposed Malthusian theory, and made that theory useless as a tool for drawing valid inferences directly from resources to population.

From 1803 onwards Malthus was, in fact, much more concerned to show the operation of the "immediate checks" in different peoples, territories and times, than he was to link them to any general principle of population, except by liturgical phrases. The reader of Malthus's accounts of the checks to population in 17 chapters in the sixth edition -- from "the lowest Stage of Human Society" on Tierra del Fuego through "the American Indians", "the Islands of the South Sea", "the ancient Inhabitants of the North of Europe", the "modern Pastoral Nations", "different Parts of Africa", "Siberia, Northern and Southern", "the Turkish Dominions and Persia", "Indostan and Tibet", "China and Japan", "the Greeks", "the Romans", to "the different states of Modern Europe" in Book II (Malthus, 1826 I pp.xvii xviii, 25-450) -- can hardly fail to be struck by the extent to which prevailing social conditions, customs and institutions (such as brutishness, viciousness, endemic warfare and disease, marriage, property rights, etc.) appeared to play a decisive part in determining both population and the means of subsistence, as part of a complex temporal process. For most prehistoric communities very little quantitative evidence of such factors would remain, and there would be few possibilities - if any - for indirect quantitative inferences
concerning population size, however tireless and skilful the field worker.

We have ourselves, as much as Malthus, to thank for our mafflement in any such enterprise. But Malthus was not the supreme maffler; that unenviable title should probably be accorded to a celebrated American biometrician Raymond Pearl who, from the "impregnable fortress" of his laboratory, in 1925 wrote:

There begins clearly to emerge the paradoxical situation that probably a real understanding of the problem to which Malthus addressed himself is going to come more from the intensive study of lower forms of life in the laboratory, under physically and chemically controlled conditions, than from any manipulation of never quite satisfactory demographic statistics (Pearl, 1925 pp.4-5).

Having got some insight into the underlying law according to which population growth occurs, and having found that this is of a sort capable of mathematical expression, we are to approach the general problem of population along several pathways not previously open. We can, for example, upon a more adequate scientific basis than mere guess-work, predict future populations, or estimate past populations, outside the range of known census counts (Pearl, 1925 pp.23-4).

Pearl's "underlying law" was the logistic curve, one of an infinity of sigmoid curves, and one which is itself an infinity, since it requires at least three and in Pearl's own illustrations, as many as six parameters to fit it by linear least-squares regression to observations of growth. It does very well for the growth of a male white rat, a pumpkin and a population of yeast cells. The length of a tadpole's tail in the course of regeneration is not so easily fitted, and *Drosophila* in a half-pint milk bottle were recalcitrant until food was added "skilfully and in small amounts". For human populations in the several countries considered, and for the world as a whole after 1650, the curve performs quite well when fitted to the data available to Pearl (1925 pp.6-44, 172-3) before 1925.

Unfortunately, these data include only one country, Algeria, in which any observations at all occur for the upper part of the logistic curve where the rate of growth diminishes as the population approaches an upper asymptote or ceiling. This upper asymptote is the distinguishing feature of logistic and other sigmoid curves (though the latter were not considered by Pearl), and it is easy to identify the asymptote with any version of Malthus's "limits of subsistence" or Cook's (and others') "carrying capacity". With his logistic curve, Pearl
was able to describe the growth of a population from small beginnings (which may have lasted for any indeterminate time, though this was not expressly acknowledged) to the ultimate ceiling -- a task beyond Malthus. But except for the fitting of the curve to the available statistical data, Pearl said nothing of the meaning of the asymptote for human populations, or of the factors that governed the statistical parameters that determine the path of growth.

The usefulness of the logistic for predictive purposes is best illustrated by world population. In 1925 Pearl's upper asymptote was 2,026 million (Pearl, 1925 p.173). By 1930, that ceiling was already exceeded so Pearl (1939 pp.257-9) took later population estimates into account, and re-calculated an asymptote of 2,645.5 million which would "be closely approached around A.D. 2100". This new asymptote was "a figure that future events and trends now wholly unpredictable may alter, just as proved to be the case with the former extrapolation ... The case demonstrates with great clarity the necessity for frequent revision of human population logistics as new data become available, a point which the writer has always insisted since his first work with these curves".

But what has become of the confident assertions of 1925 that Malthus's problems would be solved in the laboratory? Or of the prediction of future populations and the sizes of past populations by a "law" transcending "known census counts"? (Pearl, 1925 pp.5, 24). Pearl made no more predictions about the world's population, but his second asymptote - a "colossal total", 31 per cent higher than the earlier asymptotic limit - was matched in fact around 1950. The latest projections by the United Nations Population Division (U.N., 1973a) for world population in the year 2000 indicate a total of about 6.5 billion (which is more than three times Pearl's

* For Drosophila the asymptote is presumably the half-pint milk bottle, and from personal experience with such flies bred in similar half-pint milk bottles, the food would have to be fed very skilfully if none of the population was to escape.
original asymptotic limit) and tentative projections beyond 2000 (U.N., 1973b) almost double this total within a century and a half. Ultimately, Pearl's "more adequate scientific basis" is neither better nor worse that the "mere guess-work" he so despised in 1925.

Probably because most of the populations for which reasonably reliable statistical data existed in the 19th and early 20th centuries were increasing, the likelihood of populations in which the number of deaths equalled or surpassed the number of births was rarely considered by anyone. A few people were aware that some of the small populations of islands in the Pacific were not increasing, but numerically they were insignificant in every sense of that word. To deter demographers further from recognizing the possibility of living populations that did not necessarily increase, in the 1930s there was the non-event of the declines projected and predicted for the populations of both the United Kingdom and the United States because of the low birth rates of the depression years; and since about 1950, there has been the population explosion to deter still further any thought of populations that did not automatically increase. It is not only the prehistorians who have been maffled.

However, the recent work of Acsádi and Nemeskéri (1970) on skeletal material from prehistoric sites in Europe, and a further analysis of published material from both archaeological and historical sources by Weiss (1973) suggest very much higher mortality among the prehistoric groups examined than was hitherto supposed. Durand (1972) reached similar conclusions independently and estimated that, with mortality at such levels, the level of fertility needed to maintain a stationary population was commensurate with some of the highest in the world today. With this new evidence, and the thoroughly documented example that is presented here in Chapter 3, the alternatives to populations inevitably and inexorably increasing towards some hypothetical "steady state" might soon prove more generally acceptable beyond the restricted sphere of demographic model-building.

In the meantime, the fact remains that the so-called "demographic assumption" is a fallacy. That it has been assumed otherwise by vast numbers of people (including some reputable demographers) changes nothing: repetition, no matter how authoritative, adds neither
truth nor virtue to something that is fundamentally wrong. Hence, all of those studies in which it has been assumed, either explicitly or implicitly, that populations necessarily increase to the maximum size compatible with the full (or almost full) use - given some assumed level of technology - of the resources available to them are worthless. The demonstration that an area might be capable of supporting some number of people is not proof that any such number was ever there, no matter how long the area might have been inhabited.

Green's (1973) study of Tonga falls within this category, and his claim (p.72) that archaeological techniques will succeed where others have failed cannot be realised by a methodology that rests on at least one fallacious assumption. Like many others, Green has translated the valid proposition that food is essential to man's survival into the invalid one that there is necessarily a simple and direct relationship between the size of the population and the potential for the supply of its food. In addition, his extrapolation from contemporary subsistence requirements to land use in prehistoric times is another assumption of doubtful validity (vide p.16 above), the more so because it involves the further unrecognised assumption that the demographic structure of the prehistoric populations was no different from that attained more than a century after the adoption of christianity and the 19th century Protestant ethic.

Essentially, Green's estimate of the prehistoric population of Tongatapu, the principal island of the Tongan archipelago, rests on his assumed per capita acreages of arable land and the proportion of the total arable land that would be required to support populations of various sizes (Table 3, p.70). From these he argues that the several assertions that have been made about the size of the Tongan population at contact imply the cultivation of too small a proportion of the total arable land to fit with "the well settled and cultivated landscape described by all early observers" (p.70). He concludes (p.72)

In short, late 18th century descriptions of Tongatapu seem to require settlement involving regular use of cultivated lands to the extent of more than 50 percent of the arable land, but something less than 70 percent. Use to this extent implies a population between 15,000 and 19,000 if the figures of 1.8 to 2.0 acres per person are employed, as it would appear they should be on Tongatapu.
This upper limit, "which may represent a peak population", is then reduced to 17,000 "by restricting the probable decline between 1799 and 1891 to something less than 50 percent but more than 30 percent of the former population."

The excerpts quoted from the journals of James Cook and those who accompanied him on his last voyage provide no quantitative evidence for the range of 50 to 70 per cent of arable land under cultivation at the time of their visit in 1777. The total area of Tongatapu and adjacent islands is cited as more than 100 square miles and the total arable land as more than 86 square miles, but how much of this would have been seen by the visitors, sailors all? Any individual or party would have been led by native guides, and who knows where they might have been led in their excursions? It is most unlikely that they would have explored much "waste uncultivated country" (p.71) however great its extent. Altogether, Green strains several kinds of inference too far for his "best estimate" to be anything more than an arbitrary number.

A population is not, and never was, just a number that increases uni-dimensionally towards some ceiling set by a nebulous "carrying capacity". The processes and dimensions of demographic change are complex, and the recognition of these complexities challenges much of the explanation of change in prehistoric societies, because so often this explanation falls back on "population pressures" resulting from increases in population within some territorial areas. Perhaps there were increases in population which induced such changes; but surely the argument needs to rest on a sounder foundation than a fallacious "demographic assumption"? However, the discussion and implications of this are best postponed until the data concerning Aneityum have been presented and analysed.
CHAPTER 2

SETTLEMENT PATTERNS ON ANEITYUM

Because of the comparative wealth of documentary material about Aneityum and its people so soon after continuing contact, it is tempting to try to recreate the living society that was responsible for at least part of the archaeological evidence that might now be recovered. Geddie's two accounts -- "Island of Aneiteum" and "The inhabitants of Aneiteum" -- are the principal sources, and they were already published before Inglis arrived. From internal evidence, the first of these (MR 1851 pp.22-5) was written sometime between April and December 1849, but the second (MR 1852 pp.8-9, 19-22, 32, 83-4) is harder to date. Unless it was a work of fiction (and that is unlikely, given Geddie's character) the details of Aneityumese superstitions, sacred objects and gods could only have been compiled after Geddie had acquired at least some facility with the Aneityumese language, and all that is known of this is that in September 1849 he had been "able to address the Natives intelligibly, in their own tongue" for "several months" (Ed. MR 1850 p.164).

A date very much later than 1849 seems unlikely because Rev. Thomas Powell of the London Missionary Society, who had accompanied Geddie to help establish his mission, returned to Samoa in September of that year and after his departure, Geddie's only European assistant was a layman Isaac Archibald, also from Nova Scotia, whose principal role seems to have been to teach "some 4 or 5 promising lads" when he could "collect" them (MR 1851 p.87) and help Geddie with the printing press. Hence the burden of pastoral duties, visiting the outstations and the sick, "compounding and distributing medicines" (MR 1851 p.71), rescuing widows from strangulation (J 20 Nov 1849; 15 Jan 1850) and so on, would have fallen on Geddie alone. Early in 1850 he was ill for several weeks, but by October of that year he and Archibald had printed 2,000 copies of an "elementary school book" of 12 pages, 1,000 copies of a "catechism of truth" of 16 pages, and Geddie was translating "select portions of scripture" which were to be printed "in a pamphlet form of 24 pages" (MR 1851 p.89).
There are contradictions of course between Geddie's original account and some of his own comments later, and there are discrepancies between Geddie's and Inglis's accounts of the Aneityumese "in heathenism", but one cannot be sure just how much of Inglis's (1887) account relied on information provided by Geddie, and how much Inglis elicited himself independently. In addition to these two prime sources there are two more or less contemporary accounts of the island and its people by authors other than missionaries -- one by the naturalist John MacGillivray who was on board H.M.S. Herald in 1852-5, and the other by an anonymous seaman who may have been in Paddon's employment in the early 1850's. Although MacGillivray was distantly related to Mrs Geddie (J 29 Nov 1853), he seems to have relied more on Inglis than on Geddie for all that he himself could not possibly discover in the three weeks Herald spent at Aneityum.

From these various accounts it seems that, at the time of contact, Aneityum was divided into six major political districts. A map of the island which Geddie (MR 1851 p.25) sent to the Board of Foreign Missions in September 1849 shows five divisions or districts around the coast, but as Geddie was then unaware of the extent of inland settlement, his statements about "five divisions" (MR 1850 p.123; Patterson, 1882 p.182) do not necessarily conflict with Inglis's (1887 p.24) contention of "six principal districts, three on the south side, three on the north". MacGillivray (1853) depicted the island as a circle, with one district -- Itako -- at its centre, and the other five radiating out from there to the coast.

Inglis (1887 p.24) elaborated further:-

... the districts are of very unequal sizes: each of these again is divided into several sub-districts. In the days of heathenism there was a principal chief for each principal district, and an under-chief for each sub-district. It is much the same still, only formerly the principal chiefs exercised the priestly rather than the kingly power ...

A later missionary on Aneityum, J.H. Lawrie (1893 p.709) claimed that "leading coast headlands usually divided the island into districts, of which there were six, each governed by a high chief; also two inland chiefs who were in some measure subsidiary to the shore chiefs -- these were often at war with each other."

The frequency of wars between districts is an oft-repeated theme in the early missionary writings, and as there were alliances between some districts for mutual support in war, occasionally the whole island
was split into two factions (MR 1851 p.102) though more commonly only two or three districts were involved. During his first visit MacGillivray wrote "War used to occupy the natives of Aneiteum 10 months in the 12 according to those best qualified to judge". On his second visit he reported that "the inhabitants not so many years ago were supposed to spend about 9 months in every year in petty warfare" -- and perhaps even this was an exaggeration.

A war had been waged between two districts early in 1848 (MR 1850 p.24) and despite Geddie's assertion in his account of Aneityum's inhabitants that "war seems to be the rule and peace the exception" (MR 1852 p.21), there seem to have been no further outbreaks until July 1850 when two of the northern districts were at war for about two months. When a customary ally of the aggressor declined to assist, "the aggressive party, finding themselves in the minority, sued for peace. ... Three men were killed, and, I think, as many women strangled. ... A great many have been wounded" (Geddie, 1850).

On this occasion hostilities opened with "a sudden and night attack", "but the natives, if the locality will answer, prefer bush fighting to general attacks" (MR 1852 p.21).

In general the loss of life is but small. The parties at war usually fight on the boundaries of the hostile districts. The weapons of war are spears and clubs, especially the former. The spear is thrown with great precision and force, and would prove very destructive were it not for the experience of the natives in dodging it. When a man is disabled by the spear, then a rush is made on him, and he is despatched with clubs. Close combat is uncommon. When a man falls, the side to which he belongs consider themselves beaten, and usually retreat at once (MR 1851 p.102).

Patterson (1882 p.124) gave a slightly different version from Geddie's original, but as it is all included in quotation marks, he may have combined several of Geddie's accounts.

In their modes of warfare, there is neither a display of science nor system among the natives. The opposing parties, when they come within sight of each other, begin to throw their bodies into all the attitudes of defiance, and challenge and endeavour to make the most intimidating menaces, the whole accompanied with a most savage din and clamour. The bravest men then advance from each party, and engage in combat, and the conflict soon becomes general.

The consequences of defeat in war seem to have been the destruction and pillaging of houses and gardens (J 16, 22 Nov 1852), though how extensive this might have been when whole districts were involved in war
is never stated. There are also some inconsistencies in the various accounts. "A peculiarity" about wars on Aneityum was that "in fighting times [the natives] never interfere with the women and children" (MR 1851 p.102), but women widowed by wars suffered the same fate as those whose husbands died from natural causes. "If we ask a boy if his parents are alive, we can almost anticipate the answer that the father has been killed in war and the mother strangled" (MR 1852 p.21), and this cannot be easily reconciled with the few casualties alleged above.

A more kindly and hospitable interaction between districts was the custom of feasting or ceremonial food exchanges, which were deplored by the thrifty Scots, but regarded as "events of great importance" by the Aneityumese because the status and dignity of their chief was measured by the amount of food collected and presented, and to this end they "reserved all their good food for the feasts" (J 14 Jul 1852).

When a chief concludes to feast the people of another division of the island, a restriction is laid on several kinds of food; and this often continues for six months or more. After the restriction is removed, an immense gathering is made of cocoanuts, taro, sugar cane, pigs, fish, &c. The whole is collected on a spot prepared for the purpose, and piled up in large heaps. On a fixed day the people to be feasted are invited to come to the place where the food has been gathered. After a variety of ceremonies ... there is a transfer of all the food from the one party to the other, who carry it to their own land, where it is divided among the several families who eat their respective portions in their own houses. The district thus entertained is expected to give a feast in return as a recompense (MR 1852 p.9).

However, if such feasts occurred with the frequency implied by Geddie (MR 1852 p.9) and his wife (MR 1850 p.186), they could scarcely have involved only the major divisions or districts of the island, and the spatial organization of people or communities within these major divisions is variously described.

Though Geddie used the word 'village' quite often, he never described any on Aneityum, and the nearest one comes to what he meant by 'village' is in a mention of a "public place of meeting called the Intiptang" where the male heads of families and young men of a village gathered every afternoon to talk and plan communal activities. Geddie (MR 1851 pp.100-1) was pleased when some of the early converts built an intiptang for themselves close to his own house, because this would bring the people from several villages
together whereas formerly there was seldom "familiar intercourse between the people of one village and another".

His biographer attempted to remedy Geddie's omission by inserting a paragraph in what was essentially Geddie's account of the island and its people, and though Geddie may have been the source for this, it is more likely that Patterson's (1882 p.126) version is an elaboration of Rev. George Turner's (1861 pp.370-1) report on his visit to Aneityum in 1845, when two Samoan teachers were settled at Anelgauhat and three stationed at a place about one mile inland from Port Patrick on the north coast. During his five days at Aneityum, Turner

saw nothing like a decent village. Two or three huts are put up in a plantation, and when the food is consumed there, another spot is selected, and there they plant and build again; and thus they migrate from place to place within a certain division of the island.

For comparison, Patterson's description runs:-

On Aneityum there was not what could be called a village. Two or three huts would be clustered together for the convenience of good soil, fruit trees, or a canoe harbour, but which might be speedily removed to another point within a limited district.

By April 1845 Turner had lived in Samoa for about two years, but the larger Samoan villages were not his only standard of what constituted "a decent village" because he had come to the Pacific initially to work in the New Hebrides and had lived on Tanna for seven months from June 1842. His colleague there was Rev. Henry Nisbet, another missionary from Samoa, and though they were "never able to go further than four or five miles" from their house at Port Resolution, within this radius "a settlement, or a village" consisted of eight or ten families in "huts ... put up, without any rule or arrangement, among the trees; and in this place, which had its village name, you may number a population of eighty or a hundred" (Turner, 1861 pp.84-5).

However, Turner's geographic coverage of Aneityum was not very extensive. At Anelgauhat, for example, the mission party cautiously "did not venture far inland" (Turner, 1861 p.368). MacGillivray, on the other hand, spent much of his three weeks at the island on excursions to various parts to identify and record its flora and fauna. On the south he was impressed by the natives' "selection of picturesque localities for their dwellings which are
always shaded with trees, and the air of neatness conferred upon the gardens by their being invariably surrounded by well constructed reed fences". At Inglis's invitation he spent a weekend at Aname, and going around the west coast in Inglis's whaleboat he noted "wherever the shores were sandy there were groves of coconut trees, and many natives, with a school house here and there behind the beach".

From Aname he went in search of "a large bat that was to be found in great numbers at a place about 3 miles distant", and this took him first "along the shore to the SW" where "the fringing reef [was] covered with fishing weirs of loose stones formed into dykes enclosing extensive areas where the fish are taken at low water". He then passed "through a native village beautifully situated in the woods behind the beach - with numerous paths winding among the gardens which are neatly enclosed with stone walls surmounted by fences of reeds crossing each other diagonally", where "numbers of pigs and poultry were running about", and eventually he saw "a number of the bats in question mostly suspended from the branches" of a large fig tree "growing on a steep bank overhanging some cultivated ground".

Two days later (21 Nov 1853) he and a companion from Herald, with two young natives as guides, walked across the island from Aname to Anelgauhat. The journey began with their walking along the shore to the eastwards for about a mile in order to round a long spur like hill of reddish earth, nearly devoid of vegetation running down to the coast from a distance of about 2 miles inland and dividing two valleys. At length, turning off into the woods, we came upon the foot of the valley which was to lead us to the centre of the island. In this valley they "passed several hamlets and patches of cultivated ground of the usual description", and then began their ascent along "a zigzag, slippery path in the deep, red, clayey soil" through the still-heathen district of Itako towards Inrero, the highest peak on the island. "In the bottom of the valleys here and there were huts and gardens, solitary and in groups. ... The plantations are mostly on the steep slopes of the hills forming terraces walled up below with loose stones" (MacGillivray, 1853 MS pp.88-95).

By this time Inglis had been on Aneityum for more than a year, but it was not until April 1854 that he first "made the circuit of the island" on foot, in company with Geddie and "between forty and
fifty" others, including all the principal chiefs (MR 1854 p.181), and the "inland districts" were visited -- probably reluctantly -- with Geddie and "several chiefs and church members, in all about 50 people" more than a year later (J 24 May 1855). A.W.Murray's *Missions in Western Polynesia* was published in 1863 and this contained an engraving (facing p.28) by W. Dickes of an Aneityumese heathen village, reproduced here, the origin of which it is now impossible to trace. If this represents an entire 'village' it supports Inglis's (1887 pp.24-5) contention that

There is neither a town nor a village in the whole island. The system of cottage farming is in a state of full development there. ... every man sits proprietor of his own garden, and his own cultivated patches -- ... The waste lands and the forests, to the summits of the mountains, belong to the tribe. They are a kind of crown lands, but what each man cultivates belongs to himself.

In April 1854 Geddie and Inglis had visited only "the shore districts" (J 14 Apr 1854) but they "had meetings at all the villages through which [they] passed. [The] meetings in all were twenty-four in number, and two of these were held in Heathen villages" (MR 1854 pp.181-2). This total of 24 'villages' is oddly at variance with the 47 native place names shown on the Admiralty chart based on the survey made by *Herald's* Captain H.M. Denham R.N., F.R.S. and other members of the expedition [see Map 1]. In December 1853, Inglis (MR 1855 pp.52,54) boasted of the assistance given the "staff of surveyors and a corps of scientific gentlemen" on board *Herald*, and no doubt the "local information" provided to the expedition both then and later (when *Herald's* survey was completed by her steam tender *Torch*) included the place names shown on the chart and their positions relative to one another around the coast, so the discrepancy is somewhat puzzling.

Was Geddie being kind to the older missionary and sparing him difficult terrain in Anau-unse, for example, and perhaps the southern half of the east coast? Or did the mission party visit only the places which were just "behind the beach", ignoring those set back farther inland? More than half of the names on the chart do not occur in the mission records, but Inglis rarely mentioned the names of places other than the two principal mission stations and Geddie sometimes used the name of a district to identify the principal place within that district. To Geddie, Ugha (on the north-east corner) for example, was always 'Anau-unjai' which was
the name of the district; and Ehesjei (on the north-west) was 'Anau-unse', this being both the name of the district and the name for the passage through the reef opposite Ehesjei. That the names on the chart probably do represent settlements or communities might be inferred from Turner's (1861 p.476) comment in 1859 that the island was "encircled by fifty-six school-houses, eleven chapels, and sixty native teachers and assistants".

At least eight of the 56 school-houses were at inland settlements, most of which had been evangelised first by their own countrymen. During 1851 hostility towards the mission was such that Geddie rarely ventured far from Anelgauhat because he feared for his own safety and that of his family. Instead of "itinerating" himself on Sunday afternoons, he sent a few of the converted Aneityumese to nearby villages because they could "go about with greater safety" than any of the foreigners attached to the mission. They reported back to Geddie on their return, and by the end of 1851 he claimed that "all the highest chiefs on the island, and men of rank, have declared themselves on the side of Christianity"; but notwithstanding their efforts, the heathen still outnumbered the christians (MR 1852 pp.115-7).

In May 1852 there were six Aneityumese evangelists (Murray & Sunderland, MR 1853 p.83) and an Aneityumese had been stationed as a teacher at Aniblithai, about half-way between Anelgauhat and Umej on the south coast, the previous February (J 16 Feb 1852). For various reasons Geddie's movements about the island were restricted throughout the whole of 1852. His fears for his safety continued and in the middle of March he had an attack of "the island fever" which was followed by "fever and ague", so that he was an invalid for more than two months. Inglis arrived on 1 July and as soon as his house was built at Aname, work began on a new "meeting-house" at Anelgauhat (MR 1853 pp.3-4).

This was a wattled and plastered building, 62 feet by 25, with a thatched roof of pandanus, and though Geddie was working "night and day" translating his fourth book he also supervised the building of the church. In September Mrs Geddie wrote

We are very busy collecting materials for our new church. All are assisting -- the men and boys gathering wood, making lime, &c.; the women and children collecting the pandanus leaf for the thatch. The natives have never been accustomed to steady employment and we have to talk to them like children and
encourage them to persevere. Though still unfinished, the church was opened on the last Sunday in November, and on 8 December Geddie laid the foundation stone of his new house -- to be built of stone that he had bought for £5 from the sandalwood station when Paddon decided to leave Aneityum. (Patterson, 1882 p.343; Geddie, MR 1853 pp.86, 115; J 8 Dec 1852)

In 1853 both missionaries were preoccupied in building, Geddie with his stone house and Inglis with a church. The church at Aname was begun in February and though it was "well advanced" by October, it was not opened until 12 March 1854 (MR 1854 pp.166,168; J 12 Mar 1854). It was a larger church than the one at Anelgauhat -- 70 feet by 31, with a verandah 4 feet wide all round, and its floor "raised a foot with stones, covered over with small white seawashed coral, and carpeted with mats plaited of cocoa nut leaves". Built in a "pavilion form", with a double row of pillars 15 feet high to support the roof of pandanus thatch, the walls were 10 feet high and the windows "latticed with split bamboo" (Inglis, MR 1855 p.57).

Geddie's house took almost as long to build because he had to be constantly looking after the natives -- indeed he is master builder, journeyman &c. The natives are most willing to help all they can, and they do a great deal, such as cutting down the trees in woods, and carrying the timber on their shoulders. ... The natives also make the lime-wattle and put on the first coat of plaster. (Mrs Geddie,MR 1854 p.152)

The house was completed towards the end of the year and in January 1854 Geddie lamented (J 16 Jan 1854) that "the condition of the poor natives has of late cost me much thought. The building of the new church and my house has engrossed much of my time, and I have not been able to devote to the natives that amount of attention which their circumstances seem to require".

However, early in 1853 he had started two more schools with Aneityumese teachers, one at Etung on the west coast because there were "still many heathen in the neighbourhood, and it seem[ed] important to occupy it", and the other at Anauyac, on the south coast between Aniblithai and Umej (J 21, 22 Feb 1853). At least three more schools were started in Geddie's district before the end of 1853 -- at Anekra (or Anucci) inland from Umej, Anathauwai between Anelgauhat and Aniblithai, and Uje on the west coast. The
last was "always ... a difficult place. It was long a stronghold of heathenism and the people have been corrupted by their former intercourse with the sandel wood traders" (J 23 Jan, 9, 13 Feb 1854). By May 1854 there were 13 schools in the southern half of the island and 13 in Inglis's district (MR 1854 p.181).

Except in the location of the first batch of Samoan and Rarotongan teachers, the size of a community seems to have been less significant for its getting a teacher than the desire of at least some of its members to have a teacher living among them, and their willingness to provide him with a house and land for a garden. Sometimes a school-house was built to demonstrate that they wanted a teacher, and thereby pre-empt the next to become available (J 24 Jan 1854; 8 Jan 1855). In effect, Geddie's practice was probably not very different from Inglis's (1887 p.77).

As soon as in any land we had six or eight scholars, we got these and their friends to erect a school-house -- at first a small grass hut -- and I appointed a teacher to them, generally a husband and his wife. Another grass hut was erected for the teacher and his family, and thus another district school was called into existence.

Initially, the land given the teachers was often ground that had formerly been considered sacred (J 1 Dec 1851), because one of the first acts of people who abandoned their old gods - the various natmases - was the desecration or destruction of the "sacred groves" that the gods were believed to inhabit and in which they were worshipped. "These sacred spots are numerous throughout the island, and to them the natives repair in ordinary cases, to present their gifts, and offer up their prayers". When a feast was being prepared "the sacred men ... leave their homes and remain for weeks at a time in some sacred place, supplicating the natmases in order that they may have plenty of food" (MR 1852 pp.84,83). Inglis (1887 pp.29-30) also commented every little district had a piece of sacred ground that was never tilled, and on which all sacred rites were performed. "... when heathenism was given up, ... those untilled sacred grounds, were appropriated by public consent, and, as far as they went, for the support of education, and were passed over to the teachers for the time being as gardens for the planting of taro, sugar-cane, and bananas.

Just how much land was freed for cultivation by the destruction of the sacred groves is a question that can never now be answered, but the site of the first school-house or teacher's house in any place was not necessarily the same as that chosen when the original grass
school-houses were replaced by "neat plaistered" buildings, the first of which Geddie helped erect in Anauyac in May 1855 (J 30 May 1855). In January 1856 he wrote:-

The natives at the out-stations around the island are busily employed making plastered buildings to answer the double purpose of church and school-house. Some of them are completed, and have been opened, and others are in course of erection. Few persons unacquainted with the state of the islands can form a just conception of the labour of building to the natives. The wood used is all carried by men, and it must often be taken a distance of several miles. The lime and sand must also be carried a considerable distance at times. A house is now going up at one of my inland stations, and the lime and sand for it is carried by men and women a distance of eight miles. In the course of a few months our little island will be dotted with at least twenty-five snow white buildings ... (Patterson, 1882 pp.401-2)

[The "inland station" referred to above was undoubtedly Anumej, and though the distance from there to the coast may have seemed to Geddie (as to later visitors) to be eight miles, by the map it is somewhat less.]

The floors of many of these buildings are still in evidence, and though their siting may have changed the residence pattern of the Aneityumese, there is some indication that such changes as occurred were not very substantial. While the Geddies' stone house was still being built Mrs Geddie (MR 1854 p.153) reported: "We are getting quite a village around us. When we first settled here, the natives moved inland to get away from us; but they are now building all round us. They are making much larger houses than they formerly lived in, and have their fences and plantations". Inglis (1887 p.25) also conceded that the mission stations "became the germs of villages, and the arts of civilised life have largely sprung up around them".

In May 1854, after his tour around the coastal part of the island with Inglis and "several Chiefs of Importance", Geddie (MR 1854 pp.183-4) listed the "neat and comfortable grass-houses ... now supplanting the hovels in which they formerly lived" as amongst the "temporal advantages of Christianity", but he did not say whether or not these dwelling houses were grouped around the school-houses. On the other hand Inglis (1887 pp.81, 113) implied that they were not because even when there were 28 school-houses on his "side of the island", "every native was within a mile of a school" or alternatively, "every native [was] within fifteen minutes' walk of a school and within about an hour's walk to a place of Public worship..."
on the Sabbath".

At first the school-houses were merely grass huts; but by-and-by these were all superseded by beautiful lime houses, plastered and white-washed; also a teacher's house and a house containing two small rooms, for the missionary when he visited the place, and in which he might either stay all night or take a meal, as he might require (Inglis, 1887 p.81).

The anonymous seaman/historian who had lived on the island in the early 1850s -- in 1852 he had helped rescue a woman from strangulation and took her to the Isle of Pines in 1853, and was certainly no friend of "the Divines" -- described the Aneityumese "Towns or villages[as] much scattered about .. bordering on there Plantations". Whenever the manuscript was written, its author had last visited the island in 1860 when "the native was no where to be found .. that would do a days work .. the Divine had them Employed in giving them Instruction and his maxims of things he forced them to be Slavishley Copied by the Natives .. that gave them Scairce time to make good there dailey wants .." (Anon MS pp.10,7,18). A more literate, but perhaps less independent testimony that the mission had not substantially altered the traditional pattern of settlement even by August 1865 is Brenchley's (1873 pp.195-6) comment that "the inhabitants do not live collected in villages, but separately in the midst of their cultivated patches; but the island is, nevertheless, divided into districts or settlements about sixty in number".

Once a school-house was built, this was where the people gathered when the missionary went to visit and he rarely went alone. Geddie, for example, was almost invariably accompanied by "several" or "a number of natives". In November 1854 he wrote: "In visiting, I always select a few of the church members to go along with me ... But when it is known that I am going abroad, others beside those whom I select accompany me, and our party sometimes increases to 50 or 100 persons" (MR 1855 p.139). By then Geddie was visiting his 'outstations' every three months, usually several on each trip, and to facilitate these visits, the people started making roads (MR 1856 pp.276, 421): There had also been more building at Anelgauhat in the meantime -- a larger church (started on 16 March and opened three and a half months later on 7 July 1854); separate houses for the boys and girls who were then attending the boarding-school that had been started more modestly; and a printing office -- all of them
made of wood and "plastered inside and out" (MR 1855 p.136).

Despite this activity in "building, and other public work connected with the Mission" which both Geddie and Inglis believed would "do good" and "be beneficial" -- and as well, "occupy the minds of the people to such an extent that they have neither time nor inclination for feasting and other usages common in the days of heathenism" (Patterson, 1882 p.402) -- there are frequent references in Geddie's journal throughout 1854 to the amount of food being grown then as compared with previous years when whatever a man planted, "if [it] escaped the spoliation of an enemy, it was almost certain to be tabued by the chief for a feast" (MR 1854 p.184). Judging from Mrs Geddie's description of the new "plantations" near their house in October 1853, and the method of cultivation then used, this alleged increase in food production was not due to any technological changes that the missionaries might have introduced into indigenous agriculture. The two accounts are quoted in full below.

The natives are now cultivating a great deal, considering the implements they use. They have merely a sharpened stick, and yet their plantations are beautifully neat, and produce a great deal. They dig very deep and press every particle of the earth through their hands, and their plantations look as if they had been all raked with the finest garden rake. Then the pretty reed fences, wove like lattice work, and also the foliage of the different trees and vegetables, have quite an elegant appearance. There is the Banana, with its broad green leaves, - the Taro, with its soft green velvet leaf, ... and the Sugar cane towering above all. There are also numerous vegetables such as the Yam, Sweet Potato &c, with vines, these are trained over the fences and look very pretty (Mrs Geddie, MR 1854 pp.153-4).

Her husband's account was written at least three years earlier.

In their plantations these islanders display much ingenuity and taste. These are small enclosures, beautifully encircled with a fence of reeds, which are bound together by a cord made of the husk of the cocoanut. The fences are so very neat, that they would be considered ornamental in any land. The earth is dug with a sharp pointed stick of hardwood, and then it is crumbled in the hand until it is perfectly fine. The banana, sugar cane and taro are the articles of food raised in these plantations. The spots usually chosen for the purposes of cultivation are the low and swampy grounds, but it is not uncommon to find them on the sides of hills, and on the high lands. Much skill is displayed in the irrigation of those places where the ground is dry. Small canals are dug, and water conveyed to them from the nearest stream. The water courses are so constructed that the native, by opening a small sluice at the head of his plantation, can in a few minutes water the whole. I have seen ridges on the sides of hills, in the form of steps and stairs,
under cultivation, and watered in this way. The cocoanut, bread fruit, etc. grow spontaneously, and do not require any cultivation (MR 1852 p.8).

Inglis (1887 pp.22-3) also commented on the ingenuity of the Aneityumese with their methods of irrigation.

The hills are steep, and the valleys for the most part deep and narrow. A small strip of alluvial land along the shore, where the shore is protected by a reef, with the lower part of the larger valleys, include the most of the cultivated land of the island, and contain the principal part of the population. The lower and middle parts of the mountains next the sea ... are scantily covered with herbage and brushwood. On the upper parts, the soil, though stony, consists of a rich black mould, and dense forests cover the summits of the mountains. The island, as might be expected, is well watered, and the ingenuity of the natives is seen in nothing perhaps so much as in the system of irrigation by which they water their plantations of taro and sugar-cane. There are swamps in different parts of the island, which are extremely valuable as taro grounds, ...

The naturalist John MacGillivray (1853 MS pp.88, 94-5) noted on his "first walk" around the southern part of the island on 8 November 1853 "the irrigation by artificial means which here is carried on to a great extent", but his admiration was reserved for the people who inhabited the intermediate reaches of a valley on the northern side of the island, through which they passed on the journey across the island from Aname to Anelgauhat.

In no part of the island had I seen irrigation carried to the same extent as here. One of the artificial water channels was upwards of a mile in length, and, in places, as when carried along the side of a steep bank, it must have cost much labour, and a considerable amount of skill. In due time the main valley was seen to branch off into two when we crossed the stream at a dam and ascended the ridge between them. Above us was the highest elevation on the island....

It was after climbing "to the height of about 300 feet above the stream" that he saw the houses and gardens in the bottom of the valleys, and the plantations "on the steep slopes of hills forming terraces walled up from below with loose stones", and then almost immediately "encountered ... the people of this the most central district of the island - called Itako, who alone as a body retain their former heathen customs and belief".

MacGillivray's is the only direct reference found to stone-facings on taro terraces, although Geddie had earlier acknowledged that the hill-sides were terraced; but not even MacGillivray says whether or not "the artificial water channels" were lined or faced with stones. His is also the only direct reference to free-standing
stone walls enclosing gardens, as distinct from the artistically woven reed fences described by both Geddie and his wife, and if the village described by MacGillivray (1853 MS p.91) is Ijyphav -- which it probably was, given the direction and distance from Aname -- on 19 November 1853 its inhabitants were still stubbornly heathen and remained so for some time. In January 1855 Geddie described Ijyphav as "the only remaining heathen settlement on the island" (J 16 Jan 1855), and in 1857 two brothers from there were punished by the chiefs -- by then formed into a sort of governing council (J 30 Jul 1855) -- for having strangled their mother when the son of one of them died (J 17 Mar 1857).

At first sight, it seems incredible that there are such omissions in otherwise detailed accounts of the island and its people by the two men who might be presumed to know most about Aneityum and the Aneityumese, and more especially Geddie who was there first and settled at Anelgauhat within six months of Paddon's move to the mainland. But is it so incredible? Although he was born in Scotland, Geddie's parents migrated to Nova Scotia when he was still very young and his last few years there before he left for the Pacific were spent on Prince Edward Island. Both there and in other parts of Nova Scotia it was not uncommon for the stones cleared from the land to be used for building fences and houses (Mrs Maddin, 1973; T. Mackenzie, 1973), and so the reed fences of Aneityum would excite much more interest than stone walls both to the Geddies and to the readers of their letters and the accounts destined for less personal circulation. Inglis was attached to the Reformed Presbyterian Church of Scotland, and hence there was no novelty for his readers in stone walls either.

An equally common-sense and plausible explanation for Geddie's failure to mention the stone walls buttressing the "ridges on the sides of hills, in the form of steps and stairs, under cultivation" would be that he had already experienced and reported "the tremendous hurricanes peculiar to these latitudes ... which were most destructive in their effects" (MR 1851 p.23) before his account of Aneityumese agriculture was written. Early in 1849 (J 15 Feb, 16 Mar 1849) there were the two hurricanes which help date his account of the island (MR 1851 pp.22-3), and if this was written before his account of the inhabitants (MR 1852 pp.8-9, 19-22, 32, 83-4), it would have seemed obvious to anyone then on the island that terraces on hillsides needed support if they were not to be washed away in Aneityum's "tempestuous ... rainy season".
The stone facings of creeks, streams and artificial water channels would have been less visible than either the free-standing walls (whether topped by reed fences or not) or the walls supporting the terraces, but again eminently practical given the usual pattern and extent of Aneityum's rainfall. When this was excessive, the water could be channelled off so that the terraces would not be eroded, and in the drier months the streams could be dammed to conserve water for irrigation when and where it was needed. MacGillivray's party crossed a stream by "a dam" and "from this place and until we had descended to a lower elevation on the other side it rained almost without intermission but not heavily." Perhaps one needs to see the dramatic changes that can occur in the levels of streams within a relatively short time even in what is supposed to be the 'dry season' to appreciate both the necessity for, and the probable dual purpose of, the stone facings to water courses, either natural or contrived.

Apart from the stone walls, what else might the transient visitors have seen and recorded that the resident missionaries did not? The "vegetable productions" that Geddie listed as being "of most value to the natives" were coconuts, breadfruit, bananas, sugar-cane, yams, sweet potato and taro; and of these, the bananas, sugar-cane and taro were grown in "small enclosures, beautifully encircled with a fence of reeds" (MR 1852 p.8). MacGillivray (1853 MS p.93) distinguished between "the common taro and the larger or horse taro" and "various kinds of yam" in the gardens near Aname where papaya was also cultivated along with bananas, and all in "patches of cultivation neatly fenced in with stout reeds in two rows diagonally crossing and connected by horizontal bands of the same material", with paths winding between them.

Except to note once that "the drudgery and hard labour [fell] to the lot" of the women while their husbands -- "the lord[s] of creation" -- indulged in indolence (MR 1852 pp.20-1), Geddie tended to imply that the work in the "plantations" was done by the men (MR 1852 p.8). The anonymous seaman/historian was rather more critical of the men. "They do not plant and grow many yams not sufficient for their own fashions the yams is held as a luxury .. if this was the work of the females .. there would be more grown .. whereas the .. Tarroh .. they have a Sufficiency .. because this is the Entire work of the women" (Anon MS p.15). So too was the feeding
of the pigs which, at this stage, were probably still the native pigs, "of a diminutive size, but numerous and highly valued" (MR 1851 p.24). In April 1850 Mrs Geddie (MR 1850 p.186) lamented

I am afraid it will be a long time, ere the women of this dark Island will learn to read. At this place they are now all busy feeding pigs for a great feast that is to take place some three or four months hence. ... The poor women have a hard time of it, collecting food for the pigs, and they are as particular in baking it for them, as if it were for themselves.

Like the cultivation of yams, fishing may have been a male prerogative although Aneityum was "not a great Island for fish" (Anon. MS p.6). Nevertheless, "around the island, but especially at the principal harbour, fishing [was] one of the regular and almost daily pursuits of the natives" (Inglis, 1887 p.23), but it was MacGillivray who reported the "fishing weirs" close to Inglis's station. And was it only the scarcity of fish that caused Geddie's (MR 1851 p.87) first group of "free school"-boys, when told that they must fend for themselves, to "spend so much of their time in fishing and collecting food that very little [was] done in the way of teaching"?

Neither missionary suggests the kind of mobility which Turner (1861 pp. 370-1) implied for the population, but if this was still "much scattered" in 1853-4 (MR 1855 p.125), it is not inconceivable that neither was really aware of changes in house sites for any of the people except perhaps the chiefs. In his visits to the original 'outstations' for example, Geddie would probably not have gone far from the beach alone, nor strayed far from the path to the teacher's house. If the teacher was not at the beach to meet him, a messenger would have been sent to him and he would then have guided Geddie to his own house or the chief's where Geddie would wait for the people to be 'collected' or 'assembled'. Even later when he visited a place which was still heathen and the people hid in the bush, Geddie "sat down under the shade of a tree, while some of the chiefs and people who accompanied [him] went in search of the people" (MR 1856 p.420).

However the clearing of the bush and digging of new canals in preparation for new gardens while harvesting the old would help reconcile some of Geddie's apparently contradictory statements -- "plantations" which were but "small enclosures" yet "on ordinary occasions, one meal only is cooked in the day, and that towards the evening. No native will taste food, until he has completed his days labour on his plantation. A superstitious dread of eating...
before work exists, lest the Natmasses should blast their crops" (MR 1852 p.8). Only when Geddie (HFR 1861 p.247) was reporting the measles epidemic in 1861 is there a suggestion that plantations might not always have been in the immediate vicinity of the houses. "Many who might otherwise recover, die from want of food. They cannot go to their plantations for it and cook it, and there are few who can do this for them".

Perhaps they had more than one plantation, or perhaps it was the same on Aneityum as on Nguna where "the inhabitants clear only enough land to make their plantations for this year; next year they clear a fresh plot, and so on, the former plots reverting rapidly to the bush state." Certainly their houses were similar in construction -- "in shape very much like a whaleboat turned upside down" -- and with 18 villages and a population of about 1,200 in 1870 (Don, 1927 pp. 11, 15, 12), the average number of persons per village on Nguna then was about the same as on Aneityum in the 1850s. Geddie (MR 1852 p.8) described the houses as being of a small size and rude construction. -- Posts are put in the ground 6 or 8 feet apart from the bottom and bound together at the top; over this framework, reeds are placed at a short distance apart, as a foundation to the covering of thatch that follows. The one end is closed and the other left partly open to answer the purpose of a door. An ordinary sized building is 12 or 15 feet long, and 6 or 7 feet high in the centre.

Except that it had a stout ridge-pole bound to the underside of the crutch formed by the curved posts that formed the basic framework, and a lighter one lying in the fork and likewise bound to the supporting posts, an abandoned house still standing in 1972 on a sheltered terrace was just as Geddie had described more than 100 years before. Inside it measured 5.3 metres by 3.5 metres and was 2.15 metres high at the centre. There were 9 pairs of posts supporting the ridge poles, and the framework to which the thatch was attached was rather more substantial than Geddie's "reeds". Lengthwise along each side were 4 sturdy poles fairly evenly spaced down the curve, with two pairs of thinner ones between each pair to which the thatch was strongly laced and tied. The names of the man and wife who had lived there were known, and it was thought that they had died at Anelgauhat during the war when they were very old, leaving no descendants.

Except for the various stone 'walls' that may not have been present in or near every settlement on the island, the documentary evidence about Aneityum and its people within the first decade of continuing European
contact suggests that little of their material culture was likely to survive from the prehistoric into the historic period. Traces might be found of houses and house sites either because the houses had been burned when their owners had died (J 15 Mar 1849, 3 Dec 1851), or when the houses were destroyed as punishment or revenge or in consequence of war (J 17 Mar, 28 Apr 1857; 16, 22 Nov 1852), and there would also be datable evidence of occupation associated with the earth ovens used to cook food (MR 1852 p.8). However, houses were burned at death only if the owner was a person of importance, and one can only speculate on what might have happened when he was not. His wife, and any children too young to look after themselves or not wanted by others in the community, would have been strangled to accompany the husband and father to the world of the dead (MR 1852 p.21) and the house probably either left to rot, or pillaged for firewood or a new house.

If the location of plantations, and therefore of houses, changed frequently, the smaller the number of houses per 'village' (however defined) the easier it would be to change the site of the village; and so the whole island could be dotted with habitation sites that would appear to be contemporary by C-14 dating methods. An error of 40 years in dating a complete sample, or more than 50 years in a 50 per cent sample (Polach, 1974) means, in effect, that it would be impossible to distinguish between sites inhabited within any one man's lifetime.

That any one site had been occupied and then not used again for more years than the likely error in dating cannot be interpreted as evidence for the extermination or disappearance of the earlier group because they and their descendants may simply have moved around the land available to them. If this was sufficiently extensive, or if hurricanes, earthquakes or tidal waves had changed the course of streams or the contours of the land, any one particular site might not be reoccupied for many years, and in this case the archaeological evidence as to numbers of habitation sites might again prove to be misleading. Whether it was true or not, in 1849 Geddie (MR 1851 p.23) at least believed that "the land if cultivated seems capable of sustaining a population many times greater than its present number", and though he complained of "the indolent habits of the natives" he found it "almost incredible how small a spot of soil with a little labour will support a family".
Nor could one expect to find much in the way of middens associated with habitation sites because there was so much fear of sorcery. Early in 1852 when two "celebrated disease makers" became christians, Geddie (J 4 Feb 1852) explained that

When a disease maker wishes to cause sickness he endeavours to procure a portion of the person's hair or some fragment of his food or dress. He then chews up a quantity of sacred leaf, and puts the whole into his charming pot, which he sets on the fire. He then prays to his natmasee to inflict disease on the person whom he wishes to charm. The process is called naragees, and those who practise it are much feared by the people. It is not surprising that the disease makers have much influence when it is believed that the power of life and death is in their hands. This class however is hated as well as feared by the natives at large. It is their custom to go among the natives and beg their food &c, and if their demands are not granted they threaten to inflict disease.

Rev. J. Copeland (HFR 1864 p.16), who was at Aname for more than three years while Inglis was on leave in 1860-3, described the Aneityumese as "tolerably cleanly. Heathen superstitions made them so. They were afraid of the sorcerers getting hold of morsels of food or the skins of fruit, and by certain incantations producing disease. They were therefore careful to gather up all fragments of food, and even their hair."

Whether they were equally careful of their weapons and tools is not known. Perhaps because European hatchets had already displaced stone axes before Geddie's arrival, no cutting tool is listed among what Geddie (MR 1852 p.9) described as "mechanical arts".

In the mechanical arts the natives of this island are far in the rear. Their canoes are logs hollowed out, and are extremely rude. Their spears and clubs, though well adapted for their intended purpose, display but little taste in their manufacture. They excel however in making baskets, cords of various sizes, fishing nets and shell fish hooks.

The "charming pot" of the disease-makers, described in A.W. Murray's (1863 p.89) version as "an earthen jar", is the only reference found to pottery, although once again Patterson (1882 p.123) tried to remedy what he thought was Geddie's omission by adding after the above listing of the mechanical arts of the Aneityumese,"on the northern islands they still make a rude unglazed pottery, standing the fire sufficiently to cook their food in it, and probably they had the same art on the others, previous to their intercourse with Europeans". Whether this was so or not remains to be proven, but
"on the hills which surround the mountains [there was] a kind of stiff, red clay [which] bears a resemblance to burnt brick. ... It is much prized by the natives, and is used by them as a pigment for colouring their faces" (MR 1851 p.23). It was also traded to the neighbouring islands, especially Tanna (J 10 Aug 1852) for the same purpose, but what was received in exchange is not known.

Except for the "principal chiefs" there was no burial of the dead before Christianity was introduced.

As soon as life is extinct, the face is painted to conceal its ghastly appearance, the body wrapped tightly round with a bandage and weights attached to the feet. It is then carried out a short distance from the shore and committed to the deep. A fire is kindled on land opposite to the spot where the body has been sunk; the spirit is then supposed to leave the body, and after warming itself at the fire which has been made, takes its departure to the ... land of darkness, while ... the chief Natmas of Aneiteum, devours the body (MR 1852 p.22).

Even those who died in "the interior" were "slung on a long pole" and brought down "to be thrown into the sea" (MR 1852 p.167).

However,

chiefs of the highest rank were not thrown into the sea. In their case a shallow grave was dug in a house, in which the body was placed, and covered with earth, except the head and face. In this house the people were accustomed to assemble daily "to take care", as they termed it, of their chief till the flesh was all consumed from the head and face, when the skull was removed, and placed on a tree as an object of worship (A.W. Murray, 1863 p.64).

Most of their gods were believed to inhabit stones, "unhewn, and generally of a round or oval shape, with a smooth surface" and though some appeared to have "a small chip broken off as a place of ingress or egress for the spirits", Geddie (MR 1852 p.32) never learned to distinguish between "a common and sacred stone". Whichever they were, they were "numerous" in the sacred groves, but

Idols of wood are less common than those of stone, and I have only seen two since my arrival on the island. They were the large posts which supported the roof of a house built on the feasting ground, in this district where I live. -- There was a girdle of leaves tied around the middle of each post with fine black cinet, and a sash of white native tapa, the ends of which reached the ground.

Each household also had its own sacred stones which were kept in "coarse native baskets suspended from the roof" of the house, and "when the inmates are abroad, these are generally hid in the bush or buried in the ground for safety" (MR 1852 p.8).
As always, there is an insidious insistence on land not being used for cultivation -- there was "bush" in which wars between districts were fought; "bush" to which people fled and hid themselves when enemies or missionaries approached; "bush" in which trees could be cut for houses and other buildings for the mission; "bush" in which household gods and other valuables could be hidden and unwanted infants abandoned, and land that could be cultivated if the natives were less indolent. But it was Mrs Geddie (MR 1856 p.276) who noted that the land at Umej was "much more fertile and more easily worked. Here [i.e. Anelgauhat] the land is generally poor and hard to dig."

Perhaps if she had accompanied her husband earlier on his visits around the island there would be a better record of it and its people, but except for visits to Aname -- which was "not the most central" point in Inglis's district (J 5 Jul 1852) and where neither wife joined in their husband's visits to the outstations (Mrs Geddie, MR 1856 p.276) -- her first excursions seem to have been made in January 1855 (J 8, 9 Jan 1855). Within three months a "neat" cottage, with "about two acres of ground enclosed around it with a neat reed fence", had been built for them at Umej (J 4 Apr 1855) and though she joined in the visits to the various outstations accessible from there, by then such visits entailed little more than travelling to the place, either on foot or horseback or being carried in "a rude palaquin" (Mrs Geddie, MR 1856 p.275), and then "examining the school" before returning either to Umej or Anelgauhat. Perhaps the stone facings of terraces and streams had never seemed unusual to her either.

However, all of these 'walls' would have needed repair from time to time, and those lining the streams perhaps more often than others, especially if there had been hurricanes or earthquakes. At least in the immediate post-contact period hurricanes were frequent, though not all of equal severity. There was one in February 1848 which completely destroyed Paddon's station on Inyeug (Shineberg, pers. comm.), and two "severe" ones in 1849 (J 15 Feb, 16 Mar 1849) but then apparently none until 1854 (J 28 Feb 1854). The next reported was in January 1858 (MR 1858 p.512) and then there were three in 1861, the last -- in March -- being by far the worst (HFR 1861 p.248). By comparison with it, the one in 1862 warranted only a passing mention (HFR 1862 p.293) and there were no
more then until March 1865 (McCullagh, HFR 1865 p.240). There may have been one in January 1868 although there was no direct report from Aneityum, but there were then two very bad ones in quick succession, one in January 1873 (J.D.Murray, HFR 1873 p.184) and another in January 1875 (J.D.Murray, HFR 1875 p.259). In 1878 islands farther north experienced hurricanes, but there was none on Aneityum (Annand, PR 1878 p.237).

Hence, in this 30-year period there were 12 or perhaps 13 hurricanes, two of them exceptionally severe -- those of March 1861 and January 1873 -- as well as "shocks of earthquake" in November 1855 (J 16 Nov 1855) and three earthquakes of increasing severity between January 1873 and May 1875 (J.D. Murray, HFR 1873 p.185, HFR 1875 p.260; Inglis, 1887 pp.183, 194). The first of these, probably late in January 1873, created "fissures on the sides of the hills", but of the next -- in March 1875 -- Inglis wrote:

There had not been such an earthquake, or such a rise of the sea, within the memory of living man. Tradition had to be called in to supply a parallel case. The natives said that their fathers had told them that there was an earthquake in their days which loosened the rocks on the mountains, and sent the stones rolling down into the valleys, and that the sea rose and covered the low lands; but no one now living saw that earthquake -- this was the heaviest and most disastrous that any living man had seen.

However, the second one in May 1875 was even worse. "Not only did the earth quake, but the rocks were rent. In different parts of the island large blocks of overhanging rock were rent off, and precipitated to the valleys below. On other parts where large boulders were lying half buried on the surface, they were upheaved and shaken out of the earth" (Inglis, 1887 pp.194-5). For all its ferocity, there was no tidal wave associated with this earthquake as there had been with the previous one, when the sea was "raised ... ten or eleven feet above its ordinary level at spring tides ... round the whole island" (Inglis, 1887 p.183) and at the main harbour "a great wave [rolled] most impetuously in upon the shore, about 20 feet beyond the ordinary reach of high water" (J.D.Murray, HFR 1875 p.260).

As with the hurricanes, these earthquakes would have had a different impact on different parts of the island. The hurricanes which caused the most damage were of course those in which the 'eye' passed directly over the island, but even on an island as small as
Aneityum, one part usually suffered more damage than others, depending on where the hurricane struck first, whether the wind whipped up the sea to inundate the lands near the shore, or merely uprooted trees and houses in its capricious path. There would always have been torrential rain, but even this deluge would have been variable in its intensity because of the topography of the island, and inevitably some areas would have fared worse than others. In years without hurricanes, storms and heavy rains were reported for most wet seasons within this period, and as there are no records before about 1848 and that for the latter part of the century is incomplete, there is no way of knowing whether the frequency of hurricanes and other disturbances during this time was typical or exceptional.

Certainly a 30-year period is short compared with the probable life-time of the walls. No doubt some could and should be dated sometime by someone possessing the right sort of expertise, and though this will never prove or disprove that they were built originally in response to an environmental situation or as a manifestation of "nothing more than contagious zeal, inspired by the example of a single energetic individual or family" (W.Y.Adams, 1968 p.182), dating might reveal interesting modifications or extensions in the more complex systems that still exist. The interpretation would not be easy, because an extension of an existing terrace or the building of new ones might have been needed for many reasons other than an increase in population, and once built they were likely to remain.

The fallacy of attributing multiplicity or proliferation of settlements to population increase can be illustrated by a simple example. Even in a small community there will be disputes from time to time which will occasionally result in the hiving-off of one segment of the community to live elsewhere. Only rarely will all traces of their former occupancy be removed, and their departure probably would not cause any immediate change in the residence pattern of those remaining. Had such occurred at Ijyphav, for example, the stone walls enclosing the gardens of the dissident group would remain, the gardens might be used by others while their own remained fallow, but there would also be the new gardens, perhaps enclosed in similar stone walls, that had been created at the new site, and all with no more change in the size of the population than there would have been at the parent village had they all remained there.
Problems of the contemporaneity of structures are not unique to either Aneityum or other parts of the Pacific. Parsons (1972 pp.142-3) "spelled out" the weaknesses in this respect in his own study (Parsons, 1971) in the Texcoco Region of Mexico. Willey (1953) was able to avoid it to some extent by using very broad time spans, but one might legitimately question even his cautious interpretation of the numbers of sites in one phase vis-à-vis others at different periods as necessarily indicating larger or smaller populations. Perhaps they did, but to assign anything more precise than relative magnitudes such as "larger" or "smaller" involves assumptions that may not be warranted and some of these will be discussed later.

Given the lack of time control on Aneityum, at best the archaeological record there indicates the minimum amount of land that was available for cultivation by those to whom the land belonged. In the classification proposed by Beardsley et al (1956 pp.135-46) the Aneityumese at the time of continuing contact would be designated as "semi-permanent sedentary", and although there is no direct counterpart for them in Willey's (1960 p.114-5) alternative classification for New World cultures, part of the description for his "semi-permanent" category fits the Aneityumese pattern. The documentary evidence suggests that there was no material change in at least the pattern of settlement within the first decade of the arrival of Europeans, and it might therefore be assumed that what has been described for the population then was essentially a continuation of the last phase of Aneityumese prehistory.

That the area of land available for cultivation necessarily bore any simple or direct relationship to the numbers of adult males and females - and perhaps more especially the females - available for its cultivation will be investigated in later chapters. The salient facts pertaining to the land are that it was qualitatively variable in terms of its fertility and therefore variable in the amount of labour needed to be expended on it; that the amount under cultivation may have changed from year to year depending on whether a feast was being prepared and to whom the harvest was to be presented, and that the crops grown supported pigs as well as humans. The presence of stone 'walls' helps identify and define the area of land that was
cultivated and therefore suited to the crops and techniques available to the Aneityumese before the intrusion of Europeans. That any very great extension of the walls, and therefore of the land brought under cultivation after the middle of the century, was both unlikely and unnecessary will be apparent from the history of population change given in the next chapter.
CHAPTER 3

ANEITYUM'S POPULATION IN THE EARLY HISTORIC PERIOD

As in the previous chapter, the demographic history of Aneityum for the first half century of the mission will be larded with direct quotations from the writings of the missionaries, with some critical, interpretative or analytical comments inserted as they seem necessary, but for simplicity, the main evaluation and discussion of the evidence will be left until after the evidence itself has been presented. Such inferences as appear justifiable about the population prior to 1848 will be introduced as part of that evaluation, rather than in any historical sequence.

When Geddie first settled on Aneityum in July 1848 its population was believed to amount to "about 4000 people" (J 29 Jul 1848). No reason or explanation was given for this number, but the island had already been visited three times by L.M.S. missionaries familiar with Samoa and the Cook Islands, and it is probable that this first recorded estimate of the size of the population stemmed from an analogy with an island of comparable size and topography for which the number of inhabitants was known more or less accurately. For this purpose, Rarotonga seems the most likely; in 1840 two of the three missionaries then stationed there had compiled the first "censuses" of their districts (Buzacott, 1840; Gill, 1841) and one of them -- Rev.A. Buzacott -- visited Aneityum in June 1842, and the other -- Rev.W. Gill -- in 1846 (A.W. Murray, 1863 pp.29,32).

Geddie himself made no attempt to check this number until after Inglis arrived in July 1852, but by December of that year both of them had revised the initial estimate to only 3,000 (J 8 Dec 1852; Inglis, MR 1853 p.150), with no reason given for the change. However, early in 1853 Geddie at least began "taking the census of different villages" (J 1 Mar 1853) and in October 1854 he wrote "We have recently taken the census of Aneiteum. As the population is much scattered it can only be viewed as an approximation to the truth. We shall be able to get something more perfect at a future date. We have on our lists about 3,800 names, but Mr Inglis and I are of opinion that the population is about 4,000" (MR 1855 p.125). Inglis himself vacillated between this "opinion" (e.g. MR 1858 p.419; MR 1868 p.17)
and "about 3,500" which he claimed resulted from "repeatedly taking the census" (MR 1859, p.151) or alternatively "the first census" (1887 p.50).

Clearly the 'lists' were done piecemeal, and most probably by the missionaries themselves though the teachers (and perhaps others) may have contributed information about those villages which had not yet been visited by either missionary. At this time (and later) the teachers were selected as much for "character" as for "scholarship", and Geddie (MR 1856 p.180) acknowledged that "it often happens that the best men are not the best scholars". Inglis (1887 p.78) confirmed that "at first our teachers were often persons of very slender attainment in scholarship; ... we chose, as far as possible, men of character and influence, to whom the people would look up". Their educational duties -- i.e. teaching the people to read -- were subsidiary to their evangelical responsibilities, and though there were only 26 "schools" in May 1854 (MR 1854 p.181) the teachers may well have obtained information about nearby communities from curious visitors or personal visits to those communities which showed some interest in the new religion.

However, this may not have been necessary because Geddie and Inglis had spent five days in April 1854 visiting the settlements near the coast, and though this was where "the principal part" of the population lived (Inglis, 1887 p.23), they planned jointly to visit the inland settlements "at some future time" (J 14 Apr 1854) to "search out every family in the mountains and valleys of Aneiteum" (MR 1854 p.182). Even on or near the coast there were still some "strongholds" of heathenism in 1854; Anau-unse consistently rejected teachers until 30 October 1854, Imkalau had none until January 1855, and Ijyphav later still. The exact date for the latter is not known because it was situated in Inglis's district, but it may have been before August 1856 when Geddie (MR 1857 p.98) wrote: "In the district occupied by Mr Inglis the heathen are now reduced to 200 and they number about 50 in my own. They are scattered throughout the island without combination or influence".

Unfortunately none of these nominal lists has survived, and in all of the literature there are only three places for which the number of people in each is stated, and once the combined population of three hamlets. According to Patterson (1882 p.347) Anauyac had 50 inhabitants when the school there was first started in February
1853, and though he was ostensibly quoting from Geddie's journal, no number is given in the MS copy available in Australia. The second place was Anumej, which had a population of "nearly 300 souls" (MR 1855 p.124) and when Geddie first visited it in January 1854 he described it as "the largest village on the island" (J 23 Jan 1854) but in October of that year this was modified to "our largest inland settlement" (MR 1855 p.124). Perhaps it was both. The third place was an unnamed but "important inland district" on the northern half of the island which contained "upwards of 80 [people], it is said" (Inglis, MR 1855 p.183) in October 1854; and at about this time the combined population of the three "districts" in the vicinity of Inglis's station -- Epeke, Aname and Isav -- was 120 (Inglis, 1887 p.75).

Whichever of the two numbers, 3500 or 4000, might have been the more correct for the size of the population in 1854, the number of Aneityumese away from the island then was probably smaller than it had been for many years. Paddon's sandalwood and trading station had been abandoned nearly two years previously, and with fewer vessels calling at Aneityum there were fewer opportunities for the young men to leave the island. However, the mission had already begun its practice of sending Aneityumese teachers to other islands in the group, and the first two, with their wives, had been stationed on Futuna in October 1853. Two more, also accompanied by their wives, were sent to Tanna in October 1854 (MR 1855 p.86) but it was only later that any significant number were sent "for longer or shorter periods ... to assist the missionaries, or prepare the way for them" (Inglis, 1887 p.86).

During his first year on the island Geddie commented on sickness and deaths only amongst the foreigners (J 1 Mar, 1 Apr, 9 Aug 1849), but 1850 opened with "another unhealthy season yet not so bad as the last year" (J 28 Jan 1850) and by 1 March there was "much sickness among the natives and white men".

A few of the natives place much confidence in our methods of treating disease: the great majority however are still fettered to their superstitions. All sickness is supposed by them to be caused by evil spirits who possess the sick person. The patient is removed from the house in which he lives to the open air, and his only shelter is a few cocoanut leaves spread over a rude piece of framework. ... No medicine is used internally, but a priest is sent for, who by uttering some sacred words, and going through some
wild ceremonies expels the evil spirits or tries to do so. ... Cold water is also poured on the naked body and the trembling which often follows this operation is supposed to be caused by the efforts of the spirits to make their escape.

Early in 1851 Geddie stopped 'itinerating' and rarely went far from Anelgauhat, so it may have been only in its immediate vicinity that "most of the deaths" that he knew of "for several months past" were "among the christian party" (J 29 Mar 1851). In July and August there was "much sickness" from "an epidemic not unlike influenza" (J 10 Jul 1851), and "many deaths ... occurred among the heathen people", their number "sadly multiplied by the horrid practice of strangling. Several poor women have, within the last few weeks, fallen a sacrifice to this revolting and barbarous custom. In one case, three women were strangled on the occasion of one man's death" (MR 1852 p.167). The Christians who were affected in the early part of the epidemic recovered (J 10 Jul 1851).

A year later "a species of influenza" caused another epidemic which lasted for more than six weeks (MR 1853 p.66). Natives all over the island "suffered severely from it" and there were "several" deaths (J 27 Jul 1852). This epidemic probably started about the third week of July and it is therefore unlikely that Bishop Selwyn's Border Maid which arrived from New Zealand on 1 July, with Inglis and his wife on board, was responsible for it. Inglis himself succumbed to the infection, but its spread throughout the population may have been facilitated by the building of his house at Aname in which more than 100 people from different villages were engaged, and also by a feast given "the inland people" by those living near Anelgauhat (J 10, 14 Jul 1852). Whatever its source, it is unlikely that the mortality in this epidemic was high.

Later that year the sandalwood station was abandoned, and in 1853 Aneityum was bypassed by most commercial shipping. One vessel that called in April, probably unaware that Paddon's station had been moved was the barque Edward from California. Two natives went on board to pilot her into the harbour, but the wind was against her and after several unsuccessful attempts to get in she sailed for Tanna. Six weeks later the only surviving teacher of the four who had been on Tanna brought the two Aneityumese pilots back and told what had happened. There had been smallpox on board the Edward, but
The teachers and natives there were allowed to go on board without ever being warned of their danger. The passengers also landed daily and went to the house of the teachers, and partook of their hospitality. In due time the smallpox broke out on the island. Three Rarotongan teachers died of it and the wife of one died also. The Samoan teacher and his wife who lived in another place escaped. ... [The two Aneityumese] had attended the teachers during their sickness and helped to bury them.

Geddie quarantined the Samoan teacher and the two Aneityumese on Inyeug to prevent an epidemic on Aneityum, and though the Samoan returned to Tanna his stay there was short because his life was threatened (J 14 Apr, 26 May, 11 Sep 1853).

In September Towns sent a vessel from Sydney to begin another sandalwood station on Aneityum (J 13 Sep 1853; Shineberg, 1967 p.251), and though no illness was associated with either this or Herald's visit in November, by late December there was again "much sickness" from influenza and "several" deaths (J 28 Dec 1853). In June of the following year Geddie himself was a victim of an epidemic then "sweeping over" the islands; it was "neither influenza nor pleurisy, but a combination of these troubles" (J 23, 27 Jun 1854), but there is no suggestion that this increased the mortality on Aneityum any more than the previous outbreaks of respiratory diseases had done.

Before his illness Geddie had complained of "much langour of body and much spiritual deadness" (J 4 Feb 1854), but nevertheless he continued to visit and 'examine' his schools and presumably he also continued his listing of the people in various places which he had started a year or more previously (J 1 Mar 1853). In the same letter in which he reported and commented on the 'census', he explained

The great barrier to the introduction of Christianity in these islands has been a general impression on the part of the natives, that it brings disease and death along with it. ... But Aneityum has now to a large extent embraced Christianity, and what has been the consequence? The population, so far from diminishing, has been on the increase to an extent that makes the natives wonder. The abolition of war, strangling of widows, and infanticide, has saved many lives. The use of medicine by the sick, instead of the charms of the sacred men, has greatly lessened the mortality on the island (MR 1855 pp.135-6).
The allegedly 'lessened' mortality was not to last, but in February 1853 Mrs Geddie (MR 1853 pp. 167-8) believed that the natives were much more healthy than formerly, owing to their getting only their proper nourishment. Formerly, they were from their birth, fed with fish and all kinds of trash. The mothers too, take better care of their children, keep them cleaner, and do not expose them so much to the cold, rain, and night air. When we came here, there were very few infants, but now there are a great number. Because she had diagnosed teething trouble in many of the children brought to her for treatment, "now they imagine all their troubles are caused by teething, and often come to get their gums cut"; but on a more serious note, "intermittent fever ... is very prevalent here in the rainy season, especially among children. ... Natives who live at a distance, when their children get ill, come and remain sometimes for a fortnight with us, that they may have the benefit of our skill", about which she at least had few illusions.

Mrs Inglis (MR 1855 pp.183-4) also acknowledged that the Aneityumese were "very fond of their children when they are grown up a little", but she was more outspoken about the infanticide that had prevailed to a much greater extent on this island than we had any thought of. The mothers did not murder their children in the barbarous way, which the mothers in Tahiti and in some of the eastern islands were in the habit of doing. They left them, especially the female children, in the bush to die, or within the tide-mark to be drowned, or they nursed them carelessly and they died.

The result was that the first census showed, as Geddie had already discovered (J 1 Mar 1853) a "great disproportion between the male and female population, and [a] comparatively small number of children".

Reverting briefly to Geddie's comment above, one might question that the "heathen" practices were suppressed entirely either then or later. Only 2,200 of the people on the lists were christian (MR 1855 p.125), which left nearly half of the presumed population beyond the immediate influence of the mission. At least one war had been stopped by the intervention of the missionaries and the chiefs who had become christians (J 15 Jan 1853), but few were ever killed in wars. The strangling of women on the death of their husbands had always been hidden from the missionaries once their disapproval was
known, and among the older women there were some who preferred strangulation (J 15 Jan 1850) to living on in a society in which they had no place, especially if the husband had been an important man and on his death his house was burned and his gardens destroyed (J 20 Mar 1849; J 3 Dec 1851). Except in the vicinity of the various mission premises infanticide too would not be easily detected, especially the careless nursing of unwanted children. Nor would the presence of the missionaries have stopped accidents such as the one in which four young men from Anauyac were drowned (J 30 Aug 1853), or another in which four of the six who had sailed in a canoe to Tanna were killed when they were forced ashore by bad weather before they reached their destination (J 1 Oct 1852).

All in all, one must be somewhat sceptical about the magnitude of the increase in population likely in little more than two years. Perhaps there were more infants and very young children than before, some of them born to the widows who had not been strangled and may have married again, but if the birth rate in the population was about 35 per 1,000, the maximum increase in numbers over two years would have been of the order of 250 and this only if no one died during the period. This number must then be spread throughout the individual communities, and if there were 56 of these, the increase in the size of an 'average' village is hardly a number likely to excite wonder. On the other hand, the addition of the 50 or so people who accompanied Geddie on his various visits to the usual population of almost any settlement, village or hamlet would create the illusion of large numbers everywhere, especially to people whose traditional system of counting stopped at 20 -- two hands and two feet -- and numbers beyond 20 were just "many, many" (Inglis, 1887 pp.83-4).

Of the 3,800 people listed before October 1854, about 2,200 were males and consequently "no less than 600 men [were] doomed to a life of hopeless celibacy". Geddie and Inglis thereupon decided to use their influence "in promoting judicious marriages among the natives", marriages where there was no "unreasonable" difference between the ages of husbands and wives, and no "absence of affection". Formerly the young women had been monopolised by "the most influential ... men advanced in years" and "as affection had nothing to do with such alliances women often forsook one husband for another" (MR 1855 p.125).
The effect that such a capricious pattern of marriage might have had on either the birth rate or the death rate (given the strangulation of wives to accompany their husbands to the world of the dead) cannot be even conjectured.

Nor can one conjecture on the extent to which infanticide prevailed in practice, though the predominance of males in the population as a whole, with four males to only three females, was undoubtedly due more to the discrimination against females at birth than to the strangulation of widows. The comparative scarcity of children in the total population recorded might, however, have been attributable in part to factors other than infanticide, not the least of which perhaps were the missionaries' own standards of what the appropriate ratio between adults and children should be. Whatever this was, it would probably have stemmed from the experience of members of their own family circles, and perhaps of their congregations at home, where large families were the rule rather than the exception in the mid-19th century.

More than a year elapsed before the more statistically-minded Inglis began collecting the numbers of births and deaths in his district, and during that time the Aneityumese suffered much from "intermittent fever and influenza, and several deaths ... occurred" (MR 1856 p.182). By August 1856 the mortality for the past year had been "greater than during any similar period" since July 1848. "The disease of which many have died is *nemehe an pege*, i.e. the sickness of the land. It prevails at certain seasons of the year, and many of the natives fall victim to it" (MR 1857 p.97). There was also "much sickness among the natives" the following year; "few" persons escaped the "severe epidemic" which "swept over the island", and "many" died so that, by September 1857, the mortality for the year had again been greater than "at any former period" since the Geddies landed. The breadfruit crop failed that year too, and the mortality was higher among the people who relied on breadfruit for their subsistence than in the areas where taro was the staple food (MR 1858 pp.273, 321).
"But to compensate for this [high mortality] the births have also been numerous" (MR 1858 p.273). Inglis (MR 1858 p.419; 1859 pp.115-6) provided some figures. For the period 1 January 1856 to 23 July 1857 he reported 106 births and 65 deaths on the northern half of the island, and 78 births and 91 deaths for the year 1857. As the two periods overlap the numbers cannot be added, but if those for the first period are converted to an annual basis for a population of 1,900, they represent a birth rate of 35 per 1,000 and a death rate of less than 22. For 1857 the birth rate was even higher at more than 41 per 1,000, and the death rate also higher at 48 per 1,000. By then the population was classified by sex --1,075 males and 807 females -- and the death rate for the male segment exceeded that for females -- 53 cf. 42 per 1,000.

Birth rates of 35 or 40 per 1,000 are high for a population with such a predominance of males, and if they had been at this level each year since Geddie arrived and the death rates no higher than those reported for 1856 and part of 1857 by Inglis, the population would have decreased only if more than one-third of the children born had been abandoned in the bush or on the beach or nursed carelessly. If the level of infanticide was less than this, the increase in population in most years would probably have been enough to offset the losses in the occasional 'bad' year when mortality was higher than usual.

Both birth and death rates may, in fact, have been higher than the numbers given above because sometime in 1858 the mission's estimate of population was amended to 3,500, this being the number obtained "after repeatedly taking the census" (Inglis, MR 1859 p.151). However, this change in the population at risk would not materially alter the conclusion that if the birth rate was in the vicinity of 40 or 45 per 1,000 and the death rate in most years was about 25 per 1,000, the population could safely tolerate infanticide of at least a third without diminishing its size.

Except for the numbers of schools and Aneityumese teachers (and their wives) absent on other islands, there are no more statistics for a few years. To some extent this expansion of the mission to other islands diverted attention from Aneityum. In June 1857 the mission had acquired its own vessel, the John Knox; Rev. G.N. Gordon and his
wife had arrived from Nova Scotia and settled on Erromanga, and Aneityum was once again a commercial centre with two sandalwood stations and many ships calling both for sandalwood and whaling. Discharged seamen and beachcombers plagued the mission, though "the few persons connected with the sandal-wood establishment" on shore behaved reasonably respectfully towards it (MR 1858 pp.35-6, 274-5). In August and September 1858 another three missionaries arrived, two with their wives, Rev. J.W. Matheson from Nova Scotia, and Revs. J.G. Paton and J. Copeland from Scotland (MR 1859 pp.4, 36).

In January 1858 there was a "severe" hurricane and the "almost incessant" rain throughout January, February and March damaged native crops and plantations (MR 1858 p.512), but neither missionary commented on mortality that year, so the shortage of food following the rain may not have increased the death rate abnormally. In September there were "about fifty" schools which were attended "with few exceptions ... [by] old and young ... in every village on the island". There were also a "Normal Institution, under Mr Inglis' charge" and "a select school taught every day ... entirely under Mrs Geddie's Superintendence", the first of which had "upwards of one hundred pupils of both sexes" and the second "about seventy scholars, chiefly ... young men and women" (MR 1859 p.35).

Late in 1858 Geddie began building another church at Anelgauhat, larger than any of the previous ones (101 feet long by 41 feet wide) and from stone quarried nearby. Its building took "about 18 months" and some of the stones were "so large that it required 60 men to remove them to their destination. ... The walls are plaistered outside as well as inside, and are protected from the weather by a verandah which surrounds the building. The floor is plaistered and doubly covered with matting". Except for "the King's church at Honolulu", Geddie had seen none to equal it in any of the islands he had visited. The day after it was opened, "the natives, according to their usual custom, made a large collection of food which was cooked, and distributed among the people of the different villages" (HFR 1861 p.40).
The building of the church and the translation of the Bible occupied much of Geddie's time during 1859, but he continued to supervise the schools in his district, and because there were "comparatively few" coconuts on the island, he distributed 1,000 nuts each year so that they would be "planted extensively over the island" (MR 1859 p.36). In October Turner (1861 p.476) revisited Aneityum and was delighted to find it "encircled by fifty-six school-houses, eleven chapels, and sixty native teachers and assistants". Inglis went to Scotland on leave shortly afterwards and Copeland took charge at Aname (HFR 1861 p.72). By then "about 500 persons" congregated at each of the "principal stations" each Sunday, and the teachers, assisted by elders and deacons, conducted "prayer meetings" at the "more distant outstations" (HFR 1861 p.129).

However, some of the people were becoming "careless" and though nominally Christian, the young men especially were being attracted to the sandalwood stations. In January 1861 a schooner attached to one of the stations returned to Aneityum with some of her crew infected with measles and dysentery, and the disease soon spread over the whole island notwithstanding the efforts made to check its progress. The population at large were laid prostrate, and I do not believe there are half a dozen of persons on the island, who did not take the sickness. ... Many died of the disease itself, and many also from inattention and want of the common necessaries of life, as there were few able to help their neighbours. About one-third of the population were in the short space of three or four months swept into the grave (HFR 1862 p.38).

There were also three hurricanes during this period, the worst of which hit the island in mid-March.

Few of the present generation have witnessed so severe a storm. The centre of the hurricane passed directly over the island. ... The amount of damage done ... was immense, and it will be years before the island recovers from the effects of it. Trees were torn up by the roots, houses blown down in every direction, and a great quantity of food destroyed. Food will be very scarce this year, and it is only the great mortality from disease that is likely to save the island from the horrors of famine (HFR 1861 p.248).

The damage was greatest on the northern side of the island where the sea rose either nine (Inglis, HFR 1873 p.186) or seven feet above high water mark at Aname, and "upwards of 30 feet" above the level of spring tides at "a jutting headland" three miles to the west. "The
sea was greatly more [destructive] than the wind ... [and] it made a clean sweep over the whole side of the island", destroying houses and gardens and nearly all of the school-houses, and damaging both the church and mission house (Inglis, HFR 1861 p.327). At Anelgauhat, both the old church and the roof of the new one had been burned a week before the hurricane struck, but the stone walls of the church withstood both fire and storm and only a few of the school-houses in Geddie's district were harmed (HFR 1861 pp.247-8).

It is impossible to discover from the various accounts precisely where the epidemic started, or at what stage efforts were made to check its progress throughout the population, or what these might have been. With two sandalwood stations on the island -- one at Inyeug (HFR 1861 p.247) and one at Ehesjei, the owner of which also owned the schooner held responsible for introducing the disease (Shineberg, 1967 p.245) -- there might have been more or less simultaneous outbreaks both in the vicinity of the main harbour and Anau-unse. But though "the sickness broke up all [the] schools", at least Mrs Geddie's "afternoon school" (the "select school" referred to above, with about 70 pupils) continued until sometime in February, and possibly even late in February (Mrs Geddie, HFR 1862 p.76).

If the infection was introduced "about the beginning of the year", at least five or six weeks (perhaps more) elapsed before the normal routine of the mission was changed and the incubation period for measles is 12 to 14 days (Burnet, 1953 p.18). In this time the people (with few exceptions) were meeting for an hour each weekday morning for school, about one-third of the population congregated in the two main churches every Sunday, and elders and deacons visited the more distant outstations to assist with services there. There were also "weekday prayer meetings" at the main stations which all the teachers attended, and sewing classes there on most days for the women. In June 1860 Geddie had divided his district into seven parts and assigned each to a "ruling elder", who reported each month on the conduct of "church members and others under their care" (HFR 1861 p.40); and if Copeland followed Inglis's (1887 p.68) practices faithfully, there was as much (if not more) movement regularly between Aname and the outstations on the northern side of the island.
By the end of March 1861 "several hundreds of the natives" were dead, including 7 of the 21 young people who had been living on the mission premises at Anelgauhat "at the time when the disease broke out". Although "about the same proportion" of those who lived around the harbour had died, elsewhere the mortality had been "in some instances less, and others more", but everywhere it was "greatest among persons in the prime of life, while many of the old and young have been spared" (HFR 1861 p.247). The epidemic petered out sometime after 1 May (Copeland, HFR 1861 p.272) and before 26 June when Geddie (HFR 1861 p.298) reorganized his schools because several of his teachers had died.

When "the statistics" were "made up" in April or May the following year, the death toll ascribed to measles and dysentery was "about 1200" (HFR 1862 p.292), but this number may have included the deaths of some who survived the epidemic "in an enfeebled state" so that "a proportion larger than usual sink under even the ordinary diseases of the island" (HFR 1862 p.159). The size of the population then was not recorded, but "we may assume", wrote Inglis (1887 p.50), that the population "continued stationary" between "the first census" and the beginning of 1861, so that the total death rate was perhaps more than one-third, perhaps less if Inglis's assumption was wrong.

Except for the comments in Geddie's initial account of the epidemic that the mortality was variable both geographically and with respect to age, there is little more direct information about the pattern of mortality. In October 1860 Geddie had reported that there were 179 church members in his district, some of whom were teachers on other islands; and there were then 17 teachers and their wives absent from Aneityum (HFR 1861 pp.129-30). Probably more than half of them were from Geddie's district because his senior school had been operating longer than had Inglis's -- formally opened in April 1857 (MR 1858 p.419) -- and assuming that only males were eligible for church membership, there would have been 170 church members at the most on the island when the epidemic began. Of these, 68 died -- 2 elders, 8 teachers and 58 church members (HFR 1862 p.38) -- and to Geddie (then 46 years old and his eighth child just born) these were probably "persons in the prime of life", amongst whom the death rate had been especially high.
For the "young" there is only the sample of 21 males and females who were living on the mission premises "at the time when the disease broke out", and seven of them died (HFR 1861 p.247). However, these may not have been representative; the girls at least had to be old enough to help with the domestic chores of the mission house and family, and the majority of both sexes able to take charge of classes in the morning schools while attending Mrs Geddie's afternoon school for aspiring teachers and wives of teachers. There were usually more boys than girls in the boarding school because the girls left to be married sooner than the boys were taught sufficiently to become teachers, so that the range of ages for the "young" males on the mission premises was probably wider than for the females and one might guess that few of either sex were less than 10 or 12 years old.

The first school for young children was started after the epidemic when Mrs Johnston, the widow of a missionary from Nova Scotia who had died on Tanna in January 1861, was staying with the Geddies. There were 50 pupils aged less than 12 in her school, and of these, only 6 had both parents alive, 26 had one surviving parent and 18 were orphaned. "This may be regarded as a fair representation of the state of things throughout the island" (HFR 1862 p.292). If all 50 children were unrelated and both parents of each child were alive before the epidemic, the mortality among the parents was 62 per cent; if some parents were counted twice or three times because they had 2 or 3 children under 12 years of age, the mortality rate among them may have been less than this, but it would still have been higher than the rate of at least 40 per cent derived from the deaths among the church members.

No doubt this sample is biased in favour of orphans, but one cannot rule out the possibility that for some reason the chances of survival for females may have been smaller than those for males. Everyone was susceptible to dysentery, and almost certainly this was their first exposure to measles, and the disease was introduced in circumstances which were particularly favourable for the spread of infection throughout the population without discrimination of sex. Whether the lack of discrimination extended to the likelihood of surviving the illness is less certain, but Inglis (1887 p.342) -- who was away from the island from early 1860 until July 1863 -- later asserted that in this and later epidemics "the female portion of the
population suffered more severely, in proportion, than the male'.

If one assumes (perhaps wrongly) that the mortality risks for males and females were independent, a distribution such as the above for 50 unrelated children whose fathers had a 60 per cent chance of surviving would require the deaths of 80 per cent of their mothers. If the number of orphans were exaggerated, and children with both parents alive under-represented in the sample, 26 children in a group of 50 would have had one parent alive if the death rate amongst fathers had been 40 per cent and 60 per cent of the mothers had died.

Death rates of this magnitude among mature adults imply much smaller mortality risks for children and old people if only one-third of the population died, and why the old people should have had better chances of survival than their mature offspring is a puzzle. If it was true that fewer than "half a dozen of persons on the island" escaped the sickness (HFR 1862 p.38), the old had no more immunity than anyone else to measles. Perhaps they were more inured to scarcity of food, but many of the younger members of the population would have experienced periods of scarcity both before and after they became christians, either while preparing for feasts or from the ruin of crops by storms and rain. The ease with which the young recovered is more understandable, and estimates of the death rates likely for them will be considered later when the effect of selective mortality such as this is on the age and sex structure of the population is examined in more detail.

The continuing scarcity of food on the island is attested by the abrogation of the "usual custom" of feasting to mark the re-opening of the church at Anelgauhat in November 1861. "As soon as the natives began to recover" they had started rebuilding it, under Geddie's direction, remedying some of its "architectural defects" by raising the walls "a little" and arching the central window in each wall (HFR 1862 p.40), and at the end of August Mrs Geddie's afternoon school was still in abeyance because all the young men were "busy at the church" (HFR 1862 p.76). "There could not have been less than 1200 persons present" at its opening, as large a gathering as Geddie (HFR 1862 p.160) had ever seen on the island "notwithstanding the reduced number of the population". The repairs to the church at Aname were probably less arduous and less time-consuming, but all of the school-houses on the northern side had been destroyed in the
hurricane, and their rebuilding carried over into 1862 (Copeland, HFR 1863 p.70).

Writing from Scotland in October 1861, Inglis (HFR 1861 pp.329-30) predicted that "the sickness and mortality [would] be greatly less for some years to come" because only "the strong and healthy" would have survived the epidemic and the aftermath of the hurricane. Somewhat to Geddie's dismay (HFR 1863 p.125), the mortality on the island during 1861-2 was "unusually great, and the deaths ... more numerous than the births", especially on the northern side of the island. In the southern part the numbers were equal, but there had been no births at all in the northern district for "three months in the end of 1861" and in the year ended June 1862 there were only two-thirds as many births as deaths (Copeland, HFR 1863 p.70).

Given the high mortality from measles and dysentery among people of reproductive age, the number of marriages which would have survived the epidemic would be relatively small, whether there was a sex differential in mortality or not. If 40 per cent of males and 60 per cent of females of appropriate ages died, only about \( \frac{1}{4} \) of all matings survived; if the death rate among females had been higher than 60 per cent, perhaps fewer than \( \frac{1}{6} \) of marriages in existence before the epidemic survived. In the year following the epidemic, the number of births likely to occur would be reduced commensurately with these proportions rather than with the survival rate for the population or even that for females of reproductive age, whereas the number of deaths expected would be more directly proportional to the size of the population.

Unfortunately neither missionary reported any numbers, but if the mortality consequent upon the epidemic and hurricane was greater on the north than the south, relatively more marriages would have been disrupted there, and though there was much re-marriage on the northern side between June 1861 and April 1862 -- about 60 couples, and only two of the brides had not previously been married (Copeland, HFR 1863 p.10) -- few births could be expected from these unions within the ensuing nine months. And as the births that might have occurred during the last quarter of the year would have had to be conceived in the first three months when the epidemic was at its height, the absence or rarity of births late in 1861 should have surprised no one.
How much the low birth rates of these post-epidemic years might have contributed to an impression of excessive mortality cannot be judged, but certainly the fall in mortality predicted by Inglis seems not to have happened. Food continued scarce after another hurricane in 1862, "less severe" than the previous one but "very destructive" because the rain fell in such torrents that the rivers were swollen and many of the plantations covered with water and destroyed. The bread fruit, cocoa nut and other trees which were beginning to recover from the effects of last year's hurricane were again blighted, so that for two years in succession there has been scarcely anything in the shape of fruit. The consequence is a great scarcity of provisions, though the distress has not been so great here as on some of the neighbouring islands (HFR 1862 p.293).

By June 1863 food was becoming plentiful again, and collections of "taro, bananas, cocoanuts, sugar-cane and fowls or fish" were again being given the mission party when Geddie made his then twice-yearly visits to his schools. There were still "between 50 and 60 schools on the whole island, none of which are large, and others very small", still being attended by "persons of both sexes, and of every age from 70 years downwards" (HFR 1863 p.314). At about the same time Copeland (HFR 1864 p.14) was encouraging the people on the north to grow cotton, each "on his own plot of ground -- on his own paternal soil".

Inglis returned in July 1863 (HFR 1864 p.22), but Copeland (who married Mrs Johnston the following September) remained on Anéityum to replace Geddie while he took his first home leave after more than 15 years. In September Geddie reported "a severe epidemic" on the island earlier in 1863, and by December "a large number of the natives" had died, most of them "persons who had not fully recovered from the effects of the measles". Because "the statistics" showed that "the greatest mortality prevails on the low lands near the shore, whereas the hills in the interior are comparatively healthy", the missionaries tried to persuade the people to move to the "high lands, but they seem much averse to leave the spots where their ancestors have from time immemorial lived" (HFR 1864 pp.41,155).
From this and the number of schools it would seem that the traditional pattern of settlement continued despite the mission's influence and the traumatic effects of the epidemic.

Geddie left Aneityum in January 1864 and three new missionaries arrived from Nova Scotia soon afterwards in the new mission vessel *Dayspring*. Copeland then became the first missionary to Futuna and Rev. W. McCullagh replaced him in Geddie's district. There was again "much sickness ... throughout the island" late in 1864, and Geddie was warned he would miss "many old familiar faces" on his return (McCullagh, HFR 1865 pp.240, 296). Throughout 1865 there were only 20 births (11 males, 9 females) and 24 deaths on the southern half of the island (McCullagh, HFR 1866 pp.150, 152) and Inglis (1887 p.113) then reckoned "the population, by measles and other causes, has been reduced to about 2100". If this was still evenly divided between the two mission districts, the death rate for Geddie's was not very different from the rate on the northern side in 1856-7 but the birth rate was much lower than it had been then.

About the middle of May 1866 -- two weeks after the *Dayspring* returned from visiting the Loyalty Islands -- "all the symptoms of diphtheria" were noted for "eight natives, some young and some in the prime of life" who died within a week. These were the first victims of the outbreak which spread throughout the population, and "as many as thirty-two" died in one week on the northern side of the island. By August the epidemic was nearly over, but "many of the strongest and healthiest natives have been removed, while those more advanced in years have been spared". The first estimate of mortality was "at least 200 of the population" (McCullagh, HFR 1866 pp.292, 323), but late in November when the "sickness" had disappeared Geddie (who had returned on 8 September) reported that "the number who died was probably not less than three hundred" (HFR 1867 p.126). Inglis (HFR 1868 p.16) commented that the epidemic "was confined almost entirely to the strong and vigorous; children entirely escaped; any of them that died during its continuance, died from other causes".
On his return to Aneityum Geddie (HFR 1867 p.126) was struck by "the large number of children on the island. They are far more numerous than at any former period during our residence here". Geddie had been away for more than two and a half years (January 1864 to September 1866), and in one of these years only 20 children had been born in his district. According to Inglis (HFR 1868 p.17) the population had "just about recovered from the measles, and the births were equal to the deaths, when the epidemic of [diphtheria] came upon us", so there is no reason to expect that many more than 20 children had been born in any of the years between 1861 and 1865, and not all of them would have survived. Hence, a very optimistic estimate for the number of children aged less than 5 years in Geddie's district in 1866 would be perhaps 100.

Those aged from 5 years upwards would have been the survivors of the children born in the years preceding the 1861 epidemic when, with a birth rate of about 40 per 1,000 population, there might have been approximately 70 children born in Geddie's district in a year. In 1861 the mortality among children had been low compared with that for adults, but by 1866 some of these 'children' would no longer have been classed as such. This group, however, might well have been small compared with the numbers younger, especially the numbers born after the majority of the people accepted Christianity and deliberate infanticide was reduced if not eliminated, and apparently few (if any) of these died from diphtheria.

However, this begs the question of why this particular group should have been more evident to Geddie in 1866 than they had been in 1863. Was it just that they were three years older, and therefore all of them were more mobile and obvious? Did they just appear more numerous than before by comparison with the smaller number of adults (especially, perhaps females) in the various communities after the two major epidemics? Or was it just wishful thinking on Geddie's part because this was the best, if not the last hope for the mission he had founded? "The population of this island", he wrote, "has come down from 4,000 to 2,000 within the last twenty years. The chief causes of depopulation have been measles and diphtheria of late years. If these islanders could be preserved from foreign diseases, which are always fatal to them, we
might then hope for the best" (HFR 1867 p.126).

But they were not 'preserved' and the 'numerous' children were the principal victims of the next outbreak of introduced disease. About the beginning of June 1867 whooping cough "appeared" and its source was traced either to a trading vessel from Sydney via Mare or perhaps to one from Fiji. It made "something like a clean sweep of the young children" as well as "severely" affecting "half-grown boys and girls, and weakly adults of all ages" (Inglis, HFR 1868 p.16). It was "less fatal than some other foreign diseases, nevertheless a large number" died and the Aneityumese were "much dispirited". Once again the whole population was affected, but the only figure given for the number of deaths was "considerably more than a hundred" (HFR 1868 pp. 150, 182).

Probably one should not take either missionary too literally where numbers are concerned, but both Geddie (HFR 1868 p.150) and Inglis (HFR 1868 p.17) claimed that the population was halved by these three epidemics, measles and dysentery in 1861, diphtheria in 1866 and whooping cough in 1867. However, there is a discrepancy of 100 or more between 1,750 and the sum of the reported deaths and it is therefore likely that there were more deaths than births in most (if not all) years between 1861 and 1867. Confirmation is given in Geddie's (HFR 1870 p.13) annual report for 1868 in which he wrote: "This is the first time since the commencement of the mission that I can report a positive increase of population. The whole number of births on the island has been 60, and the whole number of deaths 50; leaving a balance of 10 on the right side". For a population then estimated to be 1,750 (but probably less - see page 81) these represent rates of 34 births per 1,000 population and a death rate of 29 per 1,000, neither of which is very different from the rates reported eleven years earlier.

By the end of 1868 there were nine missionaries in the New Hebrides Mission, and the islands north of Efate had been visited with a view to further expansion northwards (HFR 1869 pp.316-21). Already in 1867 the mission was aware that natives from the islands were being kidnapped and enticed away for work on European plantations in New Caledonia, Queensland and Fiji (Paton & Copeland, HFR 1868 p.154), and as this labour traffic increased so did the missionaries' concern. By comparison with the events elsewhere, the state of the
mission on Aneityum warranted little comment.

Moreover, Geddie's health was failing and Mrs Geddie's was worse, and between visits in the Dayspring to other islands and to Australia to arrange for and oversee the printing of the Old Testament in Aneityumese, Geddie spent less and less time each year at his 'station' (HFR 1870 pp.179-85, 207-8; 1871 pp.120-3; 1872 pp.13-8). When the replacement he had requested arrived in May 1872 the mission premises were "neglected and dilapidated" (J.D. Murray, HFR 1873 p.339). Mrs Inglis had not been well either, and she and her husband made at least two visits to "the Colonies" between 1868 and 1872 (HFR 1870 p.211; Geddie, HFR 1872 p.130). While the newly-formed Synod of the New Hebrides Mission was meeting at Anelgauhat in June 1872, Geddie had a stroke and was taken back to Geelong in the Dayspring where he died in December (Ed. HFR 1873 pp.13, 41, 47; Mrs Geddie, HFR 1873 p.85).

Geddie's successor at Anelgauhat -- Rev. J.D. Murray (HFR 1873 pp.184-5) -- no sooner had the mission buildings in "tolerably good repair" than they "were reduced in a single night to a worse state of dilapidation than ever" by a hurricane. Inglis (HFR 1873 pp.185-6) described it as "one of the most terrific hurricanes that ever passed over this island -- at least, since any white man lived on it", the only one to either equal or approach it being that of March 1861, when Copeland was relieving him at Aname. "The destruction of food and houses was greater on that occasion than this, owing to the sea rising so much, and inundating all the low lands ... But both the natives and white men residing then on the island, say that the wind was considerably stronger on this occasion". The Dayspring, her usual routine upset by the extra voyage to Australia with Geddie, was in the harbour at Anelgauhat when the hurricane struck on 6 January and she was wrecked on the reef.

Inglis estimated that "full three months' provisions" were destroyed in the hurricane, but nevertheless repairs to the mission premises were given high priority and within four months those at Anelgauhat were "in first rate order" again, and some "other needful improvements" had been accomplished as well (J.D. Murray, HFR 1873 p.338). Within a month of the hurricane "most" of Murray's schools had been visited and he (HFR 1873 p.185) spoke
Aneityumese sufficiently fluently to "catechise" the people "in their own tongue". Perhaps a new census was a "needful improvement"; perhaps he was aware that the Board of Foreign Missions regretted publicly that Geddie never sent it "a concise tabular view of his district" (MR 1858 p.419).

At Aname, Inglis was still keeping his records of births, deaths and population, but there is some reason to doubt their reliability. Until 1874 there are no figures available, but shortly before he retired in 1876 Inglis (1887 p.332) claimed that the population on his side of the island had decreased at an average rate of \( \frac{1}{2} \) per cent a year for the seven or eight years before 1875. In 1874 his population ostensibly consisted of 450 males and 290 females, and though there were 36 births and 40 deaths reported, and the difference between the rates calculated from these is \( \frac{1}{2} \) per cent, the rates of decrease for earlier years must have been about 4 times as high as this if the population in 1868 numbered half of 1,750. The only alternative to this greater decrease is that the population was smaller in 1867-8 than it was believed to have been; given the end point of 740 and Inglis's rate of change, the population seven or eight years previously would have been nearer 775 than the 875 alleged (see page 79).

On the southern half of the island in 1874 (or more probably 1873-4) there were only 12 births and 29 deaths reported for a population of 449 males and 299 females (J.D. Murray, HFR 1875 p.152); and though the numbers of both births and deaths are low compared with the 36 and 40 respectively reported for a population of roughly the same size in Inglis's district, Murray's (HFR 1875 p.131) only comment was that "the rate of mortality among [the natives] during the past year has been very high". Were all of the births and deaths that occurred reported?

For the latter part of 1874 and early 1875 the natives were "unusually healthy" despite a hurricane in January (J.D. Murray, HFR 1875 p.259), but on 28 March there was an earthquake followed by a tidal wave "which raised the sea ten or eleven feet above its ordinary level at spring tides. ... [It] rose round the whole island, and did serious damage at every exposed situation. In all the low-lying districts the taro -- the staff of life on the island -- was more or less injured" (Inglis, 1887 p.183). At Anelga what the
harbour was "seemingly half-emptied" and "then, in about 10 or 15 minutes, afterwards, came a great wave, rolling most impetuously in upon the shore, about 20 feet beyond the ordinary reach of high water". The only casualty was one child who drowned, but many became ill "in consequence of exposure", and Murray's annual report, dated 7 August 1875, showed 37 deaths and 14 births but no number for the population (J.D. Murray, HFR 1875 p.260; 1876 p.71).

Compared with Anelgauhat, the hurricane was "very severe" on the northern side of the island (J.D. Murray, HFR 1875 p.259) and at Aname, the tidal wave caused the sea to rise "about four feet higher than during the hurricane of 1873"; but nevertheless Inglis reported that "without any assignable reason, the death rate for 1875 was exceptionally high, and at the same time the birth-rate was exceptionally low". "As a rule", the birth rate in his district was high, but in 1875 there had been only 21 births -- "about a third below the average" -- and 46 deaths -- "about one-third above the average" -- which suggest "average" numbers of about 32 births and 35 deaths. In the first four months of 1876 there had been 12 births and 12 deaths, and the population was stated to be 713 -- 446 males, 267 females (Inglis, 1887 pp. 183, 331-2, 340).

Using the mean of 740 and 713 as the population base, these "average" numbers convert to rates of 44 births and 48 deaths per 1,000, and the actual numbers for 1875 to just under 30 births and 63 deaths per 1,000. By comparison, the figures quoted above for the southern half of the island indicate a birth rate there of less than 20 in both years, and an average death rate similar to the "average" in Inglis's district. Later censuses suggest that the size of the population at this time may have been exaggerated, and that there may have been fewer married couples on the south than in the northern district; but even with some allowance for both of these, the birth rate in the south was low compared with the rate on the northern side of the island.

The possible, or even the likely biological reasons for this are irrelevant at this juncture because by this time, with perhaps no more than 700 people present in each district, the inclusion or exclusion of Aneityumese absent on other islands
could begin to make some difference in what was the relevant population at risk for the births and deaths reported. For once both missionaries were explicit in their annual reports (for either 1874-5 or the calendar year 1875) about the numbers known and admitted to be absent; but they were not at all explicit as to whether or not the absentees were counted as part of either 'flock' in any respect whatsoever.

Inglis's (1887 p.340) total for his population in 1876 was 713, ie. two more than the 740 reported for 1874 minus the difference between the numbers of deaths and births quoted for the two years, 1874 or 1873-4 and the calendar year 1875. Yet in June 1876 there were "twenty natives male and female, from [his] side of the island living on other islands, either as teachers or as servants to the missionaries" and nine of these had been sent "during the past year". Over the same period, "one of [the] teachers on Tanna died ... and two teachers and their wives, after several years of faithful service, returned home invalided; one native servant returned from Erromanga" (Inglis, 1887 p.336).

"We also supplied the Dayspring during most of the season with five or six natives, as a boat's crew, and we supplied parties of workmen for some months on Erromanga and Nguna, to assist in house-building and other labours, for the benefit of the missionaries". One such party consisted of 67 people, and they had been on Tanna for several weeks in 1869 helping Inglis to erect a house of two rooms (the frame of which had been brought from New Zealand), "some out-houses for the natives" and a boat house (Paton, HFR 1870 p.76). "Of course these have all been paid fully and fairly for their work" (Inglis, 1887 p.336); but did all who went return? Was the teacher who died on Tanna included in the population but not the deaths? Or vice-versa? Were children born to those absent, but included in the population total, also added to both the population and the births? And what happened (statistically) if these children died before their parents returned? What happened to the children if the parents died?
From the southern district in 1875 there were "absent, as teachers and servants to the missionaries on Tana and Efate, 13; of these 6 have gone forth during the past year. Absent in N. Caledonia and Queensland, as servants to the traders, 17" (J.D. Murray, PR 1876 p-71). The "servants" might well have been females, but were the teachers accompanied by wives and children who appear in nobody's "statistics"? This is the first acknowledgement that people were in fact away from the island for reasons other than those associated with the mission, and for how long had this been going on? By 1875 the sandalwood stations had become whaling stations and Aneityumese were employed as boats' crews and labour (Steel, HFR 1875 p.75), but again how many of them may have left the island over the years, as "servants to the traders"?

The difference between the numbers reported for Geddie's old district by J.D. Murray for 1874 and by his successor, Rev. J. Annand, in about August 1876 (Paton, PR 1877 p.17) certainly suggests that if both counts were equally reliable, the losses from the population through migration were far greater than the losses through death. The two counts are as follows:-

<table>
<thead>
<tr>
<th></th>
<th>1874</th>
<th>1876</th>
<th>Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td>Males</td>
<td>449</td>
<td>386</td>
<td>63</td>
</tr>
<tr>
<td>Females</td>
<td>299</td>
<td>243</td>
<td>56</td>
</tr>
<tr>
<td>Total</td>
<td>748</td>
<td>629</td>
<td>119</td>
</tr>
</tbody>
</table>

In the two years for which the numbers of births and deaths were reported the net loss had been 40 (66 deaths, 26 births), thus accounting for only one-third of the difference in the two counts, and even if the period separating them was more than two years, at most the natural decrease would have accounted for about half of the total loss.

After Fiji became a Crown Colony in October 1874 the "so-called labour traffic" (Inglis, 1887 p.336) was better regulated than it had been, and though recruiters tended to avoid islands on which there were missionaries (Rannie, 1912 p.121), at least two showed "an unwonted audacity" in calling at Aneityum in 1875-6. Both took away "several" natives; one of them "came in after dark" and during the night "some five or six natives [were] got on board", and the other took another "several" to New Caledonia (Inglis, 1887 pp.336-7). At this rate of exodus per ship, there must have been
many more vessels calling unbeknownst to the missionaries, or more probably, the Aneityumese who wanted to leave had no need to wait for the recruiters to come and fetch them.

Inglis retired towards the end of 1876, but before he left the island he "revised and corrected" his census lists, and these showed a total population of 678 in his district in October 1876 (Annand, PR 1877 p.297), ie. 35 fewer than Inglis claimed elsewhere. About six months later there were 417 males and 258 females (total 675) and there had been 10 deaths and 6 births since Inglis's last revision. At about the same time the population in the southern half was down to 614 -- 380 males, 234 females -- and since the preceding August there had been 23 deaths and only 6 births. With 140 married couples in one district and 164 in the other, 6 births in nine months in the former and the same number in six months in the latter suggests that few of the wives in either district were both capable of producing children and willing to do so, but we shall return to this later.

Inglis was not replaced at Aname until 1879 and Annand had charge of both stations until then. In August 1878 he spent three weeks on the north coast and in the course of this visit "took a new census". This revealed a population of 622, and "anything but a hopeful prospect for the future of these people" with only 31 births to offset 82 deaths between October 1876 and mid-August 1878. "It is not the old and feeble alone that are dying but all classes, the majority being young people" (Annand, PR 1879 p.128). Converted to an annual basis, these numbers of births and deaths represent rates of about 26 births and 70 deaths per 1,000 population. As there had been "fully the average amount of illness among the natives and many deaths during the past year" (Annand, PR 1878 p.237), presumably the death rate on the southern side was no less than on the north, though with fewer married couples there the birth rate might have been even lower.

In the following year the mortality was reported to be "higher than for some years past", but chiefly because of an outbreak of whooping cough. This had started in January 1879 on the northern
side of the island, but it did not reach Anelgauhat until May and even then it had "not extended over more than one third part of the land". Probably only the children born since 1867 were wholly susceptible, and this may have accounted for the slow spread of the epidemic. By August, "over 30 children" had died, "about 20" of them in the northern district. There was "a great deal of sickness ... in addition to the cough" (Annand, PR 1879 pp.298, 323) and though no figures were given for the southern part of the island, the population on the north fell from 623 (or 622?) to 590 in the year ended August 1879 (Ed. PR 1880 p.49). If the previous birth rate had applied, there would have been 16 births in the course of a year in this population, and the epidemic may, therefore, have increased the death rate to nearly 8 per cent.

The new incumbent at Aname, Rev. J.H. Lawrie, was introduced to his charge by Copeland (who had relieved Inglis there in 1860-3) in August 1879 and Annand took leave. He returned before 1 June 1880 to report that "the health of the people has been fair. The decrease on this side of the isle has been just one a month during the last fourteen months. On Mr Lawrie's side very little if any decrease has taken place as the number of births has been higher" (Annand, PR 1881 p.127), and with 551 "souls" in the southern district, the population of the island must then have been about 1,140. Eighteen months later the total was "scarcely 1,100" (Annand, PR 1882 p.99) and though there was no further decrease before June 1883, an outbreak of influenza in June and July "cut off about 20 persons" in each district (Annand, PR 1884 pp.45-6; Lawrie, PR 1884 p.18) so that in mid-August there were "only about a thousand and forty or fifty souls on the whole island, including old and young".

If the birth rate continued at its previous level during this period, the average death rate was about 5 per cent and in some years the rate had been less than this. But this respite was short-lived and in 1884 Annand (PR 1884 p.329) grieved that "physically and numerically the race is declining. Shall we withhold the fact that within the last ecclesiastical year, ending May 31st, there were ninety-nine deaths and twenty-seven births! Within the same period about forty young men emigrated
to Queensland and the Hawaiian Islands", leaving "about nine hundred and fifty souls on Aneityum". Elsewhere (PR 1885 p.212) the number of births was given as 47 and though this is more consistent with the change in population numbers, the 27 spelled out in full is more likely in view of the birth rates reported for previous years.

A death rate of 10 per cent, in the absence of any exceptional or extraneous cause of death, following on a period during which the annual rate had fluctuated but could rarely have been much less than 5 per cent, suggests something more fundamentally wrong than merely, as Annand (PR 1877 p.271) first thought, that the people would "not learn to obey the simplest laws of health, no matter how much we talk to them about it". Early in 1864 when Geddie (HFR 1864 p.237) was taking his first home leave, the wife of the young chief who was to accompany him to Nova Scotia was "examined by a medical man [in Melbourne] who pronounced an unfavourable opinion about her case. He says that one of her lungs is affected, and any change to a colder climate is almost certain to make her disease take an active form". She returned to Aneityum with her husband but died soon afterwards, clearly of tuberculosis, and hers was not likely to be an isolated case. Widespread tubercular infection, its prevalence ensured by the daily gathering in small school-houses where the lessons were essentially reading aloud or reciting for an hour or so, would have lowered people's resistance to the endemic "fever and ague", and increased their susceptibility to any other respiratory infection.

By 1884, the number of schools had decreased. In 1874 there were only 50 teachers on the island (J.D. Murray, HFR 1875 p.152) and in 1878 Annand (PR 1879 p.128) visited "every encampment on [the] island, forty-eight in all" and there were then 26 schools in the northern district. In 1879 there were 20 day-schools in Annand's (PR 1880 p.328) parish which the next year he (PR 1881 p.127) described as "about eighteen miles of coastline, and ... three inland villages, about five miles the farthest from the sea shore". In 1886 the Assembly of the Presbyterian Church of Canada decided to withdraw its missionary from Aneityum, and leave the field to the Free Presbyterian Church of Scotland which had always supported the station at Aname (PR 1886 p.186).
TABLE 3.1 : The population reported for Aneityum 1883 to 1905: from Buxton, 1926 p. 442

<table>
<thead>
<tr>
<th>Year</th>
<th>Number</th>
<th>Year</th>
<th>Number</th>
</tr>
</thead>
<tbody>
<tr>
<td>1883</td>
<td>1056</td>
<td>1896</td>
<td>600*</td>
</tr>
<tr>
<td>1884</td>
<td>954</td>
<td>1897</td>
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<tr>
<td>1886</td>
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<td>1898</td>
<td>517</td>
</tr>
<tr>
<td>1888</td>
<td>850</td>
<td>1899</td>
<td>477+</td>
</tr>
<tr>
<td>1890</td>
<td>760</td>
<td>1900</td>
<td>482**</td>
</tr>
<tr>
<td>1892</td>
<td>710</td>
<td>1901</td>
<td>476</td>
</tr>
<tr>
<td>1894</td>
<td>678</td>
<td>1903</td>
<td>460</td>
</tr>
<tr>
<td>1895</td>
<td>670</td>
<td>1905</td>
<td>435</td>
</tr>
</tbody>
</table>

* "and the death rate is stated to be about 15% per annum"
+ "and the ratio of births to deaths is stated to be as 1 to 4.3"
** "and the births exceeded the deaths for the first time for many years"
Except occasionally thereafter the population data for Aneityum until the end of the century consist of a series of numbers, at 2-year intervals from 1884 to 1894 and annually thereafter, which Buxton (1926 pp.441-2) culled from the reports of the voyages of the Dayspring and the New Hebrides Magazine published in the New Hebrides by the Presbyterian mission there from 1902-11. Buxton believed that all of these figures were "based on actual enumerations ... made by house to house visits": if "school-house" is substituted for "house" perhaps he was right in his belief. The numbers reported are given in Table 3.1 along with Buxton's comments.

Too many of them are multiples of 10 for the numbers to be exact, but the changes in them are uneven and unless some of the losses in the mid to late 1880's were due to migration, mortality was very high in the second half of that decade. By then some of the young men who had gone to Queensland and Hawaii in 1883-4 should have been returned on the expiry of their contract period of either three or five years, but perhaps those who returned, returned to die. From 1894 to 1900 the changes in population totals are consonant with both a birth rate of about 26 per 1,000 each year, and either the relationship between the birth and death rates or the level of mortality as related by Buxton, so that although there was a resurgence of the labour traffic to Queensland in the 1890's (Robertson, PR 1892 p.8; PR 1897 p.207), the Aneityumese were perhaps not recruited.

With fewer than 500 people on the island by 1900 and diminishing numbers thereafter, one could expect some fluctuations from year to year in the numbers of both births and deaths, but it seems likely that the birth rate in the whole population was 30 or, more probably, fewer births per 1,000 for the last quarter of the century. In April 1877 when Annand was in sole charge (and so for once one can be reasonably sure that the same rules governed the taking of the census in both districts) the population of 1,289 consisted of 797 males and 492 females. A quarter of a century before there would probably have been 552 females in a group of this size; and if the sex ratio in the population had been such that only 50 per cent were
males, one would have expected about 645 females. Hence a birth rate of 30 per 1,000 when only 38 per cent of that 1,000 are females is equivalent to a rate of 34 births when females constitute 43 per cent of the base population or 40 per 1,000 when the numbers of males and females are equal.

In addition to this distorted sex ratio -- the initial distortion aggravated, Inglis (1887 p.342) claimed, by higher mortality among females in the various epidemics -- many of the marriages in existence in 1876 were "ill assorted" because of the adherence by the Aneityumese to "the principle, so rigidly carried out, that young women must be married in their own tribe or within their own districts". Inglis's successor, Lawrie (1893 p.709), also described the marriage customs of Aneityum, but with such a curious mixture of tenses that one cannot be certain whether he was writing about the past or the customs that existed during his tenure.

Marriage is usually arranged for by chiefs or heads of tribes. The girl does not pass into charge of the husband until she reaches maturity (or puberty). A feast is made when the formal marriage takes place. They usually married within the tribe, although not in blood relationship, yet the children of a brother and sister may marry, while the children of two sisters were looked on as brothers and sisters. Polygamy was common in pre-christian days.

Both Inglis and Lawrie were perhaps describing an 'ideal' situation. Copeland (HFR 1863 p.10), who spent a much shorter period than either on the island, but as a consequence of the measles epidemic in 1861, married "about sixty couples" within nine or ten months of its end, expressed a rather different (and somewhat less reverent) view of marriage on Aneityum.

Generally speaking, the natives take the affair of marriage very coolly. It is not such a great event to them as it is to the inhabitants of more civilised countries where husband and wife are much more mutually dependent. ... They think nothing of being married half-a-dozen of times, as they were constantly on the move in the days of heathenism.
Fig. 3.1 Estimated death rates from respiratory tuberculosis at each age among two cohorts of males and females born in England and Wales in 1846-51 and 1861-66.
However, these were exceptional times for the Aneityumese, and perhaps also for Copeland who was still a bachelor, with his future wife living at Anelgauhat.

But for how long could the 'ideal' marriage pattern persist in view of the decrease in population numbers? Especially during the last quarter of the century? Assuming only six traditional districts (initially unequal in size though the epidemics may have tended to equalize them), by 1877 the average number of people of all ages per district was only 215, of whom only about 80 would have been females; and with an average of 50 women already married, there would have been relatively few females available for the number of unmarried males seeking wives within the district. The high death rate each year would probably have widened the market for the survivors, but if the root cause of the high mortality was tuberculosis, it is possible that the women may have succumbed at younger ages than their husbands.

Fig. 3.1 shows the death rates from tuberculosis which two cohorts of males and females would have experienced in the course of their lifetimes in England and Wales. The cohorts represented are "synthetic cohorts", and the rates shown are annual age-specific death rates at intervals of 5 years from 1851 onwards among males and females born in the periods 1846-51 and 1861-6 as they attained each age. Though the levels of mortality are different, and successively younger cohorts (not shown here) experienced progressively lower death rates from tuberculosis, the pattern of mortality for each sex remained essentially the same for all of the cohorts born in the second half of the 19th century as for the two illustrated here. In general, females had a greater chance of dying from tuberculosis at ages 10-14 and 15-19 years than did their male contemporaries, but after a period of maximum risk at ages 20-24 the mortality for women declined as they became older, whereas the death rate for males remained high for 25 to 30 years from the age of 20 on.

There is no way of knowing whether the mortality pattern among the Aneityumese resembled this one for England and Wales because this kind of analysis requires a long run of reasonably accurate and reliable censuses and statistics of deaths by sex,
age and cause of death, and there are hardly any populations for which the data are sufficiently extensive to test the universality of this pattern by comparable analysis. However, one thing shared by all populations at that time was the lack of any effective therapeutic cure for tuberculosis, and if the differential mortality by sex was of biological rather than social origin, the pattern of risk for each sex with age might not have been very different on Aneityum.

The ramifications of a mortality pattern such as this on new and existing marriages within six small populations in which there was a surplus of males are exceedingly complicated, and in the long run, perhaps not terribly relevant. The size of the population is not known for any of the years between 1905 and 1917, but in December 1917 the missionary Dr Gunn reckoned his flock as "about 320" people which represents an average loss of 9 or 10 persons a year. Whether this was due to a continuing excess of deaths over births or out-migration or both is not known, but several of the old men now on the island worked for longer or shorter periods on inter-island trading vessels at about this time and some may have settled elsewhere in the group. None of them recalled many people dying from influenza in 1919 and Buxton (1926 p.436) thought it "curious that the great pandemic of influenza did not reach the New Hebrides until the end of 1919, and that when it arrived it was not very fatal", but in July 1923 the population was reported to be 256, though it had allegedly risen to 272 by September 1924 (Buxton, 1926 p.442).

The first "official" census in 1926 was compiled by a European settler, J.P. Wilson, who had acquired land at Anelgauhat and was felling and milling timber. The exploitation of the island's timber was not new, but this was probably the first time that anyone other than the missionary had been asked to "take the census" of the island, and the instructions he might have been given are not on record. In fact, the earliest ones on record relate to a "decennial population census of the empire 1941" in which British District Agents and British missionaries were asked to co-operate, and supply information on

(a) District, Island, Village (or zone)
(b) Number of male and female adults
(c) Number of male and female children (under 15)
<table>
<thead>
<tr>
<th>Year</th>
<th>Men</th>
<th>Women</th>
<th>Boys</th>
<th>Girls</th>
<th>Persons</th>
</tr>
</thead>
<tbody>
<tr>
<td>1926</td>
<td>106</td>
<td>56</td>
<td>26</td>
<td>32</td>
<td>220</td>
</tr>
<tr>
<td>1930</td>
<td>94</td>
<td>57</td>
<td>19</td>
<td>27</td>
<td>197</td>
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<td>1931</td>
<td>84</td>
<td>57</td>
<td>28</td>
<td>21</td>
<td>195*</td>
</tr>
<tr>
<td>1933</td>
<td>88</td>
<td>55</td>
<td>28</td>
<td>31</td>
<td>202</td>
</tr>
<tr>
<td>1934</td>
<td>91</td>
<td>57</td>
<td>30</td>
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<td>216</td>
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<td>32</td>
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<td>193*</td>
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<td>1945</td>
<td>63</td>
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<td>41</td>
<td>38</td>
<td>192</td>
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<tr>
<td>1947</td>
<td>57</td>
<td>55</td>
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<td>49</td>
<td>191</td>
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<tr>
<td>1949</td>
<td>63</td>
<td>56</td>
<td>39</td>
<td>34</td>
<td>192</td>
</tr>
</tbody>
</table>

* Either this total or one of the other numbers is incorrect
(d) Notes on degree of civilization
   i.e. "bush", "mission"

(e) Notes on education and/or literacy (if any)

(f) Notes regarding health, rises and falls
    of population, if known

(g) Population absent, if ascertainable, should
    be given separately, neglecting those absent
    on plantations who will be counted
    separately, but including those absent in
    domestic service or for other reasons.

(h) A note should be made of "foreigners" present

(i) A note of those employed on mission stations
    with origins would be appreciated.

The numbers recorded officially for the years between 1926
and 1941 and after 1941 are given in Table 3.2, but as there is
now no way of ascertaining who was responsible for the various
summaries, their reliability cannot be assessed. By then the
population was so small that one would expect some variation
from year to year in the total just by chance. After Japan
entered the war, the people were brought in from all of the
outlying districts to Anelgauhat for their greater safety, and
several of the men spent the war years in Vila and elsewhere
working for the American forces. By now there are again small
settlements dotted about the island, and the people tend to
move freely and frequently between them and Anelgauhat, but a
century after Geddie first landed on Aneityum its inhabitants
were about 1/20 of the number believed to have been there in
1848.

Reviewing this change, the evidence available provides
just as much reason to assume - with Inglis (p.72 above) - that
in some short run, or perhaps even a longer one, before the measles
epidemic the population was roughly stationary as to assume that
it was not. The scant data that now exist for birth rates before
1861 indicate that fertility was high, and just how much
infanticide could be tolerated without decreasing the size of the
population, especially in the small communities in which most of
the people lived, would depend on the magnitude of the death rate.
However, if in two consecutive years when Geddie reported that
the mortality had been higher than in any previous year, Inglis
reported numbers of deaths which convert to annual rates of less
than 22 per 1,000 and 48 per 1,000, the most one can conclude is that the death rate was usually less than the higher figure and may have been as low as 20 to 25 deaths per 1,000 or lower in years when there was no "severe" epidemic.

The evidence for the occurrence of epidemics before the establishment of the mission is at best conflicting. A.W. Murray (1863 pp.20, 29, 31) visited Aneityum twice before 1848 - in March 1841 and again in April 1845 - but he mentioned no epidemics though he admitted he was "unable to furnish a minute account of the labours and trials of the teachers during the early years of the mission". Turner (1861 pp.366-71, 433-4, 18) who visited with Murray in 1845 and again with Geddie in 1848 likewise reported no outbreaks of sickness on Aneityum, though he surely would have recognized their significance because it was "a deadly epidemic in the form of dysentery" that had forced him and Nisbet to flee from Tanna early in 1843. Nisbet and Gill visited in September 1846, and Gill (1856 pp.156, 135-6) wrote that "during the long absence of the mission-ship, much sickness had prevailed among the tribes"; two sentences later this became a "prevailing epidemic", which may or may not have been the same as the one he alleged for "most of the islands" of the New Hebrides "early ... in the year 1845".

Was this the epidemic to which Geddie (MR 1850 p.24) referred in July 1848 when describing the plight of the two Samoan teachers at Anelgauhat in the interval between September/October 1846 and his first visit in July 1848? "They were frequently reduced ... to great straits. At one time their fears for their personal safety were excited. A fatal epidemic had broken out and raged throughout the length and breadth of the island"; but at least one of the teachers had been on the island since April 1845, if not earlier, so there may have been a misunderstanding on Geddie's part. In 1861, after the measles epidemic was over, Inglis (HFR 1861 p.329) recalled that "one of the most intelligent" of his Aneityumese teachers had "oftener than once" told him of an epidemic "that passed over the island about nineteen years ago" -- i.e. 1842. Then in 1867 when the Aneityumese were suffering from whooping cough, he wrote:
Between the time the island was first visited by foreign vessels and the introduction of Christianity by the location of teachers, in 1841, a fearful epidemic swept over the island, and carried off at least one-third of the population. Between the location of the teachers and the settlement of the missionaries, in 1848, another epidemic, equally severe, passed over the island (Inglis, HFR 1868 p.17).

This timing is rather less precise than the dates given by Brenchley (1873 pp.vii, 196) for the two epidemics, and as he relied on the missionaries for all information about "the manners and social characteristics of the different islands" that he was unable to collect himself during his short stay at each, Brenchley's details of Aneityum were almost certainly provided by Inglis because Geddie was in Nova Scotia when H.M.S. Curacaoa was at Aneityum in August 1865.

Three fearful epidemics have visited the island within the last forty years. About the year 1836, an epidemic like cholera carried off above one-third of the population; about 1842 a similar one took off another third; and in 1861 a third epidemic of measles, followed by dysentery, scourgéd what was left.

The year 1836 is nearly the mid-point of the interval between 1829 and 1841, and the 1842 date tallies with 19 years before 1861; but it is curious that there were no contemporary or nearly contemporary reports of this second epidemic if the mortality from it was so high. The first two teachers were settled on the north coast in relatively auspicious circumstances in April 1841, and it seems that one survived until June 1842 (A.W. Murray, 1863 p.29). Though he then returned to Samoa, three others were left on the island, and only one of these (and his wife) died before April 1845 (Turner, 1861 p.365). From then until September 1846 there were four teachers on Aneityum, and for part of the time several more who had fled there from Tanna (A.W. Murray, 1863 p.32). Though all of them then wished to return to Samoa, two agreed to remain and they were still there when Geddie arrived first in July 1848, to report the "fatal epidemic" in a context which suggests its occurrence somewhere within the two preceding years.

In the letter quoted above, Inglis (HFR 1868 p.17) asserted that there was no tradition that he knew of of any epidemic on Aneityum before 1829, and that then "the island was populous, and
the most of the available land under cultivation. The population could not have been less than 12,000; some have thought that it might be 20,000". Brenchley (1873 p.196) merely reported that "previous to the first of the epidemics I have mentioned, the population, it is said, could not have been less than 12,000. In 1859 it amounted to 3,500". Clearly, Brenchley had trouble reconciling mortality of "above one-third", "another third" and the unmentioned third that had died in the measles epidemic with any survivors at all in 1865! However, Inglis's estimate of 12,000 is obviously based on the fallacious reasoning that if 1/3 of the population died in each of the first two epidemics, the population when Geddie arrived -- tacitly assumed to be 4,000 -- was only 1/3 of the number that had been there before the first of the alleged epidemics.

If the initial population before the first epidemic is designated by $P_1$, and the population before the second by $P_2$, the relationship between $P_1$ and $P_2$ assuming no migration over the interval is

$$P_2 = \frac{2}{3} P_1 + N_{1,2}$$

where $N_{1,2}$ represents the natural increase or decrease that occurred in the interval between the end of the first epidemic and the beginning of the next. If the population when Geddie arrived in 1848 was $P_3$, and the natural increase in the interval between the end of the second epidemic and Geddie's arrival was $N_{2,3}$ (and again there was no migration), the relationship between the initial population and the last would be

$$P_3 = \frac{4}{9} P_1 + \frac{2}{3} N_{1,2} + N_{2,3}$$

If $N_{1,2}$ and $N_{2,3}$ are both zero in this equation (i.e. the numbers of births and deaths in both time intervals are equal) $P_3$ is barely less than half the size of the initial population $P_1$. If both N's are positive because the numbers of births were greater than the numbers of deaths in non-epidemic years, $P_3$ would be greater than $\frac{4}{9} P_1$; and it would be less than this fraction of $P_1$ only if there were more deaths than births in the non-epidemic periods.
Apart from some doubts as to the reality of the epidemics and their dates, there is also some reason to doubt that any exotic disease introduced before the mid-1850's could have had such a widespread incidence as measles had in 1861. Much would depend on the nature of the disease and its mode of transmission, and Brenchley is the only person to have named a specific disease as responsible for these alleged outbreaks. "Like cholera" presupposes that either Inglis or Brenchley or both knew the symptoms of cholera; but as there had already been three epidemics of cholera in Great Britain before 1865 -- in 1831-2, 1848-9 and 1853-4 -- the second of which started in Scotland and was much more fatal than the first (Gale 1959 pp.66-8), it is not inconceivable that both were aware of the usual course of the disease in persons who had been infected with the cholera vibrio, especially the intense diarrhoea that develops as the bacilli multiply rapidly in the intestine, destroying all of the natural bacteria and inflaming and damaging its lining so that fluid from the blood leaks into the intestine, and thereby leads to death from severe dehydration (Burnet, 1953 p.312).

Burnet (1953 p.314) described cholera as "an anomalous disease", the epidemiology of which "is puzzling and has never been fully explained". Though food might sometimes be contaminated by the vibrios, "infection is usually waterborne" (Gale, 1959 p.66) and it is hard to reconcile widespread infection throughout the whole of Aneityum, or even a significant part of it, with its topography and what is known of the probable geographic distribution of population. Many of the communities inhabiting the coastal plains and lower reaches of the river valleys would not have shared their sources of water with others, and given their fears of sorcery and the lack of contact between neighbouring communities which Geddie (MR 1850 pp.100-1, for example) so often alleged, it is unlikely that an infection as acute as cholera would spread far beyond its point of introduction.

Fatal though it may have been to the people near there -- and on the evidence available Burnet (1953 pp.311-2) reckoned the likely fatality rate as between 70 and 80 per cent in untreated cases -- there is also the question of the likelihood of the survival of the organism among a boat's crew in the course of the voyage from the source of infection to Aneityum. Neither Burnet
nor Gale is specific about the incubation period for cholera, though Gale offered its "relatively short" incubation period as a "partial explanation" for why England was free of the disease until 1831 despite her extensive trade with India for more than two centuries before then. During the long sea voyage from India "an epidemic on shipboard would have burned itself out" (Gale, 1959 p.67), but it is impossible to guess whether the time needed for a voyage from Hong Kong, Shanghai or Manila might or might not have been sufficient to "burn out" cholera among the crews needed to handle the barques, brigantines and schooners involved in the sandalwood trade (Shineberg, 1967 pp.200 et seq.). However, "cholera morbus" was listed second amongst the diseases most commonly treated at Sydney Hospital in the early years of the Colony (Watson, 1911 p.7), and if cholera survived to Sydney (where it apparently did not cause any epidemics) it probably could have survived the shorter distance to Aneityum. But did it?

My scepticism about the occurrence of these epidemics is largely because of the parallels with a legendary epidemic in the Hawaiian Islands in the early 19th century. The date of its occurrence is put variously between 1802 and 1807, with a preference for 1804 when Lisiansky (1814 pp.112, 133) cancelled his intended visit to Kamehameha on Oahu because at Kealakekua Bay on the island of Hawaii he had "learn[ed] ... that a species of epidemic disease was raging in that island" (ie. Oahu). In his general account of the islands the disease merely "spread among [Kamehameha's] troops, and destroyed the flower of his army".

There are two other hearsay reports dating from this period. One is Mariner's brief mention - as reported by Martin (1827 I p.56) and therefore second-hand hearsay - that the Port au Prince was refused entry to the inner harbour at Honolulu in September 1806 because there was a sick man on board, and "the chief of the island" was afraid of "introducing disease into the country, which they said had happened on a former occasion, from an American ship". The other is a report from Kealakekua in May 1807 of "a kind of epidemic or yellow fever, said to have been brought to these Islands a few years ago, and which makes dreadful havoc among the natives" (Iselin, n.d. quoted in Schmitt,
Against these can be set at least seven other more or less contemporary accounts which fail to mention any outbreak of disease, although in two of them other details are reported which would make it likely that if there had been any epidemic of unusual severity at about this time, it would have rated a mention. The earliest of these is Shaler's (1808 pp.100, 106, 93-4) account of his visit to the islands from 22 August to 6 October 1804. He was ashore on Oahu from 1 September to 2 October, and although much of his account is devoted to Kamehameha's conquests and methods of government, there is also a passing reference to Hawaiian medicine which concludes with: "Fortunately the good constitutions and temperance of these islanders prevents their having often occasion for the skill of their physicians". Lisiansky had been at Hawaii the previous June.

By 1809 there were several Europeans living in the islands, some of whom had been there for periods that spanned the years of the alleged epidemic. Isaac Davis was one who, when Archibald Campbell (1822 pp.85, 95, 14) arrived at Oahu in January 1809, had been in Kamehameha's service for "about twenty years", and Campbell lived in his house for nearly half of the year or more that he spent on the island. About five years after he returned to Scotland Campbell's story was 'ghosted' for publication, so the omission from his account of the Hawaiian islands of any reference to an epidemic is rather less surprising than John B. Whitman's in his MS "Account of the Sandwich Islands", abstracted from his "Notes and Memorandums written during a residence of about two years" from 1813 to 1815.

Whitman toured the various islands of the group, and from "a respectable American" who was living on Maui in 1806 he obtained a graphic description of the "terrible drought" there in that year when "great numbers of the natives perished literally of starvation and thirst even the trees on the mountains assumed a sickly yellow hue ... Though some of the Islands were less severely visited, none of them had provisions to spare and all of them suffered to such a degree that many of the natives perished". To Whitman's disappointment, Isaac Davis was dead before he visited Oahu, but he called on another
American who had "resided on the Islands 10 or 12 years" and there was still no mention of any epidemic though

The natives say that the Islands were more populous in former times than at present, and the trace of cultivation in lands that are now waste, and other signs of population visible in many places, renders it probable that they are correct.

The feuds and wars, which immediately preceded the peaceful reign of Tamaamaah are the most probable causes of the decrease in population, and the now remaining traces of the barbarous custom of sacrificing human victims on the altars of their gods, presents a shocking picture to the eyes of christianity while it points out another cause of the decline of population. As it is probable that during the wars victims were more frequently sacrificed to propitiate the gods and to insure their favour to the contending parties.

The first missionaries arrived in March/April 1820, with "the strength and weakness of the evangelical Protestant, in character, conduct and theology" (Kuykendall, 1938 pp.100-2). Two were ordained ministers, one a physician, two were schoolmasters and catechists, one a printer, one a farmer and three were young Hawaiians who had been at school in Connecticut. The Americans were allowed to remain only if they split into three groups, one on each of the islands of Hawaii, Oahu and Kauai. Two years later a "deputation" from the London Missionary Society was blown off course between Huahine and the Marquesas and arrived, unheralded, at the Hawaiian islands where they remained, again unintentionally, for five months. Rev. W. Ellis of the Tahitian mission was one of the party, and he subsequently returned to Hawaii to help his American brethren with the Hawaiian language.

Rev. Daniel Tyerman and George Bennett Esq. constituted the Deputation, and the "great bulk" of their journals and letters from 1821 to 1829 were "recompose[d]" for publication rather than "abridging or condensing the mass". In the process there was some selection of what "could be published without offence to decorum". Nevertheless, under the date of 20 April (1822), there is "an eye-witness [account] of the murderous spectacle" of human sacrifices, the prelude to which was "the
yellow fever [which] broke out among the troops, and in the
course of a few days swept away more than two-thirds" of the
8,000 men Kamehameha had assembled at Oahu in 1804 to attack
Kauai (Montgomery, 1831 I pp.vii, viii, 423-4). Ellis (1827
p.23) is less specific about the "depravity and vice" of the
Hawaiians, but a contributing cause to "the rapid depopulation
which has most certainly taken place within the last fifty years"
was "the ravages of pestilence" which had "twice, during the
above period, swept through the islands".

Just before Shaler (1808 pp.104-5) left Oahu in October
1804, Kamehameha had consulted him about the Kauai expedition
and "ordered a review" of his troops. "On this occasion there was,
I believe, about 700 men under arms, 400 of which were musketeers.
... Tamaihamaiha apologised for the small number of men on
parade, which, he said, were only his guards and those
immediately in the neighbourhood, and added, that, if I would
agree to wait a few days, he would assemble ten times the
number". Five years later, Campbell (1822 pp.150, 111, 153)
"saw nothing like a regular armed force, except a guard of about
fifty men, who constantly did duty at the king's residence",
and the "great part of his navy were hauled up on the shore" at
Waikiki because the islands were "at peace ... during the whole
time" of Campbell's stay. However, Whitman recounted that
"some years ago 'Tamaamaah' assembled a large army and attempted
a descent on [Kauai] but shortly after he set sail - a heavy
gale of wind arose and obliged him to put back - with the loss
of a great number of canoes and men".

Which to believe? Shaler or Lisiansky? Whitman or Tyerman
and Bennett? Or Ellis, the "pious fraud" (Davies/Newbury, 1961
p.L) who almost certainly invented an epidemic for Tahiti in 1807,
and perhaps even "the English whaler" he (Ellis, 1831 I p.106)
named as being responsible for it? Among the 11 missionaries on
Tahiti in 1807 was Rev. John Davies, who later wrote The History
of the Tahitian Mission 1799-1830, the manuscript for which was
completed in Tahiti before 1831. This contains no mention of an
epidemic in 1807, even though the 'king' Pomare was sick twice
in the course of that year; or of the visit of a whaler called
Britannica to Tahiti in any year covered by the History. Six
other vessels are named for 1807, with their dates of arrival and departure, and at this precarious period the arrival of a ship of any description was such a noteworthy event for the mission party that if the Britannica called, the meticulous Davies would have recorded it in his journal and thence in his History. Ellis did not arrive in Tahiti until 1817, and he did not return to French Polynesia after his sojourn in Hawaii.

But in mission circles there, Ellis had already sown the seeds of the myth of massive 'depopulation' in the Hawaiian archipelago, with two undated undiagnosed epidemics playing a major role in reinforcing (as in Tahiti) "the depopulating influence of vicious habits -- ... -- the prevalence of infanticide -- the frequency of war -- the barbarous principles upon which it was prosecuted, and the increase of human sacrifices" (Ellis, 1831 I p.105). With this respectable christian background, the passing years uncovered new evidence (equally unidentifiable as to source, equally unsubstantiated) for ever higher epidemic mortality and more elaborate descriptions of the symptoms and course of the disease (Schmitt, 1970 pp.360-3). But even Schmitt's question, "How many persons did the [epidemic of 1804 or thereabouts] kill?" is irrelevant when the real question is "Did it ever happen?" And the contemporary evidence suggests more reason to doubt than to believe that it did.

For Aneityum, there is effectively only Inglis's evidence for the occurrence of two epidemics, and though there is some support for the second of these from Gill and Geddie, the dates for it differ, and on the whole, the support is weak. Gill's testimony was never very reliable: for an epidemic on Rarotonga in 1830, for example, he grossly exaggerated the number of deaths per day (McArthur, 1967 p.165) and there are numerous other instances of "very singular and wild statements" (G.N. Gordon, MR 1858 p.37) amongst his "gems". Geddie's informants were the Samoan teachers and although he had spent six months on Tutuila learning Samoan, even by his own account his command of the language was less than perfect (Patterson, 1882 p.260). Also in July 1848 he was not intending to start his mission on Aneityum, so he might well have been less attentive to detail then than he would have been had he known he would settle there; but what is perhaps more relevant is that he seems never to have referred to this alleged epidemic.
again, even though one of his informants assisted him at Anelgauhat for many years and himself died of measles in 1861 (A.W. Murray, 1863 p.402).

Probably because the Samoan teachers spoke no English, they had little or no contact with the people at Paddon's sandalwood station, but once this was operating, there may have been outbreaks of colds and influenza from time to time in the winter months, because occasionally boats from Australia would have made runs fast enough for respiratory infections to survive, or some may have called elsewhere en route to Aneityum and taken on infected passengers or crew. By more than one account the Aneityumese women and girls flocked to the boats that called, and sometimes stayed on board for "several days and nights" (Patterson, 1882 p.198).

While Geddie - and more especially Mrs Geddie - blamed the visiting Europeans for "the prevailing licentiousness" (J 12 Mar 1851), the anonymous seaman/historian (MS pp.9, 10, 13, 14) described the natives as "great Libertines .. and the females of tender years .. shows the very same Example of what has been taught them by the Elders". After living among them he decided that "the females fondness for the European man ... was a love of gains ... what would be voluntary given her what she would beg .. and what she would purlone", not for herself but her friends, "and being fond of Libertanism they would have plenty of opportunitys to satisfy there desires". Meanwhile the Aneityumese men "prefer[red] cleaning sandal wood for rice and hard biscuit, to working on their plantations" (Patterson, 1882 p.191).

In such circumstances, the females were likely to have been the major source of infection for any epidemics, respiratory or otherwise, that occurred on the island between 1844 or earlier and the abandonment of Paddon's station in 1852. How widely such acute infections might spread throughout the population would depend on how many women were infected at any one time, from how far afield they came to the boats, the size of the communities to which they returned infected, and the inter-relationships between these and neighbouring groups. If women were as restricted in their movements between districts as the men were alleged to be, any outbreak of disease consequent on the initial infection
of women frequenting the ships in the harbour would have been limited in its extent, and there could have been a widespread epidemic in those years only if these women came from all parts of the island. Such evidence as there is for this period suggests that this traffic in women was probably limited to the communities in the south-western corner of the island; although some from farther up the west coast may have been involved, it is unlikely that many to the east of the harbour or on the north or east coasts were.

Possibly more important than the acute infectious diseases the women may have contracted during these years was the likelihood of their contracting chronic communicable diseases such as tuberculosis and venereal disease, both of which were allegedly rampant at this period among the crews of sailing ships. The Aneityumese men who worked in the sandalwood trade, either as crew on boats or on shore at other islands, were also susceptible and exposed to tuberculosis, but whatever the original source of infection on Aneityum might have been or which sex was responsible, once the mission became established a crucial factor in the spread of the disease throughout the population must have been the daily gathering of old and young in the school-houses or chapels all over the island, reciting the alphabet and catechism or reading aloud from their "books".

Two people who recognized the significance of tuberculosis in the New Hebrides in the 1920's were the anthropologist Speiser (1922 pp.26-7) and more particularly Buxton (1926 pp.436-8) who was medically qualified, and he had "no doubt at all that [it was] one of the gravest causes of mortality in the New Hebrides". Neither thought syphilis was very prevalent in the group then, but Speiser (1922 p.29) was reluctant to believe that it never had been, partly because "the population of Aneityum, for instance, is said to have been to a great extent destroyed by syphilis". After visiting all of the islands except the Banks and Torres Islands, Buxton (1926 p.439) concluded that syphilis was "extremely rare in the New Hebrides, if it exists at all, and ... the disease which passes under that name is yaws".
Strangely, neither mentioned gonorrhea, perhaps because this is not easy to detect in females without a close medical examination, perhaps because it was not then known that prolonged, untreated gonorrheal infection can cause occlusion of the Fallopian tubes and thus prevent conception. Scragg (1954, 1957) attributed the low fertility in New Ireland to this cause, and in retrospect, it would help explain the low birth rates reported for Aneityum, first in the southern half of the island in the early 1870's and not many years later in the northern half as well. But if gonorrhea was the cause of the low birth rate in later years, and tuberculosis responsible for the persistently high death rate, of the two diseases tuberculosis was probably the more significant in the decline of population on Aneityum, both directly and indirectly by reducing people's resistance and increasing their susceptibility to other infections, especially those of the respiratory tract.

Whether the absence of tubercular infection among the older people was significant for their apparently greater resistance to measles and dysentery in 1861, and diphtheria in 1866 will never be resolved. Most of them would have been in their 40's at least when Paddon moved from Inyeug to the mainland and Geddie arrived in 1848, and one might surmise that they were less interested in the sandalwood trade and all that went with it than were the younger men and women, but it is only surmise. However, if about one-third of the population died in 1861, and the death rate for females of reproductive age then was 60 per cent, and that for males aged between 20 and 50 years 40 per cent (vide p.74 above), then the rate for persons of all other ages was about 20 per cent, irrespective of the model chosen to represent the age structure of the population before the epidemic.

For the diphtheria epidemic in 1866 nothing more than an approximate number of deaths is known (p.77 above), but dividing this number arbitrarily between males and females so that the rate for females aged 15 to 49 years is a little higher than that for males aged 20 to 49, the death rates for these two groups might have been 30 and 25 per cent respectively. Assuming, again perhaps wrongly, that the death rates for males and females were independent, more than half of the marriages involving people in these ranges of age would have survived the diphtheria
Fig. 3.2 Population pyramids illustrating the effects of epidemics on the structure of four populations: the outer pyramid in each case depicts the pre-epidemic population, the inner (shaded) pyramid the same population a decade after the epidemics, and with no infanticide during the interval.
epidemic whereas less than a quarter would have survived measles and dysentery in 1861. The deaths from whooping cough must also be assigned very arbitrarily, but the evidence suggests that these were confined to children aged less than 15 years in 1866, with those aged less than 10 more severely affected than the 10-14 year-olds.

Assuming that the incidence of these epidemics did not alter the ordinary chances of survival for the people who survived them, and that individuals -- and especially females -- widowed by the epidemics re-married without too much delay, the changes in the structure of the population with respect to both sex and age over the decade from 1860 to 1870 are illustrated in Fig.3.2 for four populations of the size presumed for Aneityum before the epidemics. \( B^* \) is the stable population \( B \) of Fig. 1.1 adjusted for a sex ratio of 4:3 as reported in the first 'census', and the others are merely scaled-down versions of those shown in the same figure. There was 5 per cent infanticide of males born in each of \( D, E \) and \( F \), and the infanticide of females increased from 20 per cent in \( D \) to 25 per cent in \( E \) to 30 per cent in \( F \).

The children aged less than 10 years in the inner shaded portions are those born in the decade following the first epidemic, and those aged 10 years or more are the survivors from the initial populations. In all four populations the birth rate during this decade was little more than 2 per cent, or about half of what could have been expected in the absence of epidemics and infanticide, and this with a very generous allowance for the re-marriage of those widowed in the epidemics but otherwise no change in the age-specific fertility rates. Further, although all four populations experienced identical mortality rates by age and sex in the epidemics and in the intervening years, population \( B^* \) decreased in size by only 50 per cent whereas \( D, E \) and \( F \) fell by 53, 54 and 55 per cent respectively.

If the simulation is continued for each population without change in either the risks of dying at each age, or for females in the chances of their having a birth once they are 15 years old, it was not until sometime after 1870 that the birth rate could increase significantly and even then the margin between births and deaths in populations \( D, E \) and \( F \) was so small that the likelihood
of increase in the size of those populations before 1880 was precarious. For a brief period in the 1880's there was more chance of the births outweighing the deaths, but the birth rate slumped again as the small cohorts born in the 1860's attained their prime reproductive ages, and again the likelihood of increase in size in all populations was marginal until about the end of the century or even later in populations such as E and F.

The simulations could be continued indefinitely, but on Aneityum it was already apparent long before 1900 that the population was decreasing. If the increased disparity between the death rate and the birth rate was due to a "new" cause of death such as tuberculosis, and the pattern of risk for dying of the disease at each age resembled that for synthetic cohorts of males and females born in mid-19th century England and Wales (p. 90 above and Fig. 3.1), the higher mortality among young adult females alone might have been sufficient to account for the continuance of a low birth rate. Attractive though gonorrhea might be as a simple explanation for lowered fertility, it might in fact have played no role in Aneityum's demographic history at all.

The effect of this succession of epidemics on the age and sex composition of the various 'model' populations, and thence on the numbers of households on the island, the changes in the size of villages and the amount of land likely to be cultivated by the depleted labour force, will be considered after examining what is known of the archaeological record, and in particular, that for a group within the population which lived at Anumej, an area remote from introduced diseases and European contact until 1852. This community then contributed nearly 1/12 of the total population reported for the island, and tradition now has it that the few who survived the "big sickness" left their own lands and moved down the river valley to the coast.
CHAPTER 4
THE ROAD TO ANUMEJ

The first archaeological survey of Aneityum was made by Shutler and Shutler in 1963-4. They recorded 20 sites, 17 on the coastal plain and three in the interior, but their exploratory excavations were limited to only five of these -- three "village" sites and two rock shelters. Test pits at the "village" sites yielded a few shell artifacts, and five of the seven pieces of worked shell discovered came from one site towards the eastern end of Anelgauhat harbour. This was the only place where pottery was found, mixed with broken shell and items of European manufacture. The two rock shelters, both on the west coast -- at Inmanhat and Anuonupul -- were more rewarding. Both had middens of considerable depth; at Inmanhat the maximum depth was more than 2 metres, at Anuonupul about 1.5 metres. Charcoal taken from the bottom of these middens gave radiocarbon dates of AD 1100 and AD 1480 (Shutler, 1971 p.18; Shutler & Shutler, n.d. p.4).

In 1972 Groube (unpublished) excavated a site on the bank of the Inwoumkalau river, where the most recent archaeological level was sealed by more than 1 metre of flood debris. Below this were several distinct strata indicating periodic use of this site, and carbon samples from the deepest part of the excavation yielded a date of 2000 B.P. Alongside, under the flood debris, were stone walls remarkably well-preserved, which probably marked a former course of the river and these and other walls uncovered suggest attempts to control the flow of water down the valley.

The various kinds of stone walls are the most pervasive archaeological evidence on the island. Shutler & Shutler (n.d. p.4) noted "very extensive abandoned agricultural terraces ... on hillsides all over the island", but the stone facings to terraces are not the only kind of walls extant. As already mentioned in Chapter 2 (pp.43,50), there are also free-standing walls forming enclosures, stone facings to streams and water channels, regular lines of stones which may have marked boundaries or served to support bamboo pipes or merely diverted and directed
run-off from higher ground, pathways of stones, as well as occasional house platforms or small rectangular outlines. There are also the coral-lime floors and floor pavements of the mission's school-houses. A smaller wooden church has been built on the stone pavement of Inglis's church at Aname, and the walls of Geddie's stone house and church stand still at Anelgauhat, the walls of the church more than 5 metres high and 50 cms. thick, enclosing an area approximately 30 metres by 12.

Many of the stone walls are now covered in ferns or vines and creepers or buried under a prolific growth of wild cane because virtually the whole system broke down under the stress of decreasing numbers to maintain it, and by now it is a formidable task to discover its full extent. In 1972 one site was cleared sufficiently for the walls to be mapped in some detail (though probably not completely), and two smaller sites -- where the invasion of fern and cane was less -- were examined superficially.

The larger of the latter was inland about three kilometres from the main harbour, and on high ground above the Nijiemhang river. The ground fell away steeply down to the river and rose less steeply behind, so that the 14 or 15 low and narrow terraces covered the gentlest part of the slope. At one end of the site was a tributary of the Nijiemhang and fanning out, and apparently using only run-off from the hills behind, were two beautifully constructed water channels, rectangular in cross-section, approximately 50 cms. wide and 40 cms. deep, with the terraces on either side of them so constructed that when the channels were more than about half-full, the water would flow on to each terrace. Unless it was tapped very much higher up than its apparent source during the dry season, the tributary itself seemed not to have been used for irrigation, although there were places in it where there may formerly have been dams. The name given by most Aneityumese for this place was Anatuojem, but two people also identified it as Anaparijo, a place name that occurs in the mission records. The other small site was on the bank of the Aname river, about 1.5 kilometres inland from Aname. This was very much smaller than Anatuojem, but there is an area of wild cane on the opposite bank of the river, the extent of which was not investigated.
Fig. 4.1 General map of the IIKALAU valley and plain, showing the relative locations of the two wall systems.
Fig. 4.2. The wall systems of ANELIO and ANELPAT.
Fig. 4.3 Mid-wall outlines of the enclosures at ITCANWOUMA and ANELPANPEKE
The site that was mapped in detail was the middle valley of the Inwoumkalau river and an adjoining coastal area about 300 metres to the south-east. The map shown in Fig.4.1 is based on Groube's triangulation for the area, supplemented by other observations made independently by both of us. Figs. 4.2 and 4.3 are based on my tape and compass traverses along the various walls in the Anelio/Anelpat area where the stone facings to streams and terraces are built of water-worn boulders, and at Itcanwouma where the enclosures were constructed mainly of coral heads, with some terraces inland faced with river boulders and the creek that separates Itcanwouma and Anelpanpeke also lined with stones.

There were more walls upstream and to the north-east and south-east of the area shown in Fig.4.2, and probably more that we did not discover in the time available, but the concentration and distribution within the area shown are indicative of the sophistication of some of the Aneityumese agricultural systems. The ridge on the north-west is now badly eroded, and there has been a land-slip which probably destroyed some former terraces. Heights measured by stadial readings along two traverses bearing 330°/150° and separated by 83.25 metres, suggest that the small top-most terrace at the western end of the Anelio ridge may have been part of one or other of the two long terraces farther inland, and that the house site with its pathway and associated terraces is a relatively modern renovation or reconstruction.

Almost certainly the river has changed its course, cutting through its retaining walls (walls 4 and 5 of Fig.4.2), and its break through wall 4 may have been the start of a former irrigation channel through to an earthen mound M near the head of the long central wall 8/19. For much of its 170 metres length, this wall faced a terrace 35 to 45 cms. high to the north-west, with a ditch of variable width below it which culminated in a man-made stone-faced channel, very solidly constructed and supported by an earthen bank on the north-west and a triangular mound of rubble on the other side. Half-way along its length, the base of the channel was approximately 8 metres lower than the ground on either side of mound M, which
itself was 3 to 3.5 metres below the height of the river bank, about 35 metres away to the north-east.

From the documentary evidence, taro and breadfruit were the two staple crops on Aneityum, the indigenous varieties of taro being described by MacGillivray (1853 MS p.93) as "the common taro and the larger or horse taro". Yam was also cultivated, although perhaps not in sufficient quantity (if the anonymous seaman-historian was correct) for Aneityum to compete with New Caledonia for what Brookfield & Hart (1971 p.115) described as "the apogee of technical elaboration in both wet- and dry-crop cultivation". However, informants suggested that the wild cane which covered the hill-slopes at Anelio and Anelpat so profusely originated with the canes that were used to support the yam vines. This separation of crops -- yam on the higher, well-drained hill-slopes with sloping terraces, with taro, breadfruit, bananas and coconuts grown (as they still are) on the river banks and flats of the coastal littoral -- appears eminently feasible in the context of Anelio and Anelpat.

Itcanwouma (Fig. 4.3) does not fit this pattern well. The land enclosed by the freestanding walls is sandy, and by now the walls are badly deflated. An informant who had lived for several years at Itcanwouma more than 50 years ago reported that only coconuts were grown around the house in the enclosure; the gardens were farther inland, with mixed crops (including manioc and sweet potatoes introduced by the mission) grown in patches alongside the unfaced creek, and the pigs kept in wooden enclosures not far from the house. Her son (now in his mid- to late-fifties) who spent his childhood there reported that only the inside of the walls was kept in repair, that the outside of the sea wall had already begun to deflate and was not repaired, and that a tidal wave had inundated the whole of Itcanwouma about 25 years ago.

If no more than one household occupied each enclosure and successive houses were built in different parts of the enclosure, it will not be easy to date the period of occupation at this site without excavating the whole of at least one enclosure. In his preliminary excavations in one of them, Groube (unpublished) encountered technical problems because of
the nature of the soil and the speed with which it dried out as the upper layers were removed. Because none of the people who presently own land there claims any in the Anelio/Anelpat area, though Anelpanpeke belongs to the one family still at Anelpat, it might be presumed that the two groups were distinct, but much more extensive excavation will be needed to discover whether all areas were inhabited concurrently in prehistoric times.

Unfortunately, none of this can be tied in to any population figure. The field evidence suggests that the school-house which Geddie called Imkalau may have served three communities to the north of the Anelio ridge as well as Anelio, Anelpat and Itcanwouma and perhaps also Anelpanpeke. Except for the stubborn reluctance of the people to become christians, Imkalau was never designated 'important' by Geddie (as was Ijypthav, for example), but whether it was larger or smaller than the 'average' of 60 to 65 people, or how the total number were spread over the various communities, cannot be assessed. That the whole area was not intensively settled might be presumed from Geddie's (J 13 Feb 1854) account of a visit he made with three of the chiefs "and other natives" in February 1854. "It was some time before we could collect the people. Those who did not wish to see us hid themselves in the bush. Some of them were discovered by our party who went in search of them and were persuaded to come & listen. One man was spied out on a tree". Almost a year later Geddie (J 8 Jan 1855) confessed to sitting in the shade of a tree during this search, contemplating a human skull that was "the remains of a cannibal feast", so he may never have seen any of these walls.

However, he did see and report on the one part of the island for which there is a reasonably firm and unequivocal tally of population, and that is Anumej, which was the objective of our 1973 field survey. Geddie's vivid description (J 23 Jan 1854) of the way in to Anumej on his first visit is as pertinent today as when it was written.
Left home today on a visit ... We landed at Umej, and being joined by a number of natives we went into the interior. We proceeded up the valley that runs back from Umej and after a walk of 3 miles came to Anucci. ... We continued our journey up the valley and after a walk of 4 miles came to Anumej in the heart of the island. The road was dismal and such as I never travelled before. Our only path was the bed of a small but rapid stream. We were obliged to leap from stone to stone a great part of the way. In those places where the water was too deep we ascended the steep sides of the banks, and clinging to roots of trees and assisted by the small projections in the stone we succeeded in getting along. The road was dangerous in many places and we were obliged to proceed with much caution. The mountains rose almost perpendicularly on each side of the narrow stream to the height of several hundred feet. ... Anumej is a pretty level spot in the interior surrounded on all sides by a circular ridge of mountains. It appears to be the crater of an extinct volcano. There are some pyrimidal hills rising in the centre. The scenery is bold and romantic. The only [way] out from the place is the narrow rent or gorge through which we passed.

Within a year a "road" was made, and

Instead of stepping from stone to stone along the bed of a rapid stream at considerable risk, the stones have been gathered and made into a level path along the banks, and wherever the land would admit, the river has been left and the path continued through the bush. The science of road making is but little understood here, but it must now be practised (Geddie, MR 1856 p.421).

Parts of this road still exist, but cyclones and hurricanes have taken a devastating toll, and the few Aneityumese who now visit this now-uninhabited area are as much at home still in the river as on land.

For just how long the upper reaches of the valley have been virtually if not completely abandoned cannot now be ascertained. In 1973 there were no descendants of the traditional
owners from whom permission need be sought for our incursions on to the land at Anumej, and none either from the group who traditionally inhabited the middle area of the Umej river valley between the northern boundary of Umej and the southern boundary of the Anumej district. Although Geddie always referred to this area as Anucci or Anuggi, to all of our informants it was Anekra, and there was no doubt in their minds that there were never more than three groups of people in the valley -- those of Umej, Anekra and Anumej.

Geddie's "narrow rent or gorge" is where the Umej river crosses the southern boundary of the Anumej land. To the west of the river, the boundary follows the crest of a spur leading up to Geddie's "circular ridge of mountains"; to the east the boundary is a river called Nijimahek which flows into the Umej river, but just north of the junction of the two is a tiny parcel of land enclosed (as for fish ponds, perhaps) by sets of stone walls, which formed some part of an exchange between Anumej and Anekra and this area belonged to Anekra. Just when this bargain was struck or what the Anumej people received in exchange is no longer known, and periodic questioning of informants yielded nothing more than that it was perhaps something to do with the rights to fish in the river dividing the two districts.

Even to the grandsons of people who were alive in Geddie's time, it is still known that Anumej was a "big" village, quite different in size from any others on the island. These were usually only small settlements where five or perhaps fewer families lived together, and although some were larger than this, none was as big as Anumej. But not all of the Anumej people lived in the village: some were scattered in small groups throughout the land available to them, some families lived apart from others. Because the land was fertile there was always plenty of food, and the surplus was traded with people lower down the valley. It proved impossible to discover now whether this had been the practice before their conversion to Christianity or only afterwards; and when asked what was received in exchange for the food, all informants acknowledged that they did not know because they had never been told that by their fathers and/or grandfathers.
Certainly none seemed aware that there were ever any differences between the people who lived on the coast and those who lived inland, yet Geddie (MR 1855 p.124) contended more than once that

The natives in the interior are a subdued and despised people. In a physical point of view they are inferior to those who occupy the shore settlements. In the days of heathenism if an inland man ventured out to the shore he was in danger of being killed for cannibal purposes, by his unnatural countrymen or of being thrown into the sea to make sport for them by his dying struggles.

Either Geddie or Inglis told MacGillivray (1853 MS opp. p.90, p.95) that the people on the coast considered those inland "as somewhat inferior, and [were] judged as country folks in towns at home", but when he encountered a party of them in his walk across the island, it seemed to him " - although it might be fancy - that they exhibited much more manliness of character, and a bolder and more independent gait" than the natives of the coast.

Whether they were inferior or not, the "nearly 300 souls" at Anumej constituted "the largest village on the island" (J 23 Jan 1854) or "our largest inland settlement" (MR 1855 p.124). The main part of the village was situated just north of the junction of the Umej and Nalvallo rivers (see Map 2), but there are many indications of dwelling sites southwards alongside the Umej river between the sites of the two mission school-houses.

Our base camp was situated on one such, with the tent pitched on the higher of two terraces in a bend of the river, sheltered by a tree growing on a small terrace behind and within a rectangle of stones only slightly larger than the tent. Behind and below it was a large and gently sloping area of wild cane. Elsewhere there were small complexes of stone-faced terraces and the citrus trees which, it is acknowledged by all, were planted in the wake of Christianity and usually near the houses.

The first of the school-houses was built across from the main village on the east bank of the Nalvallo river, and although it has been used subsequently as a habitation site, in the river just below it there are traces still of the pathway or
Fig. 4.4 Sketch map of ANUMEJ village site
ford that crossed the river from the village, almost
directly below a raised house platform on the western bank.
In January 1856 the second school-house was in course of
errection south of this, at the fork of the Umej and Ijyptaho
rivers. This was one of the "plastered buildings to answer
the double purpose of church and school-house", and the coral
lime and sand for it were carried in from the coast (Patterson,
1882 pp.401-2). The ruins of this building were cleared and
measured; its walls, blown flat in a storm more than 40 years
ago, cover part of the floor area and though the laths have
rotted, their imprint in the plaster remains.

A sketch plan of the main village site as it appeared
in 1973 is given in Fig.4.4 and although more extensive clearing
may have shown more terraces concealed by fern and more
especially cane, such clearing seemed not to be warranted
because there is no indication from any source what proportion
of the "nearly 300" inhabitants lived in that part of the
village. Along the Umej river side of the peninsula there was
a long low stone wall that sometimes faced a low terrace,
sometimes was just a line of separate stones of various sizes,
and probably marked a pathway along that side of the peninsula.
Much of the ground immediately inland from this wall is flat
and still has no ground cover of any description, although the
tree cover is fairly dense and includes many citrus trees.

Close to the end of this pathway was a higher wall
which topped the bank of the Umej river, sheltering two clearly
identifiable house sites. The wall was very solidly built, in
parts almost one metre high, and it ran along the river bank
for about 45 metres. From there it swung inland, curving around
to join with another curved wall, short and almost semi-circular,
which in turn joined with the stone facing of a terrace more
than 60 metres long, between four and five metres wide for most
of its length and about 35 to 40 cms. high. Behind it, on the
Nalvallo side of the peninsula, was slightly higher ground held
back from the terrace with a stone facing that ran for most of
the length of the terrace and varied in height according to the
contours of the land.
Between this terrace and the river wall was a mound, also stone-faced on the side parallel with the terrace and for part of its length on the river side, but this facing petered out as the gradient of the land changed, both towards the bank of the river and to the higher ground to the north. Excluding the two large boulders at its northern end, the mound was nearly 20 metres long and its retaining wall almost one metre high near the boulders. At the southern end of the mound, there was a pathway marked out in large stones which ran roughly parallel to the wall along the river bank, with the ground between the pathway and this wall about 20 centimetres higher than the path and the land to the east between the path and the long terrace. It was on the higher ground that the house outlines were still visible, and the lower ground was divided into plots by rows of stones, roughly at right angles to the terrace. There was enough difference in the relative heights of the three plots at the northern end to suggest that the mound had been created to channel water through to the plots lower down where its run-off was checked by a low, saddle-shaped mound of earth connecting the lower end of the terrace to a large boulder.

To the north of the village there was a large area of wild cane, but to the east of the long terrace the ground was stony, with fern and tree cover, which suggested that it had not been much used for cultivation. In this part there were occasional lines of stones larger than the ones lying on or embedded in the ground, but there were no clear patterns to suggest habitation sites. There was, however, the raised house platform just opposite the site of the first schoolhouse, with two narrow terraces between it and the river and a plot of ground clear of stones except to define it and a pathway, on the western side of the platform. The platform itself was constructed of stones, not coral, but tradition does not relate whether this belonged to the prehistoric phase of Anumej or dates only from the arrival of Christianity. In common with the schoolhouses, there are traces still of the logs that were part of the foundations for the superstructure, so it is a fair bet
that it dates from the post-contact era, and that in prehistoric times the village was concentrated on the other side of the peninsula.

A reasonable interpretation of this in terms of both documentary and archaeological evidence is that the northern part of the village site was where the chief, Karahi, lived with his taro terraces close by and other crops grown on the land to the north which is now covered in cane. His sacred grove may have been on the Nalvallo side of the peninsula above his taro terraces, and when he and his people accepted Christianity this was the land given to the teachers - "choice men, to show that however much they might be looked down on by others they [the people of Anumej] were not despised by us" (Geddie, MR 1855 pp.124-5) -- for their house and garden.

The other people who lived in the village may have grown some of their food there or their gardens may have been elsewhere, but accepting (as one must from all of the evidence presented above) that the stone walls buttressing terraces, and the presence of wild cane that may conceal many more terraces, are significant indicators of former cultivation in a mixed crop subsistence economy, ground observations in the area occupied by the Anumej people demonstrated the formidable task it would be for amateur surveyors to map and plot the area available for cultivation by this group of people. There are terraces or wild cane no matter how far upstream one goes in the Umej river; along the tributaries to the east -- Nalvallo, Ijyptaho, Nijowoik and their tributaries -- our Aneityumese needed up to two hours' walking to get to the point beyond which there was neither wild cane nor stone walls.

However, the map (of scale 1: 100,000) prepared by the Institut Géographique National, Paris showed the heights of three peaks around the encircling rim of the valley, and three points from which these and other peaks were visible were discovered and cleared to establish trig stations. The angles of inclination from them to the peaks, and the horizontal angles for each were measured, as were the angles from each station to the other two and to a clump of three Norfolk pines (Araucaria heterophylla) growing near our base camp, and from one of the stations to a point on
the bank of the Umej river, also close to the base camp. The distance between this river-bank station and the Norfolk pines (which were almost 30 metres high) was the only one that it was feasible to measure accurately, and this was done to provide a scale for the horizontal distances to be inferred from the above map. With these and the angles of inclination to the peaks, the relative heights of the trig stations and other features could be obtained, and from these one could then make inferences about the altitudes at which land had previously been cultivated, and thence estimate roughly from the contours on the map just how much land was available. For additional control, there were also tape and compass traverses of more than one kilometre of the Umej river and parts of two of its tributaries, the Nalvallo and Ijyptaho rivers.

After the field work was completed, all except these last observations were made redundant by the contour map (Map 2) prepared for us by an experienced photogrammetrist from aerial photographs supplied by the U.K. Ministry of Defence. The runs were flown by the R.A.F. in August 1972, and although we had re-photographed prints with us in the field in 1973, there had not been time before we left for the field to explore the skills and equipment available in Canberra for the production of such a map. There is, however, considerable (and needful) overlap between the photogrammetrist's interpretation of the aerial photographs and our ground observations. The positions of our trig stations, for example, and their relative heights accord well with his contours; the open patches and areas with light, or medium to heavy secondary growth which he detected and mapped fit the ground observations in the areas that we explored.

The river courses that were charted are shown as solid lines on the map, and probably some of the dashed lines there indicate streams that flow only after heavy rains, or perhaps only valleys or gullies. There is, for example, no tributary into the Umej river that was considered worthy of a name between the Nijimapek and Ijyptaho rivers, nor is there any stream flowing into the Umej river from the west between our base camp and the southern boundary of Anumej land. However, there are two more tributaries west of the Nalvallo river which have names: the
Niwa river flows into the Umej river from the north, and farther west the Nepedraili enters from the south or south-west, but these cannot be positively identified on the map. The named water-courses are presumably the more permanent streams, but there are many small creeks and rivulets feeding in to them and even in the so-called 'dry' season, one is forever wading or jumping across water-courses.

The combination of the ground observations and the contour map prepared from the aerial photographs enables one to assess more confidently than would be possible from either alone that there was a concentration of cultivation on the relatively flat land on either side of the Umej river, but that the land in the north-eastern quarter of the area was also cultivated, although perhaps less intensively because of the relatively steeper slopes. On the whole, it would seem that cultivation was feasible up to an altitude of perhaps as much as 400 metres above sea level, but most of it was probably contained within the 260 metre contour, shown here in Map 2 by the heavy line. Except on the extreme east, this contour would place most of the area of settlement within a radius of one kilometre of Anumej village. The clear or open patches higher than 400 metres on the western side of the area might well be the small upland swamps where taro was also grown and which are reported to be numerous high up near the western rim of the basin, and also farther west on the island near the saddle peak Inrero.

For a conservative estimate of the amount of land available for cultivation to the people of Anumej, the area enclosed by the 260 metre contour was measured by planimeter and the average of two measurements indicated a total area of 300 hectares. If the higher altitudinal limit of 400 metres is used, the estimate of the area enclosed is necessarily approximate because this contour line is incomplete and the southern boundary of Anumej land beyond the source of the Nijimapek river is not known. Treating both areas -- that included within the 260 metre contour and that below 400 metres -- as ellipses, the extension from one to the other would increase the area of land
available to the people of Anumej by at least 50 per cent, and perhaps by as much as 75 per cent. Although the tops of the ridges and spurs within these areas may not have been used for cultivation, the reduction of the area to allow for this would be minimal because terraces have been constructed high up the slopes, sometimes very close to the top.

For a population of nearly 300 of all ages, the conservative estimate of area yields at least 1 hectare (or 2.5 acres) per head, and the less conservative, but still feasible, estimate between 1.5 and 1.75 hectares (or from 3.7 to 4.3 acres) per head. If these altitudinal criteria are applied to the whole island, planimeter measurements on the map referred to above (p.117) indicate at least 9,300 hectares up to 260 metres above sea level, and 12,600 hectares up to 400 metres above sea level. Ignoring the variable fertility of the land reported by Mrs Geddie (MR 1856 p.276), the total population of the island might then be inferred as more than 9,000 or alternatively, between 7,200 and 8,400 people. At the time when Anumej contained nearly 300 people, the total population reported was about half the lowest of these estimates.

Clearly, the Anumej basin was not 'typical' of the island as a whole, and although on numbers alone, its population had a better chance of being 'representative ' of the total population than did any smaller community, an important proviso for its representativeness is that the mortality and effective fertility in the Anumej community were similar to those elsewhere on the island. As the crow flies, the southern boundary of their district is about 5 kilometres from the coast, and all of the area is more than 100 metres above sea level. Nowadays, in the winter at least, there are no mosquitoes and few flies; there are ticks and they are especially troublesome whenever the wild cane is cut to make tracks or traverses, but whether these are vectors of disease or not is not known.

It is not inconceivable that because of the altitude, the people of Anumej were free of the "fever and ague" that plagued the Europeans and the Aneityumese who lived in the low-lying coastal areas; but whether they were any more so than the groups living on high ground elsewhere -- in Anau-unse, for example -- is
debatable and there is no way of knowing how much these latter contributed to the population total. Cultural practices such as the strangling of widows and infanticide were not mentioned by Geddie in relation to Anumej, and as he never divided its "nearly 300 souls" into males and females, there is no way of knowing whether this allegedly despised people differed in this respect either. Hence, to infer similarity in the structure of Anumej's population with any that might be presumed for the total population involves assumptions that may not be valid.

However, if our informants were correct in their assertions about the uniformity of Aneityumese culture before Christianity was introduced, and certainly the language was the same throughout the island, a first approximation to the age and sex structure of the community at Anumej might be obtained by re-scaling the four population models re-introduced at the end of Chapter 3 (p.105-6). The model $B^*$ assumes no infanticide of males and allows only 75 per cent of females born to survive; in $D$, $E$ and $F$ five per cent of the males born are disposed of, and the rate of infanticide for females increases from 20 per cent in $D$ to 25 per cent in $E$ and 30 per cent in $F$. The numbers of males and females in broad age ranges in a population group of 300 according to each of these models are shown in Table 4.1, but all of these numbers could be rounded to the nearest 0 or 5 with no loss of accuracy.

Table 4.1: Numbers of males and females at specified ages in a group of 300 people according to each population model.

<table>
<thead>
<tr>
<th>Age Range (years)</th>
<th>$B^*$</th>
<th>$D$</th>
<th>$E$</th>
<th>$F$</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>M</td>
<td>F</td>
<td>M</td>
<td>F</td>
</tr>
<tr>
<td>Less than 15</td>
<td>68</td>
<td>51</td>
<td>57</td>
<td>46</td>
</tr>
<tr>
<td>15-19</td>
<td>18</td>
<td>14</td>
<td>16</td>
<td>13</td>
</tr>
<tr>
<td>20-49</td>
<td>73</td>
<td>53</td>
<td>76</td>
<td>61</td>
</tr>
<tr>
<td>50 +</td>
<td>12</td>
<td>11</td>
<td>.15</td>
<td>.16</td>
</tr>
<tr>
<td>Total</td>
<td>171</td>
<td>129</td>
<td>164</td>
<td>136</td>
</tr>
<tr>
<td>15 +</td>
<td>181</td>
<td>197</td>
<td>202</td>
<td>207</td>
</tr>
</tbody>
</table>
Except in B*, the numbers of females aged 15 years or more in each model are approximately equal and they represent about 30 per cent of each population: in B* they contribute little more than one-quarter of the total. Their numbers are at least matched by the numbers of males aged 20 years and over in all four models, and although in B* persons aged 15 years or more constitute only 60 per cent of the total, they account for about two-thirds of this total in the other three models. Given the random fluctuations likely in the sex ratio of births in a population of this size, there would probably be little to differentiate between any of the three models which permit male infanticide, though the distinction between them and B* would probably remain.

Apart from the marginal differences in the numbers of 'active' adults available for the cultivation and collection of food in each of the population models, and in the total quantity of food that needed to be grown to feed the populations in which the proportion of adults is highest, these numbers are less relevant to the conclusion of this exercise than they are to what follows in Chapter 5. The significant point here is that inferences based on even the largest settlement on the island, in terms of its use of the land available to it, could be so grossly misleading when applied to the island as a whole. Had there been no more information about Aneityum's population than that Anum ej contained nearly 300 people, extrapolation from the criteria established for them to the whole island would have exaggerated the size of the population. The smallest estimate would have doubled it, the largest would give a number between 2.5 and 3 times the population that was there.
CHAPTER 5

THE DEMOGRAPHIC AND ARCHAEOLOGICAL CONSEQUENCES OF EPIDEMICS

At the end of Chapter 3 (p.105) four of the population models which were introduced in Chapter 1 (p.11-4) and Fig.1.1 were re-introduced briefly to help elucidate the roles that various diseases might have played in the course of change in Aneityum's population in the latter half of the 19th century. Two of the original six models depicted in Fig.1.1 were discarded because they did not duplicate the distorted sex ratio and comparative scarcity of children which had been reported for Aneityum's population, and one of the remaining four -- the stable population B* -- was modified so as to give a total of 2,000 males and 1,500 females. Rescaled in this way, B* is no longer a stable population without infanticide, but is instead a population with 25 per cent of female infanticide. Each of D, E and F assume 5 per cent of infanticide of males, with the rate for females increasing from 20 to 25 to 30 per cent respectively. In relation to all births irrespective of sex, these various combinations produce infanticide rates of approximately one in eight births for B* and D, one in less than seven births in E and one in six for F.

As always with infanticide, it is impossible to estimate the extent to which it was practised on Aneityum during the late phase of its prehistory. At first Geddie (MR 1852 p.19) believed it was "not so prevalent here as on many of the neighbouring islands, yet it is by no means uncommon". Later, when the first 'census' showed 600 more males than females he wrote that infanticide was fearfully prevalent in the days of heathenism. It was practised on both sexes but female children were commonly the victims. The most common modes of putting children to death were to carry them to the bush and leave them to perish there, or place them on the sea shore to be swept away by the flowing tide. Sometimes persons who had no children of their own have on finding infants thus exposed taken them and adopted them and thus a few have been rescued from death. But alas! the few who have been saved when consigned to destruction by their unnatural
Fig. 5.1 Population pyramids illustrating the changes in the size and structure of each of the four populations before epidemics and 50 years after the first epidemic and the cessation of infanticide.
parents bear no proportion to the number who have perished. The reason assigned by the natives for this inhuman practice is the trouble of bringing up the children (MR 1855 p.125).

At about the same time, Mrs Inglis (MR 1855 p.184) also reported that "on enquiry we have found out that infanticide has prevailed to a much greater extent on this island than we had any thought of", while her husband (Inglis, 1887 p.274) recounted vividly how

On one occasion ... a woman near us gave birth to a daughter, that being the third daughter she had born in succession. When she learned that it was a daughter and not a son, ... she cried out to the women beside her, "Oh, kill it, kill it!" The natives around her were Christian, and hence her request was refused.

As Brenchley (1873 p.231) commented so trenchantly in a footnote -- "As usual -- black yesterday, white today".

In simulating the demographic impact of two major epidemics in which the mortality among people (and especially females) of reproductive age was as high as the evidence from Aneityum indicates (vide pp.105-6 above), and a third epidemic which affected only children (and which, for convenience of computations, was made simultaneous with the second epidemic) in each of the population models, it was assumed that infanticide stopped just before the first epidemic. In all of the models this sequence of epidemics virtually halved the birth rate for at least a decade, and usually left the population in a precarious state vis-à-vis likely increase in size for another five years at least. This closely resembled what had been reported for the Aneityumese population during this period. After a brief respite when the birth rate might have exceeded the death rate significantly, the margin between the two narrowed again and the likelihood of increase in size diminished accordingly.

In the simulations each of the population models experienced identical age-specific mortality and fertility rates, and the only difference between them was their demographic structure at the time of the first epidemic. The difference between this and their structure 50 years later is illustrated in Fig.5.1, or less dramatically in Table 5.1 which gives the numbers
TABLE 5.1: Numbers of males and females in broad age ranges in each population model at the end of 1860 before the first epidemic and 50 years later, assuming the cessation of infanticide in 1860

<table>
<thead>
<tr>
<th>Age Range</th>
<th>Males 1860</th>
<th>Males 1910</th>
<th>Females 1860</th>
<th>Females 1910</th>
<th>Persons 1860</th>
<th>Persons 1910</th>
</tr>
</thead>
<tbody>
<tr>
<td>POPULATION B*</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Under 15</td>
<td>795</td>
<td>460</td>
<td>590</td>
<td>447</td>
<td>1385</td>
<td>907</td>
</tr>
<tr>
<td>15-29</td>
<td>580</td>
<td>342</td>
<td>430</td>
<td>318</td>
<td>1010</td>
<td>660</td>
</tr>
<tr>
<td>30-49</td>
<td>485</td>
<td>259</td>
<td>350</td>
<td>235</td>
<td>835</td>
<td>494</td>
</tr>
<tr>
<td>50 &amp; over</td>
<td>140</td>
<td>151</td>
<td>130</td>
<td>130</td>
<td>270</td>
<td>281</td>
</tr>
<tr>
<td>TOTAL</td>
<td>2000</td>
<td>1212</td>
<td>1500</td>
<td>1130</td>
<td>3500</td>
<td>2342</td>
</tr>
</tbody>
</table>

| POPULATION D |
| Under 15 | 660        | 438        | 535          | 430          | 1195        | 868         |
| 15-29    | 540        | 312        | 445          | 307          | 985         | 619         |
| 30-49    | 533        | 239        | 425          | 228          | 958         | 467         |
| 50 & over| 177        | 123        | 185          | 116          | 362         | 239         |
| TOTAL    | 1910       | 1112       | 1590         | 1081         | 3500        | 2193        |

| POPULATION E |
| Under 15 | 645        | 410        | 500          | 402          | 1145        | 812         |
| 15-29    | 555        | 294        | 430          | 287          | 985         | 581         |
| 30-49    | 562        | 226        | 420          | 215          | 982         | 441         |
| 50 & over| 198        | 119        | 190          | 108          | 388         | 227         |
| TOTAL    | 1960       | 1049       | 1540         | 1012         | 3500        | 2061        |

| POPULATION F |
| Under 15 | 633        | 374        | 458          | 365          | 1091        | 739         |
| 15-29    | 565        | 267        | 405          | 264          | 970         | 531         |
| 30-49    | 600        | 208        | 422          | 198          | 1022        | 406         |
| 50 & over| 217        | 117        | 200          | 98           | 417         | 215         |
| TOTAL    | 2015       | 966        | 1485         | 925          | 3500        | 1891        |
of males and females in broad ranges of age in each model at the end of 1860 and 1910. Without the infanticide that created the differences between them initially, the female segment of each population grew much faster than the male and although there were fewer females than males at each age, after 50 years the numbers were reasonably well-matched in all age ranges as well as in the total.

It is the increase in the female segment that dominates the recovery of each population towards its initial size. Fifty years after the cessation of infanticide, the number of females in population B* is already three-quarters of the number that had been there before the first epidemic, but this proportion decreases progressively down to only 62 per cent in population F. For males the recovery towards their former number is also variable, ranging from 60 per cent in B* to only 48 per cent in F. As the only difference between the population models was in their initial age and sex structure, this alone is responsible for the different rates of increase which, after 50 years, leaves population F only 4/5 of the size of B*, and D and E respectively 94 and 88 per cent of the size of B*.

The changes in age structure are perhaps more apparent in the population pyramids of Fig 5.1 than in the condensed age distributions given in Table 5.1. By 1910 the survivors of the original population are aged 50 years or more, and in B* the numbers of both males and females aged between 50 and 59 years exceeded the numbers at these ages in 1860. At ages less than 50 years there are the small cohorts born in the decade after the first epidemic and aged between 40 and 49 in 1910, and the impact of their small numbers on the births in the late 1880's and early 1890's is apparent in the "waist" in all four pyramids, but especially B*, at ages 15 to 24 years in 1910. By comparison with the age and sex structure in 1860, there is clearly a higher proportion of children aged less than 15 years in 1910 in all populations except B*.

Returning to our original equation of the number of households in a population with the number of females aged 15 years and over (p.14-5), how do these change with the change in population structure between 1860 and 1910? The numbers are given
### TABLE 5.2: Numbers of females aged 15 years and over and total population in each population model in 1860 and 1910, (assuming cessation of infanticide in 1860) and estimated average size of households

<table>
<thead>
<tr>
<th>Population model and year</th>
<th>Females aged 15 years or more</th>
<th>Total population</th>
<th>Estimated average household size</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>B</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1860</td>
<td>910</td>
<td>3500</td>
<td>3.85</td>
</tr>
<tr>
<td>1910</td>
<td>683</td>
<td>2342</td>
<td>3.43</td>
</tr>
<tr>
<td><strong>D</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1860</td>
<td>1055</td>
<td>3500</td>
<td>3.32</td>
</tr>
<tr>
<td>1910</td>
<td>651</td>
<td>2193</td>
<td>3.37</td>
</tr>
<tr>
<td><strong>E</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1860</td>
<td>1040</td>
<td>3500</td>
<td>3.37</td>
</tr>
<tr>
<td>1910</td>
<td>610</td>
<td>2061</td>
<td>3.38</td>
</tr>
<tr>
<td><strong>F</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1860</td>
<td>1027</td>
<td>3500</td>
<td>3.41</td>
</tr>
<tr>
<td>1910</td>
<td>560</td>
<td>1891</td>
<td>3.38</td>
</tr>
</tbody>
</table>

### TABLE 5.3: Numbers of persons aged 15 years and over, and total population in each population model in 1860 and 1910 (assuming cessation of infanticide in 1860); percentage of total aged 15 years or more, and ratio between total and adults

<table>
<thead>
<tr>
<th>Population model and year</th>
<th>Persons aged 15 years or more</th>
<th>Total population</th>
<th>Percentage of total aged 15+ years</th>
<th>Ratio of total pop. to nos. aged 15+</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>B</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1860</td>
<td>2115</td>
<td>3500</td>
<td>60</td>
<td>1.65</td>
</tr>
<tr>
<td>1910</td>
<td>1435</td>
<td>2342</td>
<td>61</td>
<td>1.63</td>
</tr>
<tr>
<td><strong>D</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1860</td>
<td>2305</td>
<td>3500</td>
<td>66</td>
<td>1.52</td>
</tr>
<tr>
<td>1910</td>
<td>1325</td>
<td>2193</td>
<td>60</td>
<td>1.66</td>
</tr>
<tr>
<td><strong>E</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1860</td>
<td>2355</td>
<td>3500</td>
<td>67</td>
<td>1.49</td>
</tr>
<tr>
<td>1910</td>
<td>1249</td>
<td>2061</td>
<td>61</td>
<td>1.65</td>
</tr>
<tr>
<td><strong>F</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1860</td>
<td>2409</td>
<td>3500</td>
<td>69</td>
<td>1.45</td>
</tr>
<tr>
<td>1910</td>
<td>1152</td>
<td>1891</td>
<td>61</td>
<td>1.64</td>
</tr>
</tbody>
</table>
in Table 5.2 and there is in fact very little difference in the ratio between the numbers of adult females in 1910 and 1860 and the ratio between the size of the populations at these dates in any of the population models except B*. In this the number of females aged 15 years or more increased more by 1910 than did the total population, and therefore the average size of household was smaller then than it had been in 1860.

The increased proportion of children in all of the populations except B* means that the numbers of persons who might be considered "economically active" decreased relative to the total numbers to be supported, and any advantages that populations such as D, E and F had over B* in this respect in 1860 had disappeared by 1910. The numbers are given in Table 5.3 and for 1910 there is remarkable uniformity in the relative numbers of adults in all four population models. However, only in B* are the ratios between the numbers of persons aged 15 years or over in 1910 and 1860 commensurate with those between population size at these dates: in E, for example, the work force in 1910 was less than half the number that had been there in 1860, whereas the population to be supported per head had increased from 1.45 in 1860 to 1.64 in 1910.

At this point it is reasonable to ask what would have happened in these populations if infanticide had continued despite the epidemics? How would they compare in size and characteristics with what they would have been if there had been no epidemics? As the critical level of infanticide for this set of models is between 20 and 25 per cent of all females born (vide p. 13 ), only population D would increase over a 50-year interval free of epidemics and the other three would decrease, B* and E at an average rate of 0.16 per cent per year, F at 0.38 per cent. For D the rate of increase is 0.07 per cent per year.

Fig.5.2 shows the population pyramids for each simulation model at the same date, the only difference between the inner and outer pyramids being that the populations represented by the shaded areas experienced the first of three epidemics within five years 50 years previously, whereas those depicted by the outer pyramids suffered none. The numbers of males and females in broad ranges of age in each model according to whether there
Fig. 5.2 Population pyramids illustrating the changes in the size and structure of each of four populations before epidemics and 50 years later when infanticide was still being practised.
TABLE 5.4: Simulation of the effects of epidemics in each population model assuming the continuation of infanticide for 50 years from the date of the first epidemic: numbers of males and females in broad age ranges had there been no epidemics, and the numbers had the population experienced all three

<table>
<thead>
<tr>
<th>Age Range</th>
<th>Males</th>
<th></th>
<th>Females</th>
<th></th>
<th>Persons</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>None</td>
<td>All</td>
<td>None</td>
<td>All</td>
<td>None</td>
</tr>
<tr>
<td>POPULATION B*</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Under 15</td>
<td>733</td>
<td>345</td>
<td>544</td>
<td>255</td>
<td>1277</td>
</tr>
<tr>
<td>15-29</td>
<td>535</td>
<td>302</td>
<td>396</td>
<td>221</td>
<td>931</td>
</tr>
<tr>
<td>30-49</td>
<td>447</td>
<td>254</td>
<td>323</td>
<td>177</td>
<td>770</td>
</tr>
<tr>
<td>50 &amp; over</td>
<td>129</td>
<td>151</td>
<td>120</td>
<td>130</td>
<td>249</td>
</tr>
<tr>
<td>TOTAL</td>
<td>1844</td>
<td>1052</td>
<td>1383</td>
<td>783</td>
<td>3227</td>
</tr>
<tr>
<td>POPULATION D</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Under 15</td>
<td>683</td>
<td>329</td>
<td>554</td>
<td>272</td>
<td>1237</td>
</tr>
<tr>
<td>15-29</td>
<td>559</td>
<td>273</td>
<td>461</td>
<td>225</td>
<td>1020</td>
</tr>
<tr>
<td>30-49</td>
<td>552</td>
<td>225</td>
<td>440</td>
<td>182</td>
<td>992</td>
</tr>
<tr>
<td>50 &amp; over</td>
<td>183</td>
<td>123</td>
<td>191</td>
<td>116</td>
<td>374</td>
</tr>
<tr>
<td>TOTAL</td>
<td>1977</td>
<td>950</td>
<td>1646</td>
<td>795</td>
<td>3623</td>
</tr>
<tr>
<td>POPULATION E</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Under 15</td>
<td>595</td>
<td>287</td>
<td>462</td>
<td>219</td>
<td>1057</td>
</tr>
<tr>
<td>15-29</td>
<td>512</td>
<td>249</td>
<td>396</td>
<td>192</td>
<td>908</td>
</tr>
<tr>
<td>30-49</td>
<td>517</td>
<td>214</td>
<td>386</td>
<td>159</td>
<td>903</td>
</tr>
<tr>
<td>50 &amp; over</td>
<td>182</td>
<td>119</td>
<td>176</td>
<td>108</td>
<td>358</td>
</tr>
<tr>
<td>TOTAL</td>
<td>1806</td>
<td>869</td>
<td>1420</td>
<td>678</td>
<td>3226</td>
</tr>
<tr>
<td>POPULATION F</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Under 15</td>
<td>516</td>
<td>246</td>
<td>374</td>
<td>177</td>
<td>890</td>
</tr>
<tr>
<td>15-29</td>
<td>461</td>
<td>221</td>
<td>330</td>
<td>160</td>
<td>791</td>
</tr>
<tr>
<td>30-49</td>
<td>489</td>
<td>198</td>
<td>345</td>
<td>136</td>
<td>834</td>
</tr>
<tr>
<td>50 &amp; over</td>
<td>178</td>
<td>117</td>
<td>163</td>
<td>98</td>
<td>341</td>
</tr>
<tr>
<td>TOTAL</td>
<td>1644</td>
<td>782</td>
<td>1212</td>
<td>571</td>
<td>2856</td>
</tr>
</tbody>
</table>
were epidemics or not are given in Table 5.4, and population B* is the only one for which the population 50 years after the first of the epidemics is more than half the size that could be expected had there been no epidemics.

It is also the only one of the four in which the numbers aged less than 15 years relative to the total are very different in the population which experienced the epidemics from that which did not -- 33 per cent as against 40. For all of the other population models the occurrence of the epidemics did not change either the average size of household or the ratio between the total population and the number that might be designated as the economically active sector. Each of the populations that experienced the epidemics shows the same "waist"-ing as a direct consequence of the high epidemic mortality on the number of births for the ensuing decade, and a secondary effect a generation or so later when the small numbers of females born in the post-epidemic decade dominated the reproductive sector of the population. Echoes of this would recur with diminishing amplitude in subsequent generations.

The relatively short term effects of the epidemics on the rate of change in the size of the various populations which experienced the epidemics are shown in Table 5.5.

Table 5.5 The effects of epidemics on the average annual rates of change in each population model over a 50-year interval.

<table>
<thead>
<tr>
<th>Population model</th>
<th>Post-epidemic number</th>
<th>No. in 1910</th>
<th>Average annual rate of change (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>with</td>
<td>without</td>
</tr>
<tr>
<td>B*</td>
<td>1,789</td>
<td>1,835</td>
<td>+0.06</td>
</tr>
<tr>
<td>D</td>
<td>1,733</td>
<td>1,745</td>
<td>+0.02</td>
</tr>
<tr>
<td>E</td>
<td>1,721</td>
<td>1,547</td>
<td>-0.24</td>
</tr>
<tr>
<td>F</td>
<td>1,698</td>
<td>1,353</td>
<td>-0.50</td>
</tr>
</tbody>
</table>

It will be remembered that the size of the population in each model before the first epidemic was 3,500 and that each experienced the same mortality rates specific for age and sex both in the epidemics and in the interval between them. The deviations in the simulation models from the halving of the population as
reported for the Aneityumese after the epidemics is due to the differences between them initially in respect of their age and sex structure, and does not invalidate the comparison of their sizes after the epidemics and 50 years after the first of these. Again throughout this interval each of the populations was subjected to the same schedules of mortality and fertility, "schedule" being used here to describe the set of age-specific mortality rates for each sex or the set of age-specific fertility rates for females.

Within this interval, the incidence of epidemics in populations $E$ and $F$ aggravated their average annual rates of decrease, in $D$ the average rate of increase per year was reduced, but in $B^*$ the expected average rate of decrease of 0.16 per cent per year was translated into an average annual increase of 0.06 per cent. In the simulations at least another 50 years were needed to restore the former rates of change for each of the populations which experienced the epidemics, and throughout this interval it was assumed that infanticide continued at its former level, that the mortality and fertility schedules remained the same as those on which the simulation models were all based, and that there was no further disruption by epidemics. The only way in which any such demographic sequence might be reflected archaeologically would be in the extent of occupation of the sites of settlements or villages that existed before the first epidemic.

To examine this it is convenient to return to Aneityum as it was before the first epidemic. The population was then divided into a number of communities, the precise number of which is not known, but in 1859 there were 56 schoolhouses (Turner, 1861 p.476) attended each weekday morning for an hour after sunrise by the whole of the population. If we identify villages with schoolhouses, in some cases at least we are combining several smaller communities or hamlets. The schoolhouse at the place Geddie called Imkalau, for example, was situated near the mouth of the river Inwoumkalau and as already stated (p.110), it probably served six or seven hamlets - three to the north of the river, and either three or four to the south, the doubt arising because the most southerly group was nearer the schoolhouse at Iteng than the one called Imkalau.
The numbers of people that constituted such villages are known for only four places: there was Anumej which contained "nearly 300 souls"; on the south coast was Anauyac which may have had 50 inhabitants; on the northern half of the island there was an un-named inland village which Inglis considered "important" and it contained "upwards of 80 men, women, and children", and there were three hamlets in the vicinity of Aname which contained a total of 120 people who attended the school at Aname. However, if the total of 3,500 people was distributed over 56 villages, the average number of people per village was 62.5. If Anumej is excluded this average falls to 58 inhabitants per village.

To convert numbers such as these into numbers of households per village we must fall back again on the population models. If the number of women aged 15 years and over is used to define households, the average size of households in each of the models $B^*$, $D$, $E$ and $F$ is respectively 3.8, 3.3, 3.4 and 3.4. If, because of the distorted sex ratio, households are defined by the numbers of males aged 20 years and over, the average size of households in each population model decreases progressively in steps of 0.2 from 3.5 persons in $B^*$ to 2.9 in $F$. Hence in $B^*$ there are either 16 or 18 households per village of 62.5 people; in $D$ 19 by both definitions; in $E$ either 18 or 20; and in $F$ either 18 or 21. For the smaller average number of 58 inhabitants, the number of households ranges from 15 to 20 instead of 16 to 21 as above.

The frequency distribution of villages by size is not known, but given the equation of villages with mission school-houses, it is reasonable to assume that no village contained fewer than 10 households initially. Depending on the model chosen to represent the population, villages in the size class of 10-14 households would contain from 36 to 44 people on the average; those with 15-19 households would have from 51 to 61 inhabitants, and those with 20-24 and 25-29 households from 65 to 79 and 80 to 96 respectively. In each case the smallest number of inhabitants is associated with population $F$, and the largest with $B^*$. Whatever the actual distribution of villages by size in the late phase of Aneityum's prehistory, these four classes probably covered the majority of villages.
TABLE 5.6: Estimated numbers of people in villages of each size in each population before and after two epidemics, and the percentage change likely in their size

<table>
<thead>
<tr>
<th>Number of households per village</th>
<th>Number of People in Villages</th>
<th>Percentage change</th>
<th>Minimum</th>
<th>Maximum</th>
<th>Minimum</th>
<th>Maximum</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Before epidemics</td>
<td>After epidemics</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Minimum</td>
<td>Maximum</td>
<td>Minimum</td>
<td>Maximum</td>
<td>Minimum</td>
<td>Maximum</td>
</tr>
<tr>
<td>10-14</td>
<td>9</td>
<td>73</td>
<td>9</td>
<td>73</td>
<td>9</td>
<td>73</td>
</tr>
<tr>
<td>15-19</td>
<td>16</td>
<td>69</td>
<td>16</td>
<td>69</td>
<td>16</td>
<td>69</td>
</tr>
<tr>
<td>20-24</td>
<td>18</td>
<td>66</td>
<td>18</td>
<td>66</td>
<td>18</td>
<td>66</td>
</tr>
<tr>
<td>25-29</td>
<td>21</td>
<td>62</td>
<td>21</td>
<td>62</td>
<td>21</td>
<td>62</td>
</tr>
<tr>
<td>POPULATION B*</td>
<td>44</td>
<td>12</td>
<td>40</td>
<td></td>
<td>9</td>
<td>73</td>
</tr>
<tr>
<td>POPULATION D</td>
<td>41</td>
<td>10</td>
<td>37</td>
<td></td>
<td>10</td>
<td>76</td>
</tr>
<tr>
<td>POPULATION E</td>
<td>39</td>
<td>9</td>
<td>34</td>
<td></td>
<td>13</td>
<td>77</td>
</tr>
<tr>
<td>POPULATION F</td>
<td>36</td>
<td>9</td>
<td>32</td>
<td></td>
<td>11</td>
<td>75</td>
</tr>
</tbody>
</table>
To assess the effects of the epidemics on villages of each size, one cannot assume that in groups as small as these, the mortality rates that were estimated for the total population in the various epidemics (irrespective of population model) are applicable except by allowing them to vary within limits that are determined by the size of the population at risk. This involves very tedious arithmetic computations, first reducing the average numbers of people in villages of each size class in each population model to approximate numbers at the relevant ages, and then calculating the standard errors of the various mortality rates (assuming, as is customary for rates, that these are distributed binomially) for the numbers at risk for each rate.

Using the survival rate plus or minus twice its standard error to indicate the likely minimum and maximum numbers of survivors from the first epidemic, the proportions of marriages likely to survive can be estimated to assess the numbers of births that might occur before the second major epidemic five years after the first. For simplicity, the third epidemic which affected only children was ignored. The survival rates for the second outbreak must again be allowed to vary within the range of four times their standard errors given the numbers of people concerned, and ideally there should be some allowance for mortality at ages other than infancy in the years between the epidemics. However, this would be insignificant compared with the epidemic mortality, especially when no account can be taken of migrations consequent on the first epidemic.

Hence the figures shown in Table 5.6 may under-estimate the changes in the numbers of people in villages of each size as a result of the two major epidemics. In the smallest villages, only about one-quarter of their inhabitants may have survived if the worst happened in both epidemics; if they suffered lightly in both, their numbers may have decreased by only 10 per cent. The range between "best" and "worst" narrows as the size of the village increases, but even for the largest shown here, about two-thirds of their inhabitants may have died in the epidemics. Irrespective of the population model used, all of the minimum numbers are smaller than the smallest of the villages before the epidemics, and only in villages which contained more than 20
households initially is the mid-point of the range between minimum and maximum larger than the smallest number before the epidemics.

These numbers of people cannot be easily reconverted into numbers of households because the initial epidemic may have left no marriages intact even in the largest villages. At best, only half of the marriages in these survived the epidemic, but this fraction increased as the size of village diminished, so that almost two-thirds of marriages in the smallest villages may have survived. The chances of re-marriage within a village group would probably be better in the larger villages than in the smaller ones where the few survivors might have been biologically related or within some proscribed degree of relationship, but this introduces too many imponderables for simple models.

However, not all of the men widowed by the epidemic could have remarried if the mortality among adult females was relatively greater than for the males. A very rough guide to the extent to which re-marriage was possible for males widowed by the epidemic, and marriage for those who had not previously been married, can be obtained from the population models. If one equates the number of marriages in existence before the epidemic to the number of females aged 15 years and over, this is only slightly smaller in all four population models than the number of males aged 20 years or more, and if marriage was restricted to people of these ages, more than 90 per cent of males in all populations except F could have been married. In F the proportion was 87 per cent.

Ignoring, for simplicity, the different mortality rates for people of reproductive age and those beyond it, one can estimate the number of women who were still married after the epidemic as 60 per cent of those who survived, this being the chance of survival for males of comparable ages. As there could be only the same number of males and females married, marriage or re-marriage immediately after the epidemic was possible for only as many males as there were widows, and these might be assumed to be 40 per cent of the number of adult females who survived. In each population this number was less than half the number of presumed single and widowed males aged 20 years and over, and in F the discrepancy was even greater than this, so
TABLE 5.7: Estimated numbers of males and females aged 15 years and over in villages of each size in each population before and after each epidemic

<table>
<thead>
<tr>
<th>No. of households per village</th>
<th>Before 1861</th>
<th>After 1861 epidemic</th>
<th>After 1866 epidemic</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>M</td>
<td>F</td>
<td>M</td>
</tr>
<tr>
<td>POPULATION B*</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>10-14</td>
<td>15</td>
<td>12</td>
<td>6</td>
</tr>
<tr>
<td>15-19</td>
<td>21</td>
<td>16</td>
<td>9</td>
</tr>
<tr>
<td>20-24</td>
<td>27</td>
<td>21</td>
<td>12</td>
</tr>
<tr>
<td>25-29</td>
<td>33</td>
<td>25</td>
<td>16</td>
</tr>
<tr>
<td>POPULATION D</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>10-14</td>
<td>14</td>
<td>13</td>
<td>5</td>
</tr>
<tr>
<td>15-19</td>
<td>21</td>
<td>17</td>
<td>9</td>
</tr>
<tr>
<td>20-24</td>
<td>27</td>
<td>22</td>
<td>13</td>
</tr>
<tr>
<td>25-29</td>
<td>33</td>
<td>27</td>
<td>15</td>
</tr>
<tr>
<td>POPULATION E</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>10-14</td>
<td>14</td>
<td>12</td>
<td>5</td>
</tr>
<tr>
<td>15-19</td>
<td>20</td>
<td>16</td>
<td>9</td>
</tr>
<tr>
<td>20-24</td>
<td>26</td>
<td>21</td>
<td>12</td>
</tr>
<tr>
<td>25-29</td>
<td>32</td>
<td>25</td>
<td>15</td>
</tr>
<tr>
<td>POPULATION F</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>10-14</td>
<td>14</td>
<td>11</td>
<td>5</td>
</tr>
<tr>
<td>15-19</td>
<td>20</td>
<td>15</td>
<td>9</td>
</tr>
<tr>
<td>20-24</td>
<td>26</td>
<td>20</td>
<td>12</td>
</tr>
<tr>
<td>25-29</td>
<td>32</td>
<td>24</td>
<td>15</td>
</tr>
</tbody>
</table>
that an upper limit of 50 per cent for the re-marriage of males is reasonable.

In effect, this is not very different from the numbers of adult females who survived the first epidemic in the villages of each size, so that even if some re-distribution of women occurred, it would have little effect on the size of the village during the interval between the epidemics. The numbers of males and females aged 15 years or more in villages of each size in each population before and after each epidemic are shown in Table 5.7, the minimum and maximum numbers being derived as explained above.

By the time of the second epidemic (in which the mortality rate for females of reproductive age was estimated to be 30 per cent and for males of like ages 25 per cent) the children aged 10-14 years at the time of the first epidemic had become adults by our definition, and the discrepancy in size between those villages which suffered severely in both epidemics and those which escaped lightly is much greater than it was after the first. Irrespective of the population model, most of the villages which had the least mortality in both epidemics had almost regained their former size in terms of numbers of adult males, though not in terms of females and the larger the village initially, the greater the relative loss of females. At the other extreme, where mortality was high in both epidemics, the smaller villages of less than 20 households initially were virtually eliminated and those with more than 20 households were reduced to about one-quarter of their initial size.

The comparison between the two series of maximum numbers -- for 1861 and 1866 -- indicates the speed with which the villages that suffered lightly in the first epidemic could regain their former size, whereas others of the same size initially, but with high epidemic mortality, had little chance of recovery except slowly and if they experienced high mortality again in the second epidemic, the odds are that some would never recover. In between these two extremes would be other villages which had greater or lesser chances of regaining their former size, but the comparative scarcity of females in consequence of the epidemics would leave perhaps one-quarter, perhaps one-third of the males who might have expected to marry without wives. Restrictions as
to who might marry whom -- which, according to Inglis (1887 p.342), still persisted as late as 1876 -- would exacerbate this scarcity, and the likelihood of recovery for villages of the same size initially would therefore be very variable.

For a village as large as Anumej, it is worth dividing the adult sector of its population into those who might be presumed to be "active" members of the community, and those older than (say) 54 years who might still be active but would contribute little towards the physical regeneration of the population. Estimates of the numbers of males and females at various ages according to each population model were given in Table 4.1, and Table 5.8 concentrates on the numbers at ages from 15 to 54 years who might have been there before the first epidemic, and the numbers who would have survived to these ages if the mortality rates for males and females of each age in each epidemic were as high as the average rates for the whole population plus twice their standard errors, given the size of the population at risk. Even on this scale, there is the same remarkable similarity between all four population models in the numbers of survivors of both sexes as there was for the smaller villages shown in Table 5.7.

<table>
<thead>
<tr>
<th>Population model</th>
<th>MALES</th>
<th>FEMALES</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>Before</td>
<td>After</td>
</tr>
<tr>
<td>B*</td>
<td>97</td>
<td>40</td>
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<tr>
<td>D</td>
<td>99</td>
<td>36</td>
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<tr>
<td>E</td>
<td>103</td>
<td>40</td>
</tr>
<tr>
<td>F</td>
<td>108</td>
<td>41</td>
</tr>
</tbody>
</table>

If, in fact, the people of Anumej fared even worse than this because of their former isolation, and the epidemic mortality is stretched to the 99 per cent limit of variability (ie. plus three estimated standard errors instead of only two) numbers of survivors such as these in Table 5.8 would ensue from the experience of just one epidemic. After the second, there would be only about 30 "active" males and 10 females of reproductive age surviving.
Even on paper, decreases of this magnitude in the numbers left to inhabit a large village and a large area of land are surprising, especially the decreases in the number of females of reproductive age. With mortality rates as high as this, perhaps only one marriage in 10 at the worst, or one in eight at the best, would have survived the first epidemic.

Small wonder if, in either case, mortality such as this was interpreted as the wrath of God! By now the "misili" is only an afterthought in the oral tradition of the "big sickness" which led to the abandonment of Anumej, and the prime cause was dysentery. For the people of Anumej their isolation had ended at least five years earlier with the stationing of teachers in their midst, and although they had their own school-houses, the second of these was barely large enough to accommodate half of the population for its daily sessions. It measured approximately 9 metres by 5 inside, with an extra metre of pavement outside the walls and probably sheltered by the roof. But at Anekra, two kilometres away and well within Inglis's "an hour's walk" for an Aneityumese, there are the remains of two floor pavements, the larger measuring 12 metres by 6 inside and the smaller -- "where the books were kept" according to our informants -- 7.5 metres by 4.5 and they too had a pavement about one metre wide outside the walls.

As Anekra was a smaller village than Anumej, it is likely that on Sundays people from Anumej attended church at Anekra. Before 1860 Geddie had entrusted his pastoral duties for this part of the island to Matau-ahileth, one of the "high chiefs" and much esteemed by Geddie (HFR 1863 p.125), and although Matau-ahileth survived the 1861 epidemic, he died not long before September 1863 (HFR 1864 pp.41-2) and was buried at Anekra close to the smaller of the two pavements. It may not have been until after his death that the survivors from Anumej moved down to the coast at Umej to be nearer the missionary, but the date must remain imprecise.

If Anumej was one of the places where the mortality in the 1861 epidemic was high and numbers resembling those shown above in Table 5.8 were all that survived the epidemic, their total removal to Umej is more plausible than it might have seemed, given that Anumej was initially such a big village compared with
others. There might of course have been other amalgamations as a consequence of the epidemics which were not reported, any more than the Anumej-Umej fusion was in the contemporary accounts. The number of schools in Geddie's district once he could claim that everybody attended school daily was usually reported as being "about" some number, rather than any precise number; and Inglis, who was not on the island when the 1861 epidemic occurred, was rarely more precise than Geddie after his return to the island in June 1863.

What of the archaeological record of a traumatic experience such as this? For villages of each initial size, there would be those that had been abandoned at this time because virtually everybody had died; there would also be those that had decreased in size and could, for various reasons, increase again only very slowly if at all, so that there would be no need for any expansion of the site in terms of either houses or gardens for some number of years, if ever, but there would be evidence of continuous occupation of the site; and there would be others that may have regained their former size, either by amalgamation or natural increase, rather more quickly but probably variably, and some of these might have needed to increase the number of houses and extend the area of cultivation within some variable period of time after the holocaust.

Ultimately, the sites that had been completely abandoned would be distinguishable from the others; but it is hard to see how, if one has no time control finer than plus or minus 40 years at best, some of the remaining alternatives might be distinguished. With time control that is of necessity even less refined, such as Willey's (1953) in the Viru Valley which was based on ceramic sequences, the task of distinguishing between a site that was fully occupied and one that was only partially occupied for a period that may have covered decades or more, becomes almost impossible. Certainly any inferences that might be drawn from the size of the site alone during periods of habitation could be very misleading, because the physical dimensions of the site might reflect the size of a former population rather than the one that inhabited it at some later date.
If, for example, any two of the villages in each size class in Tables 5.6 and 5.7 which had suffered severely in both epidemics joined together at what had been the site of the initially larger village, they would not have required all of the house-sites and cultivable land available to them for some considerable time, largely because of the immediate scarcity of females for reproduction and later because of a lagged effect of this scarcity. The population may have remained stationary over a long period, but with no time control over the visible and tangible archaeological record, both villages would appear as of their original size.

A similar, though not parallel situation might arise if there was widespread infection of a communicable disease such as tuberculosis which increased the death rate beyond the highest birth rate possible. This would cause less dramatic decreases in the populations of some villages than would epidemics, but the slow diminution in the size of the various communities would also be imperceptible archaeologically, until perhaps the end-point where a long-continued decrease in size led to the abandonment of a site. Throughout this period the extent of occupation of the original site would have changed, and not necessarily according to any simple formula if there were differences between the ages at which males and females were likely to die.

It might be argued, as was done in Chapter 3, that the conditions for the spread of the acute infections which caused the epidemics on Aneityum, or of chronic infections such as tuberculosis, were considerably less favourable under the indigenous culture than they were after missionary intervention. Another legitimate argument is that the diseases concerned in these epidemics and the subsequent chronic infection were diseases of European origin. The rebuttal of the second argument is perhaps easier than the first, provided that one exception is all that is needed to prove any rule; but let us take them in the order in which they were presented.
The mission's activities undeniably facilitated the spread of the various diseases that caused the epidemics throughout the whole population. Whether the fatality in each was thereby enhanced or not for any particular community is another question. Before the establishment of the mission there were the intiptang where the men congregated regularly to plan communal activities, and there were the ostensibly frequent ceremonial exchanges of food which brought the people of one 'district' into contact with those of another. Hence, although a communicable disease may not have spread throughout the whole of the island's population before the mission was established, once introduced into one community within a 'district' it may have spread throughout that district, causing mortality rates similar to those experienced in the epidemics that followed European contact. The effect on the population as a whole would of course have been less, but the effect on the district immediately concerned may have been no different under either the traditional cultural system or the early years of some form of Christianity.

On the second point, even diseases of "European origin" originated somewhere, and there were diseases in the island populations of the Pacific at or soon after European contact which were unlike any that the Europeans who reported them knew in their own society. Some of them caused very high mortality, often higher among females than among males, and so the diseases had the double-barreled effect of decreasing the birth rate while increasing the death rate in the population affected. Probably the classic example of the apparently spontaneous occurrence of what might be termed a chronic communicable disease in a population prior to European contact is that of 'kuru'.

This is an acute, progressive, degenerative disease of the central nervous system which was first discovered among the Fore-speaking people in the Eastern Highlands of New Guinea in 1951. According to local tradition a decade or so later, kuru had either not occurred or occurred only very rarely before about 1925, and the populations affected had had little (if any) contact with Europeans before 1951. From 1954 onwards there have been intensive investigations of various kinds, but the origin
of the disease remains obscure, its mode of transmission can only be presumed and probably many more years yet will be needed to decide, even reasonably conclusively, whether or not the cannibalism of the victims of the disease was a decisive factor in its continuing incidence.

Because no such disease has been found elsewhere, there is by now a vast literature on all its various aspects, and this will probably be reviewed and summarized in a collection of essays to be published soon by the Institute of Medical Research of Papua-New Guinea, edited by R.W. Hornabrook. Its relevance here is that it occurred, and it need not be the only instance of the spontaneous occurrence of a disease in any population. But in the South Fore in the 1960's, mortality was very high among women presumed to be of reproductive ages, and although the birth rate was boosted by the immigration of young women, presumably for marriage, from the surrounding areas, the balance between births and deaths in any year in any village community and in the population as a whole was precarious. It might have remained so if cannibalism had not been actively discouraged after effective European contact.

For infections of a more "acute" nature there is one reported from the Austral Islands, south of Tahiti in the early 1820's (McArthur, 1967 pp.297-310). Although European responsibility for its initial introduction cannot be entirely ruled out, it was first reported from Rurutu, then from Tubuai whence it spread to the neighbouring islands of Rapa and Raivavae. If it was the same disease on both Rurutu and Tubuai, it may have reached the latter either from Rurutu, by a party that fled to Tubuai to escape the sickness and death on their own island or, as reported by a British naturalist High Cumming (1828 pp.114-6) by a canoe from Anaa, one of the Tuamotuan atolls, which made landfall at Tubuai with all on board diseased and dying.

Regardless of where the disease originated, it was spread from one island to others without the intervention of European ships. Between the numerous accidental or drift voyages and the deliberate trading voyages reported (Dening, 1962 pp.137-53; Parsonson, 1962 pp.28-32) for the island world of the Pacific,
and the many more that might have happened (Levison, Ward & Webb, 1973 pp.44-64) there was undeniable scope for the transmission of infectious or contagious diseases long before the penetration of Europeans into the region. How many times an incident such as the above might have happened is less relevant than the fact that it happened at all, and that when it did happen it may have left no imprint on the archaeological record, however large a dent it might have made on the size and structure of the population.

For Aneityum there is contemporary documentary evidence for a particular sequence of introduced diseases, and this historical record - inadequate though it is - contains enough rudimentary demographic data to permit inferences about the impact of these diseases on the population as a whole. From these one can then proceed to estimate the likely impact on some of the individual communities that contributed to the average experience of the whole population, and by allowing for random variations in the mortality rates in communities of various sizes, it is no longer surprising that one or more of the events in this sequence led to the abandonment of the site of the largest village on the island, and perhaps many others besides. But without the contemporary evidence, how would one know?
CHAPTER 6
REFLECTIONS

Unique though Aneityum is because of the contemporary documentary evidence for the late phase of its prehistory, it is not necessarily exceptional in other ways. Virtually the only structural evidence remaining pertains to only one aspect of settlement - the exploitation of the land. Superficially, nothing remains of their houses to indicate the spatial organization of communities, or of the distribution of such communities over the island. Their fear of sorcery eliminated middens as indicators of habitation sites, and there was probably no tradition of pottery, though this is still unproven. With some variations, conditions similar or comparable to these might be expected on other islands, and reflections on the Aneityumese situation may help re-formulate some old hypotheses, especially those on which inferences concerning population were based.

One commonly made is that changes in the number or area of sites indicate increase or decrease in the numbers of inhabitants. Examples of the proliferation of sites because of internal frictions within existing communities, and the converse coalescence of communities following some traumatic experience such as happened on Aneityum in the 1860's, have been mentioned above (pp.57, 130), but in the wider context of the Pacific there may have been other causes for both. On Aneityum, it seems likely that those defeated in war were not dispossessed of their lands as they were in Somoa, for example (McArthur, 1967, pp.113-4), or in Tanna and Erromanga (Matheson, HFR 1862 p.246; Gordon, MR 1859 p.149), where there were sometimes many battles to be fought before the land was re-conquered, only to be lost again at a later date. In the interim the dispossessed either created new villages or re-occupied old sites, or joined with friendly communities who had not been engaged in the war - and because few were ever killed in wars then, all with no more change in the size of the population than might have occurred if the wars had never happened.

Nor need the transition of a community from one location to another necessarily be abrupt and complete, especially if the
group was initially large. A series of bad seasons, or a major staple crop blighted by disease or predators, might encourage the spread of cultivation to new areas and thus lead to the dispersion of settlement. Success in some areas would attract more and more people from the original group, reducing the population at the parent site but not reducing the site area, while both the population and the areas of the new sites increased. Once again all of this could occur with no more change in the size and structure of the total population than would have occurred if everyone had stayed at the initial site.

However, the situation in which inferences from numbers of sites to population might be most misleading is that of a semi-sedentary population where the habitation sites changed frequently, leaving few surface indications. This was the situation reported for Nguna in the central New Hebrides (p.51) and there are suggestions - both documentary and archaeological (pp.38, 50, 107) - that it may have been similar on Aneityum. Where there was such mobility for a small group, archaeologically it would be indistinguishable from contemporaneous settlements of numerous small groups, and the apparent multiplicity of sites which might be revealed by changes in the course of a river, for example, or by excavation would tend to the belief that the inhabitants of a particular area were much more numerous than they had in fact been. With a chronology that must, for want of other evidence, rely entirely on C-14 dating, and habitation sites that moved every two or three years or perhaps more frequently, the dating error of ±40 years at best might lead to a 30- or 40-fold magnification of the numbers of people who occupied the successive sites.

The lack of 'time-control' except within a range that exceeds the life-time of man - and this applies to criteria other than C-14 dating - is one of the greatest stumbling blocks to inferences from sites and site areas to the people who created them. Another is the apparent absence of any habitation evidence associated with structural remains. In the Marquesas, for example Bellwood (1972) mapped the various kinds of stone platforms and walled enclosures in one entire valley, but found nothing from which to re-construct a chronological sequence or to infer, except
very broadly, the purposes of the structures presumed to have been built on the paepae.

These reflections on the proliferation of sites through dispersion for one reason or another, or the re-location of whole communities, provoke still more questions. Can one assume, for example, that there is no change in the spatial organization of village communities when new settlements are created? That the houses they build are replicas of those they have left? That the space enclosed by walls, or under some kind of roof is used as it was formerly? Soon after their conversion to Christianity the Anenityumese were reported to be building larger houses than formerly (Geddie, MR 1854 p.184), and there is no suggestion that this was at the instigation of the missionaries except in the case of a house for Nohoat, the principal chief of Anelgauhat (J 23 Apr 1855). If the larger houses were built because there was greater security of property under Christianity than when warfare was rife, could not this security also have been attained if Nohoat had achieved supremacy over the whole island by conquest? More generally, if one aspect of a culture changes sufficiently to distinguish it from what went before and what came after, is one justified in assuming that there are no concomitant changes in house styles or, more particularly, in the use of enclosed space?

Many of these questions are prompted by personal experience in organizing or helping to organize censuses in various parts of the Pacific. Because a household questionnaire is the most efficient method of collecting and recording information, the immediate problem invariably was how to define a household which might be spread over two or more dwellings, not necessarily contiguous, not necessarily inhabited by the same people all of the time, but which might be expected to contain the same people at the time of the initial recording by a census enumerator and again when this record was checked soon after the census date. The diversity in the use of physical structures was remarkable: the one constant factor that separated one household group from another was that the members of each household commonly, if not invariably, ate together. This definition was as appropriate for the Indian population of Fiji in 1956 as for the Fijians, and it was and has been used since with equal success in Polynesia,
Melanesia, and Micronesia.

The apparent universality of this throughout the Pacific region as a definition for census purposes immediately raises further doubts as to the usefulness of structural remains as a reliable basis for inference about households and thence populations in prehistoric times. What the archaeologist sees in or on the ground is an assemblage of the activities of a continuum of living populations, and had censuses been taken throughout the periods of habitation at some site, the population occupying it at each point in time would have been amenable to the same demographic logic as any population living today, no matter how long the period of occupation of the site, or how long ago the people who created it lived. In archaeology as in census-taking where people lived or live is easier to discover than how, but if it is the 'how' that is most relevant for the enumeration of a population now, so it must be for the estimation of, or inferences about a population long past.

For some islands, part of the 'how' might be gleaned from descriptions written by the early explorers or visitors and later by the missionaries who settled there. But here too this study of Aneityum points to the need for discretion, if not caution, in the use of such documentary source material because the two people who might be expected to be the most dependable witnesses for the island and its people failed to mention the most pervasive record of settlement on the island -- the stone walls of various kinds. Formerly (McArthur, 1967) it seemed there was ample evidence to doubt the reliability of estimates of the size of populations which were made by transient visitors to the various islands of the Pacific, and of the counts made by some of the missionaries stationed there later. Further reflection suggests that even this may have been naive or simplistic, and that what was recorded must be seen as a subjective opinion, reflecting both the background of the author and those for whom the accounts were written.

For the early transient visitors, the duration of their stay and their use of time spent ashore are obviously important, especially in terms of their likely geographical coverage of an island. Most of the journals written during the voyages of exploration in the Pacific which have been published are those of
officers, or naturalists like Banks and the Forsters who accompanied Cook on his first and second voyages. In most cases what the early visitors saw or were shown on their excursions ashore was in the immediate vicinity of a safe anchorage for their ships, and as a good harbour for European vessels also has advantages for an indigenous seafaring people, its hinterland and the pattern of settlement there was not likely to be 'typical' or 'representative'. Moreover, all of those on board the visiting ship or ships relied essentially on the same informants and, no doubt, exchanged experiences and opinions with one another, so that often one is at a loss to discover how much is independent observation or deduction and how much derives from others. The supreme example of collusion is probably the elder Forster's estimate of Tahiti's population (p.19 above and McArthur, 1967 p.242), but this is more easily detected than many others might be.

For the missionaries, the rules are more complex. In general, the longer they stayed on an island or in an island group, the more they identified themselves with the people and the greater the tendency to exaggerate or distort the extent of the changes wrought by the coming of Christianity. Not all of the missionaries were equally perceptive or receptive when they first arrived, and what a newcomer reported of the past was often what had been told him by his predecessors in the field or preconceptions based on their reports, and only rarely independent evidence garnered by himself. Once a mission was established on an island, the occasional visitor -- be he naval commander or trader or curious traveller -- usually relied heavily on the missionary for information, and if their accounts confirm those of the mission, it is usually because they share the same sources of information.

Nor are all of these equally reliable. The 'pious frauds' among the missionaries were detected by some of their brethren -- for example, William Ellis by John Davies and William Gill by G.N. Gordon (pp.100-1 above) -- as well as by some less gullible contemporary visitors -- for example, John G. Paton and others by Julius Brenchley who was angered by the Curacoa incident (p.5 above). But many of the missionaries were either more charitable than Davies and Gordon, or perhaps aware that at
times they themselves exhibited similar traits. Few were as blatant as Ellis or Gill, or even Paton (HFR 1863 p.296) whose address to a public meeting in Glasgow in October 1863 was reported in part thus:

Aneityum was divided between Mr Geddie and Mr Inglis, the former being appointed to attend to the printing department of the mission, and the latter ... being appointed as divinity professor, to take charge of those who would become missionaries on that island. For the missionaries saw that, unless the native agency was set to work immediately, it would be long before two men could bring nearly 5000 savages under the light of the gospel.

By the time Paton arrived in the New Hebrides, Geddie and Inglis had already made their 'census' and revised their initial total of 3,800 or more down to 3,500. By October 1863 this number had been reduced to little more than 2,000 by the measles and dysentery epidemic in 1861, but Paton overlooked this small detail as well as the population count and the estimates that had preceded this count, because he was trying to recruit more missionaries and enlist financial support for them and for the mission ship Dayspring.

In his autobiography Paton admitted to 3,500 people on Aneityum, and for him and others a natural corollary of this was that islands larger than Aneityum had commensurately more people living on them. This rationale underlies the mission estimates of populations in many islands of the New Hebrides (and elsewhere). Even towards the end of the 19th century when some of the small populations of bigger islands had been counted -- Efate, for example, which was found to contain perhaps 2,000 people in 1874 compared with the "ten to twenty thousand" believed to be there in 1865 (Brenchley, 1873 p.225) -- the old estimates for them and others were never explicitly retracted. Occasionally there was some downward revision, but that was no less arbitrary than the original guess. In cases such as these, the documentary evidence as to the size of populations could well prove more of a hindrance than a help to the archaeologist seeking to unravel the history of settlement on an island.

If one accepts the historical fact that there is no necessary relationship between the size of an island and the number
of people there at any point in time, how should the archaeologist or prehistorian seeking to establish a reliable or reasonable estimate of former populations proceed in the absence of documentary evidence? Given the historical facts, one cannot invoke the old chimera of "carrying capacity": nor, as was demonstrated above (p.122), can one safely infer from land-use criteria established for one sample area to the whole, even if the sample area was the largest settlement on an island as small as Aneityum.

The basic strategy for reconstructing subsistence settlement systems which Struver (1971) formulated presupposes much more 'ideal' conditions than one is likely to encounter in comparable studies in the Pacific region, where the logistics of collecting together at one time a team of people with the requisite skills for an adequate ground survey of "plant, animal, water and soil resources" in an area remote from all sources of food and other supplies would be formidable. But fundamentally the rules must be the same -- that one must start out with this reconstruction as an objective and that it must be an integral part of the initial planning of a programme of field work, not just an afterthought when the field work is being written up. Essentially the study of both settlement pattern in a region and population needs to be planned as one would a sample survey of any kind, no two of which are ever identical, and there are no short cuts if one wants the answers to mean anything at all.

For islands which are now virtually uninhabited, and for which there are both maps and aerial photographs, the initial phases of planning would depend on whether it was a big or a small island, whether one was interested in the whole island or only in part. Assuming that the areas of former settlement can be identified from the aerial photographs because of the differences in vegetation cover, these areas can then be classified according to various combinations of factors so as to provide a sampling frame from which areas could then be selected so that the ground survey would give the best possible coverage of the varieties of areas that have been used for settlement. That is the easy part: thereafter there are many alternatives to be weighed and decisions to be taken.
needs to be done should be done within the whole framework, and not just here and there arbitrarily. Although practical considerations would dictate concentration of effort at sites that appear to be more rewarding archaeologically, the comprehensive ground survey cannot be skimped if one wishes to extend inferences from such sites to the whole island or region. If one is less ambitious, the scale and cost of the operations are lessened, but many of the conceptual and statistical problems - especially those where small numbers are concerned - become more complex.

But to go into even a limited range of the alternatives of ineluctable compromise would be a new and different venture - another journey of exploration far beyond Aneityum in time and space.
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The Demographic Situation

NORMA MCArTHUR

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The total indigenous population of Papua and New Guinea probably numbered about two millions in 1964, 95 per cent of whom had been recorded by name in either a tax register or a village book, and the remainder were the numbers thought to be living in areas difficult of access where no listing of the people had yet been attempted. Because of the practical difficulties involved in enumerating the population in such a territory at a particular moment of time, census-taking there is very different from an enumeration of Australia's population, and the method that has evolved requires periodic 'census patrols' through the contiguous geographic areas into which the Territory has been divided, when the inhabitants of each village or cluster of hamlets in the path of the patrol are asked to assemble on some specified day for the census. The patrol carries with it the tax register (or village book in the few areas where the people do not yet pay taxes) for each such group of people, and when they are assembled the officer conducting the patrol checks through the list of inhabitants, adding the names of children born since the previous patrol and anyone who may have joined the group, and deleting the names of people who had died or had gone elsewhere to live.

When this check is completed, the numbers of males and females, births and deaths, and various other details are summarized, and such totals for each 'census unit' are subsequently aggregated to provide totals for the whole 'census division', and thence for the sub-districts which are the basic administrative units. Not all census divisions are patrolled at the same time, nor is any one census division necessarily patrolled at the same time each year. Hence the interval between successive censuses in a division may range from a few months to nearly two years, and as the population figures published in the annual reports for each Territory incorporate the results of the most recent census in each division, some of these may relate to the population that was there nearly two (and
New Guinea on the Threshold

occasionally more) years previously. Given this and the proportion of the total population that is estimated, it would be unwise to be too precise about the number of inhabitants either within the whole Territory or for any part of it.

These two sources of variability also make it difficult to assess the rate at which the population is growing. The proportion of the total that was estimated decreases each year as administrative control is extended, and there are frequently changes in the boundaries between census divisions and sub-districts which prevent the numbers of people recorded in the course of one year being compared with those recorded some years later. In a detailed review of censuses taken before and during the fiscal year 1953/4, (McArthur 1956:pt vii) it seemed that the counts for seven sub-districts were probably fairly reliable, and those of a further six were less reliable but probably satisfactory, whereas the population records for thirty-five sub-districts were either quite unreliable or incomplete, and in nine the population was either unknown or estimated. Only two of the first group of sub-districts could be followed through to 1961/2, although the boundary changes between a further three could be eliminated by combining them into a single unit for each year from 1953/4 to 1961/2. Three of the sub-districts in the second category—probably satisfactory—could also be followed through to 1961/2, although one had a suspected excess of one hundred people in 1953/4 and not all census divisions were checked in 1960/1.

The average annual rates of growth exhibited by these sub-districts ranged from 1.5 per cent to 2.7 per cent, and the average for the five sub-districts where censuses taken during 1953/4 seemed to be reliable was 2 per cent per year from then until 1961/2. In the sub-districts where the 1953/4 counts were less reliable, the average annual rates of increase to 1961/2 ranged from 1.5 to 1.8 per cent. More detailed and more accurate data from smaller groups in both Australian New Guinea and the former Netherlands New Guinea indicate that in some areas at least populations are increasing more rapidly than this, and though some may be increasing more slowly, it is unlikely that any are decreasing consistently. In some years there may be more people dying than there are children born, but even in the population which suffers most from the baffling and fatal disease known as kuru, more births than deaths were recorded between June 1961 and January 1964, though for one year of this period there was a net loss of females, but not of males. In the total population of the Territory the increases or decreases in individual areas each year will be smoothed out, but until more is known about the rates at which more sectors of the population are changing, any estimate of the average growth rate for the whole population can only be an uninformed guess.

Nowhere in New Guinea is it particularly easy to compile accurate and complete records of population, but in some areas it is
more difficult than others. Much depends on the topography and climate of the area, but even more important are the attitudes and personalities of the people living there. Many patrols must be made on foot because even where there are vehicular roads, the roads do not pass through all villages in a census division. Where the villages are large and either close to one another or separated by relatively easy terrain, a census patrol may not be particularly arduous; but if, as so often happens in mountainous areas, the villages or hamlets are perched on hilltops and separated from their neighbours by deep valleys, a census patrol can be strenuous. In sparsely populated areas the patrol may walk for days to reach the next village, only to find it deserted by all except the few too old or too sick to join in the hunting or sago-making expeditions which had begun before news of the patrol got through. A patrol officer reporting on his travels through a newly contacted part of the Western Highlands in the early 1950s complained bitterly that all he saw of the native population during several weeks of patrolling were their backs as they disappeared over the nearest ridge.

Still more exasperating to census patrols are people such as those who inhabit part of the delta region near the mouth of the Fly River on the western side of the Gulf of Papua. Their villages are built on land—if land it may be called, composed [as it is] of fluvial mud, one patrol officer wrote disgustedly—and the paths through the villages are precarious board-walks built over the mud. Travel between villages is by native canoes, some of which are very fast, but seemingly these were seldom placed at the disposal of the patrols. As one officer was paddled slowly by ‘fatalistic tribesmen’ towards a village, he saw the villagers themselves—commonly accused of being lazy or inherently lethargic—paddling swiftly away upstream (T.P.N.G. 1950/1). On another occasion, a census patrol was sadly mis-timed and the tides, when low, were really low, and several times the canoes were left sitting on the mud several hundred yards from some villages, and progress from then on through almost knee-deep mud was, of necessity, slow. During this patrol, the officer also learned of the disconcerting habit of these people not to make their village their ‘place of residence’, but to visit friends in other villages in turn, returning to their own village after an absence of several months only to act as hosts to similarly nomadic friends. As a result, many were recorded in more than one village book (T.P.N.G. 1951/2).

By now there are probably few areas where people are so reluctant to meet the patrols, but there is still considerable movement of population between villages, either for informal visits or for special ceremonies or ceremonial exchanges, and migration away from villages into nearby towns in search of paid employment. Persons absent from their village at the time of a census are designated as ‘absentees’, but are still included in the count of the
village population even though they may be away for years at a
time and their whereabouts not always known to the village head-
man. If they choose to marry and live in a town, they will probably
remain in the register for their home village, but unless they return
to home it is unlikely that any children born since their departure
from the village will be recorded anywhere because there are no
registers for towns comparable to the village lists. Nor would it be
practicable to try to institute them, and if the indigenous popu-
lations of urban areas are to be counted, different techniques of
census-taking are needed and the system for village populations
must be modified to avoid counting some people twice.

The numbers of villagers living in towns are not yet large enough
to affect the general pattern of distribution of population through-
out Papua-New Guinea (see Fig. 7 of Chapter 3). The most densely
populated regions are the three Highlands Districts, which contain
40 per cent of the population in one-eighth of the total land area.
The northern mountains between the Sepik River and the coast
contain some areas of high population density, as do the Finsch-
hafen peninsula and a narrow coastal strip along the eastern edge
of the Gulf of Papua. The vast area from the western shores of this
gulf through to the West Irian border and northward is very sparsely
populated; so too is much of the southern portion of the central
mountain range and the island of New Britain, though the north-
western tip of New Britain's Gazelle Peninsula is the most densely
peopled area in the Territory.

High or low population densities based on the total land area of
census divisions do not mean much in a country as heterogeneous as
New Guinea, especially as the census divisions range in size from
one to more than nine thousand square miles, though almost half of
them are less than two hundred square miles in area. If the popula-
tions could be related to the cultivable land area of the census
divisions rather than the total, the range of densities might be
reduced, but much variability would remain. The amount of land
available for food cultivation is not the sole determinant of where
people live, nor will it necessarily affect the number of children
that will be born or the number of people who die. Ultimately
both of these depend on the sex and age composition of the
population, and, in the absence of migration, the size of a population
at any given time depends on the relative numbers of births and
deaths that have been occurring each year for many years past;
these in turn determine the extent of the changes likely to occur in
the future.

The number of children that can be born in any year depends on
the number of women who are physiologically capable of bearing
them, and this capacity is limited to a span of thirty or thirty-five
years from about fifteen years of age. Once a child is born it is
continually exposed to the risk of dying, but this risk is not uniform
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throughout life. High at or soon after birth, the risk diminishes progressively as a child survives through each year of age to a minimum somewhere between the ages of ten and fourteen years. From then on, the risk increases gradually with each additional year lived, and the older people become the greater the chances of their dying. The number of deaths that will occur in a population in a year thus depends on the number of people who have survived from birth to each age, and if one-tenth of a population are at ages where the mortality risk is high, there will be more deaths in a year than there would be if the population contained fewer people at those ages.

Accustomed though we are to the concept of chronological age, it is quite alien to the majority of Papuans and New Guineans. Only those who have had some education are aware of the convention of measuring the passing of time by calendar years, and only those whose parents were similarly educated would be likely to know the date of their own birth. Hence the ages for most adults and many children must still be estimated, either from their physical appearance or by trying to associate their birth with an event of some importance which occurred in some known year. For most primitive populations whose history is unknown, this second method is not particularly helpful, and when the village books were compiled initially the patrol officers, complying with instructions to record sex and approximate year of birth for each individual, relied chiefly on physical appearance.

As there are no absolute standards of how a person of a given age should look, the years of birth recorded for people could only be very rough approximations. More accurate estimates would have required long and tedious questioning and cross-questioning, making comparisons between individuals which are impossible unless both questioner and respondent speak the same language. Some officers declined to assess the years in which people then obviously adult were born, and recorded them simply as either 'adult' or 'aged'. The years of birth for children born since the books were compiled in the post-war years, and particularly those of recent years, are probably reasonably accurate, provided no errors of transcription occur when a dilapidated old book is replaced with a new one and each family group stays in the village in which the post-war children were born. Should they move to another village to live, the years of birth recorded for them in that village's register may differ from the originals.

So far little use has been made of these records of age, and summaries of population by sex and years of birth are available for only a few census divisions. Some of these relate to the populations of the Gazelle Peninsula as they were more than a decade ago (McArthur 1956), and the more recent ones are of two groups of people in the Eastern Highlands, both of which are abnormal
because of the prevalence among them of the fatal disease \textit{kuru} (McArthur 1964). The village books for these two groups were compiled initially in 1958, and the dates of birth recorded showed the patrol officer's preference for such years of birth as 1918, 1928, 1938, 1948 and 1920, 1930, 1940, 1950. Because of the large numbers assigned these years of birth, the numbers of people allegedly aged 5-9, 15-19, 25-29, etc. years in 1960 were very much smaller than the numbers aged 10-14, 20-24, 30-34, and so on respectively, whereas the usual distribution of a population by age is decreasing numbers in each group as age increases. By laboriously distributing the numbers with each recorded year of birth over the range of ages within which the initial estimates might lie, more regular age distributions were achieved for both populations, and arbitrary though the procedure was, these probably represent the age structure better than the original data. To use the data that have accumulated for all other areas, similar adjustments would be needed to eliminate the compiler's preferences for certain ages or years of birth.

Several anthropologists and medical officers have tried to discover the age composition of the populations amongst whom they were working. Some have been helped by mission records of baptisms, though not all missions keep these and many of the pre-war records were either lost or destroyed during the war years. More often the ages have been estimated by comparative rankings of members of the group, using genealogical data and recognized 'age-mate' groups who played together as children or shared some ceremonial ritual. Most of the populations studied by such researchers are necessarily small, and interesting though their demographic data are against the background of their social organization and economic activities, their very smallness is a handicap to any broader interpretation.

As a simple illustration, let us imagine a village of fifty households each of which contains one woman of reproductive age. If the average interval between successive births is three and a half years, fourteen babies on the average would be born each year and because the sex of each child is determined independently, one would expect any number between four and ten of these babies to be girls. In exceptional circumstances there may be fewer than four or more than ten girls in a sample of this size, but as few as four or as many as ten would be within the normal range of variation when the chance of each child being a female is one in two. In a period of five years one would expect the annual variations in the numbers of each sex born to even out, but they may not and it could happen that at least forty-five of the seventy children born in the village over a five-year period were girls.

If three-quarters of these children survive to marriageable ages, and both males and females marry at about the same age, there
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would then be only eighteen young men to become husbands to thirty-four girls, and if the parents of the redundant females wished their daughters to marry, marriages would have to be arranged with men of other villages, the young women then going to live in their husband's village. This would leave only thirty-six people in this particular five-year age group in our village instead of the fifty-two that would be expected had equal numbers of males and females been born. If a census was taken of the village at this time, there would probably be considerably more males and females in the age groups on either side of the one that had been depleted by the out-migration of the young brides.

Knowing nothing of this background, the census-taker might at first sight suspect some error or bias in the estimates of age, for example a tendency to exaggerate the ages of married people and understate the ages of children so that few people were assigned to the intermediate range of ages. If the age estimates appeared to be reasonable on the basis of other evidence, what kind of events may have caused this distortion? Was there perhaps an epidemic of some infectious disease about the time when this small group was very young which caused exceptionally high mortality among infants and young children? Or were people of reproductive age the chief victims of the infection, so that fewer babies were born in the years immediately after the epidemic? Had the villagers perhaps suffered defeat and heavy casualties in a local war, which left many young women widowed and therefore less likely to have children for a few years?

The equal numbers of males and females in the age group in question would lend support to hypotheses of this nature and no doubt, on questioning, it would be found that the villagers had experienced epidemics from time to time, and also that they had engaged in periodic fighting. However, it would probably be difficult if not impossible to construct a precise timetable of such events because, unless one outbreak of disease was in some way exceptional, few people would be able to distinguish between an epidemic which had occurred twenty-five years previously and one of the same disease five or even ten years later. Even if different diseases were concerned, the accuracy of people's recollection would depend partly on their age at the time of the epidemics and partly on the degree of personal suffering or deprivation, so that, with each of several informants giving conflicting versions of the same event, the hypothesis that the distortion in the age distribution was due to, say, a war or an epidemic could not be confirmed or rejected.

If the matter were left there, as it might well be if the investigator was in the village for only a short time or had become discouraged by the apparent fruitlessness of such lines of inquiry, the real cause might never be discovered. The genealogies collected by an
anthropologist would contain the relevant information, but the association between population structure and marriage outside the village might not be immediately apparent, especially if only one or two young women had gone to each of several villages and the ages of some of them either could not be, or had not been, estimated. Without genealogies, the only other data likely to provide a clue would be complete histories of each woman’s pregnancies, and again the relevance of the absent daughters to the current population structure would be detected only if their estimated or assigned ages were reasonably accurate.

If the peculiar age distribution was eventually discovered to be the result of chance fluctuations in the sex of births, and not the aftermath of war or epidemics, no one would be tempted to infer that this one village was ‘typical’ or ‘representative’ of other villages in the area. Had fighting or disease been responsible, some of the surrounding villages might have been similarly affected, but before assuming typicality or representativeness it would be wise to collect comparable data from other villages in the immediate vicinity. Extension of this principle leads inevitably to the folly of assuming, without checking, that one can generalize from just one village, however its population may be distributed with respect to sex and age. The corollary to this is that if the other villages are checked appropriately, it is no longer necessary to rely on just one small population to unravel the demographic situation within some defined area.

The extension to more villages introduces a scale of operation beyond the resources of a lone anthropologist or even a small research team, especially when, to achieve the best results, the villages should be selected by some impartial and statistically valid sampling procedure. The method of selecting the villages, whether the choice is from all villages within an area or whether some areas are chosen and all villages within each are then canvassed, depends on the objectives of the survey and the personnel and financial resources available. If the operation was to be limited to a relatively small area, such as one census division or perhaps two or three adjacent divisions, it would be feasible to select villages within the area which would then be the ‘sample villages’ whose populations were to be recorded and studied. If information concerning the population inhabiting a much larger area were required, it might be more practical to select ‘sample census divisions’ from within that area and record the details required about the populations of all villages within these census divisions.

If a sample census of the whole territory was contemplated, the second scheme would have several advantages. Fewer investigators would be needed to cover some pre-determined proportion of the total population if the time spent in travelling between villages was kept as short as possible; and, perhaps more importantly, the quality
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of the data recorded would probably be higher if an investigator were dealing continually with one relatively homogeneous cultural group, about whom he could be reasonably well briefed in advance, than if he were assigned to several villages, each of which had a distinctive cultural pattern. In addition, the effects of inter-village migrations, especially those of a temporary nature, would be minimized by an 'area sample'; and it would be administratively simpler to enlarge a sample of census divisions over a period of years, to cover the whole territory eventually, rather than to expand gradually a sample of widely scattered villages.

Except that its initial areas were not selected on any statistical basis, a project such as this was started in the former Netherlands New Guinea, and preliminary reports on six areas have now been released (Groenewegen and Van de Kaa 1962-4). Two of the areas censused are the Schouten Islands and Noemfoor in Geelvink Bay; there are three widely separated coastal areas which also include some if not all of the relevant hinterlands, and the sixth is an area in the southern foothills of the central mountain range close to the border with Papua. The total indigenous population included in the censuses of these areas is more than 70,000, but nearly half of this number was contributed by just one area—the Schouten Islands—and the sum of the six populations is therefore no more likely to be a representative sample of the indigenous population of West Irian around 1960 and 1961 than are the various samples collected by individuals in Papua-New Guinea. On the other hand, each sample is relatively large—none comprised fewer than 5,000 people and the four areas on the mainland each contained 8,000 or 9,000—and diverse geographic and climatic conditions are represented, so that the data indicate the range of values for some demographic indices that might also be expected on the other side of the border.

The people of the truly inland area were the only ones with less than 43 per cent of their total number aged less than fifteen years, and they also had the lowest crude birth rate and the smallest average numbers of children born per woman at each age. The crude birth rates ranged from 38 to 53 births per 1,000 population per year, but most were within the range of 45 to 50 births annually per 1,000 population. In four of the six areas the women who had survived to ages 40-44 years had borne an average of more than six children, but in two of these areas as many as one-quarter of the children had died within their first year of life; elsewhere the infant mortality rate was one-fifth or one-sixth of all children born. The two regions where the women nearing the end of their reproductive life had had the fewest children on the average also seem to have experienced the lowest infant mortality rates in former years. Strangely enough, malaria is hyperendemic in one and the other is not highly malarial. These were also the only areas where the infant mortality rate has not declined markedly in recent years,
and the proportions of survivors to age one year amongst the offspring of women aged approximately 20-24 years in all areas was within the fairly narrow range of 0.80 to 0.90.

The subsequent fate of children who attained their first birthday is not disclosed in these preliminary reports, and the Schouten Islands was the only area for which there were records of the deaths that had occurred during some specified period. These indicated a crude death rate of about 14 per 1,000 population in a year, and as the birth rate there was estimated at about 53 per 1,000 annually, the population of the islands was increasing at about 4 per cent per year. If the crude death rate in each population is proportional to its infant mortality rate, then two of the populations are currently increasing at rates of about 1\% per cent a year, two others at about 2\% per cent per year, and the other island population at about 3 per cent.

Though this simple relationship between mortality in the first year of life and mortality at all ages may not be valid, it does seem to hold for some populations with similar age structures, especially those where deaths in infancy and early childhood contribute very largely to the total mortality. But until the appropriate data are available for populations such as these, any more elaborate assumptions would only convey a spurious accuracy to growth rates which are probably ephemeral, partly because of the age structure of the populations, partly because the health measures which were being or had been instituted in these areas may affect the incidence of disease and, in turn, the levels of both mortality and fertility. If mortality falls and the birth rate remains unchanged, growth will be accelerated, and in some areas at least the present age structure of the populations is such that current birth rates are likely to be maintained. The limit to the acceleration of growth is immortality, but the higher the initial level of mortality the faster it is likely to fall, and the more people who survive through the reproductive period the more babies will be born. Because improved health may increase the chances of conception and reduce the likelihood of early foetal deaths, the birth rates may even rise and so enhance the difference between them and the falling death rates.

Both the timing and the magnitude of changes such as these in any part of the mainland or islands of New Guinea are totally unpredictable. The only certainty is that neither will be uniform, nor even particularly regular. Though little is known of the contribution of specific diseases to the total mortality pattern, their relative incidence will no doubt be found to vary from one locality to another, even from one year to another, and the extent to which they can be controlled will likewise be variable. The principal causes of death listed in the Annual Reports for 1961-2 were pneumonia, tuberculosis, dysentery, gastro-enteritis, and malaria; but the cause of death can be ascertained for only very few among
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all who die in the course of a year. The native diagnoses recorded by some census patrols are colourful, but seldom specific or particularly informative as to cause rather than symptoms. Even in years when some particular disease assumes epidemic proportions, its incidence throughout the Territory is likely to vary: some sectors of the population may escape infection entirely, others may be protected by the immunity conferred by an earlier attack so that only people born since then may succumb, others may be so remote from the initial source of infection that the infective agent has lost some of its pathogenicity by the time it reaches them. The possible sources of variability are endless, and the only safe assumption to make about the future course of mortality is that it will either remain much the same as now or decline.

Given this and the not dissimilar assumption that the birth rate will not change much within a decade, Papua-New Guinea’s population ten years hence is likely to be between two and three millions, though the latter figure is unlikely unless the present population exceeds two millions. Between 40 and 45 per cent of the total will probably be aged less than fifteen years, and as about three-fifths of these would be children of school age, between one-half and three-quarters of a million children may be seeking primary education. There could be the same numbers of adult males in what in other communities are described as the ‘economically active’ age groups, approximately one-fifth of whom would be aged between forty-five and sixty years and consequently unlikely to be seeking employment outside their villages, though they may well be economically active there.

There is little point in elaborating these figures: a margin of 50 per cent between the lower and upper estimates makes nonsense of attempts to translate them into practical terms such as the numbers of schools and schoolteachers that will be required under varying assumptions about the proportions of the school-age population who should be receiving primary education. On the other hand, expanding the existing staff and facilities by only 50 per cent over the next decade may do no more than maintain the fraction of the population of school age which is now attending schools. If this fraction is to be raised from less than one-third to about one-half over the decade, expansion of the order of at least 100 per cent should be planned.

The only statistics currently compiled for the indigenous labour force relate to persons in wage employment, and their source is the annual return each employer makes to the Department of Labour (Labour and National Service 1964). In March 1963, 75,000 males were receiving wages and 56,000 of these were employed by private enterprise. The number employed by the Administration had increased by 4,000 over the previous year’s total, but the net increase in all kinds of employment was only 2,500. These wage-earners
would constitute perhaps one-seventh of the total labour force, and as more than half of them were employed in some form of agriculture (one-third in the production of copra and cocoa), the numbers engaged in all other kinds of employment would amount to perhaps one-twelfth or less of the available work force.

No matter what (within reason) happens to this non-agricultural sector over the next decade, by far the greater part of the prospective increase in the work force must be absorbed by agriculture, and the ease with which this can or cannot be done is an inherently local problem. If the structures of the populations inhabiting the different regions are as variable as the sparse data concerning them suggest, the work force component of each will increase in a highly individual fashion. The net gain to the work force over a ten-year period depends primarily on the number of children aged five to fourteen years at the start of the period; and if the initial age structure of the population is as distorted as those of two Tolai settlements in 1960 (Epstein 1962), the work force after ten years could be 55 per cent larger than at the start, whereas the growth during the preceding five years would have barely exceeded 10 per cent. Only by having more adequate and more reliable statistics of the populations can contingencies such as this be foreseen; developmental programmes planned without reference to such vital facts are likely to benefit neither the supposed beneficiaries nor the planners.
Map 1

- Heights in metres

ANETYUM

0 2 Km

0 2 Miles