

B.B.13

Paweł Piotr GORECKI
Ph.D. July 1983.

The University of Sydney

Copyright in relation to this thesis*

Under the Copyright Act 1968 (several provisions of which are referred to below) this thesis must be used only under the normal conditions of scholarly fair dealing for the purposes of research, criticism or review. In particular no results or conclusions should be extracted from it, nor should it be copied or closely paraphrased in whole or in part without the written consent of the author. Proper written acknowledgement should be made for any assistance obtained from this thesis.

Under Section 35(2) of the Copyright Act 1968, the 'author of a literary, dramatic, musical or artistic work is the owner of any copyright subsisting in the work'. By virtue of Section 32(1), copyright 'subsists in an original literary, dramatic, musical or artistic work that is unpublished' and of which the author was an Australian citizen, an Australian protected person or a person resident in Australia.

The Act, by Section 36(1), provides: 'Subject to this Act, the copyright in a literary, dramatic, musical or artistic work is infringed by a person who, not being the owner of the copyright and without the licence of the owner of the copyright, does in Australia, or authorises the doing in Australia of, any act comprised in the copyright'.

Section 31(1) (a) (i) provides that copyright includes the exclusive right to 'reproduce the work in a material form'. Thus, copyright is infringed by a person who, not being the owner of the copyright and without the licence of the owner of the copyright, reproduces or authorises the reproduction of a work, or of more than a reasonable part of the work, in a material form, unless the reproduction is a 'fair dealing' with the work 'for the purpose of research or study' as further defined in Sections 40 and 41 of the Act.

Kenneth W. Knight
Registrar

* 'Thesis' includes 'treatise', 'disertation' and other similar productions.

ETHNOARCHAEOLOGY AT KUK:
PROBLEMS IN
SITE FORMATION PROCESSES

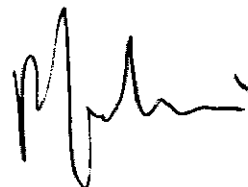
Pawel Piotr Gorecki

Volume I

This work is a thesis
submitted for the Degree
of Doctor of Philosophy
in the University of Sydney

December 1982

Except where otherwise acknowledged
in the text, this thesis is based
entirely on my own fieldwork and
research.

A handwritten signature in black ink, appearing to read 'P. Gorecki', with a stylized, cursive script.

Pawel P. Gorecki

This work is dedicated to the
memory of Carlyle Greenwell,
whose bequest has made it possible.

"... en Océanie, ethnologues et
préhistoriens ne peuvent que
collaborer pour comprendre le
présent comme le passé de ces
îles." J. Garanger 1975

"An important feature of the
archaeology of New Guinea, as
of other areas of Sunda and
Sahul, is the extent to which
it is yesterday's ethnography."
J. Golson 1977

ABSTRACT

This thesis concerns ethnoarchaeological research carried out at Kuk, in the New Guinea Highlands. It is about a community of farmers who have an intensive agricultural system in which the staple crop is sweet potato. Gardens are made in both swamplands and drylands by the means of a network of ditches used to raise the garden beds. There is no village, houses are scattered, sometimes in small hamlets. Domesticated pigs are the major source of protein and wealth.

Four distinct studies are presented: (i) a study of 20 years of human impact on the landscape; (ii) a study on houses; (iii) a study on cooking activities; (iv) a study on gardening practices. Ethnographic observations related to the last three studies have been complemented by the excavation of historic sites having various lengths of abandonment. These data are aimed at defining and evaluating archaeological site formation processes.

Through these, it is shown that processes are numerous and complex during the first five years of abandonment. Archaeological visibility of remains is discussed; overall the visibility is low, but most of the time sufficient to enable an interpretation in accord with the ethnographic activity which made it. There is a correlation between the in situ appearance and site types leading to a functional interpretation.

This research demonstrates that ethnoarchaeological studies should take into account site formation processes. This could be best achieved by including a long-term analysis of the field observations and the excavation of sites occupied during living memory.

ACKNOWLEDGEMENTS

In the course of this research, I was assisted by numerous individuals and institutions, and I wish to acknowledge my thanks and gratitude to them. My first and deepest debt goes to my "foster fathers" Jack Golson and J. Peter White. They have always kept a sharp eye on my liking to fall into traps while in the field and during the writing of this thesis. They have introduced me to Papua New Guinea, to its ethnography and prehistory, a geographical region and research interests I shall never forget. Peter acted as a supervisor and Jack took me into his Wahgi Project, and since 1977 they both have been checking all words I have written or said about Kuk. Thank you for this.

Jack and Peter had an enormous task which was to recycle my speech and writing from Anglo-French to a sort of English. This linguistic problem is a serious one and the work presented here should be seen as an additional problem in "cultural formation process". Despite their help, and the help received from many others, particularly Roland Fletcher and Tim Murray, there must still be many grammatical mistakes left. These are obviously and exclusively mine. I apologize for them.

Staff and students of the Department of Anthropology at the University of Sydney have helped me tremendously through this period of research, especially the advice and assistance received from Richard Wright. I thank them all.

I wish to thank the Government of Papua New Guinea and the Western Highlands Provincial Government for allowing me to carry out this research. I thank the entire community at Kuk, and particularly Rea El, for accepting me so positively among them and for responding favourably to all my odd queries during my stay at Kuk. The staff of Kuk Plantation, and the Department of Primary Industry, have been most co-operative. The Australian National University has been most generous in allowing me to use its facilities (vehicle and accommodation) available in New Guinea.

I owe a special debt to those who have facilitated my entry and stay in Papua New Guinea: Mary Jane Mountain, Les Groube, Pamela Swadling, Brian Egloff and Andrew Strathern. Archival research was made possible

in Mt. Hagen and Port Moresby with the help and assistance of the Staff of the National Archives and the Western Highlands Provincial Government.

The research would not have been possible without the financial assistance from grants received from the Sydney University Postgraduate Research Studentship (1978, 1979), the Carlyle Greenwell Bequest in Anthropology (1977, 1978, 1981) and the Sydney University Overseas Research Travel Grant (1982).

Excavations would not have been possible without the invaluable assistance received from F. David Bulbeck and from Rea El, Korua Rumba, Kupaki Ebuga, Pelang Mak, Aipa Mip, Pita El, Krepia Konga, Walua (Joseph) Nigints and Mek (Kanyi) Genglba.

In the laboratory, I received great assistance during the lithic analysis from Jim Baxter, Denis Gojak, Cassandra James and Sue McIntyre. Drawings of stone tools were made by Margrit Koettig; further help on some drawings was sought from Jim Baxter. All the photographic material was expertly handled by Ed Roper. The typing was done by Barbara Baxter, who had to fight against indefinite last-minute vagaries on my part. I thank all the above persons for their assistance and contribution to this research.

Severe but outstanding criticism and comments on all or various drafts were received from Jack Golson, Roland Fletcher, Tim Murray and particularly from J. Peter White. I thank them for this painstaking work and emphasize that their responsibility is not at all engaged here.

Finally, I thank my family, nuclear and extended, for their patience in coping with my growing bad temper as the research progressed. It is all over now. To all of you, and to those friends and colleagues I may have forgotten to name, thank you.



Plates 1 and 2: The manufacture of an archaeological site: planting sugar cane.



TABLE OF CONTENTS

Abstract	page i
Acknowledgments	ii
Glossary	1

CHAPTER 1

INTRODUCTION

1-1 Location of Study Area - Kuk	2
1-2 Testing the Propositions	8
1-3 Theoretical Approach	11

CHAPTER 2

MACRO-ANALYSIS

2-1 Clan Division of Kuk	26
2-2 Period 1: 1960-1969	31
2-3 Period 2: 1969-1970	33
2-4 Period 3: 1970-1973	34
2-5 Period 4: 1973-1977	35
2-6 Social Implications	35
2-7 Landscape Implications	38
2-8 Artefact Collection	43
2-8-1 Stone Axes	45
2-8-2 Small Tools	49
2-8-3 Other Artefacts	51
2-9 Macro-Picture: Discussion	54

CHAPTER 3

HOUSES - ETHNOGRAPHY

3	Ethnographic Observations	page 63
3-1	Spatial Distribution	63
3-2	House Site	64
3-3	Site Definitions	66
3-3-1	Definition 1: House in Pig Land	66
3-3-2	Definition 2: Enclosed House	67
3-3-3	Definition 3: House Inside a Sweet Potato Garden	68
3-4	Tracks	69
3-5	Site Limits - General Laws	70
3-6	Pita Homestead	72
3-7	Ru/Rea Homestead	74
3-8	Site Abandonment	78
3-9	Type of Houses	81
3-9-1	Men's Round Houses	84
3-9-2	Oval Houses	87
3-9-3	Woman and Pig Houses	93
3-9-4	Veranda Houses	96
3-9-5	Kiap Houses	98
3-9-6	Menstruation Houses	100
3-9-7	Toilets	102
3-9-8	Pig Sties	103
3-9-9	Cassowary Cages	105
3-9-10	Other Buildings	107
3-10	Conclusions on Houses	109

CHAPTER 4

HOUSES - ARCHAEOLOGY

4	Archaeological tests	page 111
4-1	Strategy: Test Priorities	111
4-2	Case Study: Pita House	113
4-3	Men's Round Houses	117
4-3-1	Site MOP	117
4-3-2	Site MOG/R	119
4-3-3	Site MOH/R	120
4-3-4	Site MOQ	121
4-3-5	Site MOL	124
4-4	Oval Houses - Site MOO	127
4-5	Woman and Pig Houses	130
4-5-1	Site MOD	130
4-5-2	Site MOE	132
4-5-3	Site MOH/WP	134
4-5-4	Site MOO/WP	135
4-6	Kiap Houses: Site MOF	137
4-7	Menstruation Houses	139
4-7-1	Site MOH/M	140
4-7-2	Site MDM	140
4-7-3	Site MON	141
4-8	Miscellaneous Sites	142
4-8-1	Shelters. Site MOK	143
4-8-2	Pig Sties. Site MNT	144
4-8-3	Cassowary Cages. Sites MOI and MOJ	145
4-8-4	Toilets. Site MOH/T	147
4-8-5	Burial Sites	149
4-9	Conclusion on Houses	151

CHAPTER 5
COOKING PLACES - ETHNOGRAPHY

5-1	Introduction	page 164
5-2	Cooking Places	164
5-2-1	Cooking Inside Houses	165
5-2-2	Cooking Outside Houses	165
5-2-3	Special Places	167
5-2-4	Cooking in Singing Grounds	168
5-3	Typology of Mumus	168
5-4	Dimension of Mumus	170
5-5	Cooking Stones	172
5-5-1	Number of Stones	174
5-6	Two Types of Cooking	175
5-7	Inter-relation Between Earth Ovens	177
5-8	Conclusion: Archaeological Testing	178

CHAPTER 6

COOKING PLACES - ARCHAEOLOGY

6-1	Introduction	page 181
6-2	Site MOH/CP	181
6-3	Site MOO/CP	182
6-4	Site MNG	182
6-4-1	Description	183
6-4-2	Analysis	185
6-5	Site MNI	186
6-5-1	Description	187
6-5-2	Analysis	188
6-6	Site MNH	190
6-6-1	Description	190
6-7	Site MOG/CP	193
6-7-1	Description	193
6-7-2	Analysis	194
6-8	Site MOL/CP	195
6-8-1	Description	195
6-8-2	Analysis	196
6-9	Other Excavations	197
6-10	New Guinea Highlands Archaeological Data	199
6-11	Archaeological Interpretation from Ethnographic Analogies	200

CHAPTER 7
GARDENS - ETHNOGRAPHY

7-1	Specific Locations	page 204
7-2	Crops	205
7-2-1	Sweet Potato Garden	205
7-2-2	Mixed Garden	206
7-2-3	Other Crops	209
7-3	Individual Gardens	210
7-3-1	Korua's Garden	211
7-3-2	Nigints' Garden	213
7-3-3	Other Individual Gardens	214
7-4	Size of Holdings	215
7-5	Size of Plots	216
7-6	Communal Gardens	218
7-6-1	Generalised Model	219
7-6-2	Muk's Block	221
7-6-3	Rea-Nung's Block	221
7-7	Swamp Gardens	223
7-7-1	Generalised Model	225
7-7-2	Size of Blocks and Plots	229
7-8	Ditches	231
7-8-1	Definitions	231
7-8-2	Formal Analysis	232
7-8-3	Functional Analysis	235
7-8-4	Blind Ditches	238
7-9	Archaeological Hypotheses	239

CHAPTER 8
GARDENS - ARCHAEOLOGY

8-1	Site MNU	page 243
8-1-1	MNU/1	244
8-1-2	MNU/2	245
8-1-3	MNU/3	245
8-1-4	Interpretation	247
8-2	Site MNV	249
8-2-1	MNV/1	250
8-2-2	MNV/2	251
8-2-3	MNV/3	252
8-2-4	MNV/4	254
8-2-5	Interpretation	255
8-3	Site MOA	256
8-3-1	Results	257
8-3-2	Interpretation	259
8-4	Site MOB	260
8-4-1	Results	261
8-4-2	Interpretation	262
8-5	Site MOC	263
8-5-1	Results	264
8-5-2	Interpretation	265
8-6	Site MNI	268
8-6-1	Results	269
8-6-2	Interpretation	271
8-7	Site MNX	272
8-7-1	Results	272
8-7-2	Interpretation	274
8-8	Site MNY	275

8-8-1 Results	275
8-8-2 Interpretation	278
8-9 Site MNZ	279
8-9-1 Results	280
8-9-2 Interpretation	281
8-10 Conclusions on Dry Land Cultivation	283

CHAPTER 9

CONCLUSION

9-1 Feature remains	287
9-2 Artefact remains	289
9-3 Nature/culture and feature/artefact	292
9-4 Local prehistory	296
9-5 Wider prehistoric context	301
9-6 Current studies in ethnoarchaeology	309
9-6-1 Yellen	311
9-6-2 P.J. Watson	314
9-6-3 Gould	317
9-6-4 Binford	320
9-7 Future orientations	322
Bibliography	325

GLOSSARY

KAPUL (Pidgin). Cuscus, opossum.

KARUKA (Pidgin). Nut of the Pandanus julianettii and Pandanus brosimos (cf. Powell et al. 1975).

KAUKAU (Pidgin). Sweet potato.

KIAP (Pidgin). Patrol officer of the Administration (Government official).

KINA (Melpa, Pidgin). Traditional valuable, crescent shaped, made from goldlip pearl shell (Pinctada maxima; cf. Hughes 1977).

KONGA (Melpa). Quartz crystal kept as magic stone (cf. Vicedom and Tischner 1943-8, Vol.I:130).

KUMU (Melpa). Loose term to describe various green vegetables tasting like spinach, including Commelina, Hibiscus manihot, Rorippa, Rungia klossii, Solanum nigrum, etc. (cf. Powell et al. 1975).

KUNAI (Pidgin). Short grass, Themada australis.

LAPLAP (Pidgin). Loin cloth.

MARATA (Pidgin). Fruit of the Pandanus conoideus (cf. Powell et al. 1975).

MUMU (Pidgin). Earth oven; cooking in earth oven.

PAT (Melpa). Winged bean, Psophocarpus tetragonolobus (cf. Powell et al. 1975).

PITPIT (Pidgin). Tall grass, Miscanthus floridulus.

PITPIT, edible. Saccharum edule and Setaria palmifolia (cf. Powell et al. 1975).

RAEM (Melpa). Ornamental Melo spp. shell (cf. Hughes 1977).

SINGSING (Pidgin). Ceremony, feast.

TANKIS (Pidgin). Cordyline (cf. Powell et al. 1975).

CHAPTER 1

INTRODUCTION

1-1 Location of Study Area - Kuk

The ethnoarchaeological research described in this thesis was carried out at Kuk, a small locality about 18 km north-east of Mt. Hagen, in the Western Highlands Province of Papua New Guinea (Map 1). Kuk was uninhabited at the time of European contact in 1933 (M. Leahy, pers.comm.; aerial photograph from 1955; Griffin 1958). Prior to its recolonisation which started around 1960, Kuk can be described as follows.

It consists of a large swamp covered by the tall pitpit grass (Miscanthus floridulus). North and east of this main swamp is a high but narrow ridge called Ep, having steep slopes covered by the short kunai grass (Themada australis). The ridge supports small stands of secondary forest regrowth found along some creeks deeply dissecting it. South of the Kuk swamp is a region of wide swampy creeks separated by small low lying hills having the aspect of dry islands. Pitpit is found in swamplands and kunai in drylands. The area west of the Kuk swamp consists of well drained hills, with numerous creeks having occasional marshy banks. The dominant vegetation there consists of kunai with few shrubs. Overall it is grassland country, with a remarkable absence of trees. Elevation at Kuk varies between 1550m ASL in the main swamp up to 1962m ASL on top of Ep ridge.

The Mt. Hagen region is described elsewhere in detail: geology (Rickwood 1954), physiography (Haantjens et al 1970), vegetation (Powell 1970) and climate (Powell et al 1975, McAlpine 1970). These data demonstrate a lack of marked seasonality in the Mt. Hagen region. Temperature and rainfall do not change dramatically in the course of the year. The mean annual rainfall is about 2600 mm (Brookfield and Hart 1966), while at Kuk itself (from 1970 on), the extremes recorded for one month are

36 mm and 394 mm. The mean temperature for the region is around 18°C (Powell et al 1975) and the extremes recorded at Kuk are -1°C and $+32^{\circ}\text{C}$.

It was the previous occupant of the Kuk swamp, the Kawelka tribe, who moved back to Kuk sometime around 1960 (Gorecki 1979a). The tribe has recolonised the area by establishing itself all around the major swamp, except along its western boundary. Later on (in 1969), the entire Kuk swamp itself was alienated by the Government and then partially drained for various agricultural ventures, including Kuk Agricultural Research Station (KARS). It is from KARS that most of the evidence on early agricultural systems in the New Guinea Highlands has been found (e.g. Golson 1981a).

From the ownership of a territory estimated at around 13km^2 , the Kawelka were left with about 6km^2 after this alienation. A gradual shift of activities from the slopes of Ep ridge to the south of KARS resulted in a dramatic and intensive use of the land in this particular area. Because of this, I have concentrated my attention on this southern section of the settlement (called here "South Kuk"), although the ridge was never totally ignored. It is from South Kuk that the macro-level analysis has been made; similarly, most of the archaeological sites excavated for this research are found in the same area.

The alienation of the Kuk swamp was made over a few years, starting with Tibi in 1964 and ending with Kuk in 1968. The alienation of the swamp for Kuk plantation (13 June 1968) was a difficult affair which involved long investigations to find the owner of the land. While it was known that its last occupant was the Kawelka tribe, the sale in reality involved no less than seven groups. Using the spelling of the time, these were: Jiga-Kilambi, Iamaga, Kululiga, Kauliga, Jiga-Milagampo, Keme and Penambi (anonymous and undated patrol report, Mt. Hagen Government Office).

Tibi plantation was the first alienated (Map 2). The investigations

carried out for this purchase are detailed here because they emphasize the complexity of problems related to land ownership.

"It seems as though the Waragi group lived on or near the land initially (Tibi), although memories on the point seem somewhat hazy. They moved further up the valley to where the Kombugga group now resides. It is probable although not certain that the Kauliga (Kawelka) group fought the Waragi group forcing them to leave their holdings.

"The Kauliga group lived on the land for some time and in fact regard it as their ancestral home. They killed a man from the Jiga-Kilampi group. Some of the Jiga groups and Moge-Kwipi groups then waged battle with the Kauliga group and forced them off the land. They went to live at Buk with a small group going to settle at Korgua.

"At this stage the Kemi groups of Kagaga and Tetagaga were residing on the banks of the Wahgi river somewhere near the land. The Jiga-Kilampi's after ridding the area of the Kauligas settled near the Kemis using a small portion of Tibi for pig grazing.

"The Moge-Kwipi shortly after fought with the Iamaga group. The Iamaga group defeated them and they went down to live with the Kemi groups on the banks of the Wahgi in relative peace.

"However a Moge-Kwipi man was killed by the Kemis which act resulted in a war between these two groups. The Moge-Kwipi group won decisively. The Kemi groups of Kagaga and Tetagaga went to live with the Jiga-Milagampo group which resided near Koglamp. The Kemi-Kunduga group went to live with the Golga-Kenjibi group near Mala. The Moge-Kwipi group began using the land in question. The Kuguliga group were at this stage residing at Kelua.

"The Kuguliga-Jaga and Kemi groups then combined and fought with the Moge-Kwipi. The Moge's were badly beaten and broke into two sections. The Ogubuga group of Moge-Kwipi went to live with friends at

Rugli. The Ogugumunaga-Gulga-Engaga-Ogambo-Agiliga groups went to live with the Moge-Depi groups who reside around the Hagen station.

"Some Jiga's went back with the Kuguliga and Kemi groups to reside on the land in question. Because of a fear that the Moge would return and kill them all the Kuguligas and Kemis intermixed to a certain extent for strength.

"During all this time the Pinambi groups were living with the Elti group on the banks of the Wahgi river near the land in question. The Rogaga groups were living on the banks of the Komun river near where Mr. D. Manton now has his plantation. Both these groups fought with the Penambi inflicting defeat on the Rogaga group who then moved down to live near Nunga on the other side of the valley. The Penambi did not settle on the land because they had killed several men from this group. They then went to live with the Kuguliga group at Kelua. Later they went back to reside at or near Tibi.

"It was shortly after their return to live at Tibi that Mr. Jim Taylor came to the area.

"The position is now that the Penambi group although not residing on the land itself have been using it for pig grazing as have the Kemi and Elti groups. The Moge-Kwipi group itself have actual ownership rights jointly with other groups such as the Kuguliga and Jiga groups. A number of families from each of these groups have returned and are now living near the area and are using it for pig grazing. A number of Kauliga have now returned and are also using the land. The Kuguliga because of their assistance to other groups in the past and the fact that a number of their people are using the land still retain some rights in the land. Certain of the Jiga groups because of assistance rendered in the past still retain some rights in the land and must receive some payments for those rights. The Waragi and Rogaga groups now have no further rights in the land.

"The above is agreed to by all groups involved. The owning groups are then as follows: Penambi-Depi, Jiga-Milagampo-Mindimpo, Kemi, Jiga-Milagampo-Komisbi, Kuguliga, Kauliga, Elti, Jiga-Kilampi, Moge-Kwipi. Certain individuals from the Moge-Depi group will receive payment." (P. O. Allwood, correspondence on file in Mt. Hagen Government Office.)

The payment for Tibi plantation was finally apportioned as follows: Penambi (17%), Jiga-Milagampo (6%), Kemi (17%), Kuguliga (12%), Kauliga (12%), Elti (12%), Jiga-Kilampi (6%) and Moge-Kwipi (18%). The long quote given above relates events which may have lasted for only 30 years. Population movements within this time span concern most of the tribes found in the Upper Wahgi and migrations back and forth within the entire region. Similar complex stories are found when dealing with one tribal group only (e.g. the Kawelka, Strathern 1972; Gorecki 1979a).

While the data above strongly suggest warfare as being the cause for these migrations, it is my belief that it is this aspect only, and particularly the loss of a battle, which is emphasized in enquiries regarding land ownership. From the archival research I have carried out concerning the Kuk swamp, I could find only one reference regarding its cultivation: "The Wahgi valley people are very much attached to their land, even useless swamp land, and they have legends of how good the land used to be an indefinite time ago" (R. W. Hallahan, P.O., 21/1/64). These legends did not attract the attention of Patrol Officers and are by now lost. But when the question of swamp cultivation is raised, an issue not directly relevant to land rights, then it is another general picture which emerges. It is possible that valley floors throughout this part of the Highlands were in fact abandoned because of a widespread epidemic (Gorecki 1979b). Such an event, although of dramatic consequences, has no value in the land rights issue and thus was marginal

or even omitted from the early Government reports.

Although the Kawelka did not receive major compensation for the Kuk alienation, it is nevertheless common knowledge in surrounding tribes that the swamp was theirs. This alienation was probably an occasion for the parties involved to settle numerous compensation disputes.

My involvement in the Kuk project (under the direction of Prof. J. Golson, A.N.U., Canberra), is rather fortuitous. When I arrived in Sydney, I tried to submit a "rough" research proposal to be carried out in the New Guinea Highlands. The study area was so vague (I knew little about it myself) that the locality or even the valley were unknown to those familiar with New Guinea; these names were not even found in the most recent maps available (it is only when my 1977 fieldwork at Kuk was almost at its end that I finally went to see that mysterious study area; it really exists).

Following that shaky introduction with Australian scholars, and possibly because of my background as an anthropologist interested in material culture, I was then offered the opportunity to participate in the Kuk project by conducting a dramatically revised research, this time on the present group of people living around the Kuk swamp. While Golson's team investigated the history of the swamp, little data were gathered from the surrounding drylands. My independent association with this team has allowed this dryland study to be done, but perhaps not in the expected way. To justify the confidence White and Golson had in me, and to thank them for the opportunity to do it, I sincerely hope that the work presented in this thesis can contribute to the Wahgi project.

1-2 Testing the Propositions

This research is centred around the problems of defining site formation processes, cultural and natural, and in the quantification of relationships existing between site types and their in situ appearance. Attempts are made to evaluate the accuracy of site interpretation, answering questions such as "what the form tells us about the function". All this concerns data derived from four methodological steps described below, i.e. data gathered by a "taphonomic ethnoarchaeologist". These are then tested against data from some prehistoric sites, including some of my own, to check the validity of trends noted as well as the extent of the applicability of specific ethnoarchaeological analogies to prehistoric sites. Finally, it is in the concluding chapter that the entire exercise is assessed and compared in its methodology with other ethnoarchaeological studies. Four of them are selected for this purpose: those carried out by Yellen, Watson, Gould and Binford.

The methodology followed in the field was complex. It involved first the intensive analysis of aerial photographs of the study area

which had been taken over a period of 25 years (although people moved into the area only during the last 20 years). Aerial photographs do not lie; if they show a specific human activity at one location in 1969, this activity existed at that location in 1969 (it is our interpretation of it which might be wrong). A summary of this study is presented in Chapter 2 and concerns more particularly the long-term view stressed in my methodological "step 1". This aerial research was followed by a ground examination of the study area, taking into account events noted on these photographs as well as activities contemporaneous with the period of my fieldwork.

While aerial photographs do not lie, by contrast, personal observations or data gathered through interviews with the most reliable informants may not be necessarily correct. Some contentious questions were solved by these photographs. Overall, eleven months were spent in the field in two seasons (1977 and 1978), including four months of intensive excavations of selected sites. During these digs, continuous observations on human activities were also made.

The excavation technique used in this fieldwork was as follows. The surface vegetation of selected sites was removed with axes, bush-knives and spades to expose the surface. A normal 1m x 1m grid system was laid over the site and then all or only selected squares were excavated. Sometimes the grid followed other dimensions, but these were always according to a metric system. In some cases arbitrary 5cm spits were removed by trowels, in other cases these spits were removed by spades. Sometimes almost the entire top soil was removed at once with spades to excavate the deposit found near and on the basal clay more carefully.

Sieving was a problem. In most cases this was forbidden by the owner of the site because of the belief that it would remove the "rich grease" from the soil. In other cases wet sieving was forbidden or

impossible because the only location where this could be done was in garden ditches. These either contained little water or more often had consumable water which could not be disturbed. Nevertheless the contents of almost all hearths were wet sieved. Despite being an important lacuna in my data, it will be shown in this thesis that in most cases material remains recovered from the Kuk drylands are of limited archaeological value. Because of this point, it tended to make the sieving problems less critical.

Most important features of a site were recorded with a theodolite in a three dimensional method. All portable finds were recorded in two dimensions. With the exception of the stone tool material, they were all identified at the site or at the field house and then discarded. The stone tool component of each site was washed, labelled and bagged in the field and then shipped to Sydney for further laboratory analysis. This material is presently on loan to the Department of Prehistory, A.N.U., Canberra, and will eventually be deposited in the National Museum, Port Moresby.

In the field I tried to deal with as many activities as possible. I tried to witness them, to complement these observations with comments by informants and to cross-check them as much as possible by duplicating similar observations. It is evident that some activities have been better observed than others because of their regular and daily occurrence. For instance I have detailed data on cooking, gardening and housing activities, but insignificant ones on burial practices or major ceremonies. Consequently the material collected probably covers fairly well the daily routine of the society concerned, while occasional or unusual events are poorly represented or missed.

Because of the nature of the observations made, as well as the time and financial support required, only remains from specific activities were archaeologically test excavated. The bulk of the tests were carried

out on housing, cooking and gardening sites. I have tried as much as possible to excavate sites of the same type which had been abandoned for various periods so that taphonomic processes could be best evaluated within each site type and micro-environment. Because of the nature of the area and its history, prehistoric sites could not be avoided and are therefore an integral part of this research. All these data cover my methodological steps 1 (short-term perspective), 2 and 3. They are discussed in Chapters 3 to 8.

I have restricted the material presented in this thesis to three major activities because I have confidence in their results: housing, cooking and gardening. This restriction allows me to make a more detailed study of the evidence by presenting details and comments which otherwise would have been left aside at the expense of interpretations. By leaving aside various activities, I am also stressing the important fact that any ethnoarchaeological study is incomplete. I am only attempting to answer questions that I wish to ask of my excavated material. For this reason, studies such as settlement mechanism and their spatial analysis are omitted (e.g. Fletcher 1977, 1978).

My last methodological step, the fourth, is the thesis itself, but more particularly its conclusion (Chapter 9). In it, I try to evaluate the relevance of such work for archaeology.

1-3 Theoretical Approach

The thesis presented here is an attempt to demonstrate that ethnoarchaeological studies on site formation processes are useful for the interpretation and explanation of specific archaeological data. It involves the collection of ethnographic material from a small New Guinea community and the archaeology of its ethnographic present. I argue that both strategies cannot be dissociated and are aimed at offering alternative interpretations of prehistoric remains. I shall attempt to demonstrate that direct interpretation can be done for some specific

activities within specific geographical areas, while general and hence more cautious interpretation can still be proposed for some prehistoric remains from other areas.

When planning my research design, the a priori approach was similar to Schiffer's second strategy of behavioural archaeology (1976:4-5), which "pursues general questions in present material culture in order to acquire laws useful for the study of the past". Subsequently I found that although this definition was loose enough for my initial aims, it became unsatisfactory. At the conclusion of my fieldwork I realised, a posteriori, that I was following a mixture of theoretical approaches. My field problem was, in fact, an exploration of the notion of archaeological visibility, a problem tackled before by Yellen and Hayden. While Yellen (1977) provides us with detailed maps on numerous activities at few campsites, these maps represent in reality only an ethnographic visibility in which there is little consideration for any time depth. It is this time depth which leads towards a more accurate archaeological visibility, as opposed to an ethnographic visibility. In contrast, Hayden (1979) looked at a more restricted range of activities than Yellen, but tested the notion of time-depth and archaeological visibility by applying ethnographic excavations to his observations in an attempt to establish a better interpretation of an archaeological visibility. Hayden was concerned with taphonomy.

As a result, I realised that the emphasis I put in my ethnographic excavations was greater than Hayden's; it was a major component of my investigation. I was involved in a taphonomic inquiry aimed at producing a better understanding of what was at work at a site between its occupation and its archaeological visibility. Conversely, it was ultimately aimed at finding out if the interpretation of this archaeological visibility could lead me back to the activities which were observed at the site.

By now I required a more refined definition for my ethnoarchaeology in which taphonomy could play a major role. In an archaeological context, taphonomy analyses the nature and rate of deposition (sediments), decay (organic remains), displacement (organic and inorganic), etc. which may occur during the formation of a site (e.g. Efremov 1940; McIntosh 1974). The study of site formation processes is the subject of a growing interest (Gifford 1980, 1981; Stern 1980; Behrensmeyer and Hill 1980). Despite becoming now fashionable (Bordes 1980a, 1980b), it is undoubtedly a tool of prime importance which should be used by the ethnoarchaeologist and the prehistorian.

I finally realised that the theoretical framework of my research was essentially centred around the question of "site formation processes" within the field of ethnoarchaeology, ultimately aimed at offering a functional interpretation of specific archaeological sites through an analysis (ethnographic and taphonomic) of their in situ presentation (archaeological visibility).

It then became a question of defining these processes. This is a rather complex topic, involving the interrelationship of numerous processes, both cultural and natural (e.g. Schiffer 1976; Binford 1981; Stern 1980; Gould and Schiffer 1981). These recent readings suggested to me that the range of processes operating at a site was widening so much that some of them appeared to be more than before beyond the scope of the archaeologist (e.g. Gifford 1981), pointing towards the increasing need for a multidisciplinary approach. By now there were geomorphic, osteologic and other "ologic" processes operating at most sites for which the archaeologist, and particularly myself, could simply miss or misinterpret.

With this in mind, I realised that to do a good job at Kuk, I should theoretically take with me a plane-load of specialists from related fields. This being impossible, I was then forced to be such

a plane-load by myself and attempt to answer the best I could all these "ologic" questions I was asking myself. By now I had become a taphonomically oriented ethnoarchaeologist prepared to gather "valuable data". Rather than questioning information which might be gleaned from material data (Rathje 1979) or providing a collection of cautionary tales (e.g. Heider 1967), attitudes which are nevertheless of relevance to this subject (Yellen 1977), my frame of mind was that data were there in the field waiting to be gathered, let's grasp them in a specific methodological way, and only then start to analyse them.

The notion that "I have witnessed the last guy doing this, it is now an extinct activity" is rather common in ethnoarchaeology (Gould 1980; Yellen 1977; Hayden 1979), sometimes for valid reasons. Regarding my field area, the only extinction I can mention concerns the period of fieldwork; what I have seen was during a limited time over a defined period which is part of the past now. People's activities at Kuk have not stopped with my departure; neither have site formation processes. New sites are being made right now while older ones are being disturbed or destroyed.

All this raises the question of the validity of my data. While I am aware that I do not have the expertise to clearly understand all the processes at work, that I have not witnessed all ethnographic activities, that I have limited time-depth, and that processes are still at work now, it is nevertheless my view that some general statements regarding site formation processes can be made. What I am interested in concerns a limited body of data, limited by my period in the field, by my observations, and essentially by the questions I asked. It is within this framework that my field data are valid.

Being interested in site formation processes, it is my view that this can be studied at its best by observing contemporary societies as an ethnoarchaeologist. This is particularly true when dealing with

cultural processes. With a similar frame of mind, some aspects of historical archaeology can also be incorporated into this type of study. Following this review of the problems faced by the "taphonomically oriented ethnoarchaeologist", and inspired by the literature given above, I have devised four methodological steps for data collecting and analysis. It is only the result of this, presented in the conclusion (Chapter 9), that should be regarded as a body of data among many others which may be useful for the interpretation of some prehistoric sites. These four steps are as follows.

The first step involves the ethnoarchaeologist acting as an ethnographer concerned with the interrelationship of human behaviour and material culture. This implies precise observations on activities carried out at and from a site, starting from its selection and occupation and ending with the abandonment of that site. This should be pure ethnography at a highly descriptive level (e.g. Leroi Gourhan 1973a,b; Steensberg 1981), where there is no question of introducing words such as archaeology, the past, or analogy. These observations should not be limited to one activity, one site, one season or one family, but should be duplicated over a large number of sites, over a long period of time and with various families.

It is with such duplication of observations that one may grasp various aspects of a single activity, various site usages, seasonality and trends within families of a single larger group. A good illustration of this is the study carried out in the Sahara on a very large group of pastoralist nomads, which was complemented by solid archival research (Cauneille 1968). While Cauneille's research was certainly not directed to ethnoarchaeologists or prehistorians, one can nevertheless extract from it some general data which must be stressed here because of their implications for studies in ethnoarchaeology or prehistory.

The group studied by Cauneille is divided into numerous tribes, themselves composed of distinct clans and families. While the study area is essentially divided into ergs and regs (sandy and rocky deserts), the nomadism of the smallest social unit, the family, appears to be traditionally bound to specific micro-environments within these two broader zones. It is a nomadism based on the ownership and availability of wells rather than one based on transhumance. By comparing the patterns followed by few of these families over a limited number of years, one might start to see a clear seasonality emerging, with a preference for some specific micro-environments according to the season. It would be possible to start postulating that such micro-environment is associated with such season. Cauneille effectively emphasizes these trends.

The long-term picture, however, appears to be quite different. Cauneille recorded over a period of up to 76 years no less than 766 major campsites (419 spring sites and 347 summer sites) occupied by various families. He did not look at this overall picture at all, and thus failed to notice that statements he made on the seasonality were based on a study at the family level were somewhat contradictory with those that could be made at the group level; the general pattern of campsites submerges the particular.

Plotting all these campsites on a map demonstrates clearly that in no way could one define a nomadic pattern based on the seasons. The short-term model is now totally obliterated and a new one emerges. While for almost the totality of the territory one cannot establish any kind of seasonal movement, there are nevertheless some extremely restricted and limited micro-environments which appear to be occupied during one season of the year and not the other. What is of importance here is that the model one might build from the long-term study is drastically different from the one deduced from the short-term study. And this is without taking into account the material culture or any

other data regarding activities at these campsites (not provided in Cauneille's monograph). I suspect this may also be similar in ethnoarchaeology: things observed over a long period at one site might be quite distinct to those observed at the same site over a short period. And this is where the entire question of archaeological visibility rests.

This problem of time-scale was taken into account in my research as much as I could. While I saw various activities occurring over a limited period of fieldwork, I was always questioning these data in terms of their long-term consequences. This is why I tried to gather a macro-picture of activities which have occurred at Kuk over a twenty year period (still far shorter than Cauneille's 76 years).

It is my strong belief that when dealing with site formation processes in ethnoarchaeology, one should take into account not only the short-term approach (limited to the period of fieldwork), but also the long-term one so that the material gathered can be put into a time frame more relevant to the question of archaeological visibility. This long-term approach can be made possible by using oral histories, aerial photographs, archival research, or by speculating on the hypothetical consequences of a series of activity duplications at or near a single known location over a time period one wishes to deal with. Glimpses of these duplications and their consequences must certainly have been observed during the fieldwork period. For instance, Yellen (plan of sites; 1977), although failing to discuss them, noted them at almost all his campsites under the label "old feature" or "successive occupations".

The second step that a taphonomically oriented ethnoarchaeologist should follow, in reality done jointly with the first one, is as an archaeologist doing a detailed surface survey. The ethnographic work (first step) may lead towards some patterns for which the archaeologist

should give guides on, i.e. should suggest what to look for. He/she must have the definite knowledge that these patterns are bound by limits in time and space, sites being still occupied or only a few days old. All activities are only local and typical of the society one is dealing with. For example the economic system, social structure, ideology, sexual segregation, nature of organic and inorganic material collected, hunted, harvested, exchanged, eaten, discarded, built, etc. are all interacting in a way peculiar to that society at that time. Altogether, these result in Schiffer's S-A processes (1976:30-4), i.e. some move into the archaeological record.

Because these activities interact with one another, they result sometimes in distinct patterns of single activities or patterns of clusters of activities. It is both these patterns which are of primary concern in the second methodological step. They need to be defined in terms of their archaeological potential only, certainly not in terms of their final archaeological evidence or visibility: how they look or may look like (material and feature evidence), where they are (spatial analysis), why they look that way (operations and mechanisms leading to them) and finally what questions can be asked of them (interpretation).

The core of these data concerns cultural site formation processes. They occur at the site itself when delimited, but with ramifications outside it. The processes result in the creation of an abundance of sites, sometimes distant from one another but all related as in a chain-reaction. Cultural processes by the occupants of one site end with its abandonment, but cultural processes of other sites may affect it, and in many cases they will, in its immediate post-abandonment period. Sometimes within a time-gap of a few days only, a site may be occupied by two different cultural groups who may leave behind different cultural remains on a single occupation floor. I have in mind here trips I made in an uninhabited area of New Guinea. We stayed overnight in

rock shelters and the same shelters receive irregularly the visit of hunting parties from both the Wahgi and Jimi people. These two New Guinea groups have few cultural and economic traits in common, and may have been even more distinct in the past. Following this, two sets of distinct cultural evidence may be left on a single activity floor, in this instance the surface of shelters. In other cases, a site may also switch within a matter of days from one function to an entirely different one (e.g. a house site being recycled by its owner into a garden site).

These examples not only result in Schiffer's (1976:34-40) S-S processes ("materials transformed through successive system states"), but also may result in his A-S processes ("materials transformed back from archaeological context to systemic context"). The new processes affecting the remains left by the first activity (cultural), although only a few days later, could theoretically be termed natural. I prefer to label both of them cultural, on the grounds that they were made "at the same time", even though not necessarily by the same cultural group or for a similar usage. Because of this, a refinement in the definition of cultural processes is required.

In my view, cultural processes are operating at a site as long as the site is exposed on the surface. Whatever the time gap is between successive occupations of a single surface site, the second occupation, which may be of entirely different nature than the first one, has the potential of exploiting the cultural remains left from the previous occupation (see all of Yellen's campsites; 1977), thus uniting the two occupations into a single one. This definition does not exclude natural processes at all; these are also operating from the start of the first occupation right through the period lasting between its abandonment and the end of the last occupation, and no doubt beyond it. Re-defining Schiffer, I would say that it is only when a site is no longer visible

on the surface, when it has been buried by a natural or cultural sedimentation process that no more cultural processes can affect it. Its remains can be disturbed or even be destroyed by a later cultural occupation. While the latter is generating new cultural site formation processes within it, those affecting the underlying occupation layer are, in my view, by now natural as long as they are unintentional. Pigs rooting or humans ploughing a field are natural processes which may affect a buried occupation layer, because they are unintentional. Post holes, roots, hearths, burrows, burials, etc. have all a similar potential effect on buried archaeological sites.

While natural and cultural site formation processes have a similar starting point, i.e. the beginning of a site occupation, cultural processes of the site (but not of the occupation) end with its burial through sedimentation and natural ones end with its excavation (archaeological or not). While some natural processes can be observed in ethnography, most of them can only be partially measured through the archaeology of this ethnography. During the gathering of ethnographic data (Steps 1 and 2), observations on natural site formation processes should be made, such as those generated by predators and scavengers, the consequences of climatic fluctuations (droughts, flash floods, etc.) as well as on the rate and extent of natural decay of organic remains operating at the time of the observations.

Processes occurring at a site after its abandonment are complex and cannot be any longer the sole interest of the ethnoarchaeologist working as an archaeologist doing a surface survey (step 2). It is now necessary to move to the third methodological step which is the one where the ethnoarchaeologist becomes a geomorphologist (the best he/she can), and more particularly a digging archaeologist. These digs are exclusively selected among a variety of known ethnographic sites, and aimed at answering specific questions. The sealing of a

site is often the result of geomorphic activities which can be measured through their archaeological excavation. This is an important part in ethnoarchaeology and is critical in the understanding of site formation processes. The archaeology of ethnographic sites (and historical ones for that matter), deals with the most important and reliable taphonomic problems. Their results may perhaps be used "as a basis for separating culturally derived patterning from distortions and bias introduced by non-cultural processes" (Stern 1980:42).

The best control for quantifying such non-cultural processes is the selection of sites from which one can gather maximum (but probably incomplete) ethnographic data, followed by the regular excavations from the day of their abandonment and over the longest period possible. Against this is that this activity would prevent others being made, i.e. the very fact of full observations would prevent other observations. Obviously there are limits in such strategy, such as the archaeologist becoming too old or fed up after, say, 30 years of excavation of the same site, or that the site has been fully excavated, or that similar sites occupied within living memory are no older than a specific time depth.

The last example is certainly the most relevant here. If within a single geographical unit one has sites occupied by a single group, say, for about 50 years, all said to have had a similar function (or at least known functions) and located in a similar micro-environment (soil, vegetation, topography), the excavation of some of these sites, but covering the full span of 50 years, should provide the best controlled taphonomic data regarding sites with related cultural and geomorphic histories. Some of these data may then be broken into defined processes which might be more widely applied.

Taphonomic processes have exclusively a destructive role in archaeology. They are there mostly to obliterate, distort, disturb, con-

fuse and destroy the material and feature remains. Because of this, the third methodological step is critical. It is the one which will correlate the ethnographic evidence with the excavated evidence. This step is also the final and concluding field approach regarding the collection of data in ethnoarchaeological research.

The fourth methodological step is carried out back in the laboratory and concerns the analysis of the data collected in the field. This task is performed by what one might call the "ethnoprehistorian". This is where patterns noted in the field, as well as new ones resulting from the outcome of subsequent laboratory inquiry, are defined and quantified. Absence of pattern must also be stressed here.

First of all, patterns noted at the end of the first step must be restated. Then, one has to tackle the second approach (surface survey) and correlate its results with those from the first step. Following this, results from the third step (geomorphology and excavation) should also give patterns, but probably quite distinct from those of the second step. These two sets of patterns must be analysed jointly so that general trends can be stressed. These are trends only, certainly not rules or laws. It is only when all this "cross-examination" work is carried out (within and between sets of patterns) that the ethnoarchaeologist can make some more general statements regarding the area and the culture concerned. Taphonomic processes in operation between the ethnographic observation and the archaeological visibility should be by now clearly understood.

The conclusions reached in any such ethnoarchaeological research should be compared with data from other ethnographic studies, and also against prehistoric data, the best suitable for this being those that may be available from within the study area itself. My problem here is that most ethnoarchaeological studies have been carried out on hunter-gatherer type of societies while my research was on farmers

having an intensive type of agriculture. In contrast, there are some important prehistoric evidence coming out from the Kuk swamp itself (Golson 1977a; 1981a). Such comparative analysis may or may not emphasize the trends noted, and if they do, these may progressively advance towards a more important label such as rules or laws.

This is also the point where the troublemaker word "analogy" appears on the scene. Its application from ethnoarchaeological data to prehistoric remains seems to raise "philosophical" objections to some. To those who are sceptical about it (e.g. Wobst 1978), I would like to stress that archaeology is made of analogies of all kinds; denying the validity of any analogy would be denying the existence of basic words such as stone tool, campsite, hearth, etc. Ethnoarchaeology is one of the tools which has been used to define such words. Consequently the right question to ask is not "is the argument by analogy valid" but rather "how much of it is valid".

My position in this debate is in favour of its use, as one tool among others which contributes towards the understanding of human prehistory, but with various degrees of confidence attached to it. A starting point to this problem would be to ask if ethnographic observations, or rather data derived from ethnoarchaeological research as defined above, can be of any use to interpret historical (but from an earlier period) as well as prehistoric data from sites occupied by the same culture or from the same geographical area. The interpretation of historical sites using ethnoarchaeological data should test the extent of the validity of the analogies used.

It is most likely that there is a point in time for these historical sites, but more specifically for prehistoric sites where analogies are insufficient to confidently interpret the archaeological record. It is when such breaking point is reached that the use of ethnoarchaeological analogies reaches a lower degree of confidence. Testing this

proposition can be done by the means of an independent check of the same evidence, i.e. by using traditional methods of site interpretation. It is most likely that the two approaches will point towards a similar breaking point, which could indicate things such as cultural or technological change. It is in approaches like these that the nature and extent of technological, material and cultural change or continuity in the archaeological record can best be defined and perhaps explained.

The four methodological steps described above result in ethnoarchaeological trends from which analogies can be used as tools to offer one interpretation of an archaeological site. It is also my belief that these analogies should be tried for sites where it appears they will not work. Such highly controversial approaches may perhaps, by looking at the data from an entirely different angle, offer new hypothetical interpretations of a site. To summarise the approach which will be followed in this thesis, four points must be stressed:

- (i) Step 1, ethnography (material culture, history, geography): collection of observations. This short-term study (limited by the time spent in the field) should be supplemented by some considerations on long-term consequences of these observations.
- (ii) Step 2, surface survey: recording in detail the remains left by specific activities at specific locations during their active life, and more particularly at the moment of their abandonment (activity or location). This step concerns essentially cultural site formation processes.
- (iii) Step 3, excavation and geomorphology: excavation of selected ethnographic sites aimed at answering specific questions. These sites should have various length of abandonment to quantify at least some basic natural site formation processes.
- (v) Step 4, ethnohistory: patterning of data derived from the above three steps; defining trends.

Presented as such, I hope the thesis is seen as an ethnoarchaeological research aimed at providing further data on contemporary societies which may be of use in the interpretation of prehistoric sites.

CHAPTER 2

MACRO-ANALYSIS

Since its discovery, the Mount Hagen region has attracted the interest of numerous scholars from various fields. The earliest and certainly still the most complete monograph on the material culture and ethnography (social, economy, politics, kinship, religion) of the region is from a study carried out just after pacification of the area (Vicedom and Tischner 1948, three volumes). It could be seen as being a witness of "traditionality", and consequently a detailed account of the late prehistory of the region, made before the major changes which have occurred since European contact. Further substantial work on the present and past vegetation is also available (e.g. Powell et al. 1975, Powell 1982). Two social anthropological monographs are of direct relevance to my research (Strathern 1971, 1972) because these deal with precisely the same tribe who lives at Kuk, the Kawelka, although the core of Strathern's material comes from another settlement in the mountains, some distance away. Finally, archaeological research on early agricultural systems is being carried out principally in the Kuk swamp (Allen 1970a, Golson 1981a), thus giving me a handy prehistoric depth for the study area. Furthermore, the Kuk area has been well covered by aerial photographs so that "snap-shots" of the settlement are numerous and invaluable for the macro-analysis presented here (as well as for the selection of sites to excavate).

Consequently the background data for Kuk is substantial, exceptional in terms of material culture, social anthropology and prehistory. All this greatly facilitated my work.

2-1 CLAN DIVISION OF KUK

The tribe's origin-myth (Strathern 1972:36-9; Gorecki 1979a) relates

the peregrinations of the Kawelka throughout the Upper Wahgi until they settled at Kuk. Following the loss of a tribal fight (see Chapter 1), probably at the turn of the century, the Kawelka left the Kuk area for a new settlement at Buk, on the Wahgi-Sepik Divide (Map 1). Kuk was probably entirely uninhabited and abandoned till well after pacification, when the Kawelka came back to recolonise it around 1960. This move being successful, a greater number of Kawelka migrated from Buk to Kuk to the point that by 1978, Kuk (535 Kawelka) overtook Buk (500 Kawelka) as the major settlement. In real figures, I recorded in 1977 at Kuk 744 people, including 191 foreigners (people from other tribes) who seemed to have settled down more or less permanently (cf. Strathern 1979:110-1).

Census figures of the tribe (Gorecki 1979b) show a greater number of males, which is consistent with the data from elsewhere in the Highlands (Waddell 1972:24). While the annual population increase is around 2% throughout the Highlands, the figures for Kuk show an unusual increase of 9.4% from 1965 to 1978, thus stressing the immigration input. On the other hand, Buk is affected by an annual decrease of 2.3% for the same period, probably as a result of emigration to Kuk. A detailed analysis on Buk and Kuk concerning demographic problems is given elsewhere (Gorecki 1979b).

The Kawelka tribe is made up of three clans (Mandembo, Membo and Kundumbo), supporting and competing among each other (Strathern 1971) to a point that the history of the recolonisation of Kuk in terms of land distribution between these clans is of interest to this research. Why did the Kawelka come back to Kuk in the first place? It could have happened because of pacification in the region, together with the realisation that the Wahgi swamps were starting to be alienated by the Government. Another reason could lie in the socio-economic situation at Buk at the time of the tribal split. It seems that it was one of

the bigmen representing the Kawelka at Buk as early as 1946, Membo Nggoimba (Grove 1946), who first came to Kuk with a party of fellow Membo. This event may have occurred in 1960. Nggoimba's decision to emigrate must have been important and as suggested by Strathern (1972: 156-8, 247-8) could have been caused by some areas of Buk having reached a near maximum carrying capacity (Gorecki 1979b). Other reasons which have influenced such a move were the established pacification of the area and the preliminary enquiries from the Government regarding ownership and possible alienation of swamplands in the Wahgi (Griffin 1958). Finally the existence of a major road linking Mt. Hagen with Minj (passing just south of Kuk, Map 2) and the growing attraction of Mt. Hagen may have precipitated this migration.

This could have led Nggoimba and the Membo to return to Kuk. Similar migrations to unoccupied lands are found elsewhere in the Highlands (Kelly 1968:53). The same could be said about the Mandembo clan who followed the Membo shortly later. These successive migratory waves have important repercussions in the land distribution and boundaries between the Kawelka clans. The resettlement at Kuk during its early stage seems to have always operated through Ep Ridge (see Map 1). Nggoimba, after agreements with the Djiga tribe living west of Kuk, crossed the swamp and took over for the Membo the area known as Kenta and Mapa, leaving Ep ridge for the Mandembo. Although there was a lack of timber at Kenta and Mapa, the area was said to be better than Ep, with low lying hills surrounded by small swamps easy to drain if necessary. On the contrary Ep has rather steep slopes dropping straight into the main Kuk swamp, with little flat dryland available. Later on, a young Mandembo who was living with the Membo in Kenta found some good gardening grounds among the "islands" located east of Kenta. He started there the Mandembo hamlet of Kukrumdi. Such was the situation at Kuk that when the Kundumbo clan made its move there, only a small amount of vacant land was left: it was south of Kukrumdi, among some dry hills

and numerous large swamps (Kum hamlet).

Air photos of the Kuk swamp before its alienation show that people were draining portions of it in Kuning, Kenta and Kukrumdi. In 1977, all the Membo territory was divided into permanent dryland and swamp-land gardens with little land left in fallow, and pigs were grazing in neighbouring bushland outside Kawelka territory. Almost the same could be said for the Kundumbo. On the contrary, the Mandembo had communal gardens only in Kukrumdi and Kuning, the remaining land being left for pigs and small individual gardens.

It is these population movements and their impact on the environment over the last 20 years that I wish to discuss in detail now. To complement this ethnographic material I have made an indiscriminate surface collection of prehistoric artefacts throughout the Kuk area over the eleven months of fieldwork. These data are discussed below and the correlations existing between this collection and the ethnographic data are discussed.

While the entire Kuk settlement could be discussed here, I have deliberately restricted myself to the area located south of Kuk Agricultural Research Station (Kuk ARS) for two main reasons. The first one is that it is the area which has been the most intensively occupied, manipulated and modified over the last 20 years, thus making it more interesting than the area north of Kuk ARS, along Ep ridge, which even today has relatively large undisturbed tracts. The second reason for my concentration on ethnographic data from the area south of Kuk ARS is the availability of aerial photographs from different periods which cover it. This is also the southern catchment area of the Kuk swamp, an area which could have been of major significance for the history of the swamp (Golson and Hughes 1977).

These photographs are an invaluable source for ground survey and mapping of present and past activities as well as for cross-examining

informants. Maps 3 to 13 were drawn principally from aerial photographs taken in 1969, 1970 and 1973, and from my research in 1977. I have now in my possession another aerial photograph taken in January 1980 which would be outstanding for follow up work; unfortunately I received it too late to include it in this thesis. While there are further extensions of activity into the swamps by 1980 (further witnessed during a short visit to Kuk in February 1982), the general trends noted between 1960 and 1977 and detailed here are not affected by them. It can be said that the 1980 photograph confirms the macro-analysis which is presented here.

The developments observed and analysed here are deeply affected by external influences, two of which must be stressed. The first one concerns the immigration to Kuk which is an area attractive because of the proximity of numerous roads, markets and plantations. Not only travels to numerous towns or settlements are stimulated by these roads, but also the possibility of wage labour and cash cropping. Kuk ARS must have a profound impact upon the Kawelka community and upon developments which have occurred at Kuk over the last 20 years.

The second major influence on the developments discussed below is also caused by Kuk ARS. It concerns the drainage of the plantation itself and its access roads resulting in the drainage of a large section of South Kuk. Of primary importance is the southern boundary ditch of Kuk ARS which traps and disposes of some of the critical outlets of the area. Added to this are major ditches found along Tibi and Kuk roads (also under the care of the Government). These ditches penetrate deeply into South Kuk and as such are not only draining extensive areas but also allow Kawelka ditches to be connected with them. The processes one witnesses at Kuk have been substantially guided and accelerated by these Government ditches.

When I started my fieldwork, I divided the area into drylands (most of the time dry although flatlands being muddy during rainfall), wetlands

(surface water during rainfall and muddy during dry spells) and swamplands (water table always on surface). I found this division to be difficult to maintain during the fieldwork, principally because of the amount of drainage which had taken place since 1960. Aerial photographs came to the rescue here to establish a redefinition of soil types encountered at South Kuk. While the three categories above are probably correct, I am following in this thesis a revised definition where only two types are recognised: drylands which become a type of wetland during long spells of rainfall, and swamplands which in contrast become often only wetlands during dry spells. I found this new soil type division to be more convenient to deal with, and this is a direct result of ditching activities coupled with vagrancies of the water table level. Its fluctuations are responding quite dramatically to minor climatic changes so that a definite statement on surface moisture can hardly be made. In agricultural terms, dry crops can naturally grow only in drylands; in swamplands and wetlands, their growth would require ditching activities to raise garden beds.

2-2 PERIOD 1: 1960-1969

South Kuk covers about 3.4 km^2 or 340 ha, which can be divided into 231 ha (68%) of swamplands and 109 ha (32%) of drylands. There is strong evidence that in 1958 Kuk was still uninhabited (Gorecki 1979b) and that by 1969 there were an estimated 170 Kawelka established in South Kuk, or 50 people per square kilometre (p/km^2). Further details are given in Table 1.

The period is marked by the beginning of what one might call the colonisation of Kuk by a few men and the subsequent establishment of families resulting in increasing activities such as house building, garden making and pig raising. For the sake of this analysis I omit any activity which may have occurred at Kuk prior to 1960 so that I

can hypothesize about the possible archaeological consequences of a conquest of a "virgin" land by a small group of people and their increasing impact over the next 17 years.

While the initial gardening activities started on drylands only, it seems that within three years there was a move towards swamp margins and swamplands in such a manner that gardens were found both in dry and swamp areas. Such a move is a consequence of two phenomena: i) firm confirmation of Kawelka's tribal rights and boundaries from surrounding tribes; ii) anticipated rapid population increase through immigration from other Kawelka settlements, principally from Buk. The location and extent of these gardens are a reflection of clan affiliation. There is a lot more happening in the Kenta-Bagla area (Map 3) which is owned by the first-arrived Membo clan than in Kum, owned by the last-arrived Kundumbo clan. The general trend is for large areas to be fenced, larger than required, within which individual and communal gardens are made. These large enclosures are a result of the expected substantial immigration.

Two kinds of ditches are taken into account in this macro-level analysis: the major ditches and the plot ditches, both of them analysed more in depth in the micro-analysis of gardens (Chapter 7). Major ditches are those which undoubtedly are dug through the top soil and into the basal clay. These ditches are generally wide and deep and have an embankment along their gardened side built from their removed spoil, with an additional fence on top of this artificial embankment. This is what I call a "ditch and fence enclosure". Plot ditches on the contrary tend to follow the surface of the basal clay, and not to penetrate it. Their spoil is thrown on either side to raise the beds which will be gardened. These ditches are regularly laid out, usually at right angles and parallel with each other, to form a general "grid-iron" pattern. They are narrow and shallow and about 3 to 4 metres

apart. Thus one might say that for one hectare being opened for gardening, a minimum of 500 metres of plot ditches and 400 metres of major ditches is required.

Following the high immigration at Kuk during Period 1, one might say that generally speaking there are more major ditches than necessary while areas gardened (consequently plot ditches) represent the minimum required to sustain the economy (humans, pigs, exchange). By July 1969, there were 17 km of major ditches dug at South Kuk, or about 100 m per head (Table 1). In terms of gardened land, by 1969 some 50 ha were cultivated, or .29 ha per head. Overall some 25 km of plot ditches were dug, or 1.5 km for every person inhabiting South Kuk. There were 40 ha of gardens located in dryland (80%) and only 10 ha in wetland (20%).

Beside these figures, one must take into account other activities such as the building sites (houses and others) and pig disturbance. Unfortunately the figures available for them seem to me to be too unreliable, and as such are integrated into the general discussion only at the end of this macro-analysis.

2-3 PERIOD 2: 1969-70

This period lasts only 15 months (Map 4). It confirms trends noted during Period 1; more major ditches are dug than required, and particularly more areas are now gardened within the enclosures. There seems to be a shift of emphasis from dry to swamp lands. The Membo clan moves right into swamplands, particularly in the Kenta and Kuk Waiga areas. Very limited gardens are extended in drylands throughout the period. On the contrary, numerous new ones are being made in enclosed swamplands. While the entire enclosed areas are not yet under cultivation, new major ditches are being made in yet undisturbed areas (in Rombugl, Bagla and Kuk Waiga).

The Mandembo clan is still mostly restricted to dryland activities,

principally in Kukrumdi and Mup. Limited swampland gardens are found in Kuk and Kukrumdi. While during Period 1 the entire enclosed areas of the Mandembo were under cultivation, one can notice that preparations for a move into swamplands are under way in Kukrumdi by the means of major ditches straightening the course of two creeks. The Kundumbo clan has no garden extension during this period (in Kum), but major ditches for future development are being dug in both dry and wet areas.

Quantitative figures are as follows (Table 1). The population is estimated to be around 190 persons by the end of the period, or a density of 56 p/km^2 ; 9 km of major ditches were made (or 450 m for every new arrival) for a total of 26 km; 21 ha have been gardened (or 1.05 ha for every new arrival), including 14 ha in wetland, for a total of 71 ha. These figures (particularly the 450 m of major ditches for every new arrival) point toward a planned extension of activities rather than a stabilized agricultural system.

2-4 PERIOD 3: 1970-73

The processes noted for the previous two periods are still operating (Map 5). Strong moves into swamplands occur, but there are also substantial new activities in drylands. The Membo have extended their gardens into swamplands of Kenta and Kuk Waiga as well as into drylands of Mapa, Gengla, Kenta and Rombugl. A few new enclosures are made, the new major ditches being generally located within already enclosed areas. The Mandembo, as noted earlier, have utilized most of their newly enclosed areas, suggesting a closer relationship than the Membo between areas enclosed and areas cultivated. The Kundumbo have increased their gardens now, with a noticeable switch to swamp gardens and planned swampland extensions.

Quantitative figures for the period are as follows: The population has risen to about 310 by the end of the period, or a density of 91 p/km^2 ;

16 km of major ditches were made, or 133 m per new arrival. This is a marked decrease when compared with the previous period. In terms of overall trends, one sees a stabilisation of major ditching activities and an increase of areas being gardened. This suggests that perhaps the anticipated peak of immigration has been reached. Some 65 ha of new gardens were made (overall 136 ha) divided between 37 ha in wetland (overall 61 ha) and 28 ha in dryland (overall 75 ha).

2-5 PERIOD 4: 1973-77

Numerous new developments and changes occur during this last period (Map 6). The Membo have almost their entire territory under cultivation, with only small pockets still undisturbed by gardening or housing activities since the recolonisation in 1960. The Mandembo have extended their swamp activities, principally in Kuk hamlet (but east of the Kuk road) where an extensive new swampland cultivated area is developing. The Kundumbo have extended their activities in swamplands, leaving their remaining dryland undisturbed as it was during the previous period.

Quantitative figures at the end of this period are of interest. The population is now 571, or a high density of 168 p/km^2 . While only 13 km of major ditches have been built (or 50 m for each new arrival), an impressive 71 hectares of new land was opened to cultivation. These are divided between 57 ha in wetland and only 14 ha in dryland. It seems that the situation at South Kuk is now entirely dependent on internal developments: new gardens are made according to local needs rather than external expectations.

2-6 SOCIAL IMPLICATIONS

The trends followed by the Kawelka at Kuk between 1960 and 1977 must be seen in their detail as being particular to that group for that period. It is probable that another group settling in at Kuk or in any other new

area would follow a pattern directly related to its history, tribal size, social structure as well as its personal view of the potential use of a particular landscape.

Thus for the Kawelka, this recolonisation of Kuk appears to take strongly into account the knowledge of an artificial (immigrant) population explosion resulting in the making of large communal gardens. This was done by a careful use of both drylands and wetlands to achieve a particular equilibrium suitable for the economy planned. Despite this, each clan was directly competing against the other for land to a point that both Membo and Kundumbo appear to have reached a critical level of land use. Important decisions were made regarding garden extensions, population growth and size and location of pig herds. The increase of one parameter (population, gardens, pigs) should mean not only the increase of the others but more importantly the encroaching into the potential expansion of another of these parameters.

For these two clans, the stability of their socio-economic system is maintained by raising pigs outside Kawelka territory and in adjacent grasslands. If this were to be no longer possible, there would be other important decisions to make. These could be the reduction either of cash crop gardens other than coffee plots (sweet potato, corn, peanut, bean, etc.) to increase the size of pig grazing land or of the size of pig herds, perhaps even keeping pigs permanently penned. One attempt under way now to solve this problem is the shifting of pig herds to Ep ridge. This requires not only Mandembo permission but also lengthy daily travels to feed pigs, the risk of having pigs stolen, or the establishment of small sweet potato gardens under the care of one person who lives there especially for the raising of pig herds. The latter process is already operating for some Membo in Prkl Robri.

A possible obstacle to such expansion on Ep ridge appears to be the regeneration of the bush on the ridge itself which has been underway

since recolonisation. I do not know if this is a deliberate attempt, but it is certain that on the 1955 aerial photograph of Ep ridge at Kuk virtually no forested areas are left and that since recolonisation (subsequent aerial photographs) the forest has expanded considerably at the expense of surrounding grasslands. Today the forest is dense, abundant in possums and bird life (including hawks) and its exploitation is carefully supervised by some Mandembo. Birds of paradise have now returned (or have multiplied) on Ep ridge (M. Gunther, pers. comm.). Overall there is great pride among the Kawelka in having that expanding forest.

It is my strong belief that the recolonisation of Kuk which I have described has been deeply influenced by the proximity of Mount Hagen township and the ability to market cash crops there. I have tried to quantify the extent of these market crops, the arable surface taken by them but have reached only approximate figures because it is an investigation which is time consuming requiring extensive cross-checking. But I suspect that it is substantial.

The figures I have suggest that there is a minimum of 40 ha, possibly 70 ha, of cultivated land being reserved for a market oriented economy. This represents 20% (possibly 34%) of the gardens not being made for a subsistence economy. The major cash crop is coffee. Its expansion started almost with European settlement (Brown and Brookfield 1959) and has developed in such an explosive fashion throughout the Highlands (Donaldson and Good, 1981; McLaren 1982) that it may soon generate major socio-economic problems.

South Kuk could easily sustain a much larger community than the present one without reaching a similar critical level by reverting to a more traditional economy (but undoubtedly less prestigious). Following this, it is probable that South Kuk could support a population density of more than 300 p/km^2 by following a socio-economic pattern similar to the one which was in use in the region just prior to European contact.

At a more general level, the recolonisation of Kuk as detailed above can serve as a framework on which data on population migration, expansion and land use can be tested. It is certainly important to be able to quantify the amount of land used by a known population size, and the potential archaeological testimony left. This Kuk example should not be seen as a detailed model to be applied elsewhere, but rather as one of many guidelines on which models particular to some archaeological data or specific geographical areas could be tentatively built.

2-7 LANDSCAPE IMPLICATIONS

From the above description of the processes involved in garden activities and extensions, many points can be raised concerning their landscape implications, i.e. their archaeological potential and interpretation. General trends only are discussed here, at the macro-level of analysis. A micro-level analysis of these and other activities is dealt with later in this thesis.

It is important to discuss what the Kuk community has done to its landscape over the 17 years of recolonisation. It seems that a strong relationship exists between dryland and swampland agricultural behaviours. A spatial analysis of the ditching network at South Kuk should give clues for a general understanding of land management and development. In drylands, the successive addition of individual gardens from one period of occupation should give a pattern distinct from communal planning. In the swamp a similar behaviour appears to be operating, although at South Kuk, swamp gardens seem to be less individual and more collective. From the analysis of this general network of ditches it appears possible to substantiate the economy involved and to quantify the size of individual blocks owned. The entire question of intensification may also be raised, although the South Kuk example given here appears to be intensive from its beginning, thus lacking the

presumed original phase of swidden cultivation. No gardens have been put into a long fallow phase; all fallow gardens or small blocks seen were under a regime of allowing kunai grass to grow back for a short period, perhaps less than six months, and then of recultivating them intensively. It was noted that at the end of this short-fallow period, tubers were in fact still harvested (although of a small size) to feed pigs.

It is generally accepted that drainage/swamp/irrigation agriculture implies a high labour input for building and maintaining the system, especially when compared with swidden systems. Is this really so? The ethnographic literature on this topic is divided, some clearly stating such hardship (e.g. Serpenti 1956; Pospisil 1963; Clarke 1971) while others do not take a firm position (e.g. Heider 1970). As soon as the relationship between input and output is involved, then there is no more dispute on this issue: in general drained fields are more productive than swidden systems, at short and long term (see discussion in Clarke 1966; Clarke and Street 1967; Modjeska 1977).

My experience at Kuk suggests that the notion of "high labour input" should be discarded or at least the underlying notion of "slave type"

sustained work should be discarded. I have witnessed numerous examples of people building new ditches or maintaining old ones, but was unable to time and quantify these activities. The overall impression is certainly not of "hard" work. In one case, a small team of up to six men, but more often a single man, was able to build a major ditch 2 m wide, 2 m deep and some 300 m long in a very relaxed style. It was done at a leisurely pace and I found it impossible to estimate the man-hours involved (e.g. Waddell 1972). There were days or even weeks when nobody was working on that ditch, and when at work, there were enormous discrepancies between one day and another.

In another case, one man alone built in 1977, and fairly quickly, another major ditch (1.8 m wide and 1.5 m deep over 160 m) before I

noticed the work being done. When I planned to time the labour involved, he never came back. When I returned to Kuk in 1978, the ditch was still as I saw it some 9 months earlier, untouched and overgrown with pitpit. During my 1978 fieldwork, the same man finally finished the ditch, an additional length of 350 m, done in a single period, but here again I missed the timing. The man came to see me only afterwards to announce proudly that he had just finished the work and asked me if I wanted to look at the exposed sections of the ditch as well as some wooden posts, prehistoric, recovered during its digging. Here again I have no data on labour input for such activities, except a general impression that the man enjoyed doing the work, but did it his own way: in two bursts some 10 months apart.

Following a series of failures to quantify the man-hours involved in the making of these ditches and their maintenance, I finally decided to rely on timing made by others, such as archaeologists doing an experiment where a "mark" is set and the worker is free to go home when the mark is reached (Puleston 1977:39-41; Steensberg 1980:89-90). A similar working method is applied by modern plantations in the New Guinea Highlands and I found the data gathered from it more reliable because there is much less pressure put upon labourers: they simply carry on a daily routine work while those working under archaeological experiment condition generally cannot sustain the early pace. The following examples are from such plantations, both of them reclaiming swamplands (one located in the Upper Wahgi and the other located in the Middle Wahgi).

Example A

Major drains: 1.5m deep, .9m wide at the top, 1 spade wide (.25m) at the bottom.

6 metres required per man per day, including cutting of grass, setting

of bamboo posts for general direction to follow and digging.

The work is done in 4 hours by the fastest men (or, as explained by the Manager, men who are in a hurry that day because of other commitments).

Example B

15 cubic metres required per man per day.

Main drain: 3m wide, 2m deep

Primary drain: 2m x 1.5-2m

Secondary drain: 1m x 1m

Tertiary drain: .6m x .6m

The general pattern is that the work of all men is done within 4 hours to 4½ hours.

In Example A, the pattern is to spend one day clearing the grass and lining the posts; the next day is entirely spent digging the required 12 metres per man. For Example B, the division of labour (decided by the labourers themselves) is different. There is one team preparing the land and a second team specialised in digging. Whatever the method is, workers usually try to be back in their settlements by lunchtime. Despite the fact that these ditches are made with steel spades, it appears that tasks performed with wooden spades (used in pre-historic times) were only marginally (half) slower (Steensberg 1980: 89-90).

Following the example of South Kuk where 54 km of major ditches were made in 17 years (of sizes smaller than those for the above plantations), and using the figures given for the plantations above, one can speculate on the man-hours involved in such work. Assuming an average ditch size of 1.5m x 1.5m, some 121,500 cubic metres were removed at South Kuk; if 3 cubic metres are removed per man-hour, there are 40,500 man-hours required; if a man works 5 hours per day, a total of 8,100 days are necessary. It would take 22 years for a single man to build the entire 54 km of major ditches at South Kuk; a team of 50 men could

do the job in less than 6 months.

What can we say about "maintenance"? At Kuk it is infrequent. Ditches are cleared only when the system is clogging up, or when there is a threat of overflow into adjacent gardens, or when foot tracks become too muddy. There is no season or regular pattern for maintenance activities. Ditches are allowed to fill up as long as possible, thus generating a natural build up of a rich mulch in them.

There is no doubt that the above 54 km of major ditches will remain there as permanent archaeological features. For reasons explained later in this thesis, all the network of ditches, tributaries, extensions and modifications are permanently imprinted in the local landscape. Beside these, people have carried out other activities which have affected the landscape. Some of these should have archaeological evidence and others not. For instance, another 1,000 km of smaller ditches were made over these 17 years, together with over 1,000 building sites including at least 500 houses which have been occupied by humans. All this is for a population which has risen from 0 to 571.

The picture left by these activities suggests strongly that major archaeological features such as houses and gardens will be interwoven to such a degree that no predictive model can be built (Plates 3 to 6, 24). It is as if the entire South Kuk should be considered as a single archaeological site, within which smaller units, or specific activity areas, could be defined. Whenever an archaeological trench is opened at South Kuk, it is highly likely that there will be a find to be made, whatever the location of the trench. While we know that this is valid for only 17 years of occupation, then chances of finding anything pre-historic are even greater because of the evidence of human presence at Kuk over the last 30,000 years, including 9,000 years as a garden area .

Before moving into the micro-level analysis which attempts to

quantify the finds in terms of the correlation between reality (occupied site) and the formation processes which have occurred in them (excavated site), a digression is made here, dealing with the prehistoric evidence of dryland occupation I have gathered at Kuk by carrying out a surface collection of all stone artefacts I could find.

2-8 ARTEFACT COLLECTION

Since the Stratherns arrived in the Highlands in 1964 to work with the Kawelka, and particularly since the alienation and drainage of the Kuk swamp resulting in the discovery of early agricultural systems (Allen 1970a) which was followed yearly by an invasion of various archaeologists and related scholars and students, stone axe blades have received such attention that by the time I arrived at Kuk in 1977, it appeared that this supply of prehistoric artefact at Kuk was virtually exhausted.

I estimate that from Kuk alone more than 1,000 axe blades have been sold or given to visiting Europeans or at the Hagen market. While little of these collections from Kuk have been analysed, but substantially more from other areas (Strathern 1965, 1969; Lampert 1972; Hughes 1977; White 1977a), the fact remains that there are no data on what and how much was exactly removed from Kuk. It is also unknown where the finds were made within Kuk, not even in a broad perspective (e.g. dryland or swampland). Most of these archaeological informations are now lost. Some of the material excavated or collected by Golson's team at Kuk is now under analysis by John Burton (A.N.U.; Golson, pers. comm.).

Despite this negative background, I decided to attempt a new collection, preferably from finds made by myself, and to plot these on a map. Knowing that the "best", or rather the prettiest items were already missing from the record, I decided to collect all stone artefacts, regardless of the size, quality and nature. It was ultimately hoped that some hypotheses and assumptions regarding their use and discard

could be made, possibly even that activity areas could be defined (e.g. White and Modjeska 1978:283-5).

The most immediate aim was to check a personal assumption I made, that the 1,000 axes I missed were in fact only a reflection of what the local community thought of Europeans' interest in stone artefacts. I assumed that it is only when this supply dries up that more important archaeological information may come out of Kuk. Damaged artefacts, axe blanks, small stone tools, etc. should be of greater archaeological value in terms of understanding a prehistoric economy and technology than specific artefacts restricted to one economic or ceremonial activity (see Bulmer 1964:250, footnote).

To have a controlled sample, I also decided to find one man in whom I had confidence and to ask him to do an intensive collection within his own garden over a period of almost one year. A man called Joseph agreed to do this in a communal garden where he had a share, located right on the NW boundary of Kuk, on Ep Ridge. This is now known as Joseph's collection. A price was set where full unbroken axe blades were worth the same as axe chips less than 1 cm long to ensure that everything would be collected. To ensure that no material would be added to this particular collection, Joseph received two bags, one of them to fill with artefacts from his garden and the other for artefacts he could find elsewhere at Kuk (for which a similar price was fixed). This experiment was highly successful and my only regret now is that I did not set a second controlled sample in the South Kuk area for a comparative analysis.

Most of the collection I made comes from areas where I mapped existing human activities and because of this there are important geographical gaps: parts of Ep ridge and some swamps along the southern boundary at South Kuk were not visited and consequently no collection was made there. Similarly, some areas have been visited almost daily, such as Kukrumdi, while others received only occasional visits; the

intensity of the collection varies accordingly. Finally, the last bias in this collection comes from my informants and the Kuk community itself; only large artefacts were pointed out to me, particularly fragments of polished axes. Only in a few cases were flakes or axe blanks shown to me; of particular interest is the fact that no axe blade or chips made of a river pebble or any small flaked stone tools or waste material made of chert was recognised as being of relevance. I had to find these myself, and in some cases where dense scatters were found, people still did not show interest in them.

All these problems regarding the collection are taken into account in the analysis below. I feel confident that its results present a fairly accurate macro-picture of Kuk regarding site prediction and location (or rather the lack of such prediction), as well as the potential of artefact recovery. The collection is divided into three classes of artefact: (i) stone axes and the material derived from them, (ii) small artefacts (tools and waste), essentially chert, (iii) other artefacts.

2-8-1 STONE AXES

After the collection was made and during its analysis, I found it necessary to divide the stone axe material into various categories. Five of them concern full blades or large fragments and another three categories concern smaller fragments and a specific flaking activity (rejuvenation of cutting edge).

The largest number of axe blades found were of the planilateral type which appears on the Highland scene perhaps 5,000 years ago (Bulmer 1975), fully ground, and was still in use at the time of European contact. It is this particular type of axe which has been the subject of most analyses. The collection I made in 1977 and 1978 (all excavated material is excluded in this analysis) contains 93 full blades or large fragments of planilateral blades. By the latter I mean fragments which

represent at least 30% of the entire blade, and within which three categories are recognised: cutting edge fragments, central sections where both cutting edge and poll are missing, and poll end fragments.

The distribution of these 93 artefacts (49 full blades, 11 cutting edges, 9 central sections and 24 poll ends) is shown in Map 14. On all the stone tool maps (14 to 23), a dot represents a single find while a number in a circle represents the number of artefacts found. These maps are adapted from Map 2. It appears that these artefacts are fairly well distributed throughout the Kawelka territory and are found in swamplands as well as in drylands, particularly nearer to these swamps.

One notable gap concerns the SW corner of Kuk and west of Kenta creek (Gengla and Mapa hamlets, see Map 1) from where not a single one of these artefacts was found. This is despite my regular visits to this area, densely inhabited and cultivated since the beginning of the recolonisation of Kuk. It is interesting to note that this particular area, together with the upper slopes of Ep ridge (also where no such finds were made, although there the surface visibility is reduced) are the only substantial drylands which are not adjacent to large tracts of wetlands. This area could represent an archaeological "black hole" (Groube 1981).

The only significantly high concentration is found in Joseph's collection (28 artefacts) along the NW boundary. The area intensively collected by Joseph over a period of almost one year is a communal garden 275 m long and 162 m wide (4.5 ha).

The second type of axe blade under review concerns the fully ground lenticular type which is present in the Highlands from at least 9,500 years ago (White 1972; Bulmer 1975) to about 5,000 years ago when supplemented with the planilateral type. I found very few of these lenticular blades or large lenticular fragments (1 full blade, 3 central sections, 1 poll end) so that their distribution (Map 15) is not very

informative. One can nevertheless note that three of these five artefacts come from three distinct locations within the main Kuk swamp, while the two others are from drylands adjacent to swamplands. None is from Joseph's collection.

The third type of axe blade is with "battered sides". Here I follow Hughes who states, and I believe rightly so, that axe blades with battered sides "exist in sufficient numbers to form a distinct prehistoric type in the Upper Wahgi Valley and adjacent areas" (Hughes 1977:149). This type of blade is strongly related to the planilateral type. I found only 8 of these (4 full blades, 4 poll ends), including 2 in Joseph's collection, and here again the number involved does not warrant a reliable distribution analysis (Map 16).

The fourth type concerns axe blades made of river pebbles. To my surprise, the Kawelka refuse to consider these as being man-made. They always state that they are naturally shaped like that or were made by spirits, but certainly not by man (cf. Bulmer 1964:251). Beside the three blades collected at Kuk (Map 17), I have purchased many pebble blades from the Wurup valley to the south. When 17 of the oldest men at Kuk were interviewed regarding axe manufacture, provenience and classification, none of them took interest in these pebble blades.

Because of this, I tend to see them as being a distinct type. My impressions are that the Wurup area, as indicated by my collection made there, as well as from a brief examination of Christensen's collection made in the same area (White 1977a), suggest a Wurup origin for them, possibly as an economic answer to axe manufacturing activities carried out at one of the most important axe quarries, Abiamp, located to the east of Wurup.

I have collected only three pebble blades (2 full blades and 1 cutting edge), two of them being planilateral in cross-section and one being lenticular. The sample is too small for a distribution analysis

(Map 17); one artefact is part of Joseph's collection and the two others come from the main Kuk swamp. The only point of interest here is that none was found in the South Kuk area, which was by far the most visited by myself.

The last type concerns axe blanks. Only nine of them were found (Map 18), all roughly shaped by flaking but not enough to start grinding them. This type, although small in number, indicates clearly that trading activities from quarries did not concern only finished products, but also semi-finished and blank blades. Their distribution appears to be uniform, with 2 specimens coming from Joseph's collection.

Numerous planilateral axes in the collection show that attempts were made to rejuvenate damaged edges by bifacially flaking them with a hammerstone. None of those recovered shows signs of grinding activity posterior to this rejuvenation. Twenty two such blades were found, including 12 in Joseph's collection. None showed fresh fractures and all are considered to be genuine. The distribution of these blades (Map 19) does not show a clear pattern, except perhaps that more of them are found in South Kuk than on Ep ridge.

There is no doubt that since stone axes were made at Kuk, as well as used and reshaped there, one should expect to find small flakes and chips derived from these various activities (cf. resharpening described in White 1977b). Small polished flakes were found in quantity (113) all over the Kawelka territory (Map 20). No less than 42 of them were collected by Joseph in his garden. One high concentration within a restricted area was noted in Kukrumdi where 16 polished small chips of a wide range of colours were found. Another concentration (14), although more widely spread than the previous one, was noted in Kuning Tip. Most of the chips and flakes from the latter could be fitted together. The finding of these conjoins unfortunately was suspicious and a close examination of all fractures showed that they were fresh except for those

for which conjoins could not be found. The latter showed a distinct patination suggesting some antiquity. I think these 14 flakes belong to a single blade which was smashed up during my stay at Kuk. I do not consider it as being a "natural" concentration. As noted for rejuvenated blades, polished flakes appear to be more frequent in South Kuk than on Ep ridge.

If one excludes one specific location in Tibi Residue, one can say that fewer unpolished flakes and chips were found (40 of them, Map 21). The concentration of polished flakes noted at Kukrumdi is complemented here with the finding of 20 unpolished ones, also from raw material of distinct colours. Apart from Joseph's garden, no unpolished flakes were found on Ep ridge.

One location in the Kuk swamp (in Tibi Residue near its boundaries with Tibi and Baisu) had an unusually high number of raw flakes: over 500 of them were found at the foot of a natural mound within the swamp, over an area about 10 m by 10 m. Such density appeared to me to be indicative of being a sort of working floor where stone axes were made (over 500 unpolished flakes, some of them large, against 3 small polished chips); this site is now coded MNN.

2-8-2 SMALL TOOLS

Beside stone axe blades and artefacts derived from them, I expected to collect throughout the Kawelka territory other stone tools, i.e. the flaked material proper (White 1972:5-9). To my surprise, such artefacts appear to follow a distribution pattern entirely different to the one noted for the axe material.

Overall 847 flaked artefacts were found, but these were not dispersed all over the landscape: most of them were recovered from only 5 small locations, now designated by a site code number (Map 23). The number of artefacts collected within each of these sites varies (Table 2)

but it must be pointed out that only 33 flaked artefacts, or 3.9% of this particular category, were isolated finds. Because of this specific distribution, and to emphasize it, only the large concentrations are represented in Map 22.

Because the aim of this thesis is not related to stone tool technology, only a superficial analysis of this material (with the expert assistance of J. P. White) was carried out. Only 11.8% of the material, or 100 artefacts (Table 2) consist of tools showing some use-wear on at least one edge, within which the great majority consists of steep angle edge flakes (78 flakes), the remainder being "scrapers" with an acute angle edge (22 flakes). Four of the five sites have steep angle edge flakes largely dominating the other type. Some of these artefacts are illustrated in Figs. 3 and 4.

Ten scalar cores (White 1968:665) were identified and as far as I am aware these are the first ones to be reported from the Wahgi region. This evidence points towards a discrete but probable Highlands "technological tradition" (White 1968:664).

The location of these sites strongly suggests that they were deliberately chosen because of their look-out properties: they all are on top of hills or mounds having a clear view over the surrounding land. Sites MNL and MNK are both found on top of hills dominating a dryland environment. These two sites are also the only significant finds made in the entire Kawelka territory west of Kenta creek. The three other sites are found in contrast at strategic positions overlooking large tracts of swamplands. I noticed this peculiar pattern while still in the field and I checked other locations in South Kuk which appeared to be in similar vantage positions but I could not find anything relevant to this question.

Site MNN is of particular interest. It was stated above that the largest collection of stone axe flakes and chips was made at the foot

of a hill located in Tibi Residue. It is at the same hill, but on top of it, that the concentration of flaked material was made. It is a small hill and despite this the two collections appeared to be not at all related. I do not know if these represent two specific activities from a single occupation period, or, as I think more likely, each of these concentrations represent one specific activity of two distinct and entirely unrelated periods of occupation. To support the latter proposition is the evidence that stone axe material (from any of the major quarries), polished or not, was never recovered from within any of the five concentrations.

Associated with the finds made at MNP were manuports, or larger rocks which appeared to have been unmodified or with no apparent shape or function. Some were soft rocks such as sandstone, others were made of basalt. These manuports were not quantified, but they largely outnumbered the flaked material. It appears that similar manuports are associated with the earliest agricultural phases found in the Kuk swamp (J. Golson, pers. comm.).

Overall, the present collection appears to be similar to a smaller one made in the Upper Kaugel valley (at much higher altitude), where steep angle edge flakes also dominate the tool component (Allen 1970b). Looking at stone collections from the New Guinea Highlands, including the one from Kuk, one cannot escape the frustrated feeling that "the dominating impression produced by a collection of prehistoric stone and bone implements from the Central Highlands is one of sameness and continuity ... It is also very similar from 9,000 BC to the present" (White 1972:148).

2-8-3 OTHER ARTEFACTS

While stone mortars and pestles are found throughout the Highlands (Gorecki 1982a), none was found at Kuk during my fieldwork. The few I

have seen are still held by the Kawelka and are considered as sacred, bringing better crops and numerous pigs and wives. Many of these mortars and pestles were in fact found within the Kuk drylands during gardening activities or pig wallowing. They were immediately collected by the finder and kept as lucky stones. This behaviour may explain why I could not find a single specimen: they are immediately recycled and hidden somewhere. One unfinished half disc, probably to be used as a stone club, was found by Joseph in his garden.

It is probable that I also have collected the "tong" of a broken tanged blade (Fig.1). It was at a location just outside Kawelka territory, on top of a hill surrounded by drylands. The blade itself appears to be missing and may have been broad. This implement could be related to similar finds, or "oddities", made from time to time in the Wahgi (Allen 1970a; Christensen 1975a; Golson 1977b). The extreme rarity of such implement in the Wahgi suggests it has not a strong cultural and technological tradition within the valley while such tradition clearly exists elsewhere in the Highlands (White et al. 1970; Bulmer 1977).

Grinding stones of two kinds were found (Map 22). One type is still being widely used today to sharpen steel implements. It is a large sandstone slab, fixed in the ground, on which a blade is sharpened by adding some water. Two grinding stones of another type were found, both from swamplands, which have no counterpart in the ethnographic present. These are long and narrow pieces of sandstone, looking very much like being grinding files. Unfinished planilateral blades found at Kuk have narrow grooves on them, strongly suggesting that this was one method used for thinning the blade. For this, it is the blade which is fixed and all the action is made by the file. This filing technique appears to be widespread because similar deep and narrow grooves were noted on some specimens I have collected at Abiamp and Pukl quarries. One blade

from the Tsenga quarry may also have been ground with a file and a slab, but because it was found at Kuk, I do not know if this grinding was made at the quarry or at Kuk. These two grinding techniques were also noted in the nearby Jimi Valley (Attenborough 1981).

It is possible that the slab technique was used for a wide range of maintenance activities, such as sharpening a cutting edge and grinding all surfaces of an axe blade, while the second technique, filing, appears to me to be related to activities of axe making, with the particular function of thinning the blades to the desired standard. These files could not have been used to sharpen cutting edges.

Despite the high number of stone artefacts of all kinds I have recovered at Kuk, only three hammerstones were found (Map 22). It appears that hammerstones were not really imported into the Kuk area. It is more likely that such a function was carried out by cooking stones which are found in quantity, lying around in the Kuk landscape. These are so numerous that their mapping would simply blacken the entire area. Because of this, cooking stones are not discussed here but are presented in detail in Chapter 5.

The last stone specimens to be discussed here concern two strangely shaped blades, very similar to each other in rock type, size and fractures (Fig.2). One was found by Joseph in the Kuk swamp (Kuk plantation) and following Joseph's explanations, it is possible that the artefact comes from a distinct grey clay layer (dated to 9,000-6,000 BP, Golson 1981a). The second artefact was found by myself on a spoil heap from channel deepening along the western boundary of Baisu, well into swamp-lands. Because of the local stratigraphy, and because of the characteristic dirt still attached to it, I strongly suspect that it was entirely covered by the same grey clay. Profiles of the channel at that location clearly showed the presence of Golson's agricultural phase 2 (6,000-5,500 BP), thus the blade may be related to that phase or could even be earlier.

Both artefacts appear to have not only their tip but also their margins ground down to a cutting edge. Use-wear on both blades is similar: a clear break at the tip and badly damaged cutting sides. The "butt" of the two blades also has a clean break, and because of this I suspect the breakage point may be the point of hafting. Thus it is possible that originally these two blades were much longer than they are at the present. They appear to be similar to two others, also found in the Kuk swamp (Allen 1970a). Because of these similarities, I support Golson's description of them as being "stone knives" (Golson 1977b). On the present evidence, I am inclined to see these knives as being specialised tools for swampland activities and having some antiquity. Against this interpretation are finds which may be similar to these "knives" made in dryland country such as Nombe (White 1972) and the Bismarck Range (Bulmer and Clarke 1970).

2-9 MACRO-PICTURE: DISCUSSION

The material presented here concerns two sets of data, both related to a macro-level analysis of Kuk. The first one concerns human activities over the last 20 years at South Kuk, while the second set concerns things that humans have left within the entire Kawelka territory of Kuk over a period of perhaps as long as 30,000 years. Are there general points which tie together these sets of data?

What people have done to South Kuk during these last 20 years (Map 6) suggests that little land is left undisturbed by human activities, while pig activities, particularly their rooting and wallowing behaviour, have left scars in those few areas less affected by humans. Overall, there should be archaeological evidence dated to less than 20 years all over South Kuk. To include the agricultural time depth known from Kuk into these data, or 9,000 years of swamp farming, one should theoretically multiply, but at a decreasing intensity, the South Kuk evidence

by 450 times! To reach the time depth regarding human presence at Kuk (30,000 years), the Kuk data should be multiplied by up to 1,500 times. Following this simplistic arithmetic, one can easily understand why prehistoric artefacts and features should be found in almost every square centimetre of Kuk.

While these prehistoric occupations may be stratified in swamplands where there is a continuous sediment build-up, the nature of surrounding drylands in contrast is such that all the human history, whatever its time depth, is stored within a perpetually disturbed top soil of an average thickness of only 30 centimetres. Below this top soil is an almost impenetrable compact clay acting as a sort of barrier for artefact penetration.

Major human activities such as housing and gardening will over a period of time expose buried artefacts from previous occupation periods. Because of this, it appears that in drylands the word "in situ" can hardly be applied to artefacts unless found as part of a structure such as a neat pile of cooking stones or found in a feature (thus undisturbed). The correlation between artefacts and nearby features may often be misleading. In a taphonomic context like the Kuk drylands, recycling of "meaningful" artefacts must be very high, even if the original "meaning" of the artefact is changed to a new one by its finder. For example, mortars and pestles have an unknown antiquity in the Upper Wahgi, and their "meaning" at the time they were made is still unclear for archaeologists. This original meaning is also lost for present-day Hageners who nevertheless keep collecting them when found, thus recycling them, on the grounds that the present meaning of these stones is of good fortune. It is possible that between the time they were made and today, these artefacts went through a series of "meanings", and it is unlikely that the archaeologist will ever be able to substantiate all of them.

Despite this bleak background, concentrations of artefacts should

not be dismissed, particularly if there appears to be some logical connection between the artefacts found: a concentration of flaked chert or fragments of axe blades may indicate a working floor, even if the floor itself has been disturbed or destroyed or buried on the clay surface below the top soil. Such concentrations may contain items which in fact are originally from an entirely different context or period: a working floor used by planilateral axe people may well contain lenticular axe material which was brought in from elsewhere, perhaps from a site 2,000 years earlier. The processes operating in the Kuk drylands are numerous and continuous, but they are essentially cultural. They include the entire systemic processes as defined by Schiffer (1976: 27-41).

While I would regard the finding of one stone axe blade as not being significant at all for the reasons detailed above, can I regard the finding of 93 or more isolated blades also as not being significant (Map 14)? Probably yes, because these finds are not part of a concentration. A study on the life of stone axes carried out also in New Guinea but in a different environment has shown that taphonomy is also complex, resulting in the fact that

"the archaeological record is composed of two groups of whole axe blades ... Whether an intensive collection of archaeological specimens would allow these groups to be distinguished we do not know, but given the probable total numbers involved, we doubt it." (White and Modjeska 1978:284-5)

I would also regard the finding of hundreds of isolated axe flakes as not being significant while I would regard the finding of a single concentration of hundreds of flakes as highly significant (Map 21). Even if this concentration has been disturbed and each flake is no longer "in situ", the concentration itself carries information that the archaeologist must try to understand. Consequently, few "significant" finds were made during my surface collection, and these are reviewed below.

A principle similar to the one for artefacts must be applied to features, with the critical difference being that all features are in situ. A single feature, whether a large and long ditch or a small post hole, is a meaningless information because the widest range of interpretations can be applied to it. It is when one has a collection of features, a "concentration", that there is prehistoric information to be grasped by the archaeologist. It is when both these parameters are found together, artefacts and features and each being in a significant context, that maximum information can be extracted. This is what I shall attempt to quantify in the micro-level analysis.

Overall I believe there are 8 important results derived from my surface collection:

- 1) It is probable that the 1,000 or so axe blades and other artefacts which were sold in the past to Europeans do not affect the macro-level understanding of Kuk's prehistory. It certainly affects data on trade (Hughes 1977) and petrology (Chappell 1966), but not site formation or location. This is particularly true since most of these artefact transactions concerned planilateral blades and other cultural items related to the most recent prehistory. I also believe that numerous axe blades sold were originally from Buk and its associated quarry, and brought into Kuk only after 1960 (Gorecki 1979b), being thus of limited archaeological significance. This large artefact "migration" may potentially distort greatly data on "prehistoric" trade at Kuk. Finally, blades which were really found at Kuk and sold were most likely isolated finds rather than parts of concentrations, thus again not affecting greatly local data.
- 2) Surface visibility within the area surveyed varied greatly, but some major conclusions can still be reached. Stone artefacts appear to be more difficult to find in swamplands than in drylands. This is not only for taphonomic reasons (and particularly suspected downward

vertical displacements in the swamps), but also because gardens in dry-lands are continually reworked (not the case for those located in wet-lands) thus making surface visibility better.

In normal weather conditions large artefacts were more often found than smaller. Under light rain, the recovery rate of all artefacts appears to be the highest, particularly with regard to the smallest. I did not try collecting under heavy rain. The pattern described here suggests that artefacts are covered with dust in dry weather and become exposed and "shiny" under wet conditions. Collections may differ substantially according to the prevailing weather conditions at the time they are made.

Organic remains recovered (not discussed in detail here) concern essentially wooden fence and house posts; one wooden axe handle was also found. All this material was exclusively found in swamplands and according to the stratigraphic context of each find, they are all posterior to a tephra dated to within the last 300 years (Blong 1982). It must be noted here that most of this recovered material was immediately recycled as firewood, thus again lost from the archaeological record. Swamps within plantation grounds (Kuk, Tibi, Baisu) were not surveyed, but artefacts mapped within them are those found by labourers during my stay at Kuk and left in situ for examination.

- 3) Not a single artefact collected by myself was in situ. They all were exposed on the surface, whether the top soil or the bottom of ditches, as a result of activities such as gardening, cooking, housing, fencing, etc., as well as pig rooting and wallowing. Even in areas apparently not disturbed recently, a close examination of the ground near a find showed signs of disturbance, particularly pig rooting. Pigs are great artefact excavators. Natural and cultural processes appear to be very closely interwoven in the formation of archaeological sites at Kuk.
- 4) The finding in Kukrumdi of a small concentration of 16 polished

flakes derived from various axe blades is in itself not highly significant (Map 20). The finding of 20 unpolished flakes at exactly the same location (Map 21), is also not really significant. It is only when these two sets of data are put together that the location of these finds becomes substantially more interesting. It suggests that someone in the past may have been sitting there, making or rather repairing various stone axes. It may not be someone specialised in such activity. My interpretation is that the scatter indicates a working floor where maintenance activities of stone axes was carried out by an individual rather than a group, and over a short period rather than a lengthy one. The evidence could also be interpreted as being the result of various occupations of the site (Bordes 1980a; Cahen 1980; Bordes 1980b), but it is most likely that one is witnessing only a single event.

5) In contrast, the stone axe material collected at MNN strongly suggests a highly specialised activity. I suspect stone axes were made from blanks at MNN, and because of its location (rather unfavourable), this activity may also have been carried out by an individual only. The rarity of such concentrations at Kuk and the peculiar location of MNN suggest a single event. The Kukrumdi concentration (point 4 above) may reflect domestic activities carried out near a house site while MNN may reflect skilled activities being carried out at a workshop.

6) Negative evidence is sometimes significant, as with the area west of Kenta creek (Groube 1981). While surface visibility there was relatively good, surface disturbance frequent and visitations regular, almost nothing was found. I suspect the lack (or low amount) of stone material there is real and has some real significance, but I am at a loss to explain it. A very cautious hypothesis would be that the area is within an environment where the major economic and social attractions are within swamplands, but found too far away from these and thus being neglected for drylands located closer to these swamps. If this is the

case, it implies that there would be a sort of transitional zone, made of drylands, between typical dryland and swampland environments (each with its specific economy) where little prehistoric activities occurred. Two chert concentrations were nevertheless found west of Kenta creek (sites MNK and MNL, Map 23). This contradicts somewhat the hypothesis above, unless chert sites belong to a period when there was no environmental discrimination (prior to planilateral axes).

7) The distinct distribution pattern noted for the flaked chert material (Map 23) is certainly of interest. It is as if these activities had nothing to do with those related to stone axes. I cannot think of any behavioural pattern to explain it, except perhaps an unlikely sexual segregation, which would end up in such a distinct dichotomy in artefact distribution. The most likely interpretation for such distinct distribution between axe and flaked material would be the one provided by ethnographic observations. Axes are used all over the place, flaked tools are used around houses; axes are not used much at house sites (e.g. White 1977b; White and Modjeska 1978). This would imply that sites with a concentration of flaked tools may be house sites.

In most cases one could think of, there could be a dominant group of artefacts at one location, but still with members of other artefact classes being represented. Site MNN is again informative because the chert material was found on top of a hill, well segregated from the axe material which was found in one location at the foot of the hill. It is as if these two classes have no relation at all, perhaps a result of a substantial time gap existing between the events which led to the two concentrations.

Based on indirect evidence detailed earlier, one could here again formulate a highly cautious hypothesis. While MNP, MNM and MNN appear to be swamp oriented sites, MNL and MNK in contrast seem to be dryland oriented. Because of some specific manuports, particularly at MNP,

which appear to be identical to others found in the earliest agricultural phases in the Kuk swamp, I wonder if all these five sites do not belong to an early phase of human occupation, a phase which was not entirely swamp oriented yet. This could be from a period earlier than the appearance of planilateral axes. It would imply that the sites must be more than 5,000 years old (no association of planilateral material) and would be of critical interest for the understanding of early events which have occurred in the Kuk swamp itself. This hypothesis can be tested by excavating at least one of the sites, and MNP would be the best candidate for this, in the hope of finding some features whose infill could be dated.

8) The last important result derived from my surface collection concerns Joseph's collection. While each artefact or each set of artefacts from Joseph's garden is not significant in terms of providing evidence of some specific prehistoric activity, the overall collection in itself is very informative. It is a collection made essentially of all sorts of small bits of things, which clearly illustrate that a wide range of human activities (and most likely those of pigs as well) has occurred there. It shows how under-representative my collection is, due essentially to the difference in time spent collecting at each location.

Overall Joseph has collected a much larger number of artefacts (and he still collects them for Golson who took over this "case study"). This has numerous implications. What is coming out of Joseph's garden appears to progressively represent a reality in artefact distribution and density. In terms of distribution, it shows that artefacts should be found all over the landscape. Because of the time span involved (30,000 years) and the various activities which probably occurred within Joseph's garden (theoretically from hunting-gathering/low population density to intensive farming/high population density), it is probable that the cultural and natural processes have deeply affected the

formation of possible sites. Some may have been destroyed beyond recognition. Nevertheless because only a few suggestive concentrations have been found (points 4, 5, 7 above), it is also possible that Joseph's collection reflects daily outdoor activities rather than highly localised and specialised events.

While my artefact collection and the South Kuk analysis directly concern the Kuk area, I am not sure how much can be extrapolated to other areas. I believe it can be tentatively applied to areas having a similar environment within the Wahgi, but I suspect only the general trends may be applicable to other areas, even those with similar environments. In other words, this Kuk macro-analysis may be directly relevant for areas such as Kotna, Banz and Minj, but may be only partially relevant for areas such as Tambul, Tari and Kandep. Only the collection of new data from other areas will help to provide general trends in site formation processes in different parts of the world and in different environments.

To gather further insight into these processes, one requires doing some close-up within Kuk, to see and understand more precisely how various ethnographic activities are carried out, what is their impact on the ground, what is happening after abandonment of a site, and finally how are our interpretations of the excavated archaeological remains derived from these ethnographic activities.

This is what I intend to do in the next chapters. It is a bit like looking at Map 6, decide on one dot in it, and then putting the dot under a microscope: what is there, why it is there and how it was made.

CHAPTER 3

HOUSES - ETHNOGRAPHY3 ETHNOGRAPHIC OBSERVATIONS

The traditional settlement pattern in the Upper Wahgi consists of small scattered hamlets or homesteads. This pattern is also found at Kuk, with isolated houses found all over the landscape. But a new trend towards house grouping has recently appeared: around new singing grounds built along roads (which are also market places), along the road itself, and around the Mission place at Bagla (Map 2).

Because this new trend will not be discussed elsewhere, it is worthwhile to quote a recent comment about it, a phenomenon encountered all around the Hagen region and probably beyond it:

"There is a gradual movement towards living in closer proximity along more formal lines and this has been encouraged by the missions especially the Lutherans. Whether this will have any effect on the living standards is yet to be seen but I would suggest that a serious health risk is involved unless rigid hygiene standards are imposed."
(Logan 1977:20)

3-1 SPATIAL DISTRIBUTION

If one excludes the above modern modifications, the settlement pattern found at Kuk is similar to the one found anywhere in the Upper Wahgi. It is still a traditional pattern, observed at the time of European discovery of the valley (Leahy 1936:243; Ross 1936:341-3; Leahy and Crain 1937:157,223; Vicedom and Tischner 1948:224-72). Father Ross' early description of the spatial distribution of houses depicts the modern Kuk situation:

"The houses are built almost anywhere, on Kunai flats, on hills, in swampy areas, along rivers. In fact, the large population makes it imperative for the people to use all the land for cultivation, and leaves them no choice for good building sites. Regardless of location, good garden ground is their living ground."
(Ross 1936:341-2).

The location of a new house site is guided not only by the location of gardens owned, but also by an attempt to find a spot reasonably close to tracks and roads, singing grounds, pig land, existing houses. The kinship and friendship ties with neighbors are also taken into account. All this leaves little choice for the new settler, especially given that the best located sites are already occupied.

The logical steps (not necessarily followed) towards the finding of a new house site are to know sub-clan, clan and tribal boundaries, then to find out where garden land is given. Personal relations with surrounding settlers further limit the final location of the new house site. Dry grounds are preferred but wet grounds are not avoided. If the house is to be built next to another, the tendency is to avoid having a doorway right opposite the doorway of another house; in settlements where a few houses are clustered, one can notice that very often houses are oriented in such a way that their entrances are not right in front of each other. This is the only evident orientation problem encountered by Hageners together with the apparent requirement that wherever possible, the house should overlook the landscape rather than turning its back to it.

Furthermore, each of the major types of houses requires a specific location (a pig house cannot be erected in the middle of a communal garden) and this is discussed below. Even though the final spatial distribution of houses seems to be anarchic, it is in fact subject to a substantial number of declared and undeclared rules.

3-2 HOUSE SITE

The boundaries of a house site are often difficult to delimit. Domestic activities, human behaviour, type of house and areas of material discard surrounding a household can vary considerably from

one place to another: number of clustered houses, type of houses, presence or absence of an external cooking place, or a kitchen garden, or an enclosure, and the immediate surrounding environment (bush, grassland, gardens), are all important parameters affecting the site limits.

As a general rule, it seems that only a narrow belt (up to 4m wide) outside the outer features is affected by human interference (i.e. rubbish dumping): along tracks leading to houses, just outside the house enclosure where present, or along the perimeter of the cleared open space if the house is built in the bush and has no enclosure. Concerning the range of material discarded, it is important to note that whenever it is possible, things are recycled: former house building materials for the erection of the new one or as firewood, old house platform for a new house or for a kitchen or mixed garden, food remains for pig consumption, ashes for surface compost, etc. Consequently the discarded items consist nowadays of almost exclusively durable material: tin can, glass, rubber, plastic, etc. Cooking stones appear to be the only durable and visible traditional artefact discarded; they are discussed in detail elsewhere. Inside the house itself, it is in the sitting room that food refuse such as sugar cane and sweet potato skin is mostly dumped, most of the time just in front of the person eating it.

From all these observations, it is assumed that very little would be found in prehistoric house sites and, theoretically, excavations of such prehistoric sites seem to require two distinct approaches according to the questions asked: the one dealing with features and spatial arrangement would require the excavation of the platform itself while the problem of material culture would require the excavation of the 4m belt located outside the outer features as described above.

3-3 SITE DEFINITIONS

Due to the complexity of the land management in the immediate vicinity of houses, and the difficulty in pinpointing the external limits of house activities, various definitions of "site limits" have to be found, aiming at a similar end result: the limits of an archaeological house site are the limits of all house activities. Derived from my ethnographic observations on material discard and human behaviour in the Kuk settlements, it seems that three definitions are necessary to cover all these activities, the external limits being delimited by the 4m wide strip of land located outside some specific settlement features.

3-3-1 DEFINITION 1: HOUSE IN PIG LAND

Some houses at Kuk which do not have any enclosure system are located in pig land (swampland and dryland). While many of them are pig houses or menstruation houses, some are normal family oval houses. They all have an open cleared space in front of the doorway. Illustrated models are examples A, B, C, and one house in E, Fig. 6. The common features are the house platform, the open space of varying size and a track; the material discard does not normally go beyond 4m outside these main features. Example A represents the simplest case found.

While this is theoretically simple, one can see that there are some archaeological traps. If we compare examples B and C (Fig. 6), there is a strong possibility of missing one building in example B, the toilet, while in example C all the buildings are integrated in the site definition: this is due to the fact that some buildings of the same site are located beyond the 4m strip and are not interconnected by a major feature or discard area. A similar case is example E, where one building (i.e. a menstruation house) could be left undetected while another building (i.e. a pig house) also located in pig land is

integrated with the main buildings site. Presumably in B proper excavation would note the gap in debris midway along the track leading to the toilet. This assumption derives from observation on human use of the site, on discard behaviour and on the nature of material debris.

3-3-2 DEFINITION 2: ENCLOSED HOUSE

Most of the homesteads in the Upper Wahgi are protected from pig disturbance by an enclosure consisting of a fence or a ditch and fence system, in which case the earth removed from ditching is piled up on the inside part of the homestead and the fence erected on top of it. In some cases, the fence is erected first then the soil from the ditch is piled on it. Very often this kind of settlement is located on the boundary between a sweet potato garden and pig land, thus allowing the full range of houses to be built on both sides of the enclosure. Such settlement is illustrated by examples D, E, F (Fig. 6).

The features which make this kind of settlement distinct from those covered by Definition 1 are the enclosure (ditch/fence), in most cases the presence of a kitchen garden within the enclosure, and the planting of trees and ornamental shrubs (for more privacy and as wind-break), also within the enclosure. The pattern of material discard is also different: while there is no change for the surrounding pig land (the "4m" rule is here also applicable), one is not permitted to use the adjacent sweet potato garden as a rubbish dump. Consequently, the limit of such a site is the inside part of the enclosure if there is a garden located just outside it.

Where is the rubbish discarded in such cases? Most of the organic material is given to pigs or used as a surface compost in the kitchen garden (especially at the foot of bananas and sugar canes). The durable artefacts are often piled up in an unused corner of the enclosure; in some cases, an old mumu or an abandoned toilet pit (in pig land or hidden

in the kitchen garden) is used as a dump, where eventually a banana tree will be planted.

The differences between Definitions 1 and 2 are the greater spatial restrictions in material discard (unless the homestead is entirely surrounded by pig land) and an increase in features from 1 to 2.

3-3-3 DEFINITION 3: HOUSE INSIDE A SWEET POTATO GARDEN

It happens that some well located gardens are extended in such a way that in the end they totally surround a homestead. Most of these gardens become communal (shared by numerous families) and are almost exclusively planted with sweet potato. Human behaviour in such homesteads is different from the pattern described in the previous two definitions, and consequently the archaeological limits of such sites should also be different.

The enclosure surrounding the homestead is now redundant, another one being erected on the distant limits of the communal garden. While some owners do keep the former enclosure (i.e. Example F, Fig. 6), in most cases the fencing material is recycled as firewood and the ditch is allowed to be refilled naturally (in rare cases it is deliberately filled back). Now, the visible limits of the homestead are the limits of the distinct grove made of kitchen garden products, shading trees and ornamental shrubs (i.e. Example G, Fig. 6).

This category of site differs little from the previous case (Definition 2), the major difference being the extreme restriction of house types and activities for the latter: anything involving pigs is excluded. The limits of material discard follow the same rules as for Definition 2, with clearer boundaries in this last case: nothing should be found outside the kitchen garden and refuse for pigs must be transported to another site.

It has been observed that in some cases, the neat grid pattern of sweet potato beds from the adjacent communal garden reaches the settlement area without a section of kitchen garden as an intermediate component (Example G, Fig. 6): in such cases, the site limits are even sharper, being the start of the sweet potato garden itself. The distinct features existing between kitchen (or mixed) and sweet potato gardens are discussed in Chapter 7.

The archaeological limits of such a site should be the external limits of the kitchen garden where present, and not beyond it. More precisely the site ends where a garden pattern clearly emerges and extends away from the house platform.

3-4 TRACKS

Tracks leading to and from homesteads are an interesting problem: do they belong to the house site? While they seem to be a very discrete feature, there is still quite an amount of material dumped along them. I have little quantitative data on this problem, but I nevertheless strongly believe that the deposition of this refuse follows the "4m strip" rule on both sides of tracks, and that the amount of deposition decreases rapidly away from the house site. Furthermore, one track could lead to the bush while another could lead to a nearby house not integrated in the site limits (Examples B, E, Fig. 6). So, to what extent is a track an "active" part of the domestic activities and should it be integrated in the house site limits? Is it as far as the track goes?

One way of solving this problem is to look at patterns related to the size of kitchen gardens, distance between houses and surrounding enclosure, and distance between isolated houses and their homestead. The rule is that kitchen gardens and enclosures will not exceed 15m beyond the house platform and its cleared space. Similarly, isolated

houses are in most cases less than 15m beyond the enclosure. This find leads to a new rule, the "15m" one, which translated in terms of house site limits gives the following prediction:

-i: Following Definition 1, there should be a dramatic drop in artefact remains 15m beyond the house platform and its cleared space along an 8m wide strip of land;

-ii: Following Definition 2, tracks leading towards isolated houses of a homestead should have a regular decrease in number of artefacts as in point i, but followed by an increase of artefacts when approaching the 4m limit associated with the isolated houses, all of this within 15m;

-iii: Following Definition 3, tracks should not have artefact remains along them. This is certainly the case in communal gardens and should be the case in pig land as well. There is no doubt that the odd artefact is being dropped at any location, but it is probably insignificant in terms of archaeological interpretation.

3-5 SITE LIMITS - GENERAL LAWS

No matter what house is erected and where, it seems that from ethnographic observations it is possible to define a house site and its limits. The general terms emerging from the above analysis are illustrated by Table 2a.

Two classes of observations are involved here, one dealing with features (house platform, cleared space, kitchen garden, sweet potato garden, enclosure) and one with material culture remains (track, artefacts, absence/presence of remains). The interaction between these two classes is made through two spatial laws described above, the "15m" and the "4m" limits. The combination of these three distinct parameters (feature, material culture, distance) allows us to determine what are the limits of a house site, but not what sort of house is

erected in it.

Table 2a works as follows. If one starts from the house platform and its associated cleared space and finds a kitchen garden along it (arrow leading to the left), this kitchen garden is included in the house site. It should theoretically end some 15 m away from the house platform (arrow leading downwards from "kitchen garden"). To test this limit, one has to excavate the area found just beyond these 15 m. If nothing is found or if the evidence of a sweet potato garden (grid pattern) is found (arrows downwards), then the site limits are the limit of the kitchen garden (thick arrow leading back to "Beyond 15m"). If an enclosure is found (arrow from "Beyond 15m" to "enclosure"), then one has to look at what is found beyond it. If there is evidence of a sweet potato garden (arrow from "enclosure" to "sweet potato"), then here again the house site limits are the enclosure itself (thick arrow leading back to "enclosure"). As can be seen in Table 2a, other possibilities are opened from this enclosure. These will eventually terminate to a 3-4m strip beyond that enclosure, or to an isolated house some 15m away from this enclosure. Overall, Table 2a is a closed system.

Various assumptions underline the model. One must be aware of the differences between a kitchen and a sweet potato garden. One must recognize an enclosure, one must interpret a strip 8m wide of artefacts as being a track (assuming that such material culture is in fact left behind).

Because of the lack of relevant archaeological data from the New Guinea Highlands, this ethnographic model remains essentially untested. While in the archaeological section of this chapter I am testing some hypotheses derived from the ethnographic model, I am not covering the whole range of tests. Some questions still remain unanswered. Before presenting the archaeological evidence however, the development of two homesteads are presented (one in dryland and one in wetland) which should stress the above restrictions in feature and material expansions. Following this, an analysis of the type of houses erected at Kuk is presented, aimed at telling us what sort of house is on the platform. It is only then that the archaeological tests are discussed.

3-6 PITA HOMESTEAD

The number and type of houses comprising a homestead vary greatly. It could be a single house located in pig land or a complex of houses erected within a communal garden. Some are found in dryland, some in swampland, some on hill slopes. The case study presented here is of an enclosed house located in a swamp, contrasting with the next case study which is of a rather big cluster of houses located in dryland.

Pita is an Enga from Laiagam who has permanently settled among the Kawelka. He came to Kuk around 1974 and has completely dropped Enga gardening and housing traditions (which are quite distinct from those of the Upper Wahgi). After living for a while with other men, he received land to garden in a swamp and built his homestead there

in late 1975. It is located at the boundary between Kukrumdi and Kum hamlets.

The buildings in his homestead consist of an oval house having a cleared space in front of the doorway (Fig. 7) and a toilet located in the southwest corner of the settlement, hidden by a screen of banana trees and sugar canes. The whole is enclosed by a deep ditch and fence. Within this enclosure, some plots have been made and are used as kitchen garden. North and east of the homestead is a communal garden, south of it is a big swamp while along the western enclosure Pita started in late 1978 to drain part of the swamp for his sweet potato garden. By February 1982, the whole swamp was entirely drained and used as a communal garden.

What can we say about the archaeological limits of Pita homestead? It seems that there are quite two distinct phases. The present phase, "before draining the swamp," falls into Definition 2 above. By combining the house platform/cleared space/kitchen garden/15m limits, we do find the total enclosure system. Then, by applying the 4m limit beyond that enclosure, one finds that the site limits effectively do not extend beyond it in north and east (because of the presence of sweet potato beds), while there is some refuse outside the enclosure but within 4m in the other directions. Theoretically one should be able to find evidence of a track quickly disappearing on the NW corner because of the presence of an 8m wide strip of artefact remains rapidly diminishing within 15m, thus pushing back the site limits at 4m beyond the enclosure (while the eastern track should be totally missing).

The second phase of Pita homestead, "after draining the swamp," will correspond to Definition 3. The site limits will be the enclosure itself all around the house. Material discard now will be restricted to the enclosed space and especially the embankment. It is obvious that the two phases are successive but can they be distinguished

archaeologically?

This question raises the problem of garden fallow cycles in the vicinity of homesteads. It seems that only two possibilities can happen. The first one is when the adjacent sweet potato garden is reverted into a short term fallow period. This event will not change at all Pita's behaviour, nothing will be discarded beyond the enclosure. The site limits remain where they are. On the one hand, if the garden reverts to a long fallow period and is consequently classified as pig land, then Pita will start discarding his refuse on a 3-4m belt beyond his enclosure, making a new site limit.

What are the theoretical archaeological consequences of such cycles, from pig land to garden land and from garden land to pig land? If a section of a house site located in a pig land is transformed into a sweet potato garden, one can expect some substantial disturbance in the spatial distribution of the refuse involved. No more artefacts will be added and the existing ones will be displaced vertically and horizontally due to the gardening method, the biggest ones even being removed and discarded on the periphery of the new garden. All this should result in an observable lack of pattern in the artefact distribution, or rather a re-patterning of existing remains.

On the other hand, if a sweet potato garden adjacent to a house site is reverted into pig land, the above forces may have been operating, but a new set of refuse, this one with a defined pattern, is added despite pigs disturbance. Theoretically, this should be quite distinct from the first cycle.

3-7 RU/REA HOMESTEAD

Rea is the Kawelka Mandembo who took over the Kukrumdi area for his clan around 1963. He was the first to settle there and his first house and gardens were located around an obelisk standing there

(Gorecki 1979a). When that new settlement was firmly established, his fellow Mandembo moved in and new settlements and communal gardens expanded. Then Rea moved to a new homestead adjacent to a communal garden and a pig land, near a swamp. This event occurred prior to 1969. Around 1971, Rea was jointed by Ru, of the same sub-clan, and the homestead was enlarged to its present size (Fig. 8). Rea has three wives and nine children, while Ru has three wives and six children.

During the first phase of this second settlement, when Rea was alone ("Rea Phase"), the homestead was entirely surrounded by a ditch and fence enclosure (eastern part of Fig. 8, bottom). At that time, only its eastern section was adjacent to a communal garden, the remaining being surrounded by pig land. It is during this phase that the communal garden gradually extends along and beyond the southern section of the enclosure.

The settlement was composed at any one time of one woman and pig house near the enclosure, one oval house, one menstruation house, one toilet, a cleared open space and a kitchen garden. The cooking place is notably missing because up to the present, Rea (and now Ru) is still using the one erected in his first homestead near the obelisk (site MN1). As illustrated in Fig. 8, more houses than those mentioned above have been erected during Rea Phase. This is because one house is replacing another of the same type through time, in a chronology detailed below. Still, by the end of this short occupation phase, the following houses had been erected: two woman and pig houses (I, K), three oval houses (J, J, J), two menstruation houses (M, M) and one toilet (L).

The second phase, "Ru/Rea", starts when Ru comes to live with Rea. The homestead was strongly modified, enlarged, with numerous rearrangements within it. Fig. 8 (top) shows the homestead as it

stands today. The western section of the enclosure from "Rea" phase has completely disappeared and only the woman and pig house "I" is still located inside the former boundary. All the present houses are in the added perimeter. The old settlement is almost entirely transformed into a coffee garden (Fig. 8, top). The new enclosure (ditch and fence) extends westward for another 30m where it reaches a swamp, and the enclosed land there is a sweet potato garden with no more building in it.

A short visit made to the settlement in February 1982, during which I could not map it, was staggering: only house D remained as it was back in 1978, all the other houses being pulled down and invisible on surface; three new houses were now erected near the emplacement of houses E, F and G, probably not even overlapping these old platforms.

If one looks at the succession of houses erected during the two phases, one realises the complexity of the potential problems left for archaeologists trying to reconstruct the culture or behaviour of such sites. The chronology of house construction is given in Table 3. It indicates approximate temporal points when houses have been abandoned, burned down, relocated, rebuilt on the same spot (the letters refer to houses illustrated in Fig. 8). Three oval houses have the same label "J" and there are two menstruation houses "M": this is because I could not find out (nor does Rea remember) their temporal order. Four major points emerge from this table, bearing in mind that each of these points have numerous counterparts elsewhere at Kuk:

-i: most of abandoned houses have been accidentally burned down (J, J, G, M, M, N, D, D, H) rather than going into a process of natural decay through time (J, K, L, P, B). From the latter, only two are not toilets (J, K);

-ii: a new house is built just next to an old one (J, J, M, M),

sometimes too close to it (see the two parallel houses "J") that it stresses a temporal difference;

-iii: sometimes houses are deliberately erected on the platform of an old one (three times for "D", twice for "G" and "H"), phenomenon which also occurred in 1977 for Pita's house in the previous case study;

-iv: not all domestic units have been shifted at once in the new enclosure, "I" remains in the old perimeter and "N" has been built there during "Ru/Rea" phase.

While Pita kept similar dimensions and orientation for the building of his new house just on top of the old one, it was noticed that Ru, for his latest house "D" in 1978, made it unconsciously with its length shortened by 15cm at each end while the width and orientation remained similar with the previous house. In Ru's case, main central posts and internal mumus did not change their position or shape. Two hearths on the contrary were totally rebuilt (fired clay and ashes) at a new location, some 80cm from the previous ones (Plate 10). There is no information about the first house "D" located there (apart from the fact that Ru tried again to follow the same size, shape and orientation) but from the above observations, we can assume that it should be possible archaeologically to find two of the three houses "D". In other cases seen at Kuk, one type of house can be erected on the platform of a previous totally different type of house.

It is interesting to note that because the eastern and southern parts of the enclosure are now adjacent to a well established communal garden, and because it is located in dryland, the deep ditch is not maintained and is being naturally refilled (mostly from soil of the embankment which is collapsing back into the ditch). As a result of this, the fence is decaying and not repaired. All the shrubs and trees growing in Ru/Rea settlement are replacing it as an effective screen.

From this settlement, only the menstruation house "N" has been archaeologically excavated (site MON).

The site limits of this settlement follow the rules given by the three definitions and are in many cases similar to those detailed for Pita's homestead. The major points to stress here are as follows:

-i: although the western boundary of "Rea" phase (ditch and fence) disappears during "Ru/Rea" phase, it nevertheless is a remarkable archaeological feature left as a warning that this site is a complex one. Although unexcavated, it is said that the ditch has been used for a while as a dump before being deliberately filled;

-ii: while the present enclosed settlement extends westward, the site limits are located at the level of house "D" and its surrounding kitchen garden. Applying definition 3, the remaining enclosed area west of that house should belong to a garden site (sweet potato) and not to a house site.

3-8 SITE ABANDONMENT

The most common reason for building a new house is when one of them has been accidentally burned down (e.g. Plate 9). Nowadays it is often due to drunkenness, otherwise women (during cooking preparations) and children (playing with hot ashes from the hearth) are blamed for such accidents. In the past, numerous houses must have been burned down during warfare, being the most obvious target; during the 1978 short fight at Kuk with Tari labourers, nine Kawelka houses were destroyed that way.

Various alternatives are used when a house or settlement is abandoned. They are all directly related to the owner's new plans, each one different from another.

It has been observed that people save what can be saved from the remains, generally only charred timber and not fully burned posts which are recycled as firewood. If the site is no longer suitable, the

vegetation is allowed to grow back at least for a short "fallow" period, then it is cut down, dried and burned to make a better soil for the planting of a kitchen or mixed crop garden. In only one observed case (for a woman and pig house isolated in the bush on Ep ridge) was the structure deliberately burned down after abandonment.

Sometimes the owner wants to rebuild a house on the same place and so the platform is cleared down to the bare clay. By doing this, the major existing house structures are exposed and tend to be reused for the new house (main post holes, mumu, hearth). While there is plundering of a burned house, behaviour seems to be very different for an abandoned house: nothing is removed by strangers, and it is closely watched by the owner. There is always a "Tambu" sign (hanging fern) in it. This is probably because of the huge reserve of firewood it represents.

Timber from an abandoned house is taken only in the quantity needed for firewood. Being a slow removal process, some houses, already collapsed and overgrown by grass, are still being recycled many months after the beginning of the dismantling. When it is finally totally abandoned, it could either be left, or a garden is made (following the process described for burned houses), or the site can be cleared and another house is erected on the same spot (or next to it, or near it, etc.).

Meanwhile, there must be a house where the occupants are relocated. If the new house is built nearby, and the old one is still being recycled, it could be as close as one meter of it: these are the only cases of such a close proximity between two houses (one being abandoned). Otherwise, two occupied houses are built in such a way that there is enough space between them to have at least a dividing track and some privacy. It is much easier to abandon or rebuild a single house than a complex settlement.

The shifting of a settlement having such things as a ditch and fence enclosure, a kitchen garden, a cooking place, a toilet and a few houses (such as the above Ru/Rea settlement) is certainly decided after long thought and preparation. It could be caused by a splitting of an association (father and married son, or one can imagine Rea wanting Ru to go and settle elsewhere). In most cases observed at Kuk, the decision has been caused by a need to be relocated near new major attraction points (economic and social) such as gardens, pig land, singing grounds and roads.

But whatever is the reason for the abandonment of a settlement, people try to rebuild in one of two locations: first, along a boundary between communal garden and pig land, or, second, isolated in pig land (bush) where a whole new economic activity starts. The abandoned settlement is left to decay, crops planted in it are harvested as long as possible and timber is collected regularly from house and fence remains and from planted trees (mostly casuarina). It seems that only the owner has the right to utilize whatever is useful from his former settlement.

The final aspect of such a homestead is of three kinds. It is recycled into a permanent coffee garden where, even in pig land location, an enclosure is not necessary and little maintenance is required. Secondly, the vegetation is allowed to grow back to a secondary forest stage. The third aspect is when the abandoned homestead is in the middle of what becomes a communal garden. It then can be levelled, all features removed or filled, and the site is planted with sweet potato following the surrounding pattern: nothing apparent would suggest a recent settlement activity.

The lifespan of a settlement is difficult to estimate. It can vary from a few months in a temporary house to more than 20 years (although in successive houses). The decision is made by the owner alone and no

pattern is apparent. This is clearly illustrated by Pita's example. It seems that Pita's homestead will remain as it is for a long time. Its geographical, economic and social location being so good that Pita does not expect ever to shift elsewhere. His house has already been burned down in 1977 (and its remains recorded in Figs. 17 to 19) and a new one has been erected on exactly the same spot. At the end of 1978, it seems that due to ongoing draining activity of the nearby swamp, only the western ditch still has a necessary drainage function, the others surrounding the settlement being partially filled, dry, abandoned.

When asked about what he expects to do when his toilet will be filled up, Pita says that first it will take a long time for this and, second, if this happens, a new one will be built just next to it or in the north-eastern bend of the site. If his house rots or again is burned down, it will always be built on the same spot, but not necessarily with the same size and shape. His only problem is when his family grows up and he has no plan yet. It is possible that by that time he will have big trees in his settlement so instead of having a kitchen garden, he will build a second house. As one can see in this case, anything might happen to Pita settlement. My personal view is that because the settlement is rather small, it will be destroyed and the surrounding communal garden will eventually cover the site. My 1982 visit to Kuk did confirm the prediction: Pita has moved elsewhere and there is no surface evidence of housing activity at the site. Instead of being integrated to the adjacent communal garden, the site is now a mature mixed garden in which coffee and casuarina seedlings have just been planted.

3-9 TYPE OF HOUSES

Various types of house are found at Kuk and are discussed in detail

below. The literature on Hagen houses is rather poor and it is difficult to evaluate the degree of change or traditionality in house types and architecture between 1933 (and consequently prehistoric) and 1977 (my Kuk data). While there are some poor and superficial descriptions of men's round houses and women and pigs houses, only Vicedom gives us an invaluable detailed description of all aspects related to Hagen houses (Vicedom and Tischner 1948 (197?):219-71). Because Vicedom was in Mt. Hagen between 1935 and 1939, one can assume that his ethnographic observations cover also (but for an unknown length of time) the late prehistoric characteristics of Hagen houses.

Based essentially on Vicedom's data, I can say that the 1977 Hagen houses have changed substantially. These changes are not always easily detectable, the external "picture" being still very traditional. It is when one deals with minute details that the magnitude of the change is apparent. This thesis is not primarily concerned with this problem and only an outline of the trends and changes are presented. They concern mostly the architecture of houses, while social and economic behaviour seems to be less affected. While Vicedom deals a lot with the religious aspect attached to the housing question (he was a missionary), I have little such data and I have not assisted in major ceremonies. Consequently my knowledge of ceremonial buildings and behaviour is limited and the literature is of little help in this respect.

According to Vicedom, there existed 26 distinct buildings of various degree of importance, some having an extremely short life (Table 4). By 1977, only 15 of the former 26 still existed, with four new types added. Following this trend and observing the decrease or increase in importance of each of these 19 types, it is possible that in the near future only 11 types of Hagen house will remain, 7 of them traditional. This general trend is caused by three major factors: First, mission and government influences in health and religious acti-

vities; second, the emancipation of women and their increasing involvement in daily family and men's life; and third, the tremendous impact of kiap style houses (European style).

While some buildings which are nowadays rarer than in 1933 (types 2, 3, 5, 7, 9, 13, 15, 18, Table 4) are discussed in detail below, one has to briefly comment on those which have disappeared since 1933 (types 1, 6, 8, 11, 14, 21, 22, 23) because I do not have records on them. It is obvious that these buildings are in the archaeological record. From Vicedom's comments, one can assume that three now extinct types of house are in fact totally similar to some existing today: 1 is 4, 6 is 2, and 8 is 9. The remaining missing types have such a peculiar architecture (and probably archaeological features) that if found during an excavation, they might be recognisable from Vicedom's descriptions and the analysis could be interpreted.

Vicedom insists on the presence of pigs in all woman (and even family) houses, confirmed by the plans presented (Vicedom and Tischner 1948:165). These houses have only one doorway, something I have never observed in the Wahgi. Nowadays pigs and women have their own doors. This difference is of importance when dealing with archaeological features. It seems to me that Vicedom might overemphasize this woman and pig association. During interviews conducted with Mick and Danny Leahy, and the consultation of their photographs from the contact period, I noted that already at that time there were two doorways in women and pigs houses. The fact remains that there are many more pig sties nowadays than there were before. Finally, Vicedom notes that some house types might be a recent introduction in the Wahgi from Chimbu (1977:259).

In more remote parts of the Melpa speaking region of the Highlands, such as the north-eastern end of the Baiyer valley or in the Jimi valley, where there is little road connection and where modern changes are occurring at a slower pace, a much greater traditionality in buildings was

noticed. Nevertheless, the new trends described above also apply to those areas.

The above comments stress the non-conservatism of Wahgi societies in building behaviour. It is not impossible that there also were changes rather than a long continuity in prehistoric times. While the Kuk houses presented here do not represent the same range of buildings as the one from 1933, one can nevertheless deal with these data with some confidence concerning spatial arrangements, dimensions and behaviour. Evidence from Wanlek (Bulmer 1977) and Eastern Highlands (Watson and Cole 1977) supports this continuity. I do not think that the problems with which I am concerned here are substantially different than those from 1933. The conclusions reached here should still be applicable for at least the recent prehistory of the Wahgi. Nowadays, houses are still built in exactly the same way as the one described by Vicedom and Tischner (in German in 1948, vol.I, pp160-7 and in English in 1977, vol.I, 243-50). These minute details are not repeated here.

3-9-1 MEN'S ROUND HOUSES

This is certainly the most impressive type of Hagen houses which has attracted the attention of many writers. This is probably so as much because of its remarkable surroundings as the building itself, and its political and ceremonial importance. No architectural differences are noted when compared with round houses observed by Vicedom.

A men's round house is always built at a prominent place overlooking part of the landscape, in a location where there is enough space in front of the doorway to add its indispensable "discussion place." This place is analysed in detail elsewhere. Beside it, there is very often (but not always) a high earth embankment and a small ditch totally surrounding the house platform except for a width of

some 1.5 to 2m in front of the doorway. This embankment is always very close to the house and cannot be used as a path. It does not extend around the outdoor "discussion place." If the owner wants one, it is built as a distinct feature. Cordyline and other decorative shrubs are planted on top of the house embankment and trees (mostly casuarina and bamboo) are planted along the perimeter which includes the house embankment and the discussion place.

Six of the twelve round houses located at Kuk are illustrated on Fig.9. The internal structures separating the sleeping quarters from the sitting one are generally made of a very light structure, such as woven pitpit, with very few small supporting posts driven into the ground. When timber is easily available (i.e. Sepik-Wahgi Divide), it is noticeable that these internal structures are made of strong planks, sometimes without any woven screen separating the quarters. As Fig.9 shows it, there are different internal spatial arrangements, apparently left to the owner's wishes. The important but logical rule is that the sitting room is in front of the sleeping room. Generally speaking, there is no bedding structure inside each sleeping room except occasionally for a log used as a pillow. A great number of men can be accommodated that way, one man lying alongside another. In some cases, such as examples "A" and "E", there is one room smaller than the others. It is the owner's private room, and in such cases, the floor can be slightly raised (10-20cm above the ground). It is said that this "bed on stilt" is a post-European introduction.

Three features are always found in the sitting room: the central main post, the hearth, and the mumu. The central post is sometimes used as a support for the internal partition walls (examples "B" and "C") and it is never located in the sleeping quarters. The hearth is always located between the doorway and the central post. It is assumed that this correlation should be a strong guideline in the finding of

archaeological structures and features. Two kinds of hearth can be noticed, one circular ("B" and "F") and the other rectangular. This difference is discussed elsewhere.

All the mumus found in this type of house are large, of the DCDC type (Chapter 5). They are located in various places in the sitting room preferably along a wall. This is a further indication stressing the boundary between sleeping and sitting rooms (i.e. "B" and "D"). Contrary to the Kuk examples, Vicedom notes that mumus are never located in Men's houses (1948(1977):255). On the contrary, Gitlow has seen and mapped a men's round house with a mumu in it; due to its location, it could be mistaken for the hearth (Gitlow, 1966:27). It is possible that it is a cultural change caused by the relaxation of women's taboos. Today the men still say that there are no mumus in Men's houses and that they are forbidden to women, while observations show that women go into men's houses and there are well hidden mumus in them.

A men's round house has various sizes, as indicated by Table 5, while the architecture is rather similar in all cases. The diameter ranges from 5.4m to 10m. The biggest house was abandoned by 1977 and is located at the head of a singsing ground (recycled as a coffee garden) in Mapa. It is unclear if it is an unusually large one (the next biggest having a diameter of 8.1m) or the gap existing between these two houses is a result of the low number of round houses recorded. Nevertheless, one can already see a clear formal distinction (and a big functional one) with the menstruation houses, which is the only other type of round house found in the Upper Wahgi.

The perimeter of the men's round house always has a double wall, inner and outer, made of strong planks driven in the ground, the space between them being filled with grass and tied with wide bark strips and vine, or filled with woven pitpit and tied with vine. Sometimes a platform is suspended from the roof just above the hearth

to store and dry firewood and to reduce fire danger. The few artifacts found in these houses are exclusively related to male activities, but it was noticed that at least in the sitting room, it was clean and free of portable artifacts. The users explained that this lack of implements and utensils avoids any temptation of theft during the frequent visits of numerous males (and nowadays females). The house floor is not swept, while the outdoor discussion place is regularly swept to give a clean appearance. Material swept aside is simply left on the margin of the place, among casuarina trees and bamboos.

3-9-2 OVAL HOUSES

Oval houses, also known as family houses, are the most common type of houses in the Upper Wahgi. Vicedom's records (197? vol.I:247-9, 252-3, 255, 258-9 and illustrated in Plate 76, 1948, vol.I:165) of this type show that the differences with present day houses are marginal. Beside the possible room-sharing of women and pigs in 1933 (and not in 1977), men also had oval houses with an apparent absence of mumu. Today, there is no such men house, all oval houses are for women or are for a nuclear family (husband, wife and children).

I have observed, measured and mapped 109 oval houses at Kuk, some under construction, others occupied or abandoned or burned down or recycled as pig sties. The measurements of most of them are presented in Table 6, and the plan of some of them are in Fig.10 (small to medium size) and Fig.11 (large oval houses). If one analyses the relationship between length and width of these houses, some interesting points emerge.

There are strong upper and lower limits in width, all houses being between 2 and 5m wide. The lower limit of 2m could be explained as being some sort of spatial minimum for people to live in. There is no obvious reason for the upper limit of about 5m, especially that trees larger than 5m are available at Kuk. It is possible that the major

reason for such a limit is to keep at optimum the amount of warmth from the hearth; one must remember that Kuk lies between 1570m and 1960m a.s.l. and that nights are rather cool. There could also be a cultural factor attached to this upper limit: because it is a tradition to have that width for an unknown original reason, people keep doing this. This point is discussed in the Kiap House section.

A second fact emerging from a length/width analysis is the lesser constraint in length limits. The length range is 2.5m-15m, with a greater concentration around 6-12m. This is discussed below. The last point to mention here is that due to the great number of houses recorded, this sample is representative for the Upper Wahgi, and that oval houses falling outside this range could be considered as odd. The mean oval house, if such a house existed, would be 8.9m long and 4.7m wide.

There are strong similarities in the spatial internal arrangement of all oval houses. They always have at least two main posts along the long axis of the house, supporting the central beam, a central sitting room containing the doorway, a hearth, a mumu and openings to the sleeping rooms which are always located at each extremity of the house. Some oval houses are too small to have bedrooms (examples I, Q, R, T, U, Fig.10) and in one case (U, Fig.10), there is no mumu (this is an almost abandoned house, used from time to time to store beer bottles and it is probable that there is a mumu in it which has been filled up). Example F, Fig.11 further stresses the occurrence of internal arrangements falling outside the above norm: the location of one sitting room and its associated sleeping room is reversed.

One important point which emerges from the smaller oval houses is that sometimes there is little difference in length between two houses and a big difference in the internal arrangement: while Example U and I or Q (Fig.10) have almost the same size, U has no bedroom but I and Q

have one bedroom each. It seems that all the internal arrangement is organised around a module 2m to 4m wide, 4m being the upper limit. The same happens between I or Q and H and S: the small difference in length between the two groups of house is translated into an additional bedroom for the second group. As the length still increases, there is a doubling of the number of bedrooms: H and S are very similar in length to N and L but there are two double bedrooms in the latter case. Some intermediate cases also exist and are represented by C and G. This correlation between increase in house length and doubling of bedrooms suggests that there is a "standard" size for bedrooms. This is controlled by the maximum warmth to be kept during the night. Bigger bedrooms (i.e. Examples C and G) and an indication of more than one person sleeping in them, or some personal wish (i.e. asymmetry in B, Fig.10).

There is also a limit in the distance between two main central posts: the upper limit (Example P) does not exceed 4.5m. I could not get an explanation for this beside the fact that beyond that limit, the central beam (joining two posts) is too heavy for one man to carry. Thus, if the length of the house is still extended, one notices the appearance of a supplementary central post (and a supplementary central beam). The critical length here seems to be at around 11m-12m. In example P (Fig.10) the house is 12m long, the distance between central posts is 4.5m and the length of the double bedrooms is about 3.5m (upper limit). On the contrary, houses A, B, E, F (Fig.11) are slightly shorter than P (Fig.10), have the same number of bedrooms but have an additional central post. This important new structure results in a supplementary sitting room. This new architecture reflects in reality two distinct and independent limits, the central post marking the division between them. Spatially, we in fact are back to the case of small oval houses discussed above, but in this case there are two small oval houses under the same roof.

Continuing this process of extending the length, one can realise

that it is a repetition of processes described above. An additional central post appears from house C to D (Fig. 11). Theoretically, this process can go on indefinitely (and indeed it does in the case of guest houses erected temporarily in singsing grounds which could be several hundred metres long). What limits the length of oval houses is the number of people living permanently in them. In examples C and D (Fig. 11), two large families share the house and it is probable that a split will occur and one family will move away to establish a new settlement, thus breaking down the house extension process. While it is theoretically possible to have three or more units in the same house, this has never been observed in the Upper Wahgi.

It is when dealing with building material and internal arrangement that the problem of upper limit in width is understood. Oval houses are built very much in the same way as men's round houses. Generally speaking, the house is built on the basal clay, its platform being often surrounded by a ditch (Plate 8). The spoil of the ditch is piled up along the perimeter of the house, consisting of a double row of strong planks (often made of casuarina). This double row is filled with plaited pitpit (a man's job) and tied with bush vines. In some cases such as in bush areas or in more traditional dwellings, the double row of planks is stuffed with grass and covered by strips of bark tied with vines. The central posts and beams are made of logs. The material used to make the roof, thus connecting the central structure with the double row of planks, is made of light timber such as branches from felled trees and shrubs. This mode of construction is extremely economical in terms of amount of trees felled and bush destruction; there is a maximum use of the cut down vegetation. This practice is of importance in areas such as Kuk where there is little forest left and most of the timber supply (construction and firewood) comes from planted groves, particularly of casuarina. This self-imposed restriction on use

of wood is probably a major factor for the width of houses being limited to around 5m: little timber is used for the construction and little firewood is needed to keep the house warm.

Not only the width of the house is controlled by this, it also affects the area of each room (thus the length as well). The internal structure modifications operate around a modular principle. This principle is demonstrated above through the operations involving house length extensions. It seems that two principles are involved, one for the sitting rooms and one for the sleeping rooms.

The area of a sitting room varies from a module about 2.5m by 2.5m to a module about 4.5m by 4m, with a greater tendency towards the upper limit. Bedrooms follow a different rule: because of their specific and limited function, and because of lack of fireplace in them, their modular principle is smaller than the one for sitting rooms. The size of single bedrooms (i.e. Example H, Fig.10) varies even less than the sitting rooms and is around 3.5m by 2m. The same can be said for double bedrooms (i.e. Example D, Fig.10), more common and smaller than single bedrooms: it should be about half the area, or around 2m by 2m maximum. One must remember that because all bedrooms are located at extremities of houses, they have one or two sides straight while the remaining are rounded off.

From all the above, what are the ethnographic features and activity areas which should go into the archaeological record, or where one should look to understand and identify the site type we are dealing with? It seems that, excluding at this stage all material and artifactual information, there are seven features which would indicate the presence of an oval house:

- 1) House platform sometimes surrounded by a ditch (Plate 9);
- 2) Double row of posts or post holes indicating the walls;
- 3) Two to four larger and deeper posts or post holes along the longi-

tudinal axis;

- 4) Presence of a hearth or fireplace between and at equal distance of two central posts or post holes;
- 5) Presence of a mumu within two meters of this hearth;
- 6) Presence of a doorway at right angles to the longitudinal axis from the above hearth;
- 7) Presence or absence of space between a central post adjacent to the hearth and extremity of long axis (which could indicate the presence of pig stalls).

It is evident from the seven above points that everything revolves around the sitting room; the more of these features that are found, the more accurate will be the classification. On the contrary, if fewer are found, the more difficult it will be to interpret the site.

Internal room divisions such as the walls separating the sitting room from bedrooms or separating double bedrooms have not been discussed. It was felt that because of their light construction they are more likely to be missed archaeologically. If found, it is a bonus added to the site interpretation. Material remains inside the house seem also to follow a pattern. Very little should be found in sleeping rooms (literally: nothing) while the sitting room should be littered with consumption items, organic remains, charred tuber skins, cooking stones, wood, ash, charcoal, etc. Furthermore, it has been observed that if a house is burned down, there should be an additional dichotomy between sleeping and sitting quarters: a lot more charcoal and ash should be found in the centre of the sitting room coming from the concentration of firewood and rack holding it which is suspended above the fireplace. All the above comments, observations and hypotheses are tested and further discussed in the relevant archaeological testing section.

3-9-3 WOMAN AND PIG HOUSES

It seems that Vicedom does not make any differentiation between this type of house and the previous one (1948(1977), vol.I:258-92 and illustrated in 1948, vol.I:165, plate 76). Back in 1933, all woman houses had pigs in them and, as already stated, these houses had only one door used by both women and pigs. In 1977, the situation is quite different. Where women and pigs live under the same roof, they have well defined and separated quarters.

All the fifty woman and pig houses found at Kuk in 1977 were recorded. Their measurements are presented in Table 7 and the plan of seven of them is illustrated in Fig.12. It is important to note that anything involving sleeping and sitting rooms in this type of house is exactly the same as for the previous case (oval houses) and is not repeated here. The differences between these two house types concern only the location and specific buildings and features exclusively related to pigs. This is discussed below. Because all the woman and pig houses from Kuk have been recorded, it is probable that this sample is representative for the Upper Wahgi as a whole.

Woman and pig houses are also oval in shape. Their length varies from 6.2m to 23.5m. This is somewhat greater than for oval houses (2.5m to 15m). The width varies from 2.1m to 5.8m, slightly greater than for oval houses (2m to 5m). A mean woman and pig house (hypothetical case) should be 12.1m long and 4.1m wide (against 8.9m by 4.7m for oval houses). What emerges from the above measurements is that woman and pig houses have a greater span of lengths and widths, with a tendency towards being longer than oval houses. Nevertheless, there is a substantial overlap.

This greater length is due to the amount of space allocated to the pigs quarter. This sty does not operate around a modular principle. Fig.12 shows the variability, from one house to another, in the

sty's size: from a length of 3m (Example G) to a length of 13.5m (Example D). The internal arrangement varies as well: no individual pig stalls (Examples C, F, G), stalls all around the internal part of the room's perimeter (Examples A, D, E), or an intermediate situation where it is partially subdivided into stalls (Example B). The width of stalls varies from 50cm to 1m.

The variability in architecture is related to the individual's will, wealth and expectations. House G belongs to an old bachelor with little ambition, and had only four pigs in 1977 and two pigs in 1978. He does not expect to have many more. Very often his pigs are left in the bush for the night rather than inside the sty. On the other hand, house D belongs to a big man and two of his four wives live in it, one at each extremity. I have never seen at any one time all the 45 stalls occupied, and probably it will never be. Because of his constant pig trading activities, he needs such a big sty. It is important to note that in all cases where more than one woman shares the responsibility of the sty (Example A to E), the doorway of the sty is the demarcation line of responsibility. In house D, the woman living in the left unit has the pigs located on the left side of the sty doorway, while the woman living in the right unit has the pigs located on the right of the sty. An asymmetry in such subdivisions stresses the status difference between two women.

I noted that there was never the same number of pigs in such subdivisions: it could happen that there are 18 pigs on the left side of house D but only three on the right side, a situation which can be reversed (and probably will) at another time. I did not pursue the investigation further. What is important to remember in the problem which does concern us, is that each stall has probably been occupied for a varying length of time during the life cycle of the house, but the number of stalls or the surface area of the sty does not give any

information on human to pig ratio. Internal architectural variation seems to be entirely left to the person looking after the pigs. For instance, nobody in house C wants stalls while each of the two women sharing house B has a different opinion on how her sty should be. Whatever the situation is, pigs are tied by one leg to the perimeter wall by the mean of a rope made of bush material.

Pig sties have no specific location in relation to the human quarters, they can be located at one end of the house or in a central position. The latter case seems to be preferred, leaving more privacy for women sharing the house. What has not been observed at Kuk is a woman and pig house with two separated sties. Sometimes there is an opening from the sleeping room (Example A) or the sitting room (Examples B, D, F, G) to the sty. This is also an option left to the person looking after the pigs.

If the woman and pig house is erected within an enclosure there is always a fenced laneway leading to the pig land. This gateway is generally narrow, about 1.5m wide, and its length varies from 1m to 15m. The only exception noted was the case of house G where the old man led his pigs with a leash between the house and the nearby swamp. But this seems to be an exceptional case. The laneway fencing is rather strong, and is carefully maintained under repair. Finally, it is worth noting that the distance separating two main central posts follows the same rule as the one described for oval houses, and that no fireplaces nor mumus are found in the sties. It seems that the material remains are extremely limited (if not nil, other than an old rope) in the sties and this should contrast with the adjacent human quarters. Furthermore, pig wallows do occur in the sties and this should be an additional information if found archaeologically.

To conclude this section, there are six points which seem to have priority in the site formation processes and interpretation of

woman and pig houses:

-i: all information from the above oval houses are also applicable here;

-ii: absence of hearth between two or more central posts;

-iii: presence of individual stalls;

-iv: pig wallows;

-v: fenced gateway;

-vi: lack of material remains.

If one or more of these points are found during archaeological investigation, one should start testing the other potential features and the hypothesis here is that there should be no difficulty in recognising and differentiating oval houses from woman and pig houses.

3-9-4 VERANDA HOUSES

This type of house was a problem in the field in terms of its geographical location, measurements, and archaeological testing. The ten veranda houses found at Kuk in 1977 are all located, for an unknown reason, within the boundaries of the Membo clan. Because I suspected it to be a traditional type, this case could illustrate the existence of some greater conservatism among the Membo. In a wider context, it shows that some traditions are harder to change within some groups (and not only individuals of a same culture. This problem was put to Prof. A. Strathern who states:

"The house type ... is traditional. It is a variant on the standard men's house type, but is used nowadays also for a family residence ... It is not specially a 'Membo' type as such, although it may be favoured by Membo men and their families ... One name for it is Klapa Manga." (pers.Comm. 1978).

It is probable that there is a different degree of conservatism within clans or tribes of a cultural unit, one aspect of it being illustrated by the above case. This is further emphasised by the much

greater number of such houses observed in the Melpa part of the Jimi valley, an area known to be more isolated and where modern changes occur at a slower pace. While Strathern mentions the change from men to family house, Vicedom observed two distinct types: one veranda house for women, distinct from a veranda house for men (1948, vol.I, p.165, Table 76; and in 197?, vol.I, pp256,8).

From all the above, one can assume that in late prehistoric times, there were two kinds of veranda house (as described by Vicedom), slowly merging to a single family type (as observed by Strathern in the 1960's to finally be in the process of total abandonment in favour of other types during the 1970's (see Table 4). So our 10 veranda houses from Kuk could well be the remains of a previously widely made house type. Consequently, our sample is not representative and this analysis should be taken with care and only as a broad guideline.

One could compare the shape of veranda houses to oval houses where one end instead of being semicircular is straight, where a veranda protrudes from this straight side; this is also where the doorway is located. The measurements of the ten houses are presented in Table 8, and the floor plan of eight of them are illustrated in Fig.13. The veranda by itself is a problem: should it be included as part of the house? Because it has a function (temporary store, shelter, tool shed) it is here integrated into the house (especially so because it is part of the house platform). On the other hand, because of the expected poor material and feature remains (it ends abruptly and there is a wall with the doorway separating it from the internal parts of the house), a double analysis is necessary: one includes it and one excludes it. The length of these houses, including the veranda, varies from 6.1m to 11.3m; excluding the veranda, it is reduced from 5m to 9.4m. The width varies from 3.3m to 5.1m. A hypothetical mean veranda house would be 8.1m long (including a veranda 1.6m deep) and

4.1 m wide. Although our sample is small, it seems that this house type is rather similar in length and width range and average to the oval house type.

The internal arrangement also has some similarities. The doorway (located in a completely different place) leads to the sitting room where there is a hearth in a central position and a mumu along a wall. The sitting room seems also to follow a modular principle, similar to the one described for oval houses. A similarity is also found in the distance between two central posts. The major differences concern the sleeping quarters. Here they are located at the back of the house (and not at the ends as for oval houses) and almost every house has a different bedroom layout. It is as if there is a greater personalisation of building, less standardisation. Nevertheless, the surface area of each bedroom seems also to follow the rules described for oval houses.

Unfortunately all the above must remain temporarily at a speculative level. It will not be further discussed in the archaeological section of this chapter, simply because not a single veranda house site has been excavated. No such site could be investigated, the few known to me being recycled by mature gardens and permission to excavate was refused.

3-9-5 KIAP HOUSES

This is an European introduced house type. These houses were originally built for government officers along their petrol tracks. They followed a design required by the Administration, with a higher ceiling allowing standing up inside the house, very often built on stilts (40 cm to 1 m above the ground). The internal arrangement was as simple as possible (being occupied only occasionally) with one large living room lacking mumu and fireplace, and sleeping rooms on the sides.

It certainly is a style not suited to Highlands climatic conditions, the architecture being such that it is hard to keep warm (particularly where there is no hearth) and by being on stilts it further allows cold winds to penetrate through the walls and floor of plaited pitpit. It is a style which has attracted the Melpa people, and in a general survey of the Upper Wahgi made in early 1977, Logan notes:

"Domestic housing generally follows traditional lines with the exception of that used by the modern leaders. These people are tending to have houses built that approximate 'house Kiap' designs. That is, a dining/living room, two to three sleeping rooms and a covered porch area with a pit latrine somewhere adjacent to the house. In these cases the houses are taller and the family lives under the one roof."
(Logan, 1977)

The general situation above is particularly true at Kuk. I recorded 36 Kiap houses there, owned not only by modern leaders, but also by ambitious young men. It is seen as prestigious to have such a house. At Kuk, the original intention is to build a Kiap house to serve as a men's house but very often the whole family soon moves in. While some men would like to see them exclusively reserved for male activity, I always have noted women, if not sleeping in them, at least carrying on daily activities in them. As pointed out by Logan, while they should be men's houses, they are in fact family houses.

The owner of such a house always complains that the house is very cold. Nevertheless, features such as being on stilts, lack of fireplace and mumu, and even the appearance of windows are practised.

Being taller than any other traditional house is another element making it colder than usual. It is probable that this house type will increase in the near future, but some essential modifications should occur: not anymore on stilts thus allowing the presence of a hearth and a mumu, all vital components for the warming up of the house.

The dimensions of the 36 kiap houses found at Kuk are presented in Table 8. Their length varies from 3.3m to 10.5m while the width has a range of 2.7m to 6m. A theoretical "classic" kiap house should be 6.7m long and 4.3m wide. This seems to be close to the oval type case. It could show that there is still some traditionality in this change, and could explain the acceptance by Melpa people of the kiap house type. The most interesting parameter here is the width, which does not really go beyond those of traditional houses.

Although the plan of kiap houses is of no use for the understanding of the Highlands prehistory, at least the material remains should be investigated for the sake of the building up of data on site formation processes and behaviour. It is also an illustration of change and continuity in a traditional society. Furthermore, it would be ridiculous to exclude the above comments on kiap houses simply on the grounds that none of them will ever be found from prehistoric times. One kiap house site has been archaeologically excavated and is discussed in the appropriate section.

3-9-6 MENSTRUATION HOUSES

Around settlements one often finds some partially concealed small houses. They are always located near a larger house occupied by at least one woman. Sometimes they are found just outside an enclosed settlement, but within the limits stressed in section 3-5. These small huts are seclusion houses where women retire during their menstruation or to give birth. The situation was the same in recent

prehistoric times (Vicedom and Tischner 1977:268-9). I have recorded 23 such houses at Kuk, representing only about 30% of the total menstruation houses. Their dimensions are given in Tables 5 and 9, and the plan of six of them is presented in Figure 14. One must point out that these houses generally have a short life cycle, being built with little material and regularly burned down (I could not find out if this was deliberate). Often an old oval house is recycled into a menstruation house (if not already changed into a pig sty), or one room in a woman and pig house is reserved for these functions. In at least one case, the pig sty of a woman and pig house was recycled as a menstruation quarter. It is said that there is only one room in them, and for some taboo reasons, meals should not be prepared in them but brought in by another woman. Thus there are no mumu inside these houses. All the doorways are obstructed by hanging ferns, a tabu sign and warning to keep off.

All the 23 menstruation houses visited have a very simple internal arrangement: a single central post (except for case E, Fig. 14, which has two posts), one hearth near the doorway and a mat used as bedding at the back of the house, generally along the central post. It seems that there is little material remains left behind. All these houses look roundish, but only a few are circular. This illusion is due to the absence of a central beam. Nevertheless, some are circular but cannot be confused with Men's round houses (Table 5): each of these two types are radically different in size and internal arrangement, and there are differences in feature and material remains.

It seems that the oval menstruation houses follow a distinct pattern. They are entirely distinct from any other discussed house type (Fig. 14). Among the 23 houses recorded, there is one, the largest, which is in fact a recycled oval house (5.5m by 3.7m). It stands well clear from the cluster of "normal" menstruation houses (Table 9) and

consequently has to be rejected from this analysis. Its original function is as an oval house (family type) and not as a menstruation one. The length of the other oval menstruation houses ranges from 2.7m to 4.2m, while the width is from 1.6m to 2.9m. A theoretical mean menstruation house, derived from our sample, should be 3.4m long and 2.3m wide, well below and clearly distinct from the oval type.

In the archaeological testing on this site formation process, one should particularly look for five points:

- i: small platform;
- ii: absence of mumu;
- iii: single central post;
- iv: no internal room division;
- v: poor material remains.

3-9-7 TOILETS

Toilets do exist traditionally in the Upper Wahgi (Vicedom and Tischner 1977:269). One of them has been excavated in the Kuk swamp and seems to have been made some 300 years ago (Harris 1977:9). Not every settlement has a toilet (a problem stressed in Logan's quote at the beginning of this chapter) and in such cases the nearby bush is used for their specific functions. Otherwise, toilets are hidden in the bush or in mixed crop gardens, but always near houses (see Figs.7 and 8). They all have a fairly standardised architecture, consisting of a square or circular pitpit screen (sometimes with a kunai roof), with a small opening leading to a central cesspit.

From the seven toilets closely observed (Table 9), only a very small percentage of the total type, it seems that the cesspit is circular and about 80cm to 1m in diameter. For obvious reasons, I have even less data on their depth. It is said to be deep, up to 2m.

The only toilet observed under construction had such depth and shape. Planks are then laid across the top of the pit, except for a narrow central opening. Then the spoil from the pit is spread over these planks as well as at the base of the pitpit screen. All this gives a neat appearance. The only obvious artifactual remain associated with such site seems to be coprolites.

In the problem which concerns us, three points have to be kept in mind for our understanding and interpretation of archaeological toilets:

- i: cylindrical deep pit within the house site limits;
- ii: circular like mound of compact clay with possible row of post holes on top of it, encircling the deep pit;
- iii: lack of artifactual remains.

These points show that theoretically toilets cannot be mistaken for other features such as mums, their closest formal resemblance, nor with any other site already discussed. This should be recognised by the archaeologist rather than interpreted by the local workers, as has happened previously (i.e. Harris 1977:9).

3-9-8 PIG STIES

It seems that in recent prehistoric times pig sties were not numerous, pigs being kept inside women houses rather than in a shelter for themselves. Only rich men could afford to have such sties. Vicedom himself noted a post-European increase in the number of sties, resulting from the fact that not only rich men had them, but also poorer men who built and shared together such buildings (Vicedom and Tischner 1977:267). Nowadays such sties are still not numerous. I have recorded 26 of them and this probably represents more than 80% of the total. Their dimensions are in Table 9 and the plan of six of them are given in Fig.15.

It is obvious that such sties should be located in pig land. Nevertheless, there is one exception where a very small sty was located inside an enclosed settlement and the only pig present being led to the grazing land by a leash. Sties are found all over the landscape, near and far from settlements. No obvious pattern in their location could be found. The same can be said about their architecture. Almost every sty is distinct one from another, in size and in shape. This is illustrated by the six examples given in Fig.15. The range in the length of recorded sties varies from 1.5m to 8.1m, while the width is from 1.2m to 4.4m. It is totally irrelevant here to speak about a classic sty (although in our data it would be 3.9 by 2.6m) because of total lack of consistency in all measurements.

It has been noted that sties are generally built with light material, suggesting a shorter life cycle for these buildings. People are also aware of the amount of excreta and top soil disturbance surrounding the sties and take the opportunity to start a new garden when the sty is destroyed. While buildings having a primary sty function are not numerous, one must point out that a great number of other buildings whose primary function is quite different, are recycled as sties when abandoned: numerous oval houses, women and pig houses, even men's round houses. This secondary function is of importance when dealing with site formation processes and could easily be misleading. Because there is no time gap, or very little, between abandonment and recycling, the additional activities, features, disturbances and remains coming from pigs will probably tend to confuse rather than to clarify the interpretation.

Unfortunately it is a problem that I cannot solve. Many observations were made for this and it was found that there is no difference between an abandoned house in pig land (thus visited by pigs)

and a house formally recycled as a sty. It will not be possible to find any stratigraphic distinction for each of the two activities. Some features such as pig wallows will indicate the presence of pigs but not the nature of abandonment or recycling of the building.

On the other hand, it seems possible to clearly define a sty. It is a building where there is no hearth nor mumu, with pig wallows in all internal subdivisions (if any), and where the artifactual remains should be almost nil. Round the structural remains (post holes) there should be no signs of activity other than pig wallows. These structural remains should show a pattern very distinct from any other house type already discussed, thus stressing its potential as being a sty. Finally, one must remember that many pigs have no shelter at all, but are left in the bush day and night, being fed once a day at a regular spot.

3-9-9 CASSOWARY CAGES

Although semi-domesticated cassowaries are much less numerous than domesticated pigs, I made many observations on them for two major reasons. First, they are native to New Guinea and could have a longer relationship with man than the imported pigs. Secondly, the nature of this relationship is such that very little remains could be left to testify it. This older man-cassowary relationship might in fact simply be expressed by the rigidity and standardisation in cassowary cages, as opposed to the almost anarchic style of pig sties. It is as if a building "code" is well established for the cages while sties might still be in various architectural trials.

There is absolutely no difference between the six cages recorded at Kuk in 1977 (Table 9) and those observed by Vicedom (Vicedom and Tischner 1977:267-8). Vicedom describes them as being on stilts 80cm high and illustrates such a case (Vicedom and Tischner 1948:178,

Fig. 79). Five of the six Kuk examples are also on stilts and the remaining one is on the contrary dug in the ground. The latter is said to be a result of the introduction of steel spades and consequently should not be found in the archaeological record.

The three largest examples from Table 9 represent in fact double cages. They are built as a single rectangular framework, divided in its centre by a row of light sticks. All cages are located next to a house, very often a men's house. The floor of both types is made of sticks, with a space between each of them. This space allows the cassowary's droppings to fall beneath the cage, thus keeping the platform clean. The structural remains from the two styles are rather different. The one on stilts should leave only a few well spaced posts or post holes, while the suspected post-European style will in addition have a central square shallow pit.

Artifactual remains should be extremely limited. The only one likely to occur (also illustrated and described by Vicedom) is the trough. It has a very distinct form and shape, being a dugout log. It is probable that such a prehistoric trough has been recovered from the Upper Wahgi, but mistakenly interpreted as being a possible "canoe" (Allen 1970a:181). Allen's "canoe" is presented in Fig.16 (Example A) together with seven troughs found at Kuk. By using ethnographic mechanical analogies (and strongly suspecting that there never were canoes in the Upper Wahgi), it seems to me that Allen's find is quite clearly a broken cassowary trough.

Troughs are essential for the survival of cassowaries. These birds need a lot of care to be kept alive and well. It is the belief in the Upper Wahgi that they will die within 24 hours without water. Troughs are in direct association with cages. Nowadays, some troughs are made from sections of car tyres and, as another example of recycling, the classic wooden trough is sometimes used to wash and

store coffee beans (Family of Man, 1979:1692-3).

To resume this section, one must again stress the possibility that cassowary cages could be found in early sites, and that the scanty evidence that would survive (beside an unexpected find such as a trough) would be few well spaced small post holes near a house platform, forming a square pattern for single cages and a rectangular pattern for double cages. If sunken cages are pre-European, then the evidence is dramatically increased with the potentially neat square shallow central pit.

This limited evidence might nevertheless be of importance. The taming of cassowaries is widespread in Papua New Guinea. In the Highlands they certainly have a great ceremonial value (i.e. Sillitoe 1979:147-8,274; Strathern 1971:135), and a social value as well (Sillitoe 1978). Beside the fact that many artifacts are made from cassowary feathers and bones (Sillitoe 1978), they are (or were) a valuable more expensive than pigs: the "equivalent traditionally to a long tube of decorating oil or 4 pearl shells and a pig" (Strathern 1971:105).

3-9-10 OTHER BUILDINGS

There are other types of building in the Upper Wahgi (Table 4) on which I have little field data and no archaeological testing was made on such sites. These types are also poorly recorded in the literature. A new type of building, widely spread throughout Papua New Guinea, is the European introduced Trade Store. It can have various sizes and shapes, built with traditional material or with modern material (concrete floor, corrugated iron sheets). This type has no prehistoric relevance at all.

The Guest Houses described by Vicedom (Vicedom and Tischner 1977:259) are of a ceremonial type temporarily built along singsing

grounds. This type still exists, with an additional but more permanent variant located in settlements. The latter is not common and is often used as a sort of temporary storage space. These guest houses are in fact simplified oval houses, with no mumu in them and often no internal subdivision between sitting and sleeping rooms. It is said by my informants to be an European introduced type, prior to that guests were received inside men's and women's houses.

Few dog kennels are found at Kuk and it is not known if they were traditionally made. None of them is described by Vicedom. They are all found inside settlements. Shooting huts (Vicedom and Tischner 1977:266-7) still exist nowadays for bird hunting, but I have never seen overnight huts in the Upper Wahgi (Vicedom and Tischner 1977:266). Such huts were observed in the thick uninhabited forest of the Jimi valley and were roughly made, good enough for hunters to have a roof and a warming fireplace. This is quite different from Vicedom's description from the Upper Wahgi (and illustrated in Vicedom and Tischner 1948:176 Fig.78). This illustrated overnight hut in fact covers also dog kennels and bird shooting huts. They are all made of a light timber frame stuffed with kunai grass. Because of their structure, building material and duration (very short), it is assumed that they will not occur in the archaeological record. Unfortunately I was unable to test this.

It is possible that there has been some substantial changes in ceremonial buildings (Table 4) but because of my very limited observations on them I do not feel confident enough to discuss them. Furthermore, none of these types has been archaeologically tested. This is a problem left open. Finally one must mention the occasional odd building which appears in the landscape as suddenly as it disappears, on which I also have limited data. These are buildings such as the marsupial cage (never seen by me but well described in Vicedom and

Tischner 197?:268 and illustrated in Vicedom and Tischner 1948:178, Fig.80), or windbreaks in communal gardens, or platforms on stilts to store sweet potatoes in pig land (when pig fodder is carried far away from settlements and stored there for a few days only), etc. None of these odd buildings has been archaeologically tested.

3-10 CONCLUSIONS ON HOUSES

Throughout this ethnographic analysis of Upper Wahgi houses, I have attempted to move from a "macro" to a "micro" approach on what house sites consist of, what are their limits, characteristics, features, etc., and what and where artifacts might be expected to go into the archaeological record.

All this was designed to inform us of the processes involved in site formation, independent of any archaeological support. The complexity of the problem is great, due to the high number of house types, as well as the very often specific geographic locations involved. There are many trends and patterns, but at the same time there is a great individualistic input making exceptions rather regular and numerous. Finally, one is witnessing some dramatic and accelerated changes, started in 1933, and the problem here is to distinguish between traditionality and modernisation. This is not eased by the very poor records available from the "early days", a gap spanning about 25 years after contact - a full generation!

It seems quite evident from this analysis that an emphasis has been put upon "function" against "form", with the help of artifactual remains and some specific feature remains. It is felt that most of the questions should be answered, and to summarise (rather than to clarify) the situation, the plan of all houses have been compared, one type against another (Table 10). It is clear that the patterns discussed in the above section are emerging, particularly the one

related to the width of buildings. While there is sometimes a substantial overlap between types (i.e. between 7m and 9m), the additional "internal arrangement" parameter (Tables 11 and 12) already helps greatly to clarify the situation. Finally, by adding the "artifact distribution" parameter, one should be able to further clarify the formal interpretation. With all this in hand, one should be able in most cases to give a functional answer.

The problem here is to evaluate how many of the various components of each parameter are necessary for the functional answer, how and where manipulations of data might break down, how far the interpretation can be pushed.

CHAPTER 4

HOUSES - ARCHAEOLOGY4 ARCHAEOLOGICAL TESTS

During the 1977 mapping of human activities at Kuk, numerous abandoned house sites were noted. These were potentially interesting for testing their archaeological remains. A catalogue of various house sites was made which included all the types. The time of abandonment was fairly well known through interviews with the owners of the sites and crosschecking with aerial photographs. Some oddities in the landscape suggesting house platforms were also noted and because these were unknown to the Kuk community, they were classified as "prehistoric house sites". These sites are found all over Kuk, in all the existing micro-environments (pig land, gardens of all kinds, Ep slopes and ridge tops, dryland, wetland, grass covered areas and forest covered areas). Permission to excavate the sites the following year was asked of most of the owners. It seemed that there would be no problem. Back in Sydney an excavation strategy was prepared.

4-1 STRATEGY: TEST PRIORITIES

Based first on the ethnographic data collected (presented above), second on a single case study also recorded in 1977 (Pita House, next section), and third on the available literature on prehistoric house sites from the New Guinea Highlands (see end of this chapter), the following strategy was prepared.

First of all, for various reasons I did not plan to test the hypotheses concerning site limits. This is a major problem found in archaeology but I decided that it was more urgent to get maximum information on site types and their archaeological appearance. A study on site limits is a research issue on its own involving fewer sites thus reducing the

available time to collect the data on house sites and also the time to study other sites. In this project, this important question had less priority than other problems.

My intentions were to excavate three examples of each type of house, one extremely recent (abandoned only a few months earlier), one site abandoned some 5 to 10 years earlier and finally one site from the beginning of Kuk's recolonisation some 20 years ago. If possible, a few prehistoric sites of unknown house types would also be excavated to have a better control on period of abandonment and possibly on change and continuity in house types. It was also expected to test various environments to assess their effects on archaeological remains (vegetation regrowth and human and pig disturbances). All the excavations would be restricted to the house platform, where it was suspected that maximum information would be gathered within a limited excavation. It was also planned to first fully excavate some platforms and then to reduce the area to be excavated to see how much had to be excavated to provide the most accurate information on ethnographic activities and archaeological remains.

This plan was simply too ambitious and did not work. The first major problem encountered was that permission could not be gained to excavate many selected sites because of the owner's absence or his refusal to allow us to disturb his land (even with financial compensation). The belief here was that either we would destroy some plots under cultivation and this should never be allowed, or we would lower the nutrients of the soil under fallow and there would be bad yields in the future. It also happened that an owner did grant permission to dig a site but had to retract under the pressures from the wife responsible for that piece of land who vehemently refused it for reasons similar to those expressed above, not because of anything that might be found. Things suddenly were not as easy as expected. So we followed a day-to-day policy of

looking for a new site only when one was almost finished.

The second major problem was that the excavations were much slower than expected and the sample had to be dramatically reduced. Nevertheless, various house types in different environments and from different periods of abandonment (including one prehistoric site) were test excavated. While there is an imbalance among the different house types analysed, these data should still cover the prepared strategy and they should answer most of the questions raised in the ethnographic section.

Overall some 872m² were excavated, spread between 16 sites (two of them entirely unsuccessful) covering 27 buildings of 9 types. They are discussed below in the same order as that in the ethnographic section.

4-2 CASE STUDY: PITA HOUSE

Pita's homestead has already been discussed in Section 3-6 and the location of his house in relation to the rest of the settlement is illustrated in Figure 7. One can see that it is a classic oval house having a central sitting room containing a hearth and a mumu and with one double bedroom at each extremity of the house. The double bedroom located at the northern end is deliberately asymmetric, the smallest one being Pita's room.

In 1977 the house was accidentally burned down while Pita's wife was cooking. The house was totally destroyed and the only item that witnesses tried to save during the fire was a bag of coffee beans (by breaking through the back wall). I came to record the remains some 6 days later. Pita told me that some items were already removed, such as partially burned house posts, big cooking stones, gardening tools, pots, etc. During the recording more items were stolen by children (with no reaction from Pita) such as wire, springs, mirrors, etc. What has been recorded is presented in Figure 17. I think that beside the few missing artefacts, these remains are representative of what one might expect to

find in a traditional Upper Wahgi oval house. A few days after the re-cording Pita rebuilt an exactly similar house on the same platform, thus obliterating most of the evidence from the first house. It would be interesting to excavate this site later on, when abandoned, to see if one is able to make a valid interpretation of the superimposed remains.

The remains of Pita's house were remarkably concentrated on the house platform itself, very little being found outside it. There was such a mess that a grid of 1m^2 was set to record it. All the finds are shown on Fig.17 except for a thick carpet (up to 5cm) of ash and charcoal covering the entire platform. The raised platform was clearly visible (see levels in Fig.7) as well as most of the perimeter wall (made of post holes or posts burned down to the base but still embeded in the clay). The internal partitions and all doorways could also be relocated. At the side of the hearth one could still see a pile of charred vegetables (for that evening meal). The mumu was more difficult to find (without excavation) because of its compact filling (deliberate) and the attempt to salvage the coffee beans could also be noted.

The first interesting new evidence was the higher concentration of charcoal and burned clay found beside the fireplace. This comes not only from the falling roof but also from the rack to store fire wood and seeds which is suspended just above the hearth. It was quite a remarkable feature, having a direct and obvious functional significance. The two central ridge posts were also clearly defined in form (bigger than the other posts) and location (central). Cooking stones were scattered throughout the site, with a much greater concentration within the sitting room. Most of these features and artefacts are repeated in Figure 18 where the internal arrangement and stone distribution clearly emerge.

Quite a substantial amount of food remains was found in the bedrooms, but each one had a different pattern. The smallest room to the

right (Pita's room) has predominantly corn and pig bones, the room next to it (2 male friends) is dominated by beer drinking evidence and corn refuse, the room located near the doorway on the other side (occupied by a female relative and Pita's children) is characterised by betel nut remains and the last room (Pita's wife) has a bit of everything. These food remains do not illustrate the last meals for two reasons: meals are taken communally outside the house or around the fireplace and one must remember that houses are never swept. I think (and Pita would agree with me) that this food refuse represents food and drink taken discreetly by individuals while no one else is around.

The few artefacts related to pig activity (items 25 and 26, Fig.17), beside the pig bones, are the only evidence that Pita, not having a sty, kept a small pig tied to the wall at night near the mumu (and item 26 is certainly convincing evidence for this). Finally, one can see with this case study how much the traditionality of Hagen life is affected by modern consumption items.

If we eliminate all these modern artefacts and food remains (on the assumption that they are not found in prehistory or they are not preserved in the archaeological record) we are left with what is illustrated in Fig.18. The entire house structure is visible, and there is not much difficulty in interpreting the remains accurately. One can see a clear functional difference between the central room (doorway, cooking stones, hearth, mumu) and the side rooms (poor in feature and material remains). One can even speculate where people used to sit around the fireplace, some absence of stones suggesting a regular sitting spot.

If one pushes the exercise further, by assuming that the platform was no longer visible (and this happens), and that all the post holes had disappeared (except for the ridge posts), we are left with a scatter of cooking stones and some basic features (Fig.19). It looks more like a hunter-gatherer campsite than a farmer's family house. Do we still have

enough hard data for an accurate interpretation of the site?

Working around the module principle described in the ethnographic section of this chapter, one can for the moment totally ignore all the artefacts. The only features relevant for the house "reconstruction" are the two central posts, the hearth and the mumu (Fig 20). The most important datum is Axis A, the distance between the two ridge posts, which regulates all the other structures. Knowing Axis A, one can estimate the maximum size of the house by drawing Axis B and C at right angles to A and outside the central posts. Assuming that the mumu is internal, Axis D is introduced in such a way that it is at right angles to B and C, parallel to A and well outside the mumu (for the maximum size). Then one has to introduce Axis E parallel to A and D, at right angles to B and C and at a distance equal to $AC-DC$ or $AB-DB$. Still to test the maximum size of the house, Axis F and G are drawn in such a way that they are parallel to Axis B and C, at a distance equal to Axis A, from B and C respectively.

It is when all these axes are drawn that the interpretation of the house starts. Its greatest size must be restricted by the rectangle DEFG. Most probably the sitting room is limited by the rectangle BCDE. Because of the positional relationship between the mumu and the hearth, the doorway must be located somewhere midway between C and B, on Axis E. If there are bedrooms at either side of the sitting room, they most probably will be single bedrooms if nearer to Axis B and C, and double bedrooms if nearer to Axis F and G. In our case, there must be two double bedrooms.

All the above interpretation seems to be extremely accurate, if one superposes the perimeter of Pita's house (Fig. 20). What is important is that so far all artefacts have been excluded and we have been dealing with a very limited number of features. It seems that by pushing the exercise even further, by eliminating the mumu and hearth, a similar

result could still be reached. This suggests that the most important features are the ridge posts. If not found, all the process of interpretation collapses (unless hearth and mumu are found). On the other hand, it is as important to record with extreme precision their exact position (something which might not have been done for Pita's house), a slight misreading resulting in a horizontal displacement of all the cubicles involved in the house interpretation. Additional data from the site is then a bonus. In Pita's case, the distribution of cooking stones will affirm the presence and size of the sitting rooms. What will give us the correct length of the house is its platform (or surrounding ditch when present), something that one can do with evidence from Fig.18, but cannot do with evidences from Fig.19. It also seems that unless some dividing posts or post holes are found (Fig.18), one would not be able to define precisely the four bedrooms (Fig.19). In such cases one would have to assume that the rooms are equally divided, following the general pattern, an assumption which is wrong for Pita's house (a rare exception).

Being aware of all the advantages and traps found in such interpretations, let us look at the excavated sites to define their general trends, form and finds compared to interpretation and function.

4-3 MEN'S ROUND HOUSES

The location of all the excavated house sites is presented in Map 24. Five of them are men's round houses: MOP, MOG, MOH, MOQ and MOL. A total of 274m² has been opened among these 5 sites, and 5,164 artefacts and ecofacts were recorded.

4-3-1 SITE MOP

MOP was abandoned around 1965 and the natural vegetation was allowed to grow back. It is a dry and naturally well drained site. Some 3m tall shrubs covered the site when the excavation started, it was bushy and no

indication of a house site was apparent beside some mature tankis. Its owner was one of the excavators and could tell us how the house was arranged.

The doorway with eaves was facing a discussion and cooking place (site MNI, Chapter 6). The sitting room had a central position with a big guest room at the back. The owner's private room was located on the right when coming in. There were a hearth and a mumu on the same axis with the central post and the doorway (same pattern as Example E, Fig.9). He did not remember if the guest room started at the level of the central post or further back. He did not have to build a platform because there was already one, from prehistoric times. He had only to clear it, to straighten the surrounding high embankment and to redig the house ditch and its exit located on the side of the house. As for his cooking place, many stone axes were found during clearing of the platform. Later on, his house was left to rot, its timber recycled as firewood. Consequently we had first hand information on MOP and before starting its excavation we knew that there should be the remains of two round houses, and we also were aware of almost all the nature and location of the internal arrangement and structure of the later occupation phase.

What was found is quite a different story (Fig.21 for features and Table 14 for artefacts). Some 47m² were excavated and we have no evidence of a prehistoric occupation. Although the platform was subsequently cleared, it still does not explain why all the evidence is gone (Plate 11). It implies that the new features were put at the exact location of the old ones (particularly for the ditch, central post, hearth, and perhaps mumu), a phenomenon which seems unlikely (total obliteration), or the new house was cut down into the basal clay. On the other hand, there are some post holes around D5 which do not have a clear pattern. Perhaps some of them belong to the earlier phase while others are the remnants of the internal arrangement of the second house. The interpretation is not clear.

The remains from the later occupation, only 13 years earlier, are extremely limited. We knew what to look for and where, but there was little surviving. It seems that smaller features such as posts and internal light structures have disappeared forever by natural processes while major features (ditch, central post, hearth, mumu and the unexcavated embankment) probably have entered the archaeological record (Plate 14). The total of artefactual remains and food refuse found (Table 14) was 1,105, with an important input of modern consumption items. This is discussed further in section 4-3-7.

4-3-2 SITE MOG/R

MOG (Map 24) is located on top of a spur end of Ep ridge (Fig.22, left). It is divided into MOG/CP (discussed in Chapter 6) and MOG/R, which is the round house itself (Fig.22, right). Although it is now located in pig land, the area is regularly burned, thus it is covered with kunai grass. Due to its location, it is probable that the site is subject to erosion. It was abandoned around 1962 and the owner's description of it seems to be of an exactly similar house as MOP and this is not repeated here. After clearance, only the outline of the embankment was visible, thus indicating precisely where the platform was. Some 55m² were excavated and 335 artefacts recorded (Table 15).

Features similar to MOP were found as well as a similar absence (disappearance) of some features: while the central post, mumu and hearth were there, many posts from the perimeter wall as well as the internal subdivisions were not found. The house ditch itself was partially missing. The hearth was lined with long slaps of Ep stone (similar to Plate 13) and just outside the doorway was an arrangement of the same slabs. This is seldom seen nowadays, as in the whole of my ethnographic observations only three such cases were noted (including two at Kuk): one having a single slab and two houses having an arrangement of upturned beer bottle

bases. Artefacts found in MOG/R were less numerous but more traditional than those from MOP.

4-3-3 SITE MOH/R

MOH is a complex site built and abandoned at the same time as MOG, but located on the lower slopes of Ep ridge. It is a much wetter area dominated by pitpit grass. Today it is also in pig land. This is the most extensively tested settlement where numerous house sites were excavated (Fig.23). Among them is MOH/R, the men's round house itself (Fig.24). Its location is marked by old casuarina trees and tankis still standing on or just outside the surrounding high embankment, easily detectable. The owner's data were that the house was big with a simple internal arrangement consisting of a central sitting room and a very big sleeping room at the back. This was the transit house "par excellence" during the recolonisation of Kuk, accommodating up to 25 men on their migration from Buk to the new Membo territory of Kenta. Some 88m² were excavated and 1,678 artefacts recorded.

The feature remains found (Fig.24) are consistent with the previous two sites above, but with an even greater disappearance of smaller features. Here the hearth was also lined with long slabs, and one of them, very big, was at the doorway. While a pile of cooking stones was found beside the hearth and there was a scatter of such stones on the platform, no mumu was found. One can nevertheless assume with some confidence that it is most likely that there was a mumu, and consequently it is located in one of the unexcavated areas of the platform (most probably in H9 or H10). The surrounding ditch was rather deep and had two exits, stressing the wetness of the area. Because of the clogging up of the natural and artificial drainage systems and because of the upturning of soil from the excavation itself, water from daily storms was trapped in sites MOH and MOG; this was an ideal feeding and playing ground for pigs

(rooting and wallowing everywhere), thus considerably damaging our neat trenches.

New features were found in MOH/R. There are seven pig wallows, three of them in the clay layer. These were made after abandonment, when the settlement reverted to pig land. Some wallows are in the clay because the house platform was exposed to the elements for a longer period, the soil deposition on it (mostly from the embankment) being a relatively slow process of a few months duration. A second hearth was located in E9. It was simply made and seemed to be of a very short duration, just posterior to the abandonment of the house, although lying on the basal clay (mixed with some top soil). Artefacts recovered (Table 16) were comparatively numerous, with an abundance of bone remains (including a human premolar). The hearth in I7 is shown on Plate 13.

The excavation of the central post hole was a problem. It was extremely narrow and deep, to an extent that our arms were not long enough to reach the bottom. So the top of the feature had to be destroyed to reach the base where, to our surprise, the base of the post was still "in situ". It was square and flat, not pointed. It could not have been driven into the ground. The final depth of the hole was 100cm. When asked how such deep holes were made, the Kawelka said that when it is getting too deep, they use a big pointed stick to dig and a hollowed bamboo to remove the dirt (something like an auger). Three small pointed sticks were found embeded in the clay to support the central post.

4-3-4 SITE MOQ

Some 34m² were excavated at MOQ and 479 artefacts recovered. MOQ is located on a dry hillside overlooking a marsh which is part of the southern catchment of the Kuk swamp (Map 24). It is in an area protected from pig disturbance (fenced settlement). When visited in 1977, there was a small round house occupied by a Tari family. It was built on the side of

what seemed to be a much bigger round platform dug deep in the hill slope. The eastern and southern limits were scarps in hill slope, or the west wall was a bank made of the artificial piling up of spoil dug from the hill, while the northern side was open and overlooked the marsh. The doorway was obviously oriented northward.

Following the tribal fight between the Tari and Kawelka early in 1978, this house was looted and destroyed, and most of the platform abandoned except along the eastern embankment where a new pig shelter with three sties was built (Fig.25). This partial abandonment was a good opportunity to test the site for various reasons, including two major ones: i) to check the remains of the Tari house observed and recorded in 1977; ii) I was convinced that the platform itself and its substantial surrounding earthwork were more suitable for an earlier classic men's round house than anything else.

The surface of the site was a loose top soil heavily disturbed by tethered pigs. In addition to the sty there were two pig wallows (Fig.25). There was no indication at all of the existence of a house seen one year earlier. The excavation was conducted in such a way that the standing sty was preserved.

A change of stratigraphy occurred some 10-15cm below the surface of the loose top soil where a harder soil mixed with brown clay was found. This was the surface of the Tari occupation (Fig.26, left). The usual features were found and the plan of this house was clearly not centred on the circular platform, nor its surrounding ditch. The hearth was simple and two larger post holes on opposite margins indicating the frame used to barbecue pig meat. The central post hole was not circular but square. The two wallows observed on the surface had penetrated this clay layer, although their size had been reduced. The surrounding embankment was now clearly delimited, with its opening (exit) northward. This brown clay-soil layer also indicated that the Tari did not bother to clear the

platform down to the basal clay to erect their house, as it is usually done (thus re-destroying prehistoric features).

When the basal clay was reached some 20-30 cm below the surface of the previous layer, new features appeared (Fig.26, right). Very few posts from the Tari phase of occupation were left, and hearth was gone as well as the wallows, but the central post remained. Quite an unexpected find within the embankment was made. Now there was an opening westward. This indicated that the orientation of the doorway from an earlier house was in that direction. Most probably the northern side was sealed with earthwork. At the centre of the platform a new major post hole was found (filled with cooking stones, said to be a well known children's game), interpreted as being the central post of a men's round house. This seemed to be confirmed by the presence of a hearth beside it, in alignment with the post and the centre of the western opening of the embankment. All these features seemed to be related. A mumu (containing modern artefacts, see Table 17) found in B4 was most probably also related with the other features, all of them stressing the presence of a classic central sitting room. A few post holes were found along the perimeter of the platform, probably also from that men's house. All these assumptions and interpretations were in fact a posteriori confirmed by the information received from the owner of the former house.

The surrounding ditch this time totally enclosed the platform. It was square with a flat and smooth bottom, suggesting the work of a steel spade. But along the western and southern external limits of this ditch there was the remnants of what appeared to be a prehistoric circular ditch. This one was irregular, shallower and made of hundreds of small holes, a characteristic of men's heavy digging sticks. It was assumed that this was a prehistoric ditch, surrounding a missing house which probably had the same orientation and size as the one from the men's round house phase.

The site was much more complex than thought. Added to this, it is possible that in the near future the sty will be replaced by another house, thus continuing this process of addition and obliteration. Artefacts found throughout the stratigraphy (Table 17), from the surface down to the basal clay, had an important component of modern items. By themselves they tell little on the various occupations. But the analysis of the shallow stratigraphy and the feature remains allows us to define at least four periods of occupation:

- i): a prehistoric men's round house oriented east-west, at least 100 years old (from oral history evidence);
- ii): a men's round house of the same size as the first one, built around 1967, occupied for about 2 years only (from the owner's information), also oriented east-west and using most of the features available from the previous phase;
- iii): a Tari occupation phase with a small round house, built around 1976 and destroyed in 1978, oriented south-north, using little of the existing features;
- iv): a pig sty built in 1978, which uses even less the existing features.

4-3-5 SITE MOL

This is the only prehistoric house site that I excavated at Kuk. It is located on dryland, opposite site MOQ, on the northern side of the same marsh (Map 24). Even before clearance it was possible to see a huge oblong embankment which at the time of occupation must have been very high. It was said to be prehistoric by the Kawelka, nobody knew what was there. Because of the oblong shape of the site (Fig. 28), I suspected that it was most probably the remains of a woman and pig house. Its excavation revealed a men's round house. The site was covered with kunai grass and sparse shrubs, the platform was used as a dump by the occupants of a house adjacent to the site, sometimes pigs were tethered

there, on top of the SW part of the embankment a new toilet was built by the people from the adjacent house, and its northern limit was cut by a modern garden ditch (Fig.28). The principal objective was to recover and analyse data from a prehistoric house site. It didn't really matter if it was from a men's round house or a woman and pig house or any other house type.

Altogether, 64m^2 were excavated (Fig.27), divided into 50m^2 on the house platform and 14m^2 on its cooking place. Some 1,567 artefacts were recorded (Table 18): 610 in or around the house, 957 in the cooking place. Only the data related to the house are discussed here, the cooking place being discussed in Chapter 6.

The features found in the house section of the excavation (Fig.27) are extremely limited but consistent with what has been noted with the other sites above. The surrounding ditch, extending on one side to the cooking place (in N7), showed the little holes made by digging sticks, but less clearly than usual. These holes were almost invisible or even absent in some parts of the ditch, suggesting that both digging sticks and wooden spades were used to make the ditch. The hearth was very rudimentary, consisting of only a shallow depression, nothing comparable to the two types found nowadays. Beside it was a pile of small cooking stones penetrating the basal clay, while no mumu was found on the platform. I cannot explain this. The central post hole was like any other found nowadays, narrow, deep, with some cooking stones in it. Not a single post hole from the walls was found. They all have disappeared. The axis of the central post and hearth is extended to the opening in the ditch, stressing the location of the doorway (further confirmed by the layout of the embankment). This is a relationship consistent with present day men's round houses.

Artefacts found consisted mostly of cooking stones. There is no doubt that some of them must be contemporary to the house itself (evidence

from those found in situ in features), while others may have been dumped there later on, by the occupants of the adjacent house. Pigs rooting on the platform have probably also disturbed the distribution of artefacts, not only horizontally but also vertically. Modern items were found throughout the stratigraphy. All these activities, subsequent to the abandonment of MOL, could explain the amount of stones and European introduced items found not only on the surface but also down to the basal clay. One way of solving this problem would be to check the origin of stones to see if there is any difference between those used nowadays (Wahgi river and Kubor range) and those used in prehistoric times (few from Ep ridge and many from unknown sources). I did not try to answer this question.

Another manifestation of the vertical displacement is the finding of one small piece of plastic lying on the basal clay. This is one of the best representatives of modern artefacts, but found "in situ" in a prehistoric house platform (see below for dating). It is probable that the plastic found its way down through pigs digging for worms. This example, added to the known disturbances described above and to the unknown disturbance which must have occurred since the abandonment of MOL is a good warning on our understanding of the relationships existing between features and so-called associated artefacts (never numerous in the Highlands).

Two charcoal samples were collected for dating purposes. The one collected in Mumu N5 (SUA-1328) unfortunately was too small after removal of contamination and could not be dated. But the one collected at the base of the ditch in C8 (SUA-1329) was of pure charcoal (all the ditch bottom was filled with compact charcoal). At the Radiocarbon Laboratory (Sydney University) it was boiled in dilute hydrochloric acid, soaked overnight in dilute alkali solution twice and boiled once more in dilute phosphoric acid. Its dating is 260 ± 90 BP. It was then calibrated

according to the method of R.M. Clark (1975) and its ideal age is 330 BP (1620 AD). With 95% confidence interval on calibrated age it becomes a meaningless 260 ± 260 BP (M. Barbetti, pers. comm.).

Because of this date, one must consider MOL as being a very late prehistoric Men's house, probably from around 1900 AD when Kuk was last occupied. This would explain why so much surface evidence is still visible today. At the time of occupation of MOL, it is probable that women and pigs were housed elsewhere, possibly following a pattern similar to the present one. There is nevertheless quite a substantial difference from present Men's round houses in the layout of the surrounding embankment. In MOL it includes the house and the cooking/discussion place, while nowadays such embankment includes only the house, the discussion place being on the contrary left deliberately open.

Because we have so far a single example of such prehistoric layout, one cannot make general statements about this change in spatial management. Nevertheless the hypothesis here is that if MOL is representative of its time, it stresses the possibility of an extremely late change in landscape management around these Men's round houses, with a much greater earthwork involved in the earlier phase.

4-4 OVAL HOUSES - SITE MOO

MOO (Map 24) is the only site where oval houses were excavated. Two such houses were found, together with the remains of an earlier Woman and Pig house (Fig.29). The latter is discussed in its appropriate section. A total of 90m^2 was excavated and 3,670 artefacts recovered. The site is located on Kukrumdi plateau, between MOP and MOL. Most of the site was covered by a coffee garden and the only surface evidence of it was an eroded embankment along its eastern limits. While the owner did not remember the existence of a woman and pig house on

the same site (he did so only during its excavation), I was told that there were two oval houses built at the same time, and that the eastern one was occupied for a longer period of time. The total time of occupation was said to have been rather short, less than three years, and the site was abandoned around 1969.

Figure 29 illustrates the features found. They are consistent with those found in previous sites described above. One must notice a better preservation of features from the eastern house which could be the result of three distinct phenomena. First it was abandoned later; second, it could be the result of a quick covering up of the platform from the spoil of the nearby embankment; third, the western house after abandonment was exposed to the elements for a longer period of time, thus accelerating the natural decay of features and organic artefacts. A more plausible explanation for this difference in feature remains between the two oval houses is that the western house was demolished immediately after abandonment (its timber stored elsewhere as firewood) to make way for an extension of the cooking place previously located between the two houses. This action results in a releveling of the platform and removal of almost all artefacts, thus altering significantly the normal evidence left after a simple abandonment and decay of the site.

A careful examination of some features shows the order in which these transformations have occurred: for instance, Mumu E4 is later than its adjacent post hole; Hearth J16 is also later than its adjacent post hole. An analysis of such strategic features shows that there was first a long house oriented NE-SW, followed by two oval houses, the eastern one lasting longer or being later than the other, because of the existence of a cooking place extending on and beyond the western house platform (Mumus E2 and E4) cannot belong to that house.

The layout and distance between some of these features are also informative. While there is not much difficulty in defining the two oval houses, only a "modular" analysis of the central posts and Hearth H13 (Fig.30) will tell us that it corresponds to the contemporary buildings described as woman and pig houses. While we know the exact sequence of occupation as told by the owner himself (Figs. 30 to 32), there are some interpretation problems which need to be further discussed. They derive from the archaeological remains themselves and I think one cannot reconstruct the real situation from them.

The interpretation one can correctly make is the one of phases 1 (Fig.30) and 3 (Fig.32), while there is nothing to indicate that there were two oval houses occupied at the same time during phase 2. The site can be wrongly interpreted as having only the western oval house with a cooking place in front of it during phase 2, followed later on by the occupation of the eastern house during a third phase, which not only uses the same mumus as during phase 2, but extends its cooking place over the platform of the previous house. The ditch located along the southern boundary of the site might be rightly (but not necessarily) allocated to the second phase of occupation because of the eficence of the dumping of hearth ash in A2, which most probably belongs to the western house because of its peculiar location (hidden behind the house). Because of this find, one can assume that the ditch was not operating during the sole occupation of the eastern house. Since the hearth ash had been dumped on the bottom of the drain it strongly suggests that the drain was functioning in the absence of a kitchen garden during phase 2 and that therefore there was no kitchen garden immediately adjacent.

Can the artefacts tell us more about the site (Table 19)? Probably not. Following the custom of cleaning the top soil down to the basal clay, it is evident that theoretically all artefacts recovered

belong to the last period of occupation. This implies that artefacts belonging to earlier phases are to be found, most probably mixed up, in the unexcavated embankment. This seems to be confirmed by the evidence from Table 19 where most of the artefacts found can logically be attributed to cooking and social events (indoor and outdoor) by the occupants of the eastern oval house. This platform clearing being done roughly, it is probable that some smaller artefacts belonging to earlier phases are embeded in the basal clay, thus "in situ". Some of them follow the same process through human stamping. The "in situ" find of artefacts from earlier phases is illustrated in site M00 by what was found in hearth D5 (44 artefacts) which undoubtedly belong to the western house while inhabited. Artefacts in general are consistent with those discussed in the above Men's Round House section; they all stress the high input of modern items and the rather poor evidence of traditional ones.

4-5 WOMAN AND PIG HOUSES

Four Woman and Pig houses have been excavated. They are MOD (53m^2), MOE (37m^2), MOH/WP (116m^2) and M00 (90m^2). The first three sites are located on the lower slopes of Ep ridge while the last site (M00) is located in South Kuk (and has already been partially discussed in the previous section). A total of 296m^2 has been excavated and some 5,332 artefacts and organic remains were recorded (including the data from M00).

4-5-1 SITE MOD

MOD (Map 24) is one of the very first such houses built by the Mandembo around 1961. Its owner said that it was in use for an unusually long period of time because of its function as a transit place for pig herds of new immigrants. He could not remember the details

concerning the internal arrangements. Prior to the excavation, the vegetation consisted of an advanced secondary regrowth, dense, with tall trees and shrubs. We were permitted to clear the area with the exception of casuarina trees. Before the work started, the only visible indications of previous human activity were these casuarina trees, an unusually large flat area (suspected to have been an artificial platform dug into the side of the slope, see for instance Plate 7), and the sharp embankment caused by this digging activity. After clearance of the vegetation, the presence of such a platform was confirmed, and one could also see a shallow pig wallow in C9-10 (Fig.33), a concentration of ash in C12, and a depression suggesting a ditch in F10-12. Against the owner's statement, my personal feeling was that it could be the site of two contemporary oval houses, one of them being burned down.

Some 53m² were excavated (Fig.33) and 1,167 artefacts recorded (Table 20). Numerous post holes were found, and some posts "in situ" with charred tops. From these remains, it was possible to say that there was a double row of posts forming the perimeter wall, and a clear division between human (westward) and pig (eastward) quarters could be made. Pigs were kept in well preserved stalls, and occupied the eastern two thirds of the house. Within the human occupation area, two central post holes were found together with a hearth and a mumu. Their spatial arrangement also indicated where the doorway was. It was possible to make a distinction between sitting and sleeping rooms. From these data, a reconstruction of the house has been attempted (Fig.34), which is probably fairly accurate.

One must note that the surface indications of charcoal concentration in C12 were only a recent phenomenon, probably from an occasional fireplace, and that there is no ditch in F10-12: this is interpreted as possibly the result of a drip-line formation, the water dropping from the artificial embankment.

The above interpretations (sleeping room, sitting room, pigs area and reconstruction) are further confirmed by the distribution and nature of artefacts recorded (Table 20). From these, one can make a sharp distinction between sleeping and sitting rooms, and between sitting and pigs rooms. Due to the low quantity of artefacts found, their nature and location, it can be assumed that the contents of the house had been removed to a new site prior to abandonment and subsequent burning down. All the cooking stones left behind are small, unrecyclable. The only accident which probably occurred is the loss of a blue necklace (unusual quantity of beads in a restricted area). Mistakenly, no observations were made on the 89 pieces of glass to find out if there were use-wear on their edges.

It is probable that shortly after abandonment the site reverted to pig grazing land because of the presence of well defined features in C1, D1 and E6 interpreted as being pig wallows. These wallows were not visible on the surface of the top soil, and penetrated clearly into the basal clay: such events must have occurred when the platform had just been abandoned, before it was naturally sealed with top soil built up and washed in from the slopes above.

45-2 SITE MOE

MOE is located inside Kuk Agricultural Research Station, right on its north-eastern corner. It lies at the foot of a low spur of Ep ridge near its junction with the Kuk swamp. The owner of MOE said that he built the house just at the time of the swamp alienation (1969) and consequently his wife lived in for only a few months before the house was pulled down and rebuilt elsewhere.

The area was entirely covered with pitpit grass and the only surface indication of any human activity was the presence of a faint ditch. Some 37m² were excavated (Fig.35) and 52 artefacts were

recorded. The surrounding ditch was built across the slope, at the foot of the scarp found by the rear of the platform, then turned at right angles at each end of the long axis of the house to disappear downslope towards the swamp. The excavation of the house ditch revealed the existence of an earlier ditch (Fig.35), of unknown age since it did not contain any particular volcanic ash. One must note that the modern ditch does not take advantage of the existing prehistoric one. While they both are in the same alignment, there is still a slight displacement, the modern ditch is dug deeper into the basal clay (15cm against 12cm; top soil 30cm thick), the modern ditch is narrower (35cm against 55cm) and both have straight faces and a flat bottom.

Numerous posts and post holes were found showing clearly the house pattern. It is fairly distinct from the previous site (MOD). It seems that one end of the house is straight rather curved (similar to the veranda type) and instead of having that end occupied by humans, it is allocated to pigs: stalls were clearly defined. A small hearth was found in C7, a central post in C4 was just adjacent to the perimeter wall and the remains of two distinct doorways (in C4 and A7) were noted. All this evidence led to the hypothetical reconstruction of the house (Fig.36) which was approved by its owner himself. It is probable that there is a mumu somewhere in the unexcavated part of the sitting room.

The artefacts found were too few to assist the understanding of the internal organisation of the house. This probably stresses the short life of the house, and its deliberate and planned abandonment. Only 52 artefacts were found in MOE, consisting of 27 small cooking stones (18 in the hearth, 8 in the human quarters and one in the pig quarters), one piece of unworked glass from the human quarters and one stone axe chip from the pig quarters. In square D10, between the

house and its ditch was a pile of 23 large unburned Ep stones. It is probable that they were collected from the ridge in preparation for a new set of cooking stones when abandonment occurred.

4-5-3 SITE MOH/WP

This woman and pig house is part of a complex of excavated house sites (MOH) already described in section 4-3-3 (Fig.23). It is not repeated here. Some 116m² were excavated in MOH/WP (Fig.37) and 443 artefacts were recorded (Table 21). The location of the site in relation to the rest of the settlement conforms to the ethnographic evidence: because pigs are kept in the house, it must be erected near the enclosure system so that pigs can be released into the adjacent grazing land without interfering with the enclosed settlement. The features found (Fig.37) are also similar to those described above (sites MOD, MOE). It is clear from this feature evidence that the house had one big pig quarter in the centre, and one flat at each end. Nevertheless, one must note the very poor preservation of minor features such as small post holes. Most of them have disappeared forever from the archaeological record, while so far the perimeter of the platform is very well preserved. Excavation just outside the platform did not reveal the presence of a house ditch. The poor artefact evidence (Table 21) does not give us any clue on the internal subdivisions.

Two new features, not discussed previously were found in MOH/WP. The first one is the embankment found eastward, which is part of the enclosure system. This embankment is interrupted opposite the location of the doorway of the pig quarters, so that the fenced pathway linking the house to the embankment allows pigs to be in direct access to the grazing areas (see examples A, B, E, Fig.12). Undoubtedly there was a fence on top of it; not a single post or post hole was found.

The second new feature is rather unusual, and as far as I am aware,

unique in ethnography and prehistory of the Upper Wahgi. It is a sort of paved stone pathway for pig use only (Fig.38, Plate 12). It is made of large natural Ep stones, some of them weighing more than 20 kg, arranged so that there is almost no gap between them. It seems to me that there is a substantial labour input involved here (such rocks are not available in the immediate vicinity of the stone) as well as engineering (planning and finding rocks so that edges match with each other). At first it seems strange that such a large feature concerns exclusively pig activities: the adjacent Men's house has only one such slab in front of its doorway (Fig.24) while site MOG/R, another Men's house, has a comparatively small stone arrangement (Fig.22).

But knowing the site's peculiar history, and the workers' interpretation of the paved stone pathway, its presence seems to be very functional. MOH was a transit site for Membo migrating from Buk to Kenta, and at times had to accommodate numerous families and their pigs. Although located on the lower slopes of Ep ridge, the ground was (and still is) often muddy and wet. It is said that to avoid pigs messing the pathway even more, the stone arrangement was built: the top soil was scraped off, then parts of the basal clay were dug to accommodate the stones and stabilize them. While the above explanation is unconfirmed, I cannot think of any other. The question this pathway does raise concerns its uniqueness. Why are there no others observed elsewhere in the valley? One might say that it represents another example of Hagen individualistic enterprise. Following this, it is reasonable to assume that more of such arrangements could be found, some related to pigs (and perhaps for reasons similar to those above), and others related to human activities (domestic or ceremonial).

4-5-4 SITE MOO/WP

This site has been discussed in section 4-4, where two oval

houses were found postdating a woman and pig house. What must be added here concerns only the latter. Although the woman and pig house has been destroyed to be replaced by two oval houses, one can notice that the major features have penetrated the archaeological record (Figs.29, 30). On the contrary, most of the minor features have disappeared forever (very few post holes found).

It is probable that there is a mumu belonging to the eastern room (Fig.30) which has not been excavated. My guess is that it is located in G14 or G15. One hearth was found in H13 which seems to belong to this period of occupation. It contained 101 artefacts. The range and amount of artefacts found in it are clearly distinct from the other hearths or specific areas of the site (Table 19). It seems to me that only these 101 items are "in situ" and belong to the woman and pig house. All the other artefacts found on the surface of the basal clay, within the woman and pig house perimeter must be considered as belonging to the last period of occupation (Fig.32). This assumption is made because of the Hagen practice of clearing the area down to the basal clay before any building activity starts. Thus artefacts from Phase 1 (Fig.30) have been discarded towards the edges of the site limits of phase 2, then some (and in other cases all) artefacts from this second phase (and consequently mixed with those from Phase 1) were themselves displaced and discarded at the boundaries of the occupation of Phase 3.

Just as I questioned the owner's interpretation of site MOD (prior to its excavation), and I was wrong, for site MOO/WP a similar conflict occurred (but subsequent to its excavation): the owner's statement is that the woman and pig house had a western as well as an eastern room (as illustrated in Fig.30), but the archaeological evidence points strongly towards an absence of a western room. I interpret the house as having only one room, at its eastern end, with the remainder

of the house being the pig quarters.

4-6 KIAP HOUSES: SITE MOF

Only one Kiap house (Site MOF) has been archaeologically tested. Although this type of house has no equivalent in the prehistory of the Upper Wahgi, the data collected at MOF were aimed at evaluating and testing changes in housing pattern. What is the magnitude of this change, how is it reflected archaeologically, how much do the artefact and feature distributions differ from an earlier pattern? Whatever the answers are in this case, one must look at them only as a broad guideline for the understanding of real prehistoric changes which must have occurred in the past (but sites as yet to be found).

MOF is located in Kuning Meten (Map 24), on the lower slopes of Ep ridge. The surrounding vegetation is dominated by kunai grass, suggesting a dry surface. The house was visited in 1977 while occupied by Enga migrants, and by the time it was excavated in 1978, it had been pulled down for less than two months. Overall, its occupation lasted less than two years. It was an unfenced house located in pig land (such as example A, Fig.6). Surface evidences of human activity were numerous, due to its recent abandonment. The embankment and its associated platform dug into the slope were visible, as well as the exact dimensions of the platform. One could also see a faint, partially filled ditch surrounding the platform and disappearing down-slope. The platform was covered by a thin top soil layer, post-dating its occupation, up to 4cm thick and sealing the remaining features. From this surface evidence, one might predict a house site, most probably of the "oval" type.

Various crops grew out of the food refuse left on the platform: sweet potato, corn, peanut, tomato, tobacco, kumu kundr and kumu kimpukl. Altogether, it looked as though the site was reverting into

a mixed crop garden. Unfortunately these young plants were destroyed by our excavation: it certainly would have been interesting to monitor the growth process as well as the human and pig interferences on them.

The whole of the platform was excavated (77m^2 , Fig.39) and 1,064 artefacts and food refuse were recorded (Table 22). The length and width of MOF conform to the oval type, while the general shape differs only at each "corner", where MOF is squarer. The module principle for the internal arrangement is also standard, with an oddity for the location of the southern central post hole (in D8) in relation to the southern bedroom partition wall (along squares E7 to B7). While in most cases these two features are adjacent to each other, there are numerous instances where they are not (see Chapter 3). One must also note the absence of a mumu in the sitting room, while the hearth is square and stone-lined. Although the site was less than two months abandoned, the process of disappearance of minor features was already well-advanced: numerous post holes from the double row of perimeter wall and from the internal subdivisions were already missing from the archaeological record.

The archaeological reconstruction of such a site does not differ greatly from that when dealing with oval houses. But one must point out one piece of information which is totally missing: the lack of evidence of raised beds, which is known for MOF because of its ethnographic record. On Fig.40 where ethnographic is combined with archaeological evidence, it is translated into regularly spaced posts within each of the three bedrooms, but the archaeological interpretation points towards three bedrooms only, without raised beds. One might say that there is a change in pattern when considering the visual aspect of the house, but there is no change in terms of archaeological site formation. One can group the two house types into a single one (Tables 6 and 8), as long as there are no further new trends

in the Kiap houses. One must remember that in numerous ethnographic examples, the entire house floor is raised above the ground, thus preventing the use of features such as hearths and mumus.

The artefacts recorded (Table 22) are of no help for further understanding of the site. What has been found confirms the features evidence, it adds nothing. The few cooking stones found (only 21) supports the absence of a mumu on the platform. A second comment concerns the organic material found. There is an unusually large amount and range of food refuse, rather similar to Pita house described above (section 4-2, Fig.17). Because both these sites are extremely young, the food refuse is still well preserved and indicates its potential as an indicator of human diet on house sites. But this refuse, from the evidence gathered at other house sites excavated at Kuk, shows such a quick natural decay and destruction (in less than 5 years) that one must see the food refuse as not entering the archaeological record. Perhaps an early house site in the swamp will have some of its food refuse preserved, but certainly none on dryland other than those found charred in hearths or mumus.

4-7 MENSTRUATION HOUSES

Three menstruation houses were excavated. They are MOH/M, which is part of the complex site MOH discussed above, located on the lower slopes of Ep ridge, and sites MOM and MON located on the Kukrumdi plateau (Map 24). My interest in these sites was not only to look at the processes involved in their archaeological formation, but also to try to define them the best I could because of their specific cultural identity.

The excavation of this kind of ethnographic house raised some objections and problems among the workers. Being a forbidden place for men, they were rather reluctant to excavate them. Eventually the work

went ahead, and ended up with continuous dirty jokes about possible finds one could make there. None of the three sites had surface indication of previous housing activity.

4-7-1 SITE MOH/M

Parts of this site have been discussed above (MOH/R and MOH/WP). The remains of a menstruation house belonging to this settlement were difficult to find. Although three men at the site knew of its existence, it was necessary to open trenches at four locations (Fig.23) to finally discover what could be half of a menstruation house (Fig.41). For this result, 50m² was excavated.

What was found at MOH/M consists of a semi-circular shallow depression (less than 2cm into the basal clay) with a concentration of ash and charcoal in a central position. When compared with ethnographic menstruation houses, the poor evidence from MOH/M suggests that we have excavated the northern half of such a house: the circular depression must be the ditch surrounding the house platform and the concentration of ash is most probably the poor evidence left of a hearth. One must point out the total absence of post holes, including where there should have been the perimeter wall, while the central main post is probably located outside the excavated area. Not a single artefact was found at MOH/M.

4-7-2 SITE MOM

The menstruation house MOM is located about 20m behind the Men's round house MOP. While MOM was occupied by woman from settlement MOP, one cannot include the two sites together because of the definitions given in section 3-3: ethnographically they are part of the same site, archaeologically they are not (as defined in Chapter 3).

Some 20m² were excavated (Fig.42) and 50 artefacts were recorded

(Table 23). In terms of feature remains, MOM conforms to the ethnographic record. One has a light structure consisting of a double row of perimeter posts (well preserved in this case), a central post and a hearth located between the main post and the doorway. The odd feature found here consists of an elongated shallow depression (less than 5cm in the basal clay) in A3-A4. Because of its location, size and shape, it is unlikely to be a pig wallow. It is interpreted as being the spot where women slept, making this depression through long term use. The workers had no explanation for it.

As at MOH/M, the hearth of MOM is a very simple structure which did not contain a single artefact. Very few artefacts were found scattered inside the structure, consisting mostly of cooking stones (34 out of 50 artefacts). There is not (nor should there be) a mumu inside MOM. No artefacts were found outside the structure. One must point out the pig bone found because women are not allowed to eat meat in such places. It represents probably an "illegal" meal offered by another woman.

4-7-3 SITE MON

MON is within a fenced settlement which is still active (house N, Fig.8). It had been burned down and reverted to a kitchen garden. Thus its excavation was impeded by various standing casuarina and banana trees (Fig.43). Overall 21m² were excavated and 51 artefacts recorded (Table 23). In terms of artefactual remains, no specific clusters were found, there being an unstructured scatter of 51 items all over the platform. The burning down of the house is emphasised by the great quantity of charcoal found and by the charred tips of the few posts still in situ.

The features found in MON consist of a ditch surrounding the platform levelled down to the surface of the basal clay, some evidence of a

double row of perimeter posts (poorly preserved for such a recent site), one central main post, a hearth and a mumu. While all these features have been discussed at length, some comments must be made on the mumu because it should not have been there.

According to Hagen traditions, food is brought to the secluded woman, thus making cooking activities in such houses irrelevant. When the mumu in MON was found, it raised astonishment and anger amongst the workers. The woman involved in the occupation of the house were questioned, especially since the hearth itself was suspected to have also been a mumu (see section, Fig.43). The women knew nothing about all this and said that these features were dug by the men for planting bananas after the house has been burned down. And to prove this, they pointed to standing bananas around MON. The matter settled down quickly, but what is relevant here is that feature D2 is a mumu built at the same time as the house and belongs to it, and that the hearth was effectively redug to plant a banana tree in it. A rotten stump was found at its base; such recycling is commonly observed at Kuk.

4-8 MISCELLANEOUS SITES

A number of other sites have been excavated at Kuk for which there is little ethnographic data to complement the archaeological evidence. Some structures or processes could hardly be observed (activities surrounding burial grounds and toilets), others are of limited quantity (shelters, pig sties and cassowary cages) so that one does not really know if what we have is representative.

Because of this limited ethnography, I have deliberately limited the archaeological tests, putting a greater emphasis on the more elaborate and substantial structures described above. Therefore one must see the various sites described below as being a glimpse only of their range in form, size and formation process.

4-8-1 SHELTERS. SITE MOK

Shelters do not follow any specific design in the Upper Wahgi. They can be the shade of a casuarina tree, a light windbreak stuffed with kunai grass in the corner of a garden, or a complex structure containing well built hearths, mumus and even special cooking places in front of them. It seems that there are as many types as there are shelters.

There is even a rock shelter, Kentupai Kumanga, located in Kuning Kantupai, which contains archaeological deposit. At some stage in the early part of the Kuk recolonisation an artificial extension of it was made in the form of a timber framework filled with dirt to increase the living/working area. The extension collapsed some time ago and the only evidence of it today is a line of cordyline growing at its outer limits. Because it could be a major excavation, it was left undisturbed.

On the other hand, a platform said to have been a shelter has been excavated in Kuning Kaklong (Site MOK). It is located at the border between sparsely gardened grasslands at the base of Ep Ridge and forested zones uphill. The shelter seems to have been used for both gardening and kapul hunting activities. The only surface indication of a previous building was the levelled platform in the hill slope, well overgrown by a dense vegetation. Some 15m² were excavated (Fig.44) and 146 artefacts were recorded (Table 24).

The feature remains indicate a single row of perimeter posts forming a circular structure about 3.5m in diameter. The post holes were circular in plan and of a greater diameter than usual (around 6cm). Inside the structure there was a disturbed hearth with only one side stone aligned, a pile of cooking stones and a pig wallow. One could not stratigraphically make a temporal distinction between the wallow and the house. The notable missing feature here was the central main post. Outside the structure, a big log was held in place by wooden pegs and was located between the house and the scarp. It is said that such logs

prevent water penetrating the house floor, something which could have occurred at site MOD where there is not such evidence (neither is there a ditch to trap water) while it is in a similar topographic location.

The artefacts found are not informative (Table 24). They were scattered all over the platform except for the pile of 15 stones neatly left beside the hearth. If one compares MOK with the menstruation houses described above, one realises that a distinction between them is impossible. The only clue here for a possible functional distinction would be the lack of central post in MOK, thus stressing its informal nature. Only a macro-excavation would tell us the difference: there must be other houses near the menstruation houses while there should be none near MOK.

4-8-2 PIG STIES. SITE MNT

As with the shelters, pig sties can have any size and shape. One of them, Site MNT, has been test excavated (Fig.45). It is located in Kuning (Map 24) at a boundary between wetland and dryland. It was built around 1962 and abandoned only a few months later. The artefact remains which are durable are limited and consist of three small cooking stones, one glass fragment, four nails and one konga. But there was also a surprising high amount of organic remains (theoretically non-durable), not quantified but described below.

Numerous posts and post holes were found, including a row of very small ones (less than 1cm in diameter) in C2. The structure emerging from the excavation suggests a roughly made sty with two major posts located at the front (in A3 and D3) supporting a frame sloping down towards the back of the building. While most of the posts or post holes were found at the surface of the basal clay, it was evident during the excavation that the floor was not at that level, but higher, mid-way between it and the present surface. That floor level was covered with

well preserved grasses, bark and leaves, suggesting bedding for pigs to rest on as well as material collapsed from the roof. It was noticed that the floor was much harder, thus easy to follow while digging, probably caused by pig tramping. It seems that the sty could accommodate only two pigs at any one time.

What is unusual here is the presence of a third major post hole in C2, along the front wall of the sty, together with a simply constructed hearth found in front of the building (in C/B 3). One possible interpretation for the third main post is that it could have been added (or even be there from the beginning) as a supplementary support for the roof frame. If the interpretation of the sty offered above is correct, then the location of the hearth is a problem: it is an outdoor one. While the workers stated that such association is not unusual, I personally have never seen it in the New Guinea Highlands. What is sure is that it was very simple (not dug in nor stone aligned) and from the limited charcoal and ash evidence, it was little used.

4-8-3 CASSOWARY CAGES. SITES MOI AND MOJ

As predicted in the ethnographic section (3-9-9), "classic" cassowary cages built on stilts are hard to find archaeologically. One major reason for this is probably because they are built on top of the top soil surface and not at the surface of the basal clay like most of the buildings found in the Upper Wahgi. Thus few or no feature penetrate the clay layer, while the top soil is most probably disturbed by various activities such as farming and pig rooting (especially since the location of the cage is composted by cassowary's droppings and food refuse). A second major reason for the absence of remains could be the fact that most of the posts used are fairly small in thickness and are driven into the ground thus increasing the chances of disappearing after abandonment.

Two such cages on stilts were excavated in Kuning and Mapa, at locations pinpointed by the owners themselves. They are sites HK1 and HK2 (Table 25) which do not have an official PNG site number because no feature was found. Although we were excavating at the presumed right spot, and trenches were extended to 6m^2 for HK1 and 4m^2 for HK2, only some artefacts were found. The evidence suggesting a possible cassowary cage at HK1 (site abandoned around 1964) comes from three preserved posts lying there and looking as though they could belong to such a cage. For HK2 (excavated 5 years after abandonment), the evidence is even more scanty and consists of one cassowary feather. While we know ethnographically that there were cassowary cages at HK1 and HK2, one must conclude that archaeologically one certainly cannot be so sure: artefacts found there could come from all sorts of activities, including those associated with the keeping of cassowaries.

Two other cages were excavated, sites MOI and MOJ (Fig.46). While we knew that MOI was a dug-in cage from numerous surface indications, it was expected (from the owner's information) that MOJ was a cage on stilts. Its excavation revealed that it was also a dug-in cage. Both sites are located in Kenta, MOI being abandoned for less than one year and MOJ some 4 years earlier. Some 1.5m^2 were excavated at each of the two sites.

MOI had a neat dug-in cage base, with a depth of 45cm below the surface, including 16cm into the basal clay. It was a neat rectangular feature surrounded by 8 posts well aligned (Fig.46) which remained in the top soil layer. A curved sheet of aluminium was also found, half-buried across the up slope side of the pit. It most probably was there to divert run-off water. The wooden frame covering the pit and used as cage floor was also found, still in situ, its sections tied together with bush rope (frame shown in Fig.46). Posts used for the building of this cage were larger than usual. The artefacts found were all in the pit

(Table 25) suggesting its recycling as a dump. When visited the following day, the pit was full of water from the overnight storm, suggesting a lack of hygiene and possibly stressing its non-traditionality.

The excavation of MOJ revealed features similar to those found at MOI (Fig.46): a central pit surrounded by a series of post holes, some of them arranged in pairs. The size of posts and pig is smaller than for MOI. The depth of the post holes was also within the top soil layer. The unexpected find here was that the basal clay was found only along one side of the pit, while the remaining sides as well as its base were still in the rich black soil (cross-section in Fig.46). It was suspected that the cage was built into an abandoned and filled mumu, a circumstance acknowledged by the owner when questioned (thus retracting his previous statement that the cage was on stilts).

Artefacts recorded at MOJ (Table 25) suggest as for MOI that the pit was used as a dump after abandonment: not only 13 fragments of glass and unbroken bottles were found in it, but also 8 posts suspected to belong to the cage itself. Thus MOJ could be classified as a complex recycling site: from a mumu to a cassowary cage (perhaps with an interphase of using the mumu as a dump) to a dump before final abandonment.

4-8-4 TOILETS. SITE MOH/T

Only one toilet has been archaeologically tested. It belongs to the complex site MOH already discussed above (Figs.23 and 41). Surface indications of human activity consisted in the presence of a raised platform, some unusually large stones and perhaps a single tree growing there, in the middle of a kunai grassland area. Some 6m² were excavated at MOH/T.

In terms of features found (Fig.41), the site consisted of a circular pit about 1.5m deep from an artificial surface, being the top of a platform raised about 50cm above ground level (thus the absolute depth of

the pit is about 1m). The platform was made of hard clay, overlying a buried soil (cross-section Fig.41). It seems that the clay originated from the spoil extracted while digging the central pit. The standing tree was growing from the top of the raised platform, not out of the pit. This could only be a coincidence, as it could come from the original screen which was most probably built there, of which is no surviving evidence (not even one post hole was found at the site). There was a very large Ep stone slab across half of the pit opening, while a second one, broken in two, was partially collapsed in the other half of the pit (these stones are not represented in Fig.41). Each of these stones were so large and heavy that they could not be carried by a single man. Their presence there indicates a planned undertaking and a selection of suitable rocks somewhere in Ep ridge.

Although it was suspected that the pit itself contained numerous human coprolites, not a single one could be recognised or identified. It seems that they have decomposed and mixed with the soil and clay content of the pit. Similarly, the cesspit should have been an ideal dumping ground and not a single artefact was found in it. It was entirely sterile. In what seems to have been the entrance of the toilet was found a well formed bean-shaped pig wallow, quite recent because still functional.

Although only one toilet has been excavated, it seems that in terms of feature remains MOH/T could well be representative. In terms of artefactual remains, one does not know if the total absence of them here is a reflection of some sort of taboo or it simply means that our case is not representative. What is certainly odd is the presence of the two large and heavy stones (instead of a wooden frame) used to cover the pit. Again this is a reflection not only of the availability of the material on nearby Ep ridge, but also an unusual functional behaviour on the part of the owner of the whole MOH site (which includes the

cobble stone pathway for pigs at MOH/WP and the large slab at the doorway of MOH/R).

4-8-5 BURIAL SITES

The Kawelka have three cemeteries at Kuk, and are aware of at least five others within their territory which existed prior to the recolonisation. Consequently they can be labelled "prehistoric". They certainly look much older than those in use now, judging only by a forest-type vegetation in them as opposed to a rather juvenile bush surrounding the current ones. This observation was also noticed by Vicedom who states without exaggeration that:

"The burial grounds, however, always remain where they are. Only they, therefore, still have primary forest with gigantic trees." (Vicedom and Tischner 1948(197?), vol.1:239).

One of these prehistoric cemeteries was said to be located near site MNH (Chapter 6, section 6-6) where we did find human remains. Thus this burial ground at least extends beyond the surface (vegetation) evidence. A review of the literature concerned (Gorecki 1979c) reveals the occasional association of mummies and graves as found in MNH (e.g. Macilwain 1957, Luzbetak 1956) as well as the numerous and complex activities surrounding graves and human bones (e.g. Aufenanger 1961). This is why I believe now that there was nothing odd with our finds at MNH where numerous mummies and cooking stones were found associated with at least two graves. In the field I was disturbed by this association, suspecting two distinct uses of the site. I am now satisfied that the finds made at MNH reflect a single event, namely the ceremonial cooking of pigs in honour of the dead in a prehistoric burial ground.

At MNH, in one grave were found two human long bones, probably tibiae of an adult. They were in a shallow pit and the body was probably in a lying position. The remains of another adult individual were found in a second grave and consisted of an adult skull, its back facing

the sky with the mandible missing. It was in a deep pit, most probably in a crouching position. For further details concerning the site, see section 2-6 in mumu chapter.

In three other instances, I could make limited observations on burial practices at Kuk. One of these is also prehistoric and was made during the testing of a small rock shelter in Prkl Rangorong. I expected the shelter to have no deposit at all and no depth. The contrary happened when at a depth of about 15cm I identified a thin layer of volcanic ash known as "Tibito Tephra" and dated to 300 years B.P. (Blong 1982). Just under it, I struck what I thought was a rock and when removed, there were human bones. They consisted of part of an astragalus, one distal end of a tibia, and one metatarsal from a foot. The three bones were from a left leg of an adult (R.V.S. Wright, pers. comm.). While the bones from MNH were left in situ, those from the above shelter were collected; their catalogue number is 77108. I backfilled the hand test hole and left. The base of the deposit was not reached. I suspect it was a grave in a niche which through time was buried by volcanic ash and soil eroding from above. Here again it is not unusual to find such exposed corpses; graves in niches are common in the Highlands when cliffs are found (Gorecki 1979c).

Another observation on burial practices at Kuk was made one morning when walking on a track familiar to me (in Kukrumdi). I noticed within a coffee grove some surface disturbance which was not there the day before. After making inquiries, I was told that it was the grave of a baby who died overnight only hours after birth. For reasons unknown to me (perhaps the young age) the body was not buried in a cemetery but alone among coffee trees in a discreet location. I was told that the grave was very shallow, less than 25cm deep. I could see that a small ornamental shrub had been planted in the centre of the feature.

The last evidence I have collected is rather different, but in a

situation similar to the case above. I noticed fresh disturbance along a track in Kenta and by observing it I realised that it was square and aligned with flowers. Beside it, but much older and also with ornamental plants around them, there were four other similar features. I was then told that it was there that dogs were buried, peacefully under the shade of a casuarina tree. It seems to me that there should be very little difference, if any at all, between these features and the one of the baby above.

While I have little field data on burial practices, they nevertheless stress the great range of customs. The potential of finding human and animal (at least for dogs) graves anywhere in the landscape is great, as detailed elsewhere (Gorecki 1979b). While I cannot comment on archaeological processes involved in this kind of site, simply to read Aufenanger (1961) or Luzbetak (1956) will stress the complexity of the problem and the urgent need for a systematic collection of ethnographic data.

4-9 CONCLUSION ON HOUSES

Rather than doing a review of the evidence and interpretation extracted from the various sites excavated, and their analogies with ethnographic counterparts, I shall try to draw some general conclusions on house sites excavated at Kuk and what they tell us about their formation processes. For this I divide the discussion into three distinct phases, all of them related to a time depth: sites less than 5 years old, sites between 5 and 15 years old, and sites more than 15 years old. I shall try to incorporate the human, animal and environmental factors involved, as well as two separate sets of evidence (as described throughout the chapter), namely feature remains and artefact remains. It is only then that I shall introduce the analogies with ethnographic data. Following this, I shall make some comments on the limited published material on house sites in the New Guinea Highlands to finally raise some general

questions concerning problems the excavator would like to answer.

The first phase, arbitrarily fixed to sites less than five years old, deals with what might be called "short-term" processes. It illustrates events occurring at a site just after its abandonment and during the partial or total sealing of surface evidences related to those housing activities. One must take into account two distinct processes related to the site formation. The first one is the disturbance process, closely linked with the second one, the disappearance process. Sometimes disturbance leads to disappearance, while both of them are a critical part of the site formation: what is the condition of the site five years after abandonment (Table 26).

It is obvious that during its occupation and particularly on the day the site is abandoned, all the features, artefacts and organic remains are archaeologically best represented and the interpretation of the site should not raise many problems (e.g. Pita House). Dramatic damage in terms of disturbance and disappearance processes occurs at the site during its first few days or weeks of formation, sometimes lasting a few weeks. The site is a favourite playground for children who not only displace numerous items but also remove some or bring new ones to the site. Children's activities at such an early period of site formation are greatly underestimated (G. and N. Hammond 1981), difficult to assess and sometimes of great archaeological consequences (e.g. removal of some critical evidence, the making of new structures or features, the deposition of foreign artefacts).

Together with these children's activities, there is a substantial plundering by children and adults, particularly when the house has been burned down. It involves disturbance and disappearance processes. Finally, the last major disturbance occurring so early in the site formation may come from new man-made earthworks: a new garden or house is made at the site, thus affecting features, structures and artefacts.

It is also during these early days of site formation that recycling activity will be at its maximum: whatever is seen as suitable will disappear from the site and transported elsewhere.

Following this great rush of activities, the next few months are also quite active but comparatively quieter: children have moved to another playground, there is not much left to plunder, the growing grass starts to hide things left. It is time now for pigs to move in, tethered or not. They are rooting all day long, disturbing the site and chewing away much organic evidence, particularly food refuse. They might make new features such as wallows. The environment is also involved in the disturbance and disappearance processes through erosion and/or sealing up of the site. If the site is located on flat dryland, it seems that the natural elements (sun, rain, wind) have little erosional effect, but have a great impact in the rate of decay of organic remains; things rot away quickly. The site being exposed for a longer period before being buried, natural, human and animal factors have a greater impact on the site. On the contrary, when the site is located on a hill slope, the tendency is for there to be greater erosional activity, particularly in terms of platform levelling, but this is compensated for by a rapid sealing up of the site by soil eroding from up hill. It results in better preservation of organic material (even better in a bushy environment than grassland). The menace of new man-made earthworks is still present, and will always be present, with the potential of greatly affecting the site.

It seems that there is a correlation between the nature of the site and the amount of evidence left behind: human activities leave far more archaeological evidence than activities related to animal domestication (pigs and cassowaries). Some 5 years after abandonment, disturbance and disappearance processes have reached a peak, at a rate that will never be equalled later, whatever the time span is. The majority of the processes

have occurred. Most of the driven-in features (e.g. post holes) have disappeared forever for the reasons given above, but also possibly through a natural process of decompression of the basal clay sealing the feature. There is a substantial decay of organic remains, many artefacts have been displaced or are missing through plundering and recycling activities.

What is left at the house site after a short-term formation process (5 years) consists mostly of dug-in features, some artefacts (especially inorganic) and little food refuse. One must stress that the above comments on short-term formation processes are a general trend only, noted on Kuk's drylands, as the direct comparisons between the data derived from sites relevant to this phase will testify. These sites are Pita House, MOQ, MOF, MOK, MOI, MOJ and HK2.

It is evident that all the processes described above do not suddenly terminate five years after abandonment; there is a continuous site formation process until the site is archaeologically excavated, probably even if a site is totally worked over by a garden, a new house, etc. It is only then that the site is considered as being definitely destroyed, that all formation processes end. The second phase, "medium-term" process, which is of interest here concerns the processes occurring between a period of 5 to 15 years following abandonment of the site (Table 26). It concerns sites MOP, MOQ, MOO, MOE, MOM, MON and HK1.

While the peak of formation process has occurred within the first five years (and even the bulk of them within the first year) following abandonment of a site, events related to the disturbance and disappearance processes are still going on, but at a slower pace.

In terms of disturbance, the new major parameter operating now is vegetation regrowth. It seems that where trees are allowed to grow back, where a bushy vegetation starts, there is some damage done to existing features (particularly those dug-in): roots have a strong tendency to

grow in them, the black soil infilling post holes, mums, hearths, etc. being better than the surrounding basal clay. But this damage is difficult to evaluate, and I am not in a position to do this. I can only state that it is in these features that the root systems of trees are found and that there is some damage done. On the contrary, when the area at the site is controlled grassland, it seems that the vegetation has little effect (or less effect) on the site.

On hill slopes, one can still have some erosion occurring at the site, particularly if the site is on an artificially built platform. Sometimes the front part of it, consisting of fill dug from the back, may collapse if the retention wall at the front is weak or if it is not made of a regenerating timber such as pitpit or cordyline. Finally, sites more than five years old are sometimes selected for new gardens, particularly for mixed crops. As in the first phase, these man-made earthworks can destroy or/and add features to the site.

Some of these disturbance processes lead directly to disappearance processes where during this second phase there is an almost total loss of driven-in features and some small dug-in features. If not all the organic remains have disappeared during the first phase, they will do so during the second one. In terms of artefactual remains, it seems that there is a stabilisation and fixation of inorganic ones; I have not found substantial changes in pattern and number between phases 1 and 2.

In summary, one can say that at the end of the second phase, the formation processes lead to sites with a reduction of features (particularly those driven-in), with a stabilisation in inorganic artefacts and an almost complete disappearance of organic remains.

One could say that the second phase is the last one involved in house formation processes. The picture from a site 15 years old is similar to the one from a site 400 years old. Nevertheless, I have added a third phase, loosely called "long-term" processes, for all sites older

than 15 years. It adds little to the second phase (Table 26). In this research, it concerns sites MOG, MOH, MOQ, MOL, MOD, and MNT.

What happens at such sites, whatever their antiquity, is not affected by natural phenomena: droughts, floods, secondary regrowth, etc. will most probably have little impact, if any at all, on the site formation process; all the processes related to these events have taken place during the first 15 years of site formation. On the contrary, I must again stress the importance of new man-made earthworks and the part they play. Gardening activities (ditching, planting, burning, etc.) and housing activities (top soil removal, important displacement of artefacts, making of new features such as post holes, hearths and mums, etc.) have a direct and substantial impact on the site formation process (e.g. sites MOO and MOQ). In a sense, if such new activity involves important manipulation of the basal clay, one could say that it is ending the formation of the site of one specific period, but at the same time it is starting the formation processes of a new period of the same site. This is particularly important for the Kuk (and in a wider context for the Upper Wahgi) area where on drylands such obliteration happens all the time and where one has few or no stratigraphic check points.

Throughout the archaeological tests, it has been noticed that beside cooking stones, not many other inorganic traditional artefacts were found. It seems that the modern items now found in all dwellings are replacing traditional ones, few of them inorganic, or are added to a repertoire which does not contain many durable artefacts. The material culture of traditional Hagen societies can be classified as basically organic (Vicedom and Tischner 1948 vol.1, Golson 1977b), relying on a sort of "wooden and woven" technology where stone is little used: kongas, mortars and pestles are rare and have only religious significance (mortars and pestles being recycled as such, their original function and antiquity being unknown), stone tools are limited in number (White 1972, Watson

and Cole 1977), including stone axes which are of high quality but again limited in number (per head, per site, etc.). Only cooking stones, uninteresting judging by the lack of attention to them in the literature available on the Highlands, are found in great quantity, not only on house sites but on almost all sites.

It is in these cooking stones that I think we should concentrate our archaeological attention and research, as well as collecting ethnographic data for possible use as analogies. In the past, it is my impression that they were an entire part of a trading network for the majority of Hagen tribes (although there is not a single mention of this in the literature), due to the strong tribal borders cutting the supply for some groups such as those living at Kuk, due to their scarcity and specific natural locations in the Wahgi landscape, and finally due to their high demand and consumption in numerous daily and ceremonial activities. Because these unattractive river pebbles carry much information, and because they are found clustering in specific locations in and around house sites (and elsewhere as discussed in the mumu chapter), it seems to me that they are the prime artefact to be analysed archaeologically and petrologically. Any other artefact found at a house site, organic or not, should be seen as unusual and a bonus for site interpretation.

In terms of feature remains, one can realise the strong correlations existing between the few parameters stressed in the ethnographic section of this chapter and the archaeological features emerging from the tests above: they are identical. Consequently it is my belief that one should try at all costs to find these features, because they are there, permanently left in the archaeological record (unless the site has been destroyed by subsequent man-made earthworks as explained above), and these few critical features should be enough to offer a plausible interpretation. These features are based on a modular principle (fig. 20) and consist of the location, number and distance between central posts,

hearths and internal mums. Any additional feature found (and the most frequent ones are the outline of the platform or the surrounding ditch) which conforms to an ethnographic analogy described in this chapter should also be tentatively added for the testing of site interpretation. All this (which is in reality little), together with information given in Tables 10 to 12 concerning internal arrangements should give not only a formal interpretation but also a functional one.

The question raised by this, assuming that the theory is correct, concerns its applicability in antiquity. It is fair to say that one can apply ethnographic analogies to sites contemporary to this ethnography. But can we apply it for prehistoric sites, and how far back can the model proposed above still be operating? To answer this, I turn my attention to four areas where prehistoric house sites have been excavated.

Two houses have been partially excavated in the Arona valley, east of Kainantu in the Eastern Highlands, by Swadling (1973). Sites NIE and NMH are interpreted as having been Men's Round houses, this from surface indications, feature evidence at NIE, ethnographic analogies and oral history. Both these sites, less than a century old, have left poor features behind.

In an area south of Kainantu, also in the eastern Highlands, Cole has excavated at least seven house sites (Watson and Cole 1977). Some of them could have an antiquity as great as 18,000 years B.P. Watson had to analyse Cole's field notes, a difficult task, but managed to identify different types of house and some changes through time.

Five raised platforms have been excavated in the Kuk swamp by Lampert in 1972 (permission to consult field notes and original plans was kindly received from Lampert and Golson). Lampert's interpretation of the platform is that all five houses were "contemporaneous and of the same social complex ...laid out in the pattern of a formal village", less than 200 years old (Lampert 1973).

The last site described here is Hed Mound (Harris 1977) which is a natural mound raising out of the Kuk swamp, just outside the northern boundary of Kukrumdi. A very large area ($1,100\text{m}^2$!) was excavated in less than two months (judging by the content of the report, it seems that speed was more important than the understanding of what was going on) and numerous dug-in features were found. Harris's interpretation is that there were four distinct periods of housing activity as testified by the remains of five house platforms, occupied between 270 BP and 220 BP (assumed dates).

What are the above four reports saying in the light of my Kuk data? Watson stresses her problems in interpreting field notes written by someone else (Cole) and stresses the lack of ethnographic data for her understanding of the features found. Swadling has good ethnographic data but could not use them because she had limited archaeological evidence. Lampert, using week-end ethnographic observations, makes important statements concerning his sites. Finally, Harris has obviously not understood his site and makes free use of ethnographic analogies based on almost no such observations. While I understand Swadling's and Watson's reserved approaches, I think Lampert (who is nevertheless cautious) but particularly Harris should have been more careful in the application of their analogies. A Sunday stroll (rare for Harris) in the native gardens and settlements making furtive observations is not enough to interpret archaeological sites.

The nineteen house platforms reported above have common feature remains, similar to those found by myself. This suggests that the site formation processes observed at Kuk could be similar for the whole of the New Guinea Highlands. If this is the case, the "module" principle presented here may work for the Lampert and Harris sites, though not necessarily for Swadling and Cole sites. A module principle is probably culturally, geographically and spatially delimited, thus it could be a

different one outside the Upper Wahgi. It is known ethnographically that house structure (Steensberg 1980, Marshall 1979), shape and size are not the same in Goroka, Kundiawa, Mt Hagen, Wabag, Tari, etc. Settlement patterns are also different, the most evident in mind being that there are villages in Chimbu but scattered homesteads westward. Thus I am not prepared to comment more on the Swadling and Watson/Cole sites (no detailed study on ethnographic houses available), but some comments can be made on Lampert and Harris sites.

The data I have concerning Lampert's five platforms are limited. They consist of a plan indicating the size, shape, and location of the platforms, a detailed plan of features found all over platform B and partially over platform C, and some field notes. From what I understand about site formation processes at Kuk, it seems to me that the site is a complex one, where successive man-made earthworks have obliterated and complicated many dug-in features. Nevertheless, I can state that at the site (at least for platforms B and C) there was an original gardening activity, itself composed of various phases, followed by a distinct cooking activity and only then followed by a succession of house-building phases. It is also evident that on platform B there was not one house built but at least three, of at least two types. Similarly, on platform C there was not one but at least two houses of perhaps the same type.

Contrary to Lampert's statement that four platforms belong to women and pig houses and the remaining one to a men's house, I do not find convincing evidence for this. Only the suspected length of some houses (especially the earlier on platform B) suggests that one might be dealing with woman and pig houses, but these would be successive rather than occupied at the same time, one shifting slightly after another. More limited excavation is necessary at precise locations to give a confident functional interpretation. While I agree with Lampert that the houses are of the same social complex, I reject the notion of a formal village.

It is more likely that the site was occupied by only one family, with no difference at all from the present-day settlement pattern, for a surprisingly long period of time (probably much longer than 15 years). This points towards a stabilisation and equilibrium of garden and pig lands owned by the occupants of the Lampert site.

More analysis of Lampert's field notes is necessary, complemented with small excavations to better understand the site. In conclusion, it seems that there is no change in site formation processes, in settlement pattern and in some house types for the last 200 years. A similar conclusion is reached for Hed Mound discussed below.

Hed Mound is a confusing report, where the interpretation of hard evidence is mixed with assumptions, guessing and dubious ethnographic analogies. What is evident is that the site is a complex one, similar to Lampert's, where numerous successive man-made structures have been put down at the surface of the basal clay. Site formation processes here again are the same as those described for present-day Kuk. I cannot go into a detailed analysis of Hed Mound; it needs a full-scale reinterpretation (and possibly a full reexcavation) with field notes in hand. What emerges from it and is worthy of consideration is that there probably are at least four more houses on Hed Mound (their features wrongly attributed to gardening activities), while numerous central post holes were not excavated (and they must be there). One does not have a spatial distribution of cooking stones, which could be very informative for such a complex site, nor a description of the infill of mumus found. Beside the house platforms, there are clusters of hearths and mumus. Special attention to these features should be the starting point for rectifying the errors made at Hed Mound. Its reinterpretation probably will show that there have been more occupation phases than recognised, which could be distinguished one from another.

To conclude this brief analysis of some prehistoric house sites, it

must be stressed that site formation processes for Lampert's site (swampland) and Hed Mound (dryland) seem to be similar to the present processes. Settlement pattern and house types seem to have remained the same for the last 200 years. The Lampert and Harris sites also point towards an intensive use of house sites at specific locations which has no equivalent in the present context. It suggests strongly that the economy, social and political life at the time of their occupation was stable, permanent and intensive. The modern Kuk situation has not yet reached this phase, where one is observing rearrangements of the centre of activities creating an instability in site locations. This is caused by the regular influx of immigrants. I suspect that soon things will settle down at Kuk, and one could witness a similar intensive use of house sites because of their location regarding economic (gardens/pigs), social and political activities which are themselves stabilised.

The questions to be solved at a house site are directly related to the problems the archaeologist wants to solve. Each one may necessitate a different approach, sometimes with limited excavation rather than large scale enterprise. I think these questions can basically be narrowed down to three:

- i): questions on house types and their changes through time require only the excavation of the house platform;
- ii): questions on material culture require the excavation of the house platform and the site limits (dumping ground) as defined in section 1-5;
- iii): questions on spatial arrangement, behaviour and settlement patterns require large-scale excavation, where the entire site as defined in sections 1-3 to 1-5 should be analysed.

As far as the application of the house site formation processes goes, the model proposed above is applicable for the last two hundred years in the Wahgi. Earlier sites have yet to be found, so the model awaits further testing. The lack of evidence of earlier house sites at Kuk could

well be the result of the excavation strategy applied so far: little work has been done on drylands while the focus is on the swamp itself. I do believe that earlier houses will be found at Kuk, some of them probably earlier than the beginning of agriculture in the swamp. These sites will be found in the swamp itself just by sheer luck and more in the surrounding drylands as a result of a methodic and specific sampling strategy.

CHAPTER 5

COOKING PLACES - ETHNOGRAPHY5-1 Introduction

Ethnographic observations on cooking practices and dietary habits in the Upper Wahgi show that the traditional patterns as described in the early literature on the New Guinea Highlands have been greatly altered by European introduced items. Nowadays, tinned foodstuffs such as fish and meat, rice, and other trade store purchased items are consumed almost daily, cooked in steel saucepans. Coastal products are also consumed such as betel nut and coconut. The consumption of these, together with that of beer, increases or decreases according to the price of coffee, the major cash crop of Hageners.

Traditional cooking in earth ovens, or mumus, is probably now much less practiced than formerly, daily sweet potatoes being cooked in the ashes of the fire and daily greens being boiled in pots often hanging above the same fire. Nevertheless, earth ovens and traditional cooking places are still found everywhere in the landscape, inside or outside settlement areas. Bearing in mind the above technological and diet changes, this chapter will discuss only the traditional way of cooking and the activities surrounding it.

Apart from daily food, traditional cooking is particularly practiced for ceremonial occasions, for big harvests of crops such as corn and pat (Psophocarpus tetragonolobus) and for meat preparation; not only pig meat, but nowadays also beef and chicken.

5-2 Cooking Places

Observations of the Kuk community show that there are four major places where food is cooked in earth ovens:

- 1) Daily food can be cooked inside a house;

- 2) Food can be prepared just outside houses on the already cleared ground;
- 3) Food is also cooked at specific cooking places where are spaces cleared mainly for that purpose but also used for social gathering;
- 4) Ceremonial grounds are used as cooking places only on specific occasions such as Moka feasts, paybacks and for the death of big men.

5-2-1 Cooking Inside Houses

Generally speaking, each house where a woman lives has a single internal earth oven, always located in the sitting room. Men's houses can also have one, in the same location, but exceptions are found in both cases: some houses do not have an internal earth oven.

When present, the mumu is generally located along the back wall of the sitting room, opposite the doorway, preferably in a corner. Cooking stones, when unused, are left in it or in a pile beside it, along the wall. Cooking stones are heated in the hearth. In consequence there is no surface indication (charcoal, burned timber) of that activity other than the oven and the stones. It is often hard to find the location of the mumu even in an inhabited house, as it is being hidden by the carpet of leaves and dry fragments of chewed sugarcane.

The foods cooked in mumu are greens, small harvests of pat, yams, sweet potatoes, edible pitpit (Saccharum edule), etc. The fact that vegetables are cooked inside a house has no particular social significance. It could be because of bad weather (rain, cold) or because of the unexpected visit of friends or relatives. But it is said that when meat is cooked inside the house, it is generally a sign that the owner wants to eat it with his family exclusively and that he does not want to share the meat with visitors attracted by the smell, as it is often the case when meat is cooked outside.

5-2-2 Cooking Outside Houses

Earth ovens located near the doorway of a house, but outside it,

are generally complementary to the one located inside a house. It is a substitute when one is not present in a house. In the majority of cases recorded, there was only one mumu just outside a house (27 cases recorded: 18 with a single mumu; 7 with 2 mumus; 2 cases with 3).

Their specific location is varied, but as a general rule they are close to the house, in a handy position within the settlement enclosure. No special ground is cleared for them. While the mumu located inside a house is limited in size (small room, small fireplace to heat the stones), the outside mumu has no size limitation and for that reason it is often bigger. According to the quantity of food to be cooked, one oven will be preferred to another. But no "ceremonial mumu" is found near a house, except in specific cooking places which will be discussed in the next section.

Associated with external oven are cooking stones and a concentration of charcoal resulting from the fireplace used to heat the stones. Cooking stones are found on a pile near the oven or inside it; some others might still be in the concentration of charcoal (unused when heated) or are scattered in the settlement (human disturbance, especially children). Within the settlement enclosure or just outside it, one can find a pile of discarded cooking stones, so broken by successive heatings that they have become too small to be reused for cooking.

One can also find items such as split wood used as tongs to carry the hot stones, burned timber, bamboo knives, and remains of wrapping such as steamed banana leaves. Nowadays, modern items of consumption such as beer bottles and tins are also associated with cooking activities.

Of the food, very little remains: perhaps a forgotten sweet potato and a few greens. Ethnographic observations show also that pig bones are still carefully collected and taken away from the site, except for the jaws which are hung on a tree nearby. Some food uneaten is given to dogs and pigs or it is thrown in the kitchen garden and spread out as compost.

5-2-3 Special Places

Places specially designed for cooking also exist, whether or not an owner has mumus inside or near his house. Only 15 such places exist in Kuk. They average 14m x 8m (range: length 9.6m-19.7m; width 5.5m-9.8m).

They are also used for social gatherings. The creation of these areas depends on the owner's influence amongst his kin. I interpret these places as a transitional step towards the building of a true sing-sing ground. If the cooking place attracts numerous people and the owner's prestige is growing, it could well be extended and rebuilt in such a way that finally a new singsing ground emerges; if the cooking place has little importance for the people living around, it will remain as it is, only occasionally used. Looking in other places in the Upper Wahgi, I have seen but unfortunately not measured other places where it would be difficult to say whether they were small singsing grounds or big cooking places.

Because of their social importance, these cooking places are built in front of men's houses. Specific vegetation such as bamboo, casuarina and cordyline is planted around their environs. The ground is always levelled, often to the bare clay, and in such places as on Ep ridge, there is a lot of earth moving to build these platforms out of the hill slope. When the owner decides to shift his settlement elsewhere, these places often continue to be used unless a cooking place is erected in the new settlement. Thus such places can now be found far away from houses, in gardens or in pig land, with house remains being hidden by the growing vegetation.

Special cooking places always have numerous ovens of different sizes, including a large ceremonial one. Generally speaking, these are located on the perimeter of the place, the centre of it being left for fires used to heat the cooking stones. Large mumus are for domestic use as described in sections 5-2-1 and 5-2-2, but also for the cooking

of individual or communal big harvests of pat, for preparation of big meals for ceremonial feasts, or when near an occupied men's house, for social gathering. Nowadays, when favourably located, they also are market places. Because of their open space, they are also used for storing building material and for pitpit wall plaiting.

Material remains left in a cooking place are numerous and varied, all directly derived from the activities described above. Charcoal is concentrated where stones are heated (central position) and cooking stones follow the same pattern as the one described previously.

5-2-4 Cooking in Singsing Grounds

As can be guessed, cooking in singsing grounds is reserved for ceremonies performed there. Although some people will cook their feast meals in their own settlements, the majority of people grouped by kin or friendship ties own a ceremonial mumu in the singsing ground.

These ovens which are very big, are located on the perimeter of the ground, the stone heating fireplaces are beside them, squeezed between two mumus. This pattern allows people to walk freely up and down the centre of the ground.

Many big stones are necessary for these ovens and none of them are found scattered, littering the ground. When there are preparations for a feast, each owner of a mumu relocates it by pushing a stick in the ground till it hits the stones lying in them. When the feast has ended, some stones, usually the smaller broken ones, are taken home and recycled in domestic use while all the others are reburied in the oven which is filled up with earth to allow a quick regrowth of grass. In such a way, singsing grounds look neat and are kept clean. The stealing of cooking stones is also avoided.

5-3 Typology of Mumus

Classifications of earth ovens from the New Guinea highlands

available in the literature are from the Eastern Highlands (Watson and Cole 1977:114-5) and from the Upper Wahgi (Harris 1977:app.II). These data are from archaeological sites. The classifications differ so much from each other that a comparative analysis of them or a comparison with ethnographic earth ovens observed at Kuk is impossible. For these reasons, I propose the following classification (tables 27 and 28) which is not ideal, but in which the above archaeological and ethnographic data can be fitted.

The initial division is functional: domestic pits (table 27) and ceremonial pits (table 28). Another arbitrary differentiation made is that oval and circular pits having a top dimension over 110cm are classified as ceremonial, while below that mark they are considered as domestic ovens. Each group is then subdivided into formal modes, taking into account: i) horizontal plan (surface feature) where an arbitrary division is made between circular pits (less than 10% difference between top length and top width), oval pits (difference between 10% and 50%), and elliptical pits (length 50% more than width); ii) vertical plan, where another arbitrary subdivision is made: shallow pits (depth less than 35cm) and deep pits (over 35cm). In this vertical plan, the modes "vertical walls" (bottom of pit as wide as top of pit) and "sloping walls" (top wider than bottom) are also represented.

In this classification, the mode "depth" is omitted for ceremonial ovens because ceremonial ovens are always deep. In "domestic" ovens, the formal "elliptical" mode is not represented; elliptical mums are always ceremonial. Finally, contrary to Harris' conclusion about split-level pits (1977:app.II,i), I consider them, on ethnographic and operational grounds, as being two pits and not one.

Not all mums found at Kuk were recorded (because of insufficient time). For this reason, a detailed statistical analysis has not been made, but my impressions are that all types of domestic mums are fairly

represented. I also think that elliptical mumus could well form over 50% of the total population of ceremonial earth ovens though this is not demonstrated in my counts. The last horizontal line of tables 27 and 28 gives the codification of each type of mumu which will be used from now.

5-4 Dimension of Mumus

Earth ovens found during the excavation of cooking places (except for sites MNH and MOL) are included in this section because they all are post-European contact sites. Thirty seven earth ovens located inside houses and seventy four located outside houses were recorded in Kuk. Their typological distribution is presented in Tables 29 and 30. From it, two patterns clearly emerge: 101 out of 111 recorded (91%) have sloping walls, and 88 of the 93 domestic pits (94.6%) are circular. One can also notice that types DCDS and DCSS are predominant among domestic mumus.

Table 29 gives the range and the average of the dimensions of mumus found inside houses while Table 30 gives similar figures for external ovens. When looking at Table 29, one can notice that all four types represented are about the same size. The mode "sloping walls" allows to have greater variability in dimensions than the mode "vertical walls" which give a more compact and standard pit, whatever its depth is. The fact that internal ovens are rather small appears to be due to the size limit imposed by the room itself. Internal ovens are always built in sitting rooms, which, in the Upper Wahgi, are small.

The two types DCDV and DCDS show that a clear formal distinction can be made between internal and external pits: the latter are much bigger and deeper. The two other types, DCSV and DCSS, show on the contrary very little difference between them, the dimensions of internal pits overlapping in many cases those of external pits. One can also

notice that the mode "sloping" is much more accentuated in external pits than in the others, whatever the depth. This can be explained by the fact that internal mums being limited in size and depth, owners tend to increase the volume of ovens by building them with rather straight walls. This seems to be irrelevant for external mums, where cylindrical shapes sharply contrasted with conical shapes. The reasons given for these technological variations are rather simple: cylindrical pits are more difficult to build; the spades make vertical contact with the hard clay in digging those pits while in making conical pits spades slice into the clay at an angle. The need to increase the cubic capacity of internal small ovens is quite a good explanation for their rather cylindrical shape, even if only three such internal pits recorded have straight walls.

One can notice that the single type of example of DOSV recorded is in fact elliptical and not oval. But it is an exception, a very odd pit, which was made by a child. Its general shape is not suitable as an earth oven (the heat cannot be preserved for long enough because of its large surface and small depth) and may, in fact, be confused by the archaeologist with a hearth (but unused as such because no ash was found in it). Three of the DCSS ovens were certainly made by children. They are the smallest and shallowest, and this lowers the average dimension of the type. Without this child disturbance, the average size would be very close to that of internal ovens, where none were made by children.

All the ceremonial mums are more impressive than the domestic ones. In the field, there was no difficulty at all in recognizing them. All have sloping walls and the bathtub shape of type CES is exclusively ceremonial.

Figure 47 presents the horizontal plan and the vertical section of the average of each type of oven presented in Tables 29 and 30.

5-5 Cooking Stones

I notice when walking in the Upper Wahgi country that cooking stones are found everywhere on the surface of the landscape: they are in cultivated gardens, in fallow gardens, in pigland, in creeks, in grassland and in the thick bush. They occur as isolated stones, as clusters, and their size is generally small. Greater concentrations and larger sizes are found in and around settlement areas.

It is obvious that cooking stones are inseparable from earth ovens. The aim of this analysis of stones is to find out if some conclusions useful for archaeological interpretation can be derived from the ethnographic activities surrounding these stones: if some meaningful patterns exist in their size, location, etc.

Cooking stones are either brought to settlements from nearby creeks when present. Nowadays they are picked up from roads. Occasionally travels by Public Motor Vehicle (PMV) are organized to distant creeks where pebbles are known to be better and stronger: the best stones for cooking are from creeks running down the Kubor Range, with a strong preference for the Tuman River (in the headwaters of which was a major stone axe quarry: Abiamp), while the Wahgi River pebbles are avoided if possible, being known as fast-breaking stones. When stones are collected they are carefully selected, each one being examined. Soft stones are immediately discarded. Only big unbroken hard stones are selected, up to 7kg each, the biggest being reserved for ceremonial ovens, particularly for type CES. Collection of cooking stones in the Upper Wahgi is very similar to that in Chimbu described by J. Knuttson (1978:22).

The new stones are piled up in the settlement and when people are preparing a mumu, a selection is individually made amongst the old stones, the smallest being discarded. I did not weigh cooking stones, but I made the following classification:

- 1) big stones; unbroken pebbles or broken in such a way that the main core is almost intact;
- 2) medium stones; smaller unbroken pebbles, or rough halves of big ones. They are still in use;
- 3) small stones; very small rocks, which will not be reused but discarded when new cooking stones are brought in.

The discarded stones are piled up within the settlement or just outside it, where they cannot obstruct movement but are still in a handy position. This is because when people are going into gardens where they know they will spend some time (weeding, harvesting, re-fencing, etc.), they will take some of these stones and leave them there: this way, they are not stolen or if they are, it does not matter. Thus one can often find in gardens, under the shade of a tree, a fireplace associated with a small mumu and a few small cooking stones.

Another activity which seems to be marginal but must have some long term consequences is "stone throwing". Knowing where piles of stones are in the landscape, children (and sometimes adults) are often seen throwing stones, toward a target or as far as possible. When a hawk or other bird is spotted, other activities suddenly end, and all males in the vicinity throw stones at the victim. It is fun more than anything else, but the immediate result is that many small cooking stones are scattered around the landscape.

When people go in the heavy bush such as on Ep ridge, to find firewood, construction timber, or for hunting, cooking stones are not carried because Ep ridge is the only area around Kuk where rocks are found, though they are soft, not very suitable for cooking. When it is the time to prepare a mumu, such stones are collected, an oven is dug and the meal is prepared. Being now a "cooking place", people tend to come back to those places till the reserve of firewood or stones runs out. The throwing activity is also practised there, a similar pattern

to that in gardens is found, although less people go on Ep ridge, while the visibility there is limited.

5-5-1 Number of Stones

Just after my arrival in Kuk in 1977, I had the opportunity of visiting a young couple who were settling in a new isolated house in the swamp, just outside the southern boundary of Kuk Plantation. The husband had just built the oven and I had the luck to assist at the very first meal to be prepared in it (Table 31).

The oven was type DCDS (90;30;72). All the cooking stones had been picked up on the Plantation road, a few metres away. I decided to visit this family regularly to follow the life history of the stones, bearing in mind that there was no problem in finding new stones. The road pebbles brought in from the banks of the Wahgi River, break more easily than those from the Kubor Range creeks.

In June, there were 101 medium size unbroken river pebbles ready to be used for the first time as cooking stones. In July, 37 medium and 90 small broken stones were recorded in and around the oven. In August, the number was 79 big, 80 medium and 163 small. In September we had 44 big, 127 medium and 109 small, while 288 small stones were discarded some four metres away in the growing grass. In October there were 85 big, 60 medium, 86 small and 448 discarded. In early November we had 54 big, 42 medium, 70 small and 926 discarded rocks. Late November, the final counting gave 78 big, 70 medium, 110 small and 1054 discarded cooking stones. These figures show that during a period of about 6 months, one family of two persons has managed to bring and scatter no less than 1312 cooking stones at its house site. By that time, the size of the mumu had increased to a top of 94cm, a bottom of 32cm x 30cm and an unchanged depth of 72cm.

In the above case, even though the family had very easy access to

the stone source (the road), the discarded stones were not bigger than the average of discarded stones observed elsewhere in Kuk. This means that stones probably were brought into this settlement at the usual rate and not wastefully. I attempted to record the number of times the mumu was used between two countings, but gave up because of the very odd and apparently unreliable answers given. Important information is therefore missing.

If the above data is extrapolated, assuming that two persons will discard 1000 stones in one year (less than above) will mean that for a small tribe of 500 persons the amazing figure of one million stones will be discarded every four years! Although they are seen scattered all over the landscape, this must have some archaeological implications in the naturally stoneless Kuk environment.

5-6 Two Types of Cooking

Ethnographic descriptions of cooking from various parts of the New Guinea Highlands (F.E. Williams 1940:157, G. Linsley 1951, Clarke 1971: 135, Waddell 1972:121, H.A.C. 1976:5, Steensberg 1980:194-202) are rather similar to the Upper Wahgi ones. This suggests that the Kuk data are relevant for other parts of the Highlands. There are however some local variants, such as in the Bismarck Range (Clarke 1971:117) and in the Eastern Highlands (Salisbury 1962:50) where above-ground ovens are found.

All these descriptions are for daily meals or for pig meat cooking (sometimes for singsings) which I call Type "A" (Plates 17, 18), while, to my knowledge, there is no description of another distinctive type of cooking, which I classify as Type "B".

In Type "A" (Fig.48, top), stones are heated near the oven and then put into it with wooden tongs (Plate 15), layers of stones being intermixed with layers of food. Generally speaking, big and medium stones are on

the bottom and the top of the pit, small stones being in the middle. The stratigraphy shown in figure 48 (top) is of a small pig cooking, for 5-6 people. When the oven is opened, the bottom layer of stones remains untouched while the other stones are stored nearby or thrown back in the pit after the food has been taken out.

Type "B" (Fig.48, bottom) is both very different to the former, and less frequent. It was observed only for the cooking of pat, and nowadays corn. It is often the occasion for social gathering, people bringing their own bunches of food. Each participant ties his bunches in such a way that it can be recognized when cooked. When a big harvest of pat is made from a garden, the owner will cook it in the same way, the social gathering will lead to the expectation of a reciprocal invitation, in a similar way to the Eastern Highlands (Salisbury 1962:58). Nowadays, big harvests of pat and corn are also cash crops.

With Type "B" cooking, all the stones are put on a scaffolding of timber built above the mumu, and the fire is lit in the scaffold. When the scaffolding has burned, it collapses into the pit, together with all the stones. The biggest unburned logs are quickly removed, a big pole is put vertically in the centre of the pit, all the bunches are laid in it and covered with green grass. At this stage, the central pole is removed, water is poured in the hole left, and very quickly the whole is covered with more grass, banana leaves, earth (especially on the sides), and timber and stones are put on top of all the whole. Small escapes of smoke or steam are quickly blocked (see also Steensberg 1980:199).

Once cooked, all the food is removed, everybody picking his bunches with no disputes. In the bottom of the pit a heavy mixture of cooking stones, charcoal, ashes and charred timber is left.

One can see that what is left in the pit of Type "B" is quite different from in the pit of type "A" and consequently this should be

checked archaeologically. If the distinctive pattern is found, one might start to speculate on problems such as the introduction or domestication of pat, if in fact this is the sole food which was treated this way in the past, as it is the sole food today (except for corn).

5-7 Inter-relation Between Earth Ovens

When a settlement is abandoned peacefully, the good cooking stones are generally collected and taken to the new place. But if the site must be abandoned in a hurry, it is obvious that people will not spend their time picking cooking stones. Whatever the reason for the abandonment, ovens are not deliberately filled up, but left as they are. For this reason, walking in the grassland area, I often fell into such pits: they are everywhere, some of known ownership, and others said to be pre-historic.

In the Upper Wahgi there is a reluctance to reuse cooking stones of unknown ownership (although this was done for site MNI below). When an unexpected surface feature is found while clearing a new cooking place, people do not usually dig into it: it might be a grave, or even if it is an oven the stones might have been used in the past by somebody ill or maybe a sick pig was cooked there. So new ovens are built and new stones are brought to the site. These stones are regularly moved from one oven to another till they are no longer useful. Small and medium sized mumus are used more than big ones, which are reserved for big harvests and feast cookings. If stones are discarded inside a mumu, it will usually be only the smaller ones. Sometimes, the cooking of pat is done with small stones being heated for the last time: because it is comparatively hard work to clean a mumu used for pat cooking, this is a good opportunity to leave all the debris inside it. The oven can be re-used in a shallower form or a new one is built nearby.

These observations raise some questions to be borne in mind by the

archaeologist:

- Surface collection of cooking stones from a site hardly answer the question of successive settlements on the same site; this is because of the tendency to clear a site down to the bare clay, or at least to level the floor;
- Short term periods of nonoccupation between two successive settlements on the same spot also could not be distinguished because of the greater probability of recycling cooking stones from an earlier occupation to a later one (presumably earlier tenant known);
- Long abandonment of a site could be established by successive sets of cooking stones being found inside ovens, where sets of broken stones from different pits fit together (same occupation period) with sets of reassembled stones belonging to another period of occupation;
- Unless some pits are cutting into others of an earlier phase, chronology will be established only by C^{14} datings or by occurrence of an external phenomenon such as a volcanic ash fall or European items;
- Presence of big stones might suggest a precipitate abandonment of the site or even destruction of it, i.e. during warfare;
- Infilling of ovens occur naturally and the stratigraphy and the remains in the bottom of them might give information on dietary behaviour.

5-8 Conclusion: Archaeological Testing

Apart from the points raised in the above section, there are other ethnographic observations which must be stressed here and treated archaeologically. They should give data on the artefactual and economic behaviour of prehistoric societies. The following are the major relevant points to be tested in archaeological sites:

- External mums are bigger than internal and are surrounded by ashes and charcoal (features missing from internal ovens);
- Almost no bones are left around a cooking site;
- Various sizes of pits occur in cooking places with only big ones found in singsing grounds;
- Charcoal concentrations occur in a central position in cooking places but are located in between mums in a singsing ground;
- No surface cooking stones in singsing grounds, though present in cooking places;
- Internal ovens are rather shallow with straight walls while external pits are deeper, with sloping walls: the fact that people protect their steel spades by making conical pits suggest that in prehistory, with the more fragile wooden spades, conical mums are likely to have been almost the only type made;
- Very small and shallow pits are the result of children's activities;
- All oblong pits are ceremonial and, if present in prehistory, might indicate the existence of big social ceremonies such as the Moka;
- The amount and weight of cooking stones are much less relevant than their size: it is the presence or absence of big and medium stones which might give some information;
- Because of the scarcity of stones in an environment like Kuk, a study of origin of cooking stones (which I did not make) might show them to be a prehistoric trade item, assuming that tribal territories were of the same size as ethnographic ones (which implies that many tribes do not have a stone resource within their territory);
- Presence of cooking stones in the landscape does not imply a residential site;
- It would not be surprising if each individual discards 1000 cooking

stones every year;

- The importance of looking at the remains left in the bottom of ovens to see whether the two types of cooking exist.

CHAPTER 6

COOKING PLACES - ARCHAEOLOGY6-1 Introduction

Seven major sites were excavated with the aim of archaeologically testing the ethnographic data. To complete the data on these activities, other minor excavations were made, some of them being single ovens, others during the digging of house sites.

The location of the seven major sites in relation to the whole Kawelka settlement is shown on Map 25. One can see that they all are on drylands, surrounding the Kuk swamp. All but sites MNH and MOL are of the resettlement phase, with a known approximate date of use and abandonment, and pre- or post-excavation explanations by the owners and users of these places were recorded. Site MNH, suspected of being a cooking place, was excavated with the aim of testing and interpreting a truly prehistoric site on which no information was available: we shall see that it was rather unsuccessful because of unexpected finds being made there.

6-2 Site MOH/CP

As stressed above, the cooking place was excavated as part of a site where other structures were also excavated. It is located on one of the most southern spurs of Ep Ridge (just north of Tibi Residue), overlooking the whole Kuk swamp southward and Kuning eastward (Fig. 23).

A thick topsoil (30cm) covered with grass was removed in thin layers with spades down to the bare clay layer: six strips, each 1x7m (overall 42m²) were excavated and ... nothing was found except three shallow depressions. Half of each of these was excavated but they were not deeper than 7 to 10cm. The infilling was only top soil, and they certainly were not ovens or hearths. They are interpreted as being pig disturbances.

No more testing was made to find the cooking place.

While excavating the site earlier, a lot of bush was cut down and top soil removed to locate the menstruation house known to have been there. We had to use three informants who "remembered" where the house was located, including the owner himself, finally to be possibly successful (see site MOH/M). Similar inquiries were made concerning the cooking place where one to three ovens were said to have been used. The informants all agreed on a spot located north-east of the former men's house entrance. When the 42m² excavation revealed nothing, it was said that the cooking place probably was further north or east, and the possibility that it was north-west of the house entrance was also advanced. We did not test further. This example will be discussed elsewhere in relation to informants' reliability.

This former settlement, site MOH, is one of the earliest, if not the first, built by the Kawelka when they migrated back to Kuk around 1960. It was used as a transit settlement by the Membo clan during the earliest stages of the recolonisation of Kenta and it was abandoned before 1963.

6-3 Site MOO/CP

Site MOO/CP is located in Kukrumdi plateau. It was complex residential phases and the cooking place found there cannot be analysed as such: it cannot be dissociated from the house sites and consequently is discussed in sections 4-4 and 4-5-4 (sites MOO and MOO/WP, Fig. 29).

6-4 Site MNG

This is located in Kenta and was partly abandoned 3 years earlier. The cooking place here is analysed as an entity, even though the owner's house still stands right on its western boundary and should be integrated in the site (archaeologically). The owner has moved away to the mission

place at Bagla where he has a ceremonial oven located in the singsing ground and also has a domestic mumu for daily use. Nowadays, he sometimes comes back in his former cooking place for some family or big harvest cooking (Fig.49).

The site is in the middle of a huge casuarina grove, where the first Membo resettled, and which is today a coffee garden. The site itself was covered by grass. When first constructed, it was levelled to the bare clay, the top soil removed being used to build a now eroding embankment. The site limits are the abandoned house (west), the embankment where bamboo and casuarina are growing (north) and which seems to be a "natural" site boundary, a small pig shelter and coffee trees eastward (artificial limit) and the coffee grove southward. It was forbidden by the owner to cut down or to dig close to coffee trees.

6-4-1 Description

A recording grid system was established, with squares 3x3m and rectangles of 3x2m (see Fig.49). A surface collection of artefacts was then recorded before starting the excavation of ovens, all visible on the surface except the elliptical one in squares B1/C1 and the smaller circular one in C4.

The surface collection produced (Table 32) 65 scattered small cooking stones (with a concentration in B2 and B3 where 19 stones were found in both squares), 5 beer bottle fragments, 2 timbers (firewood), 1 half coconut, 1 house post, 1 bamboo knife and 1 plastic bag. During this surface collection and the removal of the thin top soil (0-3cm), the concentration of charcoal (Fig. 49) was recorded and found to be in a rather central position. Surface features were recorded before half of each oven found was excavated and the infilling stratigraphy recorded. Then, it was fully excavated, mostly for the tally of cooking stones (Table 32).

The oven in B4/C4 is type CCS. It was half filled. Steamed banana

leaves were found stuck to the walls, pig fat was found in the infilling as well as adhering to some stones. The bottom was covered with rotten banana leaves, above which was a layer of stones: 479 of them (156 big, 158 medium, 165 small, see Plate 19).

The oven in B3/C3 is type DCSS. Burned banana leaves were found along the walls and in between two layers of medium and small stones were some banana leaves and kumu (edible fern) (thickness around 9 cm). Seventy three stones were found (2 big, 52 medium, 19 small).

The feature in D3/E3 is an oven of type DODS. The stratigraphy of it was unusual, since it was used first as an oven then recycled as a hearth (Plate 20). The lowest part consisted of oven remains (layer I), the middle part was the hearth (layer II), and then came the top soil fill (layer III). In layer I, 213 stones were found (1 big, 79 medium, 133 small) together with 5 charred sweet potatoes, 1 piece of corn, 54 glass fragments, 5 pig ribs, 1 big pig molar, 1 lower jaw of a small pig, 7 unidentified pig bones, 4 cow bones, 5 beer bottle caps, 3 pieces of silver foil, 1 fragment of a storage gourd, 1 steel pocket knife, 5 nails, 2 tin cans. To the astonishment of the workers, 2 big fragments of a kina shell were also recovered. In layers II and III not a single artefact was found.

Feature A1/A2 is type CES. The excavation of it was difficult because of coffee trees growing nearby, some roots already penetrating it. The stratigraphy showed a layer of compact black soil having very few small stones in it, with underneath it a heavy concentration of stones, some very big, lying in the bottom of the oven. There were 288 stones (44 big, 86 medium and 158 small), 5 pig bones, 4 glass fragments, 1 unbroken beer bottle and 1 bamboo knife.

Feature B1/C1 is also type CES. The same stratigraphy was found throughout the pit, right to the bottom, composed of top soil mixed with clay and scattered artefacts: 117 stones (65 medium, 52 small), 5 pig

bones, 6 pig ribs, 2 pig teeth, 5 glass fragments, 1 piece of rubber and 1 unworked chert fragment.

Structure C3 is the type D0SV stressed in section 5-4. No stones or cooking remains were found in it. It was partially filled with top soil and charcoal. This charcoal comes from the stone-heating fireplace located in all of this area and does not belong to the structure itself.

The small feature B2/C2, type DCSV, was made by a child. Not a single artefact was recovered from it. The stratigraphy was similar to structure C3 above, charcoal found here being also an intrusion from the stone heating fireplace.

The last feature, in C4, is type DCDS. The stratigraphy was composed of a heavy concentration of stones with little top soil above. A little mouse which lived in it was killed by a worker then cooked and eaten by a child. Four hundred and fifty one stones were recorded: 2 big, 150 medium, 299 small. No other artefact was found.

6-4-2 Analysis

The 156m² excavated in MNG show some interesting data dealing with inter-relationships between mums, the discard of rubbish and the abandonment of ovens.

It seems that six ovens were fully abandoned (D3/E3, A1/A2, B1/C1, C3, B2/C2, C4) and only two were or could still be fully operational (B4/C4, B3/C3). Because of the presence of two elliptical ceremonial pits B1/C1 and A1/A2 (the former replaced by the latter which has itself been now replaced by the one located at the Mission) it seems that the owner cooked his ceremonial pigs in his settlement for a while and that the third ceremonial pit present, B4/C4, was used for smaller feasts and for harvests of pat. It probably was still used that way right to the time of excavation, the last cooking being for pig meat. The other still

operating oven, B3/C3, seems to be for small meals and is interpreted as being probably used by the owner when gardening nearby or picking his coffee beans.

Oven D3/E3 seems to have been long abandoned and to have been used as a rubbish dump before being recycled as a hearth. This suspected "external" hearth has no local explanation nor ethnographic equivalent. On the basis of ethnographic data it could be said that it was at one stage a hearth inside a house, strangely built above a mumu used as a rubbish dump, but there is a total lack of archaeological features suggesting a house site and informants also denied it. The find of a kina shell in the same pit is a problem too. Here the logical explanation would be that somebody was trying to cut the shell in its traditional half-moon shape, failed, and discarded it (which itself is odd), and later on it was thrown in the pit together with the European items found. No further interpretation is given here.

From the analysis of the 1686 stones recorded, 795 (47%) could still be reused: the owner did not leave in a hurry nor was he killed in warfare, but moved peacefully to the mission place where the source of cooking stones (again the road) is very close and where people organise trips by PMVs to get good stones; it was therefore not necessary for him to worry about that question. It seems that, if the site was used for as long a time as is suspected, many stones are missing.

One can also note that apart from European items very little remains of material culture are left behind.

6-5 Site MNI

Some 200m north of site MNJ is found the earliest place of the Kawelka resettlement in Kukrumdi. Although the site is also a complex one, having most probably been used in prehistoric, historic and contemporary times, one can, arbitrarily, isolate the cooking place from

the rest of the settlement (see sites MOP, MOM, MNL).

The first Kawelka who settled there around 1963 found the surface remains of a men's round house where he decided to build his own. In front of it he built his cooking place which apparently overlapped the prehistoric one. When the owner moved to successive residences in Kuk-rumdi, he kept operating his cooking place and nowadays it is still used as such, even though he lives some 500m away. I analyse this place without dealing directly with the rest of the site because it can be considered as an entity in its own right (Fig. 50).

Since it is still in use today, I did not record all the junk littering the surface of the site. The owner, one of my workers and my major informant, told me that when he cleared the site, together with that of his men's house, numerous stone axes were found which were later on sold or given to a social anthropologist visiting them. The site was apparently prehistoric, with not a single disturbance suggesting a recent or post-European interference.

6-5-1 Description

As can be seen on Fig. 50, 81.5m^2 were excavated and recorded by a grid system of one metre squares. The site had been levelled down to the clay layer, but no embankment was built, the top soil being thrown on the northern side where the ground slopes down. All finds made are recorded in Table 33. Although charcoal was scattered all over the place, it was mostly concentrated in the south-eastern part, still associated with logs surrounding the stone heating fireplace.

Structure J7 is an oven type DCDS. Neither food remains nor charcoal were found in it. The stratigraphy consisted of a layer of cooking stones in the bottom of the pit with little top soil above. Two bamboo knives were found amongst the 606 stones (95 big, 330 medium, 181 small).

Structure I8 is type DCDS in which the stratigraphy was similar to

structure J7: only 35 small stones were found, together with a little kumu.

The oven of type DCDS found in A3 had only 2 small stones and one tin can. Right in the bottom of the pit was a thick layer (10cm) of ashes and charcoal, the remainder of the pit being filled with top soil.

The structure in H2 is a double feature. It was not fully excavated southward because of a big tree growing there. The circular feature is type DCDS and the other is CES. Both had the same depth (65cm). Very few stones were found (6) and they were located in the bottom. The other finds were 1 pig tooth, 1 pig long bone and 1 unworked piece of chert. A careful examination of the stratigraphy at the junction of the two features showed clearly that DCDS antedated CES.

The structure in K5 is also a double feature, one being CES (depth 75cm), the other being COS (depth 105cm). From the latter, 118 stones were recovered (65 medium and 53 small) while only 12 small were in the former. No artefact was found in CES. The other oven, COS, had a heavy concentration of charcoal in the bottom, and in the light top soil infilling above it, there were 7 big pig bones, 4 pig teeth, and 2 pieces of chert (neither of them with use wear). CES was built first, then abandoned for COS.

Later on, while we were excavating a men's house just east of the cooking place, children came to watch us working and decided to cook some sweet potatoes for themselves. So they built 3 small mums (H1, J1, L2) which were all of type DCSS (depth 16, 20 and 26cm). When they left with their food, 18 small stones were found in the "oven" H1, 52 in J1, and 53 small cooking stones were left in L2 and 6 medium size beside it. It was interesting to observe the extent of child disturbance.

6-5-2 Analysis

The major characteristic of the cooking place at MNI is the presence

of two double ovens. The owner told me that when he settled down in that area, he wanted three distinct ovens: a long ceremonial one, another smaller one to be used also for pat harvests and the last for daily meals. He found that the site had already two of the three and he decided to use them (Fig. 51, Phase 1): DCDS in A3 and CES in K3/6. He had to build only DCDS in H2 (Fig. 51, Phase 2). Later on, in a rearrangement of the place (said to be for reasons of convenience), he had to abandon the oven in A3, replacing it with a similar one in J7, and to build a new CES in G2/I2 above the existing DCDS oven in H2 (Phase 3). This he replaced with a bigger one for pat cooking: the best place to do this was in the eastern part of CES in K5/6. This CES was no longer necessary because it was replaced by the new one in G2/I2 (Phase 4). These complicated modifications could explain why the double feature G2/I2 has a single level bottom, while the other double feature, in K3/6, has a split level bottom. Altogether, they constitute four distinct and independent ovens, two of them being abandoned.

Another point to notice is that cooking stones in this place have been well utilised, moving from one oven to another. When I saw the site in 1977, DCDS in I8 did not exist and the pig cooked in J7 was with stones picked from COS in K5/6. Stones utilised in the three children's ovens in H1, J1 and L2 were from the mumu in J7.

Because of the lack of "good" stones in the two ceremonial elliptical ovens, one can suspect that they are fully abandoned; this was confirmed by the owner's statement that his ceremonial oven is now located in a singsing ground.

Finally, ovens DCDS in A3 and COS in K5/6 have their bottoms filled with ashes and charcoal: this strongly suggests the peculiar way of cooking pat, even if stones (especially in DCDS) and charred timber are missing. This was confirmed by the owner who said that the last use of these mumus was for pat and added that stones and timber from them were recycled.

6-6 Site MNH

This site was opened with the intention of examining a truly prehistoric cooking place, about which no ethnographic or historic data could be recorded. It is located in Prkl Waike on the slopes of Ep Ridge.

The site was chosen amongst others because the platform constituting it was clearly artificial, dug into the slope and levelled. It had also clear limits, except in the west, and by ethnographic analogy its dimensions showed that it was best suited for a small family house or a cooking place. Nothing was known historically about the site, but people living near the site found evidence around it of house sites, while a little downslope evidence of prehistoric graves. The surface evidence of human interference at the site was the platform itself, some bamboo growing along its eastern limit, and a single old cordyline still growing in its south-western corner. Since the western limit of the site was not evident, probably due to the erosion of the embankment, an arbitrary one was set.

Before the excavation started, the men guessed that it was a cooking place. Some said for the prehistoric house sites around, others that it was built to mumu pigs to honour the dead buried nearby. Some men, to the amusement of the others, were scared to excavate because of the possibility of unearthing graves.

6-6-1 Description

All of the platform was cleared of shrubs and tall grass. The top soil was stripped off over 96m^2 , of which 67m^2 were partially excavated. As seen in Fig.52, a recording grid of one metre squares was set up.

Numerous small cooking stones (68) were found littering the platform surface and a few rotten posts emerged out of the ground around square D16, with a drain-like depression west of it. Two stone axe

flakes were also collected (C10 and C13), together with one pig tooth (C5). All the finds made are recorded in Table 34.

All the squares with the letters D, F, H or the numbers 5, 9, 13, 17, 20 were spade-scraped to the bare clay, a top soil 20-30cm thick being removed. This confirmed that there was a building structure around D16 and surface features suggesting mums were found in C20, D8/9, F8/9, F20, G13, G17-F17 and G20. In squares F18/19 an assemblage seen as structured was found and a hearth is suggested but no charcoal or ash was found, nor was the ground burned.

Squares H20 and C5 were dug deep into the clay to check the stratigraphy. It confirmed that the platform was artificial: H20 showed only a thick continuous layer of hard clay, while C5 demonstrated that the ground dug to build the platform was thrown downslope (south) to widen it, a thick mixture of top soil and clay overlying the natural clay deposit.

Excavation around D16 recovered the remains of a very small structure, with numerous posts and post holes, partly surrounded by a small shallow drain. It was interpreted as being the remains of a shelter for a single pig. Later on, an old man visited us and confirmed this, adding that the small ditch was built because the ground was very muddy during rain. The structure was built around 1965 and was used only during a very short period of time (a few months). This informant knew nothing about the platform itself, also guessing that it was a former cooking place.

Feature D8/9 is an oven type D0DS, in which was found one stone axe flake, three pig teeth (2 molars, 1 canine) and 70 small cooking stones.

Feature C20 was interpreted as being a small oven, type D0SS. One burnt pig bone and 21 small cooking stones were found in it.

The stony feature in F18/19 was excavated a further 5cm deep, but

no artefact was found. The soil was much darker and softer than the clay surrounding the structure, suggesting an artificial infilling.

The half feature excavated in F20 gave major trouble. Only one stone axe flake was found before we came on human long bones at a depth of 64cm. It was obvious that the feature was a grave and not an oven and that it extended right underneath the bamboo grove. This suggests quite an antiquity for the grave, the bamboo itself being seen as a very old stand. The excavator working there stopped in a panic, washed his hands and the trowel in the water held in the excavated feature C20 from the rain overnight, while together with him, half the labour line stopped and asked for the site to be refilled and to go elsewhere. Because the bones found were covered quickly, I kept saying that they belonged to pigs, as many others had already been found at the site. A few men agreed to go on excavating. Thinking about it today, I believe the men were not convinced by my arguments but were themselves interested to dig more to find out how their ancestors were buried before the European penetration of the Highlands.

Meanwhile, half the feature F8/9 was excavated (in F9). It looked like being an oven type DODS: one half lower pig jaw with 4 teeth was found, 3 pig bones, 1 loose pig tooth, and 320 very small cooking stones. At a depth of about 75cm, there was a change in soil structure, the top part (rather brownish and compact) being replaced not by the natural clay but by a very loose black soil. At a depth of 86cm, we had our second and final crisis: a human skull was unearthed, the back of it facing upwards and the lower jaw apparently missing. It was not possible to argue any more; we knew that we were in the middle of a cemetery. The site was back filled and we left the place for another excavation far away (cooking place of site MNI). Two men fell sick after that and we waited to be called before the village court, which, luckily, never occurred.

Assuming that we were digging in a cemetery, one can logically speculate that the unexcavated features observed on the ground surface were also graves. Nevertheless, we also found scattered pig bones and convincing evidence of ovens in D8/9, C20, and above the grave in F8/9.

One can also add that the site is probably much bigger than originally thought, extending eastward (where the bamboo is not a boundary mark) and westward, where charcoal mixed with top soil is continuous (so the absence of embankment does not mean the end of the site).

6-7 Site MOG/CP

Site MOG comprises one Men's round house (MOG/R, section 4-3-2) and one open space in front of the house which has been labelled MOG/CP. A total area of 153m^2 was excavated down to the basal clay (top soil thickness 30 cm). Prior to its excavation it was assumed that MOG/CP would have been suitable for a large cooking place, perhaps even illustrating the transition between such a place and a small ceremonial ground. The local topography is suitable for this, being at the end of a flat spur top overlooking the valley. The presence of a men's house (MOG/R) oriented towards this spur rather than the valley supported the above assumption. For the same topographic reasons it was also assumed that the site may have been occupied in prehistoric times.

6-7-1 Description

Despite its advantageous position (overlooking the entire Kuk basin), only a limited number of features and artefacts were found (Fig. 22, left). The excavation of a large section of this natural "place" revealed a contour ditch running W-E, joined by a smaller tributary one, running N-S some 14 m in front of the house. The infill of these ditches was not excavated, but following an

interview with the owner of the site it appears that he made these ditches. Only one large oven was found despite careful examination of the remainder of the 153m².

The oven found is large, of type COS (155x115; 35x30; 82) and contained a mixture of soil, stones, wood and charcoal. Of interest is the high concentration of stones noted in the lowest 50cm. Overall about 800 stones were recovered, all big and of the local Ep type (soft). No other feature or artefact was found at the site.

6-7-2 Analysis

The scarcity of the remains at MOG/CP suggests that the site has been occupied for only a short period. It is as if the entire settlement was built to last (orientation of the house, levelling and ditching of the area) but failed to do so. This is confirmed by the history of its owner who is a member of the early Membo migration first to Prkl and then to Kenta. The ditching network suggests plan for a large discussion place in front of the men's house, and some concerns for water run-off (the house being located downslope from the place). The lack of domestic and ceremonial ovens points towards three main reasons: i) the short occupation period; ii) ceremonial cooking carried out at another location; iii) domestic cooking carried out at women's quarters.

The only evidence of cooking at MOG/CP comes from a single large oven, probably used only once (large size of stones, poor quality of stones and lack of small or discarded stones), most likely for the cooking of pat. Overall the site reflects an interesting warning against assumptions one could make based on what appears to be an attractive location for the establishment of human activities. Not only this is negated for the historical period but also for prehistoric activities. There is a total lack of evidence for the latter at MOG.

6-8 Site MOL/CP

Site MOL/CP is part of site MOL described previously (Figs. 27, 28; Table 18). It is a prehistoric site composed of a men's round house and a cooking place, the whole being surrounded by a high earth embankment. Two ovens and two piles of stone were found in the 14m² excavated in front of the house on what is interpreted as the cooking area of the site (Fig. 27).

6-8-1 Description

For reasons detailed elsewhere (dumping ground of a present nearby house; tethered pigs), it appears that at least the top soil layer of the site is disturbed. Consequently all unstructured finds of artefacts may belong either to the prehistoric occupation of the site or to the present adjacent settlement. It is only the structured finds found on or in the basal clay that can be definitely attributed to the prehistoric occupation.

In the disturbed top soil, only cooking stones were found. All of them (169) were too small to be of any further use. Scattered on the surface of the basal clay (Table 18), another 350 cooking stones, also of a small size, were found together with 8 other lithic artefacts and 2 pig bone fragments. All these are not directly related to one another and it cannot be said to which occupation period they belong.

Two circular ovens were found, both of them of type DCDS. The oven located in N2 (80;20;50) contained 51 cooking stones, all found in the lowest 10 cm of the pit and mixed with a high concentration of charcoal. Only half of the oven located in MN6 (105;30;60) was excavated and 99 small cooking stones were found within its lowest 15 cm, also mixed with charcoal. Two neat piles of stone were found. The one located in M2 had 212 small cooking stones, in which were embedded

two fragments of pig bone. These bones were clearly in situ, they could not have come from above. The other pile of stones, in M4, had 91 small and 8 medium cooking stones. Finally, in the ditch found in N7, which is presumably an extension of the one surrounding the house platform (Fig. 27) were found 14 unburned Ep stones.

6-8-2 Analysis

Because of the unknown extent of the disturbance which has, and still is, occurring at MOL, I do not regard the 519 cooking stones found scattered through the top soil and the surface of the basal clay as belonging necessarily to the prehistoric occupation of the site. It is in this kind of situation that a petrological analysis as well as a conjoin analysis (e.g. Julien 1972; Van Noten 1978) may provide convincing evidence of two sets of stone, one prehistoric and one modern (Bordes 1980a), originating from distinct sources.

For similar reasons there is some doubt that the two fragments of pig bone, the seven stone axe chips and the chert scraper belong to the prehistoric occupation. It is most likely that they do, but since one piece of plastic found its way down to the basal clay on the house platform, some reservation must be made concerning in situ location of isolated finds. Despite this problem, I am confident that the pig bone fragments found in M2 can only belong to the prehistoric occupation of the site. It is interesting to note that 923 out of the 931 cooking stones (or 99.1%) found at MOL/CP are small. This suggests not only a full use of the site but also a careful selection and removal of medium and large stones made at the time of the site abandonment. While all the above stones are imports from sources outside Kuk, 14 unused Ep stones were also found. The latter evidence could indicate a single transport from Ep and it did not suit at all the owner of MOL.

The scarcity of artefact variety at MOL also suggests a planned abandonment. The neat stone piles found (two in MOL/CP and one inside the house) indicate little disturbance at the basal clay level (thus restricting most of it to the top soil layer), and that the site was kept tidy at the time of its abandonment (and obviously during its occupation). Because of this it would have been interesting to check artefact density and variation just outside the suspected site limits, i.e. outside the embankment. I suspect to find there a relatively high variation and density compared with the one found within the embankment. Finally, it is important to stress the total disappearance of driven-in features, certainly at the house platform and perhaps also at the cooking area, together with the disappearance of organic remains. Such evidence supports strongly the site formation processes discussed throughout this research.

6-9 Other Excavations

Close to the Kuk Plantation gates, three big earth ovens were excavated in 1977. They all belonged to a big man and were said to be abandoned (a sweet potato garden was growing above them in 1978). They were used for ceremonial purposes and for the harvest of pat.

The first oven, called "A", was type CCS (top: 127x126; bottom: 54x52; depth 115cm). The lowest layer in it, 21cm thick, consisted of some light coloured ash, above which was a very black mixture of charcoal and ash. Above that layer was another one, 10cm thick, where 3 small cooking stones, 15 tin cans, 3 glass fragments and 3 unbroken beer bottles were found mixed with cane grass and top soil. The remainder of the pit was filled with top soil containing some clay.

Oven "B" was type CCS (179x175; 71x70; 137). The bottom of the pit to a depth of 17cm was filled with a rich black soil containing

16 small stones and one tin can. The remainder of the pit was filled with top soil mixed with a little clay. No more artefacts were found in it. In the Mt. Hagen context, it was a big impressive circular oven.

Oven "C" was type CCS (148x145; 65x65; 130). A rodent was killed during its excavation. Five layers were established in it. The lowest (layer 1), in the bottom of the mumu (20-25cm thick), was a concentration of almost pure charcoal. Layer 2 (15cm) was a mixture of ash and charcoal in which 235 stones were found. Layer 3 (20cm) was a mixture of very black soil and charcoal in which was a dense concentration of 376 stones. Layer 4 (30cm) consisted only of stones (568) with no soil or organic matter in it. The remainder of the pit (layer 5) was filled with top soil. Although no record was made of the stones in the field, from memory they were of small size with very few medium ones amongst them. Overall, this pit contained 1179 stones!

In 1977 a cooking place (10x10m) containing 5 ovens was recorded at the southern tip of Kukrumdi. Three of the mumus are presented here. The first of type CCS was open, with only the bottom of it filled. Two layers were recorded, the lowest (17cm) being ash and charcoal and the upper (25cm) being top soil probably fallen from the surface. Altogether, there were 813 stones (51 big and medium, 762 small).

The second oven, type CCS, had its lowest 54cm filled with ashes and 158 stones. Above it, was a loose layer of top soil 6cm deep, and the remaining 80cm of the pit was open.

The third oven, DCDS, was freshly reopened and was completely cleaned and empty: 10 big stones and 115 small ones were found beside it. The owner of the cooking place was expecting to cook soon in the third oven and was glad that I took out for him the stones and cleaned the first and second ovens; he was not intending to do

so but to collect (again) stones from the road.

6-10 New Guinea Highlands Archaeological Data

Outside the two references given in section "5-3", which will be discussed here, one might also add Lampert's report in Powell (1970a, appendix 5) and Lampert's field notes on the excavation of house sites at Kuk in 1972 and 1973 (permission to consult these data was very kindly given by R. Lampert and J. Golson). I summarize here the data dealing with cooking places. In 1967 Lampert excavated a site at Minjigina (north of Mt. Hagen) where he found cooking stones and a depression filled with charcoal, in which stones were not concentrated. It was interpreted as an oven.

A preliminary analysis of Lampert's excavation at Kuk shows that at least five earth ovens were found and that more than 2400 stones were littering the surface of the site, many of them being located in the bottom of garden drains post-dating the habitation phase. It seems that the ovens found were rather shallow, 15-16cm deep, with a top diameter of 60-80cm. One pit seems clearly to be type DCSS (70;30;28), containing stones, charcoal, and possibly charred wood. The other pits contained ash, cooking stones and charcoal.

The ovens found by Harris (1977) are tentatively classified in Table 35, together with the data found in Watson and Cole (1977).

Harris' ovens are mostly of the DCDS and DCSS types and all are younger than 250 BP. Some 3500 stones were littering the excavated hillock. Unfortunately, little information is available about the size and infilling of the 30 pits recorded.

Watson and Cole describe 13 pits classifiable as DCDV, DCSS and DOSS. The last has two distinctly different representatives (see Table 35). Here also there is a lack of data dealing with the size and number of stones associated with ovens, and the infilling of

pits is also barely described. Nevertheless, it seems that at least two pits had stones mixed with charcoal in the bottom of them (Watson and Cole 1977:33). There is also a description strongly suggestive of a stone-heating fireplace, or even a cooking place predating a house site (Watson and Cole 1977:51), unfortunately not fully excavated.

6-11 Archaeological Interpretation from Ethnographic Analogies

To what extent can the points raised from the ethnographic observations and summarised previously be taken into account in the interpretation of prehistoric cooking sites? The archaeological data from historic sites presented here seem to be an interesting link between the use of ethnographic analogies for archaeological sites, although the latter are not numerous.

The most important point that I would like to raise concerns the size and infilling of ovens: it seems that big ovens, those classified as ceremonial, provide information on cultural behaviour. In the New Guinea Highlands at least, they are closely associated with two events: ceremonial feasts and cooking of pat, the difference between the two being sometimes clearly defined by the infilling of the pits.

Few open sites from the Highlands with any great antiquity have yet been found and none clearly analysed. But in a long sequence of habitation sites, I believe that the ovens could give clues to problems such as the introduction or establishment of pig killing feasts or big ceremonies comparable to the Moka.

Oven fill could also give information on plant domestication through the presence of charcoal, ash and charred timber, in ovens now solely used for the cooking of pat amongst the traditional foods. Ovens could be one of the better archaeological features left of

early sites, escaping some erosion and disappearance which affects most of the other testimony of cultural and material behaviour in house sites.

Contrary to Harris's assumption (1977, 16), houses are often rebuilt on the same site and using the same cooking place, and are not necessarily relocated. As a result of this, we have a difficult problem in establishing a relationship between occupation phases. One way to test this will be through a distributional study of cooking stones, with reconstitution of broken pieces of the same stones from different ovens. It would not be surprising if that way we find different clusterings of ovens which might represent successive phases of occupation. This method would be tedious, but has been successful elsewhere (M. Julien, 1972).

It seems to me from Lampert's excavations in Kuk and in Watson and Cole's data, many of the ovens may be internal. No ceremonial mums have yet been found in an archaeological context, which possibly led Harris to say that the "modern cooking pit tends to be a much larger affair than its predecessors on Hed Mound" (1977, app.II, ii), a statement with which I disagree. What seems clear is that Lampert and Harris have both excavated sites of minor social significance or that ceremonial ovens of the inhabitants are elsewhere.

A detailed analysis of cooking methods may greatly contribute to our understanding of New Guinea prehistory, as shown by other studies from within the Highlands (Steensberg 1980) or elsewhere (e.g. Sutton 1971).

Although some interpretations or statements presented here might be seen as trivial, one cannot forget two points: (1) The urgent need to make maximum ethnographic observations about a society rapidly losing its traditional ways; (2) The scarcity of archaeological data for testing these ethnographic analogies. I believe that some of the

hypotheses which I have developed may be discarded later on, but others may well be verified.

CHAPTER 7

GARDENS - ETHNOGRAPHY

Upper Wahgi gardens are remarkable for their generally large size, in which many people have a share, with blocks neatly cut into regular squares by means of narrow and shallow trenches often running into a bigger and deeper ditch, which, together with a fence, often surrounds the whole. Their visual aspect has been the wonder of everyone seeing them for the first time (Plates 3 to 6 and 21 to 26). They have been described in detail by a great number of people, some of whom have compared them to Belgian (Leahy and Crain 1937:163) or Dutch (Gitlow 1966) fields. Seen from the air, they seem to illustrate the orderly planning and land management skills of the Wahgi farmers. But these big communal gardens represent only one of the garden types found in the valley.

Other gardens, although similar in shape, are much smaller, owned by a single individual or one family. They will be referred to as "individual gardens", as opposed to the "communal gardens" described above. Depending on their location, these individual gardens can also be surrounded by a fence and ditch. Still other gardens will be found isolated in the bush, representing classic examples of slash-and-burn shifting cultivation, and sometimes lacking the orderly planning. The last type of garden, less frequent and looking as though of spontaneous growth, is often isolated near creeks and wet patches and is dominated by pandanus trees.

A good description of gardens in the Mt. Hagen region is given by Powell et al. (1975:4-12), including details on land clearing, fencing, ditching, weeding, crop planting and harvesting, and land fallowing systems (see also Steensberg 1980). I

do not intend to give a similar ethnographic description for Kuk, rather I shall concentrate on the archaeological potential of this agricultural system. This means looking less at human activities disturbing only the top soil and more at those reaching the subsoil (clay layer), being as such potential archaeological features.

Different crops are grown in these different sorts of gardens. Around Kuk, five types were observed: sweet potato gardens, mixed gardens, market crop gardens, coffee gardens, and pandanus gardens. Kitchen gardens surrounding houses will not be discussed in this section, as I consider them as part of house sites, in connection with which they are discussed. While most of the crops may be planted in any kind of soil or landscape, some of them require a specific location. Throughout the discussion on gardens (ethnographic and archaeological) I shall refer to "plots" and "blocks". By plot I mean a single garden bed surrounded by ditches, while block means a section of a garden owned by an individual. For example, a communal garden is divided by numerous blocks, owned by different individuals, each block being composed of a number of plots.

7-1 SPECIFIC LOCATIONS

While most of the plants cultivated are grown in dryland as well as wetland or on hill slopes, there are some of them which require or prefer a specific location. Pandanus groves will be found mostly in the bush, along creeks. Because of the little care needed for their growth and because of their seasonality, they will often be the only crop grown, with no other work involved than the initial bush clearing and very occasional weeding. When the spot chosen is considered good and not too far away, the site

can be used as a mixed crop garden, though dominated by pandanus, and consequently it is necessary to fence the garden, often with the heavy logs from the initial clearing.

Mixed crop gardens dominated by banana trees and sugar cane, as suggested above, will also be found in other similarly moist environments, but within or very close to settlement areas. They will be located in newly reclaimed wetlands, along creeks and sometimes also on dryland. In order to avoid pig intrusions and theft, it is necessary to fence them, if not already within a major enclosure (Plates 21 to 23). Unlike pandanus groves, mixed gardens have to be close to human activities because of the continuous harvesting, weeding and watching of crops grown.

The remaining crop which needs a specific location is taro. It is mostly grown in the bottom of ditches having some water in them. Consequently it is also found within settlement areas. At Kuk taro is not planted in dry ditches but only in reclaimed wetland or dryland ditches which retain moisture for its growth. All the other traditional crops are grown anywhere. There is no apparent selection regarding the type of soil best suited to them.

7-2 CROPS

This section will examine what the different crops grown in the Wahgi could potentially affect the soil, particularly to the clay layer below the top soil, and so be recorded archaeologically.

7-2-1 SWEET POTATO GARDEN

The New Guinea Highlands staple crop is unfortunately a tuber which does not disturb the subsoil. Its planting, weeding and harvesting with a light woman's digging stick do not penetrate deeply into the top soil. In wetland, it grows well fairly close

to the water table level and the only requirement is that the tubers do not remain wet too long. Complementary observations I made in swamplands in the Upper and Middle Wahgi, Tari, Koroba and Kandep regions indicate that its cultivation in the swamp is not avoided (Plate 6). These observations also confirmed the lack of subsoil disturbance by its growth, apart from the garden pattern itself (squares or mounds) which will be discussed elsewhere.

7-2-2 MIXED GARDEN

In this type of garden, sometimes referred to as "men's garden" (Vicedom and Tischner 1973(2):127-8), three vertical levels of crops are generally found: at the bottom native and introduced greens, at a mid-level crops such as taro, pitpit, cassava and yams occur, and at the highest level banana trees and sugar cane with one or two pandanus. Only the first two of these "high" crops seem to do something of archaeological significance to the subsoil. Bananas and sugar are planted in a deep hole dug by a heavy man's digging stick, up to 55 cm deep in the clay layer (Plates 1 and 2). Furthermore, to keep sugar cane and banana trees straight during maturation, up to 5 poles (and possibly more, though I saw no examples) are set deeply into the ground around the plant. They can be alongside it, or more than one metre away from it. Finally, some varieties of bananas have the bunch covered and tied with leaves for complete maturation and to protect against bats and birds. This is done by putting in the ground a small cut tree, big enough to support the weight of a man, on which branches are left so it can be used as a ladder. This ladder is carried from one banana plant to another, and when not needed it is left where it was last used. It is also used when tying mature sugar cane to supporting stakes. The holes made by these ladders being deep and numerous in these gardens, they could be a distinct

archaeological feature.

The two mixed gardens recorded in Figure 53 and Figure 54 were fully mature gardens, having already some small casuarinas and coffee shoots planted in them in the expectation of a change from a mixed garden to a coffee garden. They are both located in Kukrumdi on land reclaimed from small marshes. They are about 250 m from one another.

Figure 53, noted in the field as "Ru's mixed garden", represents only a segment of the whole garden. It may be considered as a single unit, being well delimited by two narrow and shallow ditches located north and south, dropping into a much deeper one to the west, while there is a track on its eastern boundary. Numerous banana bunches and sugar cane had already been harvested, and because of the other crops growing there (unrepresented on the Figure) neither a complete record and excavation of post holes was possible.

Figure 54, "Konga's garden", has been arbitrarily delimited, since in reality it extends in all directions. Otherwise the same remarks for Figure 53 can be made.

These two examples illustrate the great number of narrow but deep holes found in this type of garden, especially considering that only a minimum of stakes or stake holes was recorded. One can suspect that at least an equal number was not recorded (the gardens being fully matured for more than two years) and none of the bigger "ladder" holes is recorded.

Figures 53 and 54 show clusters of stakes or stake holes for the two interplanted crops. It is not possible to decide from the holes themselves what is banana and what is sugar cane, but it is clear that gardens with these plants have a great number of holes.

Table 36 represents a formal analysis of holes recorded in

currently used mixed gardens at Kuk. It seems that a relationship exists between the top shape and size of holes: they tend to become more oval when larger. This is a direct result of the technique used to put stakes into the ground: each time it hits the ground, the man moves the stake about to take it out and strikes again in the same spot, but deeper. The bigger the stake is, the more it is moved about and the more oval will be the hole made. One observation made in the field and confirmed by Table 36 is that stakes to keep banana trees straight tend to be bigger than those for sugar cane. But there is so much overlapping between the two sizes that a generalisation here would be dangerous. Another observation was that there were more stakes further away and set obliquely into the ground for bananas than for sugar cane, but this was a tendency only.

Ladder holes seem to be larger and deeper than the two others. This is a direct correlation with their greater weight and diameter; they must be strong enough to support a man. These ladders are always put straight into the ground, never in an oblique position.

Finally, it was my impression in the field that there were more clusters of stakes around sugar cane than banana. But this impression is not confirmed by the comparison between Figure 53 and 54: it is true for the first example, but in Figure 54 there are more clusters around banana trees.

It was impossible to find a suitable mixed garden site to excavate, due to the tendency to convert them into coffee gardens while still maturing. One site, in Kuning Kaklong, suspected of being a prehistoric mixed garden or a house site with kitchen garden (suggested from the vegetation growing there), was excavated for the purpose of analysing the relationship between the numerous stake holes which should have been there. Some 42 m² were excavated down to the bare clay (top soil averaging 25 cm) and only

one small rotten post was found in a corner: it seems that there was neither a mixed garden nor a house (Site MNW). Consequently, the observations made above on ethnographic mixed gardens remain at a hypothetical level, or that such holes do not survive in the archaeological record.

7-2-3 OTHER CROPS

Yam is found in numerous dryland gardens, but always in very small quantity. There is no plot reserved for its sole cultivation, but one or two yam vines are found in the centre of some plots, surrounded by the crop dominating the garden (sweet potato, mixed, etc.). Its planting is done by a woman's digging stick not penetrating deeply into the ground, and when maturing, a small pitpit stake, up to 2.5 cm in diameter and 2 m high, is put into the ground to allow the vine to climb. All these activities seem to be ephemeral in an archaeological sense, and they would disappear as soon as another crop is planted.

The cultivation of pat (Psophocarpus tetragonolobus) has the same physical features as that of yam. The major difference is that it is more cultivated, full plots or even blocks being reserved for it within sweet potato (or peanut) gardens. I suspect that the amount cultivated nowadays is well in excess of the local need, a substantial quantity of pat being sold in markets. Numerous stakes of the same size as for yam are found in each pat plot. Four of them, some 12 m² each, were recorded and respectively 64, 55, 83, and 36 stakes were found. But all of them would also be ephemeral features.

No other traditional crops are of interest in this section. Market crops are not discussed because they do not penetrate deeply into the top soil.

7-3 INDIVIDUAL GARDENS

The difference between individual gardens and communal gardens is not always obvious and an arbitrary distinction has had to be made.

Only one or two men usually own an individual garden and blocks are given to various woman (mothers, wives, etc.). In all cases, these gardens will be found isolated, often ditched and fenced against pigs. Some of them will be found inside substantial enclosures but well separated from the communal garden by uncultivated land. In this latter case, they will not be fenced. When well located, an individual garden may be the starting point of a communal one: relatives and friends will come and make their gardens alongside it and a single ditch and fence will surround the whole. Consequently, there is no clear division between a big individual garden and a small common garden.

For this reason, I was forced to establish from field observation an arbitrary one. My definition for an individual garden is based on its size: it should be owned by no more than two men, and have a maximum area of around $2,000\text{m}^2$, a figure derived from the numerous field observations made at Kuk.

Figure 55, labelled "Korua's Garden", represents a big individual garden, and Figure 56, "Nigints' Garden", a smaller one, but more representative. The two gardens are located within a communal enclosure and consequently have no surrounding fence or deep ditch. If an individual garden happens to have

its own enclosure, this is built in the same way as described in the "Communal Garden" section. More ethnographic details on the building, maintenance and land distribution of individual gardens can be found in Powell et al. (1975:4-12).

7-3-1 KORUA'S GARDEN

This garden is located in Kukrumdi (Figure 55). It is on a gentle slope (12.5%) surrounded by pitpit and kunai. It joins the swamp level at its south-eastern boundary. It represents a big individual garden, some 1760m² including the ditches.

Figure 55 shows that the garden is entirely surrounded by a ditch which runs off in the middle of one side. This is in the bottom part of the garden, which includes the seven bigger plots where the swamp is reached. Between the garden and the major ditch is one of the main foot tracks found at Kuk and for this reason, the owner of the garden, Korua, did not want (or was not permitted) to extend it right to the major ditch. Being located inside a communal enclosure, it was not necessary to build a fence and ditch around the garden.

All the ditches in this garden have the same width (30cm). To build them, Korua put some vine ropes as a guide and then dug them "one spade" wide. To find the depth he scraped the top soil down to the bare clay and followed it. In the lowest part of the garden this method had to be modified due to the disappearance of the clay underneath the swamp peat. There Korua started digging the ditch uphill from the major one to the ditches already established. In doing so, he left black soil and swamp peat at the bottom of the ditch instead of exposing the clay.

During these operations a very few small cooking stones

were found in the dry part of the garden while in its swampy section eight rotten posts said to be made by a stone axe were found, suggesting a fence line. No volcanic ash was seen. Because this was the first garden made there since recolonisation, it is reasonable to say that the finds are prehistoric. The swamp extending southwards is almost fully drained and gardened now. Numerous artefacts were found in it, such as fence posts, heavy digging sticks, stone axe blades, stone axe handles and cooking stones. In the profile exposed in the walls of ditches dug there, one can see a full sequence of the prehistoric agricultural phases found by Golson (1977a), where the grey clay deposition (9000-6000 BP) together with other phases marked by characteristic volcanic ash layers are found. Clearly agricultural activity around Korua's garden has a great antiquity.

In the garden itself, I noticed that sometimes the spade scraped off the surface of the clay itself (1-3cm) and also that often shallow depressions (2-5cm) were made at the junction of two ditches. This was not deliberately done but was the direct result of smoothening the ditch bottoms at their junction. The depth of the ditches consequently varies between 25 and 35cm in the dry part of the garden. In its wetter part, ditches are slightly deeper, up to 50cm, while the major ditch is 2m wide and 1.2m deep.

Sweet potato was planted in all the plots except in the seven bigger ones at the bottom of the garden which were planted with mixed crops: banana, sugar cane, pit pit, cassava, taro and greens.

Two people own this garden, Korua and his father. The landmark dividing the two sections is the run off drain coming from

the top of the garden down to join the major drain.

7-3-2 NIGINTS' GARDEN

Nigints' garden, Figure 56, is located at the foot of Ep Ridge in Kuning Tip. It is within a communal enclosure and has no surrounding ditch and fence system. It is smaller than Korua's garden (some 675m^2 including the ditches), located on a gentle slope (13.3%) and dropping into the swamp southward.

All the ditches in it have a width of approximately 40cm and the depth is variable, from 20 to 40cm, the top soil being scraped down to the bare clay in the same way as in Korua's garden. In contrast to the latter, Nigints' garden has no major ditch in its vicinity and it has three run-off ditches which disappear into the swamp. In some parts of the garden, ditches are dug slightly into the clay (up to 6cm). No observations were made at the junction of two ditches.

Sweet potato has been planted in each plot except the four bigger ones found at the bottom of the garden (south), which, following the Korua pattern, are planted with mixed crops (sugar cane, banana, yam, cassava, taro and greens).

As with Korua's garden, the middle run-off ditch separates the garden in two parts, marking the dual ownership of the garden. This is visually expressed by the transverse ditches which have different alignments on each side of the run-off ditch. This deliberately made displacement is a feature which will be found especially in communal gardens to stress the boundaries of blocks allocated to different owners.

One can also see (Fig. 56) that the pattern of a previous garden is visible on the clay base of the modern ditches. Being prehistoric, it will be discussed elsewhere. No artefacts were

found during the construction of Nigints' garden, outside of a small broken piece of stone axe blade. No volcanic ash was seen and permission to excavate the prehistoric ditches found in the garden was not granted. Numerous stones from Ep Ridge were littering the surface.

In the swamp just south of the garden, numerous prehistoric fence posts were found while gardening there, and when looking at profiles exposed by these modern ditches it was possible to find numerous prehistoric ditch profiles, the bottoms of which were filled with the last volcanic ash fall dated to 300 BP (Blong 1982). All this suggests major gardening activity in and around the swamp at least in recent prehistoric times.

7-3-3 OTHER INDIVIDUAL GARDENS

The other individual gardens observed in Kuk follow a pattern similar to the two gardens described above. Some have only sweet potato growing in them, others have plots allocated to peanuts, beans and corn, others (but smaller) are only planted with mixed crops dominated by banana and sugar cane. As a general rule, there will be a boundary marker of block ownership, such as a distinct dividing ditch or non-aligned ditches.

Individual gardens located on steep hill sides have garden plot and block patterns similar to those found on flat drylands. Differences were noticed only in the ditch and fence system surrounding them: light fence in grasslands, fence made of heavy logs near forested areas.

All individual gardens observed in Kuk were located on dry land. Some gardens found in the swamps were within the 2000m² limit fixed for the size of individual gardens. I do not consider them as belonging to this category because they are

all directly articulated with major ditches and there is a continuous process of other "individual gardens" being added to those already existing to give an overall "communal garden" aspect (Plate 25). This is done to take advantage of the existing drainage system. Consequently individual gardens in swamplands are ephemeral, being quickly integrated into a network of gardens and ditches to form an articulated system. In contrast individual gardens located on dry land are not always the starting point of communal gardens. Some of them are abandoned after years of cultivation with no other human activities having taken place around them during that period of time.

7-4 SIZE OF HOLDINGS

The size of holdings owned by individuals in individual and/or communal gardens has been estimated. Due to the complexity of the problem and because of the time necessary for its solution, accurate measurements were not made. To do so would require another full-scale research project.

To give a general picture of the land owned by individuals, a survey was made in 1977 in the Kenta-Kukrumdi area, south of Kuk ARS. Some 90.6 ha of land ($906,876 \text{ m}^2$) were under cultivation since re-colonisation. Of these, some 49.1 ha ($491,250 \text{ m}^2$) were cultivated with sweet potato, 18.9 ha ($189,063 \text{ m}^2$) were plots with market crops such as peanuts, beans and corn, 6.8 ha ($68,438 \text{ m}^2$) were in mixed crops (predominantly sugar cane and banana), 6.4 ha ($64,375 \text{ m}^2$) were mature mixed crop gardens in the process of being converted into coffee gardens (with casuarina and coffee seedlings already planted), and 9.4 ha ($93,750 \text{ m}^2$) were taken by coffee gardens. The area supported 573 people, so that altogether there is .16 ha ($1,600 \text{ m}^2$) per head of total land under cultivation, reduced to less than .09 ha (900 m^2) per head when excluding cash or market crops. Consequently, when one considers the overall

900 m² per head left for local consumption, it includes some 100 m² of uncultivated garden ditches. These figures seem to be in accord with other data available for the New Guinea Highlands (Macewan 1978: app.II). The figures given above suggest that there is about 40 ha of cultivated land being reserved for a market oriented economy. It represents a high 45% of the gardens not being made for a subsistence economy. The major cash crop is coffee. Its expansion started some time ago (Brown and Brookfield 1959:32) and has developed in such an explosive fashion throughout the Highlands (Donaldson and Good, 1981; McLaren 1982) that it may soon generate major socio-economic problems.

These 900 m² of subsistence gardens per head are never planted with the same crop nor located in the same garden. They will be found scattered in individual and/or communal gardens, similar crops being at different stages of maturation to secure continuity in harvesting. If possible, they will be planted in different types of soil and in different locations in the hamlet; this is for security, guarding against such things as pig destruction, human plundering and climatic disasters such as long dry or rainy spells. All gardens owned are within one hour walking distance.

7-5 SIZE OF PLOTS

While it is often assumed that all gardens have a similar geometric pattern (especially when seen from the air), a ground survey shows a greater variability than expected.

It has already been said (sections 7-3-1 and 7-3-2) that plots tend to be bigger for mixed crops than for sweet potatoes. This observation is confirmed by Table 37 which shows the correlation between plot size and number of garden plots recorded in various areas within South Kuk.

One must note that the upper size limit of gardens was not recorded. Furthermore, a strip some 15-20 cm wide around each block has to be de-

ducted because of the small ditches separating each of the blocks (30-40 cm wide).

Table 37 illustrates some field observations of the difference in plot sizes between mixed crop gardens and sweet potato gardens (as shown in Fig. 55 and 56). Mixed crop plots are bigger and more constant in size. At the same time there is a tendency for larger gardens to have smaller plots, the size of plots decreasing from 35 m^2 to 20 m^2 . But a great number of exceptions are found.

If we look only at the mixed gardens detailed here, Fig. 53 represents a single mixed crop plot and is 243 m^2 , while Fig. 54 is even less than a single plot (and at the same time block) but is 460 m^2 ; the mixed crop plots represented in Fig. 55 and 56 also show differences in sizes between them, but are much smaller.

While the sweet potato gardens recorded in Table 37 show a less tight correlation than mixed gardens between garden size and number of plots, one can say that overall there is a greater regularity in plot size in sweet potato gardens than in mixed gardens: from 12.5 m^2 (4 plots for 50 m^2) to 13.2 m^2 (156 plots for 2060 m^2). One garden located in the swamp had very big sweet potato plots and for this reason is also illustrated in Table 37. Two other gardens made by Enga immigrants at Kuk, in the Enga traditional way of mounding, are also shown in Table 2. Interesting enough, they are not very far off the average size of Hagen square plots.

I noticed that while most sweet potato gardens can be described as "geometric and regular" in the layout of plots, some people have radically changed that layout. The new pattern was noticed only on dry land. When the garden is estimated as "good enough" and "dry enough", plot ditches will be less regularly spaced (see the difference between Fig. 59 and Fig. 60 where a garden is in the process of taking on a new pattern). The new pattern is very much linear,

with strips recorded elsewhere at South Kuk up to 20 m long and 9 m wide (looking like the unit recorded in Fig. 53 which is 27 m x 9 m).

Thus one can say that while there is generally a clear distinction between mixed crop and sweet potato gardens (Table 37), Hageners are at the same time individualists and opportunists. They are not tied to strict traditional garden layout rules. It seems that they are not afraid to make experiments and try new crops.

One such crop trial took place at Kuk when somebody tried to plant coffee right in the swamp, with no ground preparation. He wanted to see if the coffee would mature quicker. All his neighbours watched this experiment with interest. The coffee trees grew slowly and eventually died before maturation. Although people laugh at the result they say that the experiment was worthwhile. If an experiment is successful, such as the change from a square to a linear gardening pattern, people adopt it progressively rather than radically. It can be suspected that such trials of new crops and gardening techniques also occurred in prehistory and were not always successful.

7-6 COMMUNAL GARDENS

When one looks at the recent recolonisation of Kuk, one realizes that making communal gardens is a complex problem.

They can start from a well located individual garden alongside which are built other individual and small communal gardens. This pattern was followed in the Kenta-Mapa-Gengla area.

They can be planned as a communal garden big enough to sustain a population with high annual population increase due to immigration, which can simply double its size when a shortage of land is seen within the communal garden. Thus a new land management is planned for the expected growing demand. This success was particularly seen in the Kukrumdi-Kum area.

All extensions, modifications and changes have left indelible marks on the landscape along the lines of the big surrounding ditches, many of them deliberately filled when no longer required. However it should be possible to understand the process of extension of a communal garden as long as all major ditches are mapped.

The major difference between individual and communal gardens is size. After a long-term, say 20 years, agricultural use of the landscape, some communal gardens can reach enormous sizes. For instance, one can consider the whole area bordered to Kuk ARS, the Guga River, the Kuk Road and Kenta as a single giant communal garden.

Ownership of blocks within these gardens seems at first not to be discernible other than by crops being at different stages of maturation. But there is a very sophisticated block management which has been pointed out when discussing Fig. 55 and Fig. 56. To understand this, a generalised model is proposed (Fig. 57) which is not a field record but an interpretation of field observations.

7-6-1 GENERALISED MODEL

The communal garden represented in Fig. 57 could have been built in different ways. It could have started from Block "C" which would originally have been an individual garden surrounded by a ditch and fence enclosure. Its location being good, relatives and friends make gardens around it. Consequently, a new ditch and fence has to be built around the new perimeter and those on three sides of the former Block "C" enclosure are no longer necessary. They can be deliberately filled in or be allowed to do so naturally. But being dug deep into the clay, it will be possible to find their remains.

A second way of looking at the creation of this communal garden would be as a planned undertaking. The location being good, a group

of people decides to build the whole garden at once and makes the block subdivision within it. This means that there should be only a single enclosure surrounding the whole garden instead of one enclosure within another as in the previous case.

Finally, the garden represented in Fig. 57 could be only a segment of a communal garden. This implies long-range planning or, more probably, the expectation of a major change, such as immigration or land redistribution. It means that an enclosure system does not surround the section presented in Fig. 57 because it does not represent the whole communal garden site.

In drylands, foot tracks to reach individual blocks are the block boundary ditches shown by a thicker line in Fig. 57. While they have a functional meaning, these block boundary ditches do not necessarily have a distinct formal size. Often there is no difference at all in size and shape between the boundary block ditches and the plot ditches. Sometimes one block ditch becomes a leading foot track and seems to become more eroded and wider. These block ditches can also be slightly wider and/or deeper than the plot ditches.

To understand the block ownership, one has to look at the ends of plot ditches: along each side of the block, they will either end in a block ditch (thick line) at right angles to it (open) or terminate short of it (closed). The only exceptions found were when block ditches were much deeper and wider, thus stressing their function as boundary marker; plot drain displacement between two blocks in these cases is not necessary.

Block "A" has two open sides (right and bottom) and Block "C" has three open sides. Between "A" and "C", plot ditches are not set in a line. The same displacement is found between "C" and "D", and "C" and "F". Block "F" shows irregularities in the relationship of

plot ditches and block ditches (as in Fig. 55). Block "E" illustrates a change of gardening pattern from a square system to a more linear one. Finally, one block in Kukrumdi, brought to my attention by Dr. J. P. White, was entirely closed, with no openings at all (White and O'Connell 1982, Fig. 66). When asked about this, the owner said that it was deliberately made, no openings being necessary. One can also imagine blocks "A" and "C" as being previously a single block which has been parcelled out when inherited by two sons (e.g. Brookfield and Brown 1963). Sometimes a single cordyline, yam or cassava is planted in each corner to accentuate block boundaries.

7-6-2 MUK'S BLOCK

Muk's block (Fig. 58), recorded in Kukrumdi, has only one opening, westward. It has 350 m² and to reach it one has to walk through a labyrinth of block ditches.

It belongs to a young woman, Muk, and it was said that this was her first block allocation by her husband within a communal garden. Plot ditches were dug "one spade" wide (around 30 cm) down to the bare clay (top soil 20-25 cm). Outside a few small cooking stones, no prehistoric features nor artefacts were found. This block has a formal similarity to block "B", Fig. 57. It could be considered as a classic example of an individual block for sweet potato within a communal garden.

7-6-3 REA-NUNG'S BLOCK

This block (Fig. 59, 60, 61), also in Kukrumdi, is about 859 m². It has a major foot track along its southern and western limits. It represents two activities widely observed in Kuk.

First, when a block has been cultivated for long enough and

the owner does not want it fallow, the block is reworked in such a way that the new plot ditches (Phase 2, Fig. 60) are slightly displaced and cut through the plots of the previous gardening phase (Phase 1, Fig. 59). It is said that by doing this the gardening cycle can be extended without loss of yield, avoiding a "short-fallow" phase. This plot displacement can also be made after such a short-fallow phase (Plate 26).

An immediate consequence of this plot displacement activity is that if the plot ditches penetrate the clay layer, as they do at the Rea-Nung block, the potential archaeological feature left will not be a regular grid gardening pattern (Fig. 61). Furthermore, as is the case for this block, shallow depressions (up to 10 cm recorded) are made at the junction of two ditches (as mentioned for Fig. 55) as a result of smoothening their bottoms. The general archaeological picture could be an intricate pattern of numerous small ditches with some man-made basins in them.

The second activity exemplified by this garden, and already discussed above, is illustrated by Fig. 60 where there is a change of pattern from a regular grid system to a more linear one. One can notice that this change does not affect the whole block. While Phase 1 is a block having two openings (south and west), in Phase 2 there is only one left (westward). These two phases were for the cultivation of sweet potato.

From these ethnographic observations, I suspected that the archaeological picture left in the clay layer could be wrongly interpreted as stemming from numerous small ditches, some of them having man-made depressions, surrounding "islands" of various size and shape. Particular investigation of this question was planned for the excavation of garden sites located in a similar environment of flat dry land.

7-7 SWAMP GARDENS

The Kuk area has been so much gardened since recolonisation that it is hard to find out what was originally dryland, wetland or swampland. But an examination of air photographs complemented by a survey of the artificial ditching network established at Kuk by myself throws some light on the matter. Nevertheless it is difficult to find out the exact limit between dryland and wetland and a wetland and a swampland. This physical aspect is further complicated by weather variations, where long dry or wet spells are regularly encountered. I am tempted to divide the area into drylands and swamplands only, most of the drylands becoming wetlands during the wet season, while swamplands become wetlands, and even drylands on their margins, during dry spells.

The creation of gardens in swampland can take on various aspects but it always follows an important rule: it must start at a location where water can run off the garden site into an outlet. This can be another ditch, a creek or the swamp itself. When this outlet is chosen, a drain is dug upstream to form a big square or rectangle, being the perimeter of the future garden; within it plot ditches are dug.

If it is an individual venture, the garden is preferably

built on the swamp margin. This allows the owner to have its surrounding ditch feeding straight into the swamp. Although only observed in the Haibuga swamp near Tari (Southern Highlands Province), small isolated gardened "islands" can be made in the middle of a swamp as long as the principle of having a run-off channel is observed.

At Kuk, swamp cultivation seems to have often been a planned communal project. The drainage pattern indicates that the major ditch is often dug longer than necessary for the gardens it sustains (see Chapter 2). This is principally due to the land being subdivided into blocks prior to its drainage and it is more economic to dig at once the total amount forecast; especially when the work is done by foreigners settled down among the Kawelka.

All this planned work is a direct consequence of the big population immigration in Kuk. Sometimes a major ditch is dug and the block division indicated by lines of posts but the ditches are not dug and nothing more happens for a while. This was the case in Kum and Kukrumdi where a major ditch was finished in July 1977 and in November 1978 when I left the area nothing had changed. It was due to the extreme wetness of the area and people were waiting for the water table to lower. By November 1978 it was said that the ground was ready for gardening and block and plot drains would be made soon. An aerial photograph of the area shows that by January 1980 there was still no garden made. In other cases, the sequence from the digging of the initial major ditch to the planting of the first crop is uninterrupted.

To gain a better understanding of swamp garden formation, a generalised model is presented. It summarises major patterns found at Kuk.

7-7-1 GENERALISED MODEL

One can consider a creek running in the middle of a swamp (Fig. 62) which is also a boundary between two clans (or tribes, sub-clans, etc.). Clan "A" located below the creek has two sub-clans, one on the left, "Aa", under a strong land pressure, the other sub-clan, "Ab", located on the right under a moderate pressure. In contrast clan "B" located above the creek, also divided into two sub-clans, "Ba" and "Bb", has no pressure at all. Before any gardening took place, the whole swamp was considered as excellent pig grazing land.

At the first stage a major ditch dug from the creek and at right angles to it is the boundary ditch between "Aa" and "Ab" (e.g. Plates 21 and 22). Another major ditch well located for "Aa" joins the creek in a favourable bend. Part of "Aa" land is immediately subdivided into blocks and is gardened. Block drains are of two kinds, one being directly oriented towards the creek and the major ditch, the other being at right angles. While all these block ditches are planned by the male community concerned though not necessarily made by it, plot ditches within blocks are totally left to the individual owner's will.

Needing less garden land, "Ab" makes only a narrow garden strip at right angles to the creek during this first phase. Its drainage management is similar to that of sub-clan "Aa". The boundary ditch being totally artificial, its construction is under joint "Aa" and "Ab" direction but its maintenance is the sole responsibility of "Ab", being the only group using it.

On the other side of the creek, "Bb" is not interested at all in these new gardens in the swamp. The same can be said for "Ba" where only one pioneer is found gardening along the creek.

The land being good for gardens, clan "A" decides to increase

the number of its gardens and to do this it has to canalize the creek to have better control of the water flow, which incidentally increases its arable land. The creek can be partially controlled or, as in Fig. 63, totally. This work is done only by members of clan "A", people of clan "B" being not directly concerned by this need of new gardens; some people from "Ba" might join in, but with no obligation. Although "B" is interested in the fact that the creek canalization is lowering the water table in its part of the swamp. It can be seen in Fig. 63 that the new major ditch does not follow the creek bed.

During the second phase, sub-clan "Aa" has totally reclaimed and developed its swamp section. All the blocks are planned and laid out regularly, with a change of the drainage orientation in gardens located along the boundary ditch with "Ab". This now being utilized by both sub-clans, its maintenance is under the responsibility not only of "Ab" but also "Aa". By totally developing its land, "Aa" has eliminated the pig grazing land previously located there. This implies a complete pig emigration to other grazing areas.

"Ab" has developed a strip along the former creek and still has some pig land left. It can be seen that this expansion also has been a planned exercise, plot and blocks being regularly laid. For clan "A", most of the land drained has been gardened.

On the other side of the creek, new blocks have been spontaneously added to the pioneer "Ba". The lay out is much less regular than with "A" and illustrates individual ventures with no overall planning. One block is added to a previous one to take advantage of the existing ditching network and in the end block ditches will be seen as broken lines in "Ba" as

opposed to a regular pattern found in "A". In Fig. 63 five block ditches terminate in the creek in "Ba" land. One could easily show a similar combination of swamp gardens having only one outlet. In this second phase, there is still no boundary ditch between "Ba" and "Bb" and there will not necessarily be one. The canalized creek is now the boundary between "Ba" and "Aa" and between "Bb" and "Ab".

With successful gardening it is to be expected that planning will take place within "Ba". This will improve not only the drainage network but also foot access and garden supervision. On the other hand, if the pioneer garden had failed, people would laugh at the owner's experiment and no more gardens would be made there for a while.

After an interested wait while the creek was canalized by people of other clans and sub-clans and the water table on its own land lowered as a result, "Bb" finally decides also to make gardens in the swamp. At the end of the second phase presented in Fig. 63, "Bb" has only dug two parallel ditches with the intention of making gardens between them, very much in the pattern of "Ab" during Phase 1.

Once groups of people have a common ditch, whatever is its importance, they have to share the responsibility for its maintenance. Disputes are settled by men of the involved sub-clans, clans or tribes, depending on the boundary ditch location. Nowadays it can be settled by the official councillor during village courts.

During an inquiry on this topic conducted for me by Prof. A. Strathern, in Melpa to avoid any ambiguity it was stated that at boundaries and critical outlets the people involved will most probably have friendly relationships if not some kin ties. Kin

ties often exist even if the groups on each side of the demarcation line are major or minor enemies. By making such socio-political arrangements along major and boundary ditches as well as important outlets, people are assuring the good operation of the whole drainage system.

If for any reason (warfare, clogging of ditches, land abandonment, etc.) the drainage system no longer operates, people will abandon the gardens to open new ones at a suitable outlet in the same way as described above. While this seems not to be a problem in a big swamp such as Kuk, where people could easily open one section of it and later on abandon it for another suitable section within the same swamp, it is a different question for people owning small marginal swamps such as the Kawelka nowadays after land alienation. There, a breakdown of the drainage system would automatically mean a land shortage. I think that in this case there would be "attempts to acquire land by force" if no alternative solution was found, such as partial or total emigration or the doubtful possibility of a radical economic change.

The generalised models presented above are drawn from numerous examples seen at Kuk. A swamp dividing two clans exists between Kukrumdi and Kum (Plate 22). The Mandembo have not yet gardened their section of it, which has been drained for them by the boundary ditch dug by Tomba and Tambul settlers within the Kundumbo clan, which has fully reclaimed and gardened its portion of the swamp.

A regular planned garden lay out such as that exemplified by "Aa" and "Ab" is found all over Kuk but especially in Bagla, Kum and Kuning. An irregular individual garden lay out is encountered in Kuning Tip, Kuning Meten and Kuk Waiga. A change

in drainage orientation as described for "Aa" (Phase 2) is found in Kuk hamlet and Kum. Major ditches with no gardens as yet articulated, such as for "Bb" (Phase 2), exist in Kukrumdi and Mup. Partially canalized creeks are to be found in Kuning and Kum while totally channelled creeks exist all over Kuk but especially in Kenta, Kuk hamlet and Kuk Waiga.

One can also refer to Maps 3 to 13 which illustrate such a cumulative drainage network over a period of more than ten years (20 years in Kenta), though referring to dry land as well as wet. Finally, one could have added a third phase to Figs. 62 and 63, showing the swamp totally drained and gardenized, with a labyrinth of major, block and plot ditches.

7-7-2 SIZE OF BLOCKS AND PLOTS

The size of individual blocks found in swamp gardens follows the pattern for dry land blocks described in the "individual gardens" and "communal gardens" sections above. One can see from the above swamp garden model that the block pattern depends on two factors: spontaneous development gives an irregular pattern and is the result of individual activities comparable to those described in the "individual gardens" section; planned management gives a regular lay out and results from a group activity similar to the one described in the "communal gardens" section.

But however the swamp gardens are made, the pattern follows the general "communal" line; individual gardens are not isolated but clustered to form a final pattern of individual blocks within a communal garden.

The tendency is for people to have blocks in different locations and types of soil. Consequently the amount of land

cultivated in swamps is strongly related and complementary to the amount of dry land cultivated.

The size of plots is also the same in individual or communal gardens, the only difference being that there does not seem to be much experimentation with new block patterns in the swamp. Two swamp gardens, one located in Kum and the other in Kuning Tip, had enormous plots. They have a strong similarity to gardens made by Tari immigrants, especially in the very swampy illegal settlement in Tibi Residue. I suspect that these two gardens belong to or at least were made by Tari settlers, although it was stated that they were made by Kawelka, not convincingly, however, since foreigners settled at Kuk are now "Kawelka".

It was observed in Tibi Residue that Tari settlers have a different gardening pattern. The gardens are much bigger, with a single deeper and wider surrounding ditch difficult to cross. Within these big "islands" blocks are allocated and huge mounds are raised. Direct observations in the Tari-Koroba region have confirmed that this pattern is traditional, there too "islands" in the swamp (particularly at Haibuga) do not have raised mounds during the first planting of sweet potato which are grown very close to the water table, with mounds appearing during the second planting.

Regarding the Hagen square plots, I noticed that in the swamp they follow the same size dichotomy as on dry land: they are bigger for mixed crops than for sweet potato. Swamp gardens are a copy of the gardening pattern found on dry land, with the exception of the ditches themselves (compare Plates 6 and 24).

Although I never saw the "plot displacement" technique practised in the swamp, I was told that it occurs. This is,

however, suspicious information due to the relative instability of swamp plots (erosion, shrinkage) where the dryland practice illustrated in Fig.59-61 could jeopardise the whole system. This instability is reflected in the numerous plot and some block drains connecting with other ditches through a tunnel. Tunneling leads to greater stability than the open connections found in drylands. (See section 7-8-4).

In contrast to dry land gardens, the bottoms of ditches in the swamp generally does not follow a distinct soil layer. Often they cut through the top peat layer and consequently previous garden patterns can not be readily picked up as they were in the dry land situation of Fig.56. But when walking along these plot ditches one can sometimes notice that at regular intervals the soil underfoot collapses; by plotting these occurrences a grid pattern emerges and indicates the presence of some sort of prehistoric gardening activity. It also suggests that these prehistoric channels are still operating as drains in that water is oozing from them.

7-8 DITCHES

Ditches are found all over Kuk, in dryland as well as in swampland, and have some formal and functional differences. Some functional descriptions of ditches were given above and need clarification. In the following I intend to define the different types of ditch found in Kuk, both dryland and swampland, taking into account the formal and functional parameters.

7-8-1 DEFINITIONS

Considering both formal and functional aspects, five types of ditch were observed. As will be discussed below, some types

are characterised by their form, some by their function and others by formal and functional differences and/or similarities. The following ditches are defined:

- A Major Ditch is the deepest and biggest, having the most important drainage function and its outlet will be a natural stream (creek, river), or swamp (Plate 21);
- A Tributary Ditch terminates in a major ditch and is second to it in size and drainage capacity (Plate 22);
- A Boundary Ditch surrounds gardens and should terminate in a tributary ditch (Plate 23);
- A Block Ditch should be enclosed within a boundary ditch and should terminate in it;
- A Plot Ditch is the smallest of the ditches made in the Hagen region and should be enclosed within and terminate in a block ditch (Plate 26).

In theory, there is a decreasing importance in form and function from the major ditch to the plot ditch. Some are found only on dryland, some only on swampland, others are present in both swampland and dryland.

7-8-2 FORMAL ANALYSIS

Contemporary ditches almost always have vertical walls and flat bottoms, whatever their size. Very few ditches have their top width greater than their bottom width and none has a "V" shape, where walls are joined together at the bottom of the ditch. Those having the bottom slightly narrower than the top are encountered among major, tributary and boundary ditches. This formal distinction has no functional meaning; it is the result of individual enterprise and, as explained by many Kawelka, of laziness.

Almost all the ditches having vertical walls and flat bottoms, a formal classification is made according to measurements of width and depth of ditches.

In dryland, three types are found: boundary, block and plot (Table 38). Boundary ditches are found surrounding individual and communal gardens and very often are part of the ditch and fence enclosure system. They are big and dug deep into the hard basal clay: the width varies from 50 to 120 cm and the depth from 80 to 100 cm (Tables 40, 41). As a general rule, the wider they are, the deeper they are. Outlets of boundary ditches are a major ditch in the swamp or they may connect directly with it.

As described during the discussion on individual and communal gardens, there are no differences between block and plot ditches located in dryland, so that they cannot be distinguished in the formal classification. A clear formal distinction between them appears when block ditches are used as foot paths: they become wider and their bottoms show signs of erosion not present in plot ditches. Both types are barely dug into the clay. Plot ditches connect with block ditches and sometimes with boundary ditches; block ditches almost always connect with boundary ditches. The depth of block and plot ditches varies between 20 and 40 cm, the depth of the topsoil, while their width is between 30 and 50 cm. When used as a foot track, a block ditch can be up to 100 cm wide, and even wider but in this case it is difficult to see it as a ditch but simply as a major track.

In swampland the pattern is different. Major ditches receive all the other ditches and at Kuk they connect with the ditches of the plantation. In other places in the valley, they

have natural outlets in uncultivated swamp, creeks or rivers. Major ditches are dug deep and people have a tendency to reach and follow a harder soil such as a clay layer, if present. Their width varies from 100 to 200 cm, and their depth from 120 to 220 cm. Because of the silting and weed growth in them they often look less impressive than they are: once I found myself waist deep in a ditch I thought was shallow.

It is often hard to distinguish a major ditch from a tributary one. It is at their junction that the difference is clear: tributary ditches are shallower and narrower and fall into the major one; often this junction is made at right angles. When two major ditches join each other, it is often at a narrow angle and there is no fall at the junction; they have a similar depth and width. Drinking and washing water points are often found at the fall from a tributary ditch into a major ditch; in these cases the tributary ditch is kept clear from human (children) and pig disturbance. The fall can be anything from 10 cm (in the swamp) to 200 cm (in Prkl, where a dryland boundary ditch joins a creek). The width of tributary ditches varies from 80 to 150 cm, their depth from 100 to 150 cm.

A block ditch is also defined by its smaller size when compared with a tributary one and it is best seen at its junction with its outlet. Furthermore, this is often accentuated by the tunnelling of the former into the latter. Width and depth vary from 30 to 100 cm, but the upper limits are rarely reached. Outlets are more often at a tributary ditch than a major ditch.

Plot ditches are the smallest feature found among swamp ditches. In a continuous garden starting in dryland and ending in swampland, one can see that plot ditches keep the same width all through their length while the depth is greater in the swampy

section of the garden (cf. Fig. 55). At other times plot ditches in swamps are wider than their dryland counterparts but narrower and shallower than the swampland block ditches. The width and depth of plot ditches are respectively 30-50 cm and 30-70 cm. Their outlets can be any of the above three other formal swampland ditches, with a preference for block ditches. Formally, there are no major or tributary ditches in drylands, while there are no boundary ditches in swamplands.

7-8-3 FUNCTIONAL ANALYSIS

A functional analysis of the Kuk ditches gives interesting differences when compared with the above formal study. On drylands, only the boundary and some block channels might be considered as operating as drains. Boundary ditches located in drylands are the same as those described in the "formal" section. They have the function of draining off water from dryland gardens, particularly after rain. But one must remember that rain water remains in the top soil layer and does not penetrate the clay layer underneath it. Consequently, it would be enough to scrape the top soil down to the bare clay to achieve this, the action of digging deep into the clay not being necessary for drainage purposes. Encircling garden sites, they are, together with a fence, an artificial barrier against pig intrusions and destruction rather than "drains". The drainage effect is secondary, complementary to the "barrier" function.

Dryland block ditches are in a similar difficult position. Their first function aspect is to carry off the surplus of water from cultivated blocks. They receive plot ditches and are connected with boundary ditches. But block ditches surround garden blocks and in communal gardens they often have a very

zigzag run from the top of the garden down to the outlet (see Fig.57). Following the top of the clay layer, they do have the marginal function of carrying away rain water, while their major function is as block boundary marker and to raise adjacent garden beds. They also can be seen as not having a primary drainage function.

I do not consider plot ditches as having a functional drainage role. While they can be interpreted as having a plot drainage function, field measurements of ditch levels show some contradictory results. Some ditches have a slight slope down towards an outlet, the result not of man but of the natural configuration of the clay layer. Numerous plot ditches recorded with a theodolite in Kukrumdi, where there are some very flat drylands, did not have any slope between their ends. They are level and cannot be seen as having a drainage function.

Furthermore, the block found in a communal garden which has not outlets at all (mentioned in 7-6-1) emphasises the non-functional identity of plot ditches as drains. The water trapped after rain in a corner of that block is not removed but forms a useful source, near the house, where the owner can temporarily wash her children and vegetables.

Block ditches in swampland are difficult to interpret functionally and could not be represented in this classification. Some block ditches function as boundary, tributary, and major ditches. They effectively carry away water, often permanently, and consequently are classified as having a drainage function.

Other block ditches have no functional differences from plot ditches although there are formal distinctions. Functionally they all are classified as plot ditches. There is stagnant water in them and it suggests that these plot channels, like the

dryland equivalents, do not have a primary drainage function but one of raising the garden bed, resulting in lowering the water table level. The bottom of these ditches being quickly silted, it was difficult to take accurate levels to check the slope. The few measurements taken in new or newly cleaned plot ditches also revealed some with a lack of slope. Only few measurements being taken, it is dangerous to generalise but still tempting to see a strong functional relationship with block and plot ditches located on dry land. Furthermore, the mounding practice in swampland observed in Tari, Koroba and Kandep suggests similar conclusions, though more data are necessary.

Foot tracks in swampland are found along the plots where one has to jump from one plot to the next; the track network follows a pattern similar to dryland tracks, from major tracks along major ditches, then along tributary and block ditches to reach individual block allocations. The major difference between dryland and swampland is that one walks in the bottom of block ditches in dryland while one walks along them (on top of raised beds) in swampland. In Kuning Tip there is a track which is permanently beneath the water level of a swamp, where one has to perform the difficult exercise of walking on large logs tied together, which are not only slippery but invisible, so that one needs a guide. This was also observed around Tari. Sometimes swampland tracks follow the bottom of ditches (in drier areas) and in many cases I observed that tree branches and twigs were put down there to consolidate the soft path.

In conclusion, one can say that there is a behavioural relationship between dryland and swampland gardens: the need to have "enough" gardening soil above the clay in dryland and above the water table in swampland. This seems to be the major function of agricultural channels in the Highlands today, drainage being of primary concern only in swampland through the network of major ditches.

7-8-4 BLIND DITCHES

One type missing from the Papua New Guinea Highlands repertoire of ditches is the "blind ditch", a ditch having no outlet at all. From air photographs of the Baliem Valley in Irian Jaya (Elsmore 1945:676; Holmes 1953:14; Heider 1970:37; Robert Mitton's private collection), it seems that blind ditches are prolific in swamp gardens there. They seem to be present only in the swamp and suggest a technique of raising garden beds. The effect is a reduction of outlets resulting in a stabilization of garden blocks. The practice certainly avoids the erosion and collapse to be expected at outlet points. Such ditches probably also are a reserve of mulch and good gardening soil which must inevitably build up through silting and weed growth.

The closest similarity at Kuk to the Baliem blind ditches could be the technique of tunnelling for the outlet of block ditches in swampland. There are no published reports, from contact times till the present, of blind ditches as such, nor have they been found archaeologically. However, during the consultation of files kept at Kuk ARS (thanks to B. Rowson, Manager), the following account was found in a letter from A. McGrigor, who as an officer of the Department of Primary Industry (formerly Agriculture, Stock and Fisheries) was responsible for the drainage of many Western Highlands swamps, including Kuk, about the Kindeng resettlement scheme located in reclaimed swamp in the Middle Wahgi:

The construction of "blind drains". This carry over from traditional cultivation practices has no merit under swamp conditions. Having no outlet, these channels fill with water following rain and may remain full for up to a day. All drains no matter how small should be made to discharge water not store it. (A. McGrigor, 20/3/1969)

There is a strong similarity between these Baliem and Kindeng blind ditches and one certainly cannot look upon a diffusion or acquisition phenomenon from the Baliem. Both these areas have high population densities relying on gardens located in reclaimed swamps. At Kindeng it could be, as suggested by McGrigor, a revival of a technique known previously.

While little more can be added in the context of this research, blind ditches have to be kept in mind since prehistoric intensive swamp gardening practices of great antiquity have been found in the Highlands (e.g. Golson 1977a, 1981a).

7-9 ARCHAEOLOGICAL HYPOTHESES

From the above gardening activities observed at Kuk, many points can be raised concerning their archaeological potential and interpretation. Although there are no archaeological dry land garden sites as yet recorded in the Highlands, these data may be presented as hypotheses for future research and testing.

It seems there is a strong relationship between dryland and swampland agricultural behaviour, and for the latter we have archaeological data from the Upper Wahgi (Allen 1970a, Golson 1977a, Harris and Hughes 1978). Consequently, the above drylands data, and the archaeological findings which follow, could be of interest in defining similarities, differences and changes throughout the phases known from the Wahgi swamps. If this seems dangerous or ambitious, one must stress that because of the great lack of artefact and pollen remains from the excavated swamps, it is a worthwhile exercise.

A spatial analysis of the ditching network at a macro-level, taking into account formal and functional parameters of the biggest ditches found, could give clues in the understanding of

land management and development of gardening activities. In dryland, the successive additions of individual gardens from one period of occupation must give a pattern distinct from communal planning: numerous short but deep ditches criss-crossing the landscape opposed to fewer, long and deep ditches. In the swamp a similar behaviour should be found with an irregular juxtaposition of connected ditches opposed to a duplication of regularly connected ditches having similar size, shape and length.

It is through a micro-level analysis of these archaeological features that one should be able to clarify human behaviour and needs, the scale of garden units, the crops grown and the problem of intensification. Here an examination of dryland patterns compared with swampland patterns should stress their relationships: one might define dichotomy, complementarity, similarity or exclusivity between them. Garden units could be defined down to block and plot levels. In the New Guinea Highlands, direct correlations with ethnographic analogies could reveal things such as:

- Individual and communal gardening behaviour through features located on both sides of deep ditches;
- Size of blocks to estimate areas individually owned;
- Size of plots for changes in gardening practices and crops cultivated;
- Ditch and fence enclosures as indicating protection against pigs not humans;
- Sedentarization through communal tasks and "plot shifting" practice;
- Major mixed crops (banana, sugar cane) distinguished from any other crop through the presence of post hole concentrations;

- Taro cultivation in bottom of ditches or on plots through regularly spaced planting holes;
- Basins in ditch bottoms not being always made for planting purposes.

CHAPTER 8
GARDENS - ARCHAEOLOGY

Different strategies were applied in garden site excavation with the aim of testing remains left in different micro-environments. It was decided that tests would be made in a variety of slope situations, from flat land to steep slopes; trenches would be opened in land gardened since recolonisation and in land never gardened in historic times but left as pig grazing areas; finally, on swamp margins, trenches would be excavated from the wet up into the dry ground.

It was expected that through all these excavations a better understanding would be obtained of human (and possibly pig) activities on the landscape: to what extent observed remains are similar in different environments; if archaeological features and artefactual remains allow a reconstruction of human behaviours; if changes in dryland patterns occur throughout prehistory and what are their behavioural implications (technological/social change; finally, a direct comparative analysis with prehistoric swamp data available which could give the information necessary for the understanding of land management and behaviour of agricultural societies of great antiquity (Golson 1977a).

For reasons of finance, equipment and the like, it was decided that no excavations would be made in swamplands, on which more data are already available (Allen 1970a, Powell 1970a, Powell 1970b, Golson 1977a, 1977b, 1981a, 1981b, 1981c, Golson and Hughes 1977, Harris and Hughes 1978). In contrast, conclusions about prehistoric dryland agricultural systems were still only hypothetically based (Powell 1970a, Golson 1977a). Here was reason enough not to excavate more in swamplands but

to concentrate on dryland sites, partly as a test of all the hypotheses advanced.

Since most of the archaeological features would be imprinted in the clay layer just underneath the top soil, the difficulty was that an indecipherable network of ditches and other features would be found. While there is a tendency in the swamp for an agricultural phase to be sealed by a layer of clay, volcanic ash, soil or peat before another phase is found (Golson and Hughes 1977:16), in dryland no such deposits were to be expected between two phases, but a direct superposition of one above the other. If swamp patterns extended on to dry land, it might be that successive phases could be recognised at junction points, as well as through major technological changes in garden features already recognised in the swamp and through characteristic volcanic ashes filling channels of different phases. The location of excavated garden sites is shown on Map 26: they are all adjacent to small marginal swamps and to the big Kuk swamp from where most of the known archaeological data has come.

8-1 SITE MNU

Nowadays, many ridges in the Wahgi are uncultivated but are regularly burned grassland areas which show on their flanks numerous linear marks from previous gardening. This is the case all over Ep Ridge, where regularly spaced downslope features are found up to the highest grassland locations. Only in a few places are contour or transverse features also seen. But just after grass burning and before its regrowth, a clear pattern of regular plots of the classic "grid" type emerges: contour and downslope ditches are articulated and the ability to recognise this is a direct consequence of the burning and exposure of

surface ground. In Kuk, it is not known who made these gardens, the answer being invariably "the ancestors".

Site MNU is located in Prkl Robri on the flank of a spur leading towards the top of Ep Ridge; it had a low grass cover. Prior to its excavation, a pattern of regularly spaced downslope lines suggesting gardening activity could be seen from a distance. Since recolonisation, the land had only been used for pig grazing.

Due to the presence of a small swamp at its foot and the varying slope of the ridge flank, the site was divided into three units. Trenches were dug 50cm wide, down into the clay layer (a few centimetres) and to various lengths. Overall, 26 m² were excavated.

8-1-1 MNU/1

Unit MNU/1 (Fig. 64) was excavated near the top of the spur, at its steepest location: a bearing of 30° was recorded with an abney level. Some 5.75m² were excavated by the form of a contour trench AB (6 m long) and trench BC (6m long) set at right angles to AB and downslope.

After grass clearing, no features were visible on the surface of BC while there were clearly two parallel features cutting through AB and continuing downslope. On oral history grounds, they are presumed to be prehistoric. The excavation trenches were cut into the clay subsoil and, as can be seen in the cross-sections of Fig. 64, the stratigraphy was simple, consisting of a top soil some 20-25 cm thick overlying a yellow clay.

The excavation of AB confirmed the presence of the two features, which cut into the clay with the bottoms eroded and not flat. They are interpreted as two prehistoric garden ditches located 4.8m apart and running down along the maximum slope.

The western feature, MNU/1-1, lacked top soil at its base and erosion resulting from rainfall was noticed. The eastern feature, MNU/1-2, showed a thin deposit of top soil in the base and overall a more stabilized aspect than MNU/1-1. Nevertheless, the two features are still function as drains, as observed when it rained during their excavation. The excavations did not reveal any more features or any artefacts. The substantial thickness of top soil for such a steep previously gardened slope was, however, noted.

8-1-2 MNU/2

This unit is located 30m below and 10m westward of MNU/1. The slope is much more gentle, only 16° . Three trenches, overall of $5m^2$, were opened at MNU/2 (Fig. 65), AB and CD being each 5m long and running downslope; they were cut in their middle section, and at right angles, by EF which is 3m long. No features were visible on the surface of MNU/2 after grass clearing.

On excavation the stratigraphy (Fig. 65) was seen to be similar to that found at MNU/1. No artefact was found, but for two parallel features, 2.5m apart, oriented obliquely across the slope. They were both dug slightly, and irregularly, into the clay and were filled with compact top soil. The northernmost feature was labelled MNU/2-1, the other MNU/2-2. As with the previous unit, a substantial thickness of top soil was observed in MNU/2, increasing southward (downslope).

8-1-3 MNU/3

Trench EF of unit MNU/3 is located 55m below point C of MNU/1 and down the maximum slope from it. Some $15.25m^2$ were excavated in various trenches as illustrated in Fig. 66. No

features were visible after grass clearing and prior to excavation. MNU/3 is located on the swamp margin right at the bottom of the spur flank on a rather flat area (7^0). Trenches EF and MN were opened first and towards the middle of MN the water table level was reached.

The profile of EF (Fig. 67) shows one feature to the east (MNU/3-3) and a complex double feature to the west. Because of this latter, extension trenches were opened above (AB, CD) and below it (GH, IJ). It was found that there were two ditches, clearly separated in AB and CD, which coalesced in EF to form a single drain line in IJ. The critical points were in EF and GH where a close examination showed that the westernmost ditch in AB (MNU/3-1) was made later than the other (MNU/3-2), with which it came to form a single ditch dropping straight into the swamp. This suggests that above the junction point, the earlier ditch was no more in use when MNU/3-1 slightly but deliberately changed its straight course to follow the line of the previous ditch. This does not necessarily imply a different agricultural phase; it could simply mean a reshaping of garden ditches during the same phase. Similarly the very narrow angle separating these two ditches suggests that one of them is superfluous. MNU/3-3 is located some 3.5m east of the junction point of the other two ditches in trench EF.

New layers appear in the stratigraphy of MNU/3 trenches. There is a very thin (up to 3cm) gravelly layer overlying the clay west of MNU/3-1 in trenches AB and CD, which is also found in EF but located east of it; it extends as far as MNU/3-3. This gravelly layer disappears as the ground becomes wet and swampy (in GH, IJ, MN). In EF another layer composed of a mixture of clay and top soil was recorded above the gravelly

layer and distinct from the topsoil itself. In the field this was interpreted as showing the relationship between MNU/3-1,2 and MNU/3-3, that they were operating at the same time. This seems not to be a valid argument. The presence of clay mixed with top soil could well result from the making of the ditches themselves, which at this lowest part of the spur flank are deeper: not only in view of the top soil thickness but also the depth dug into the clay. This latter evidence also suggests that the gardens extended into the swamp; otherwise the ditches would shallow before disappearing into it.

Because no features were found in MN, trench KL was opened, equally with no result. Altogether, over a downslope length of 13.5m, there was no evidence of contour ditches in unit MNU/3 (Fig. 68). The gravelly layer encountered in MN does not exist in KL. Fig. 68 shows a thickening of the top soil towards its lowest part.

Features found in MNU/3 do not show signs of erosion of the clay at their base, which suggests a quick infilling with the surrounding top soil, probably naturally.

8-1-4 INTERPRETATION

Features MNU/1-1, MNU/1-2, MNU/3-1, MNU/3-2 and MNU/3-3 seem to be related and functioned to drain excess water during rainfall, raising garden beds and to a certain extent controlling erosion. In this context, one could also include features MNU/2-1 and MNU/2-2 which also run downslope even if they are less steep. Not a single contour ditch was found in the 24.5m of trenches opened down the natural slope. This could mean that MNU had a linear garden pattern composed only of downslope ditches. It could also be, and more likely is, that there were contour

ditches, thus forming a grid pattern, for which their base followed the surface of the basal clay. Through a natural process, obviously fast in gardens located on slopes, these contour ditches must be filled in rapidly, while the downslope ones are filled at a slower rate, suffering more erosion damage.

In recent gardens, it was observed that at higher altitudes (such as Buk), in a slash-and-burn agricultural environment, Melpa people have the choice between two techniques: a "grid" one where downslope ditches are relatively close spaced and dug in the clay with contour ditches rarely reaching the clay, opposed to a "mound" technique where there are no ditches apart from the boundary ditch and fence enclosure; in a very few cases seen on both sides of the Wahgi-Jimi Divide and around Buk, some of these gardens had one or two small downslope drains. With these analogies, one can assume that MNU is a prehistoric "grid" garden where the contour ditches cannot be recognised because they were not dug into the clay.

One must also point out that the earliest aerial photographs available for Kuk (1955) show Ep Ridge much less forested than it is today. It is probable that at one stage Ep was an almost completely grass covered ridge and that prehistoric grassland gardens have been invaded by woody regrowth. All features from MNU being apparently interconnected as a system, it seems that there has been only one agricultural phase, and that not long in duration, in that area of Ep Ridge; or that any other phase is missing archaeologically.

It is not known when and for how long MNU was in activity. But a remarkable aspect is its lack of erosion, even in its steepest section. Downslope ditches have little changed through time (at least 45 years of abandonment) and contour ditches have

been filled with top soil, probably through natural slope wash. Only one, unexcavated, downslope ditch showed signs of erosion, its base being irregular and sometimes 60cm deep in the clay. It could have been a boundary marker. It was also noticed that prior to excavation it was used as a pig track and that during excavation it was the easiest way for the excavators to move from one unit to another: all this could also be a cause for its erosion. Under the grass cover, nothing suggested the presence of a ditch and fence enclosure system. MNU could well have been part of a bigger communal garden.

8-2 SITE MNV

Site MNV is also located on the flank of an Ep Ridge spur, but in Kuning Emdel. It is on the circle of hills surrounding the Kuning swamp. From a distance one can see a modern ditch and fence barrier all along the flank and on the ridge top it extends eastward then southward. This barrier is a sub-clan boundary (Mandembo clan) to keep out rooting pigs, one clan settled all around the Kuning swamp, the other settled south of it in Kuning Meten and Kuning Kaklong. There is no gardening at all on MNV or any of the surrounding spurs and there has been none since recolonisation. It is all low-grass country.

From a distance one can also see a scar, looking very much like the modern barrier (minus the fence), which is almost parallel to it but located higher on the spur flank. It was inferred that it is the remains of a similar functional feature, but prehistoric. These two barriers slope down slightly; they both start on top of the spur (around 1770m A.S.L.), follow its flank, finally to turn sharply to swamp level (1580m A.S.L.), just before the spur itself does. Overall the features are more than

one kilometre in length. Many linear marks, running straight downslope and similar to those described for MNU, are also seen on both sides of the prehistoric feature, suggesting gardening activities. At one location, where the spur flank curves and has its steepest slope, such features are totally absent. On the other near the top of the ridge above the barrier (some 1720m A.S.L.), a grid pattern seems to emerge at a rather steep section. Because of the archaeological potential of this flank, it was decided to excavate four units, each of them at strategic points as described above. Overall, 46.5m^2 were excavated.

8-2-1 MNV/1

This unit was opened where contour and downslope marks suggesting grid-type gardening activity had been observed from a distance. Two trenches were opened (Fig.69, Plate 27), AB straight downslope and 10m long; and BC, 20m long, a contour trench at right angles to AB. After grass clearing, two features were visible on the ground surface and immediately interpreted as "downslope ditches". The depth of trenches was one metre, well into the hard clay. The slope angle was 35° .

Not a single feature was found in AB, where an uneven clay layer underlay a topsoil some 15-25cm thick. The features observed at a distance and inferred to be contour ditches not only did not penetrate the clay but were not even seen nor felt after grass clearing.

In trench BC, three features were recorded, but two visible on the surface (MNV/1-1 and MNV/1-3) and feature MNV/1-2 (Fig. 69). They were all dug 15-16cm into the clay and filled with compact topsoil averaging 17cm in thickness. As for trench AB, both top soil and clay surfaces in BC were undulated. The three features

are interpreted as small downslope ditches fairly regularly spaced, 5.3m and 4.9m apart.

By doing a simple arithmetic exercise and looking closely at the stratigraphy around 5m south of MNV/1-3, it was found that 5.6m away there was an undulation of the clay surface which was different from and more accentuated than the others. It is feature MNV/1-4 which could be interpreted as another downslope ditch, although it is shallower and wider than the three others.

From a distance a landscape "anomaly" was observed just above trench BC. It was suspected to be the possible remains of some sort of boundary marker, a trench or/and a fence. Consequently, an extension trench 4m long and one metre wide was excavated down to the bare clay. Nothing suggesting a former ditch or fence was recovered, but there was a sharp change in the slope profile. This is interpreted as a natural phenomenon of the spur.

8-2-2 MNV/2

Unit MNV/2 is located on the steepest part of the spur flank where nothing is seen even from a distance. One contour trench 8m long was opened and at right angles to it another trench, 6m long, was dug downslope. Ten metres above the junction of the two trenches, a third one was opened uphill (6m long). The pre-historic barrier runs between the two trench units and is clearly felt when walking on it: it is a narrow but flat strip of land contrasting with the steep slopes surrounding. Overall 10m² were excavated in MNV/2, where the slope angle recorded was 43°.

Not a single feature was found, which seems to confirm the lack of gardens in such a steep landscape. The other interpretation would be that any gardens made there have left no remains.

During my fieldwork the steepest garden at Kuk was observed in Prkl Rombil on a 38° slope. It was a mixed garden dominated by sugar cane and edible pitpit. There was no preliminary ground preparation outside grass clearing, not a single ditch was made and only a pitpit fence was erected against pig intrusions. Probably very little in this garden had an archaeological potential. Using these ethnographic analogies it is possible to say that if in the past a garden had existed in MNV/2 it could well have been of the type seen in Prkl Rombil.

The stratigraphy of MNV/2 was an uneven clay surface above which was an uneven topsoil. The thickness of the latter, 7-20cm (averaging 15cm), raises the question of erosion: on such a steep hillside, frequented by pigs, one would have expected to find something approaching an eroded bare clay, instead of this topsoil thick enough to start a garden. While the excavation of this unit confirmed the landscape observations and impressions, one must remember the total failure of the excavation of site MNW, which is discussed in section 7-2-2 above, where from the observations of micro-environment details one would have expected to find much more than a single post.

8-2-3 MNV/3

Some $12m^2$ were excavated in MNV/3 which is located on a 20° slope, across the prehistoric barrier at a point where this runs across the slope. Trenches were set along and across the flat strip of land as shown in Fig. 70. Two features were found, MNV/3-1 and MNV/3-2, whose cross-sections are presented in Fig. 71. Some 10m above the site, a recent small platform, said to have been built by children, had a fireplace and a few cooking stones.

Feature MNV/3-1 is the prolongation of the flat strip

observed in unit MNV/2 and is part of the barrier observed from a distance. It had a clay exposure on its embankment (eastward) which showed signs of erosion. An extension trench was opened towards point "A" on and above the embankment, to examine the stratigraphy and look for the remains of a fence which could well have been erected there. Nothing was found in or above the clay.

An extension westward, towards point "B", was opened to record the stratigraphy of the site. Feature MNV/3-2 was found, not having been visible on the surface. It was interpreted as a contour garden ditch. Its location is rather close to the barrier platform, only 2m away. Its base just reaches the clay.

The stratigraphy of the site shows a rather thin top soil above the embankment with no traces of clay spoil from the making of the scarp and platform. The platform itself had a little soil deposited on it, probably a result of wash-in from above. The topsoil extends downslope and completely fills feature MNV/3-2. On both sides of this, the basal clay is covered by very thin layers of top soil and clay before the surface topsoil is found. These thin layers could be spoil from the platform. With a scarp some one metre high, it is possible that the spoil was not piled above it but thrown down slope. But this would not be a difficult obstacle for pigs.

From modern examples, it is more likely that the spoil was piled just above the scarp and a fence of pitpit and tankis was built in it. All these evidences would be gone archaeologically. This type of barrier would be more difficult for pigs to cross. As a possible testimony of this loss information could be two cordylines still growing, located well apart but just above this prehistoric barrier.

It seems that contour fences were in prehistoric times made by slicing away a section of the slope, with the spoil piled above and a fence erected in it. Ethnographically, a similar barrier is made by means of a ditch, above which the spoil is piled and a fence erected (Fig. 73). This could well be a direct result of the technological change from wooden implements to steel spaces.

It seems that feature MNV/3-2 postdates the building of the barrier, since it cuts through its spoil, but there is no evidence for a temporally distinct phase. The garden could be directly associated with the fence, erected to keep out pigs. A few small cooking stones were found on the surface or in the upper part of the topsoil below the embankment. They probably come from the modern platform made above the site.

8-2-4 MNV/4

Unit MNV/4 is located near the bottom end of the prehistoric barrier and across it. At this point, the barrier has turned sharply to become a downslope ditch which nowadays is connected with the artificial ditching network of the gardens located in the Kuning swamp. Some 5.5m² were excavated and the cross-section of the site is presented in Fig. 72.

Prior to its excavation, it was obvious that there was active erosion from rainfall running down the slope (16° at MNV/4), with the ditch widening through bank collapse and deepening to give a "gully" aspect. The clay was exposed on one flank. Its transformation from a flat narrow platform to an eroded ditch penetrating the clay already starts before the downslope bend and consequently it is hard to say both whether it was deliberately made and where it started. From ground observation, it seems that the flat contour platform ended almost at right angles to the downslope

ditch, the latter deliberately made and starting at this junction point. It is impossible to evaluate its initial depth and width.

As Fig. 72 shows, it has now a top width and a depth of more than 150cm. From ethnographic analogies, it could originally have been less than one metre wide and deep.

The stratigraphy of MNV/4 shows a compact top soil 25cm thick on both sides of the ditch, together with an unstable deposit of top soil in its bottom and over one flank. To one side there is a very thin layer of clay in the middle of the topsoil. This being irregular, it was not possible to conclude that it was part of the ditch spoil piled up for fencing. It is a possibility one cannot reject because it is located on the same side as the suspected fence described for MNV/3. Unit MNV/4 is the only one clear example of erosional activity for the whole MNV site.

82-5 INTERPRETATION

The suspected prehistoric barrier seems to be confirmed as such, not only from ethnographic analogies but also from archaeological evidence. It might also instance a correlation between technological change from wood to steel and formal change in contour barriers (MNV/3), but probably not in downslope barriers (MNV/4). As with MNU, one can say that overall little erosion has occurred in MNV, even on the steepest slopes which have been for more than 45 years grass-covered. MNV was gardened above the barrier (MNV/1) as well as below it (MNV/2 and MNV/3).

Similar conclusions as for MNU can be reached about MNV. It seems that there is a strong formal relationship between MNU and MNV: contour plot ditches missing, downslope plot ditches dug slightly into the clay, substantial top soil, no artefactual remains. Neither of these two sites shows signs of intensive

gardening activity, such as one would expect to find from people gardening around the Kuk swamp for more than 6000 years. There seems to be no overlapping of successive or different gardening phases, or perhaps only one of them has left archaeological evidence.

8-3 SITE MOA

Site MOA is located west of Guga creek, outside Kawelka territory on Djiga land. It is on the flank of a low-lying plateau overlooking the Kuk swamp to the east and Ep ridge to the north. Trenches were opened all along the flank in such a way that the bottom of the longest trench (Trench NSI, Fig. 74) reached southward to the water table level of a small marginal swampy creek which eventually drops into the Guga, while northward it almost reached the top of the slope. From this major trench, some extension trenches were opened to cover a greater surface. Overall 224 m² were excavated in MOA (Plate 28). The site was mostly grass-covered, with a few shrubs, and used as pig grazing land. It was said never to have been gardened within living memory.

Various objectives were in mind prior to the excavation of MOA. It was deliberately chosen outside Kawelka territory with two aims: there was no reason why in prehistoric times tribal boundaries would be similar to those of today, and second, the Guga creek seems to be a natural boundary marker which could indicate archaeological differences in land management. Finds could be similar to or quite different from those made within Kawelka territory, such as more intensive gardening or, on the contrary, "permanent" pig grazing land. The landscape suggested that the location of MOA would be very suitable for dryland gardening extending to the small marsh, with some sort of settlement on top

of the hill where an old bamboo grove was still standing. MOA borders on the Kuk swamp and could well have been integrated into the gardening systems found there. If this was the case, a dense network of ditches overlapping and obliterating each other was expected to be found. Finally, if there was at one stage a settlement on top of the hill, some artefactual remains discarded from there could be expected to be found on the hill flank.

Since the slope at MOA was more favourable for human activities than sites MNU and MNV, it was decided to modify the excavation strategy by doubling the trench width (one metre wide instead of 50cm). It was also felt that the two previous sites had not been sufficiently excavated and more of the flank would be examined here. Although there was an uneven slope at MOA, it could be seen as 13.5% overall (see levelling in Fig. 74).

8-3-1 RESULTS

Numerous features were found and they are all represented in Fig. 74. One can classify them as belonging to three distinct activities: housing, gardening and pig rooting.

At the junction of trenches EWI and NSI were found a hearth and a post hole filled with charcoal, while some 3.2m westward from the hearth in EWI was found a shallow curved feature interpreted as being part of the surrounding ditch of a house. The clay surface in all this area was levelled rather flat, and could be interpreted as the house platform. Elsewhere in MOA, it was found that the clay surface in various places had small disturbances which could hardly be interpreted as gardening features and even less as the remains of garden ditches. The feature found at the junction of NSI and EW3 is an exact copy of contemporary pig wallows and is interpreted as such.

A substantial topsoil was present all over the site, up to 32 cm thick, averaging 25 cm. Some finds were made in association with the hearth - one stone axe blade chip, cooking stones, charcoal, ash - but for the rest of the site, in the 224 m² excavated, only one small cooking stone was found. The water table level was reached in the lowest part of NS1 and at the junction of NS2 and EW2.

Several features interpreted as garden ditches were found in all trenches and none of them ran straight downslope parallel to the line of trenches NS1 and NS2. Except for the ditch located just below the level marked "100.3" in trench EW2, which was dug some 15-18 cm into the clay, no other feature penetrated the clay by more than 1 or 2 cm. They were often hard to define and it was suspected that other ditches were missed simply because they were wholly in the topsoil. Two ditches (one in NS2 and one in EW2) were not continuous and this was probably more accidental than deliberately made. Many of these garden ditches showed an irregular bottom full of closely spaced small holes suggesting digging stick activity.

All the ditches found are set at 45° (NW-SE) or at 135° (NE-SW) to the natural slope, being parallel within each set and at right angles to the other set. In eight instances one ditch joins another and does not cross it but terminates in it: four NW-SE ditches join NE-SW ditches and four NE-SW ditches join NW-SE ditches.

An analysis of parallel ditches shows that one can make two groupings among the NW-SE set: one group with ditches spaced at around 4.8-5.6 m, and the other with ditches spaced at around 3.6m. Some other ditches within the NW-SE set were noted as being at half the distance of the first group (2.4 m) and others at twice

the distance of the second group (7.2m). Similarly, the spacing of ditches in the NE-SW set suggests two groupings: one spaced at 3.6-4m and the other at 4.8-6m. There is no evidence remaining of the possible existence of the two other categories noted in the NW-SE set.

8-3-2 INTERPRETATION

The garden ditches located in the house platform indicate that the house pre-dates them. The same can be said for the pig wallow, which was cut by one garden ditch. No conclusions can be reached about other pig disturbances, which had no stratigraphic connections with other features. Very few artefacts were recovered in and around the house platform excavated and certainly no discards from the hypothetical house located on top of the hill were found. The site was conspicuously lacking in artefactual remains, whatever were the activities taking place there and their time depth.

The features found in MOA, if correctly interpreted as garden ditches, certainly do not illustrate an intensive use of the site for gardening purposes, unless the evidence has gone. What was found in MOA suggests the presence of a big garden (no boundary marker found) of the grid pattern similar to the recent sweet potato garden pattern in the area.

On the basis of the measurements given above, one could say that there were at least two distinct gardening phases in MOA: one with square plots of more than 25m^2 each and the other with smaller square plots of less than 16m^2 . Compared with the ethnographic data from Kuk, one can say that the latter phase is much more analogous to the present situation, while the former would be a garden pattern with rather big plots. It is not known if the

two phases are really different aspects of the same gardening routine or whether they are chronologically distinct and if so, which is the earlier. But because the smaller grid is closer to present pattern it is tempting to consider it as being later than the other.

Again relying on contemporary practice, it is probable that the earlier system includes the "plot displacement" practice (described in section 7-6-3) not only because of the halving of the space between ditches, but also because with a system of blocks less than 6m^2 , the ratio between cultivable raised plots and uncultivable ditch bottoms would not justify the labour input. Consequently, it is possible that there was a sort of intensification in gardening practice during the "big plot" phase. There is not enough archaeological evidence on the "small plot" system to be conclusive on this matter.

It seems that ditches were made by pointed digging sticks rather than by implements such as digging sticks with expanded blades, the so-called paddle-shaped spades. It seems that the gardeners did not want to dig into the clay but, as nowadays, only to remove the top soil and follow the clay surface. Because of this method, it is suspected that many ditches belonging to the two prehistoric gardening phases described above are now missing. Finally, one cannot speak of any sort of erosion having occurred at MOA since it was gardened.

8-4 SITE MOB

The other site excavated in Djiga territory is site MOB, which is on a hill located directly opposite and south of the same swampy creek bordering site MOA. In the case of MOB, the water table level was not reached and the northernmost part of trench

NS1 (Fig. 75) is still some 30m away from the swamp. The environment of MOB was exactly the same as that of MOA.

The preliminary assumptions made for MOB were similar to those made for MOA. I expected that what was found in MOA should also be found in MOB, that there should be strong correlations between the two sites. A landscape oddity was noticed along the small spur leading towards the top of the hill: a small platform, clearly observable, broke the profile of the spur and it could have been artificially made. It was decided to investigate it to find out if it was man made. My workers and I assumed that the platform would have been an ideal position to erect a house, nicely overlooking the surrounding landscape, and we were certain of finding house remains there. Various trenches were opened and overall 211m² were excavated.

8-4-1 RESULTS

As Fig. 75 shows, numerous prehistoric features interpreted as garden ditches were found, with a peculiar lack of them in the vicinity of the junction of trenches NS1, NS2 and EW1. As with the previous site, ditches were dug very little into the clay and many of them showed signs of digging stick activity. Here also ditches were set at right angles to each other between them but the three sets of ditches joining each other are clearly crossing and not terminating.

Spacing between ditches having similar orientation are less clear than for MOA. Ditches from MOB have a wider range of spacings and no pattern clearly emerges. Nevertheless, one could consider a spacing of about 4.4-4.6m as being more frequent.

No disturbances attributed to pigs were found and though the suspected house platform was indeed a platform it gave no evidence

of any housing activity. A natural rock outcrop was just emerging from the clay in the eastern part of it. The top soil had various thickness, from 10 to 35cm, averaging 25cm. Only one small cooking stone was found in MOB, together with numerous small unworked and unheated stones similar to the very soft rock outcrop located on the platform.

8-4-2 INTERPRETATION

The house platform inferred from topographic observations has not been confirmed archaeologically and it is an interesting warning against theoretical assumptions when dealing with human behaviour. Similarly, a reconstruction of prehistoric behaviours based on artefactual remains cannot be made when considering the two Djiga sites. The only evidences of previous human activities are the garden ditches, which would have been totally missing if they had not penetrated the clay by a mere 1-2cm.

The nature of the garden ditches found in MOB strongly suggests that they are only part of a greater number. They are not very visible and it seems that, as today, people wanted to remove only the topsoil down to the bare clay and to follow its top. Consequently, when operating, garden ditches could well have been regularly spaced, set at right angles to form a general grid pattern. This is not so evident when considering exclusively the excavated features and without any ethnographic knowledge of Highlands agricultural systems.

Considering the spacing between the ditches at MOB, it seems that there could have been quite intensive gardening. Various spacings were found and very small distances between some ditches could indicate a "plot displacement" practice from a single phase or a superposition of two more gardening phases. Looking only at

MOB ditches, it is hard to estimate the size of plots; but when compared with MOA, one can say that plots of 25m^2 and 16m^2 are also represented in MOB, but together with others. It is possible that there was greater gardening activity around MOB than around MOA, giving a more "anarchic" ditch spacing. If this was the case, one has to conclude that many features are definitely missing in MOB. Finally, formal ditch similarities between the two sites may demonstrate a strong cultural and temporal link between them.

The agricultural characteristics found in MOA as well as in MOB are grid systems with beds raised for cultivation, separated by unused ditches. The primary function of the ditches is as top soil supply rather than drainage. This could explain the numerous missing ditches and the total lack of them in one area of MOB. No deliberate action towards clay removal was taken and it is probable that when gardens were reverted to fallow land, ditches were naturally filled back with top soil erosion prior to any possibility of clay erosion.

8-5 SITE MOC

Site MOC is located in Kukrumdi, in an environment very similar to that of the two Djiga sites. It is on the flank of a low grass-covered hill looking southward over the swamp (almost fully drained and cultivated) which divides Mandembo and Kundumbo territories. The hill has never been gardened since recolonisation, although it is no longer pig land, being located inside the enclosure surrounding the whole of Kukrumdi. It is probable that MOC will soon be gardened for the first time, since the whole of the uncultivated land surrounding it has already been allocated to various Mandembo and future plot boundaries have been marked with bamboo poles.

Prior to its excavation, it was assumed that there would be considerable similarity between archaeological features found at MOC and those at MOA and MOB; the objectives of the excavation of MOC were therefore the same as those described for MOA (section 8-3). The main differences concerning MOC were that there was no obvious possible house site in its vicinity and that the swamp bordering it was much bigger and could influence its agricultural pattern.

Indeed, numerous evidences of prehistoric agricultural activities were found in this swamp (see "Korua's Garden", section 7-3-1, for further details). MOC being some 80 metres west of Korua's garden and on the same hill flank, it was assumed that at MOC there could be a continuation of prehistoric activities noted in the swamp and the recovering of artefacts during the excavation of MOC was expected. Some 132m² were excavated by means of trenches one metre wide, in a lay-out illustrated in Fig. 76.

8-5-1 RESULTS

All the features excavated in MOC seem to belong to gardening activities and are interpreted as garden plot ditches. They are sometimes articulated with and sometimes cut through other ditches.

Two sets of ditches emerge from a formal analysis. One set is dug very little in the clay, often less than a centimetre, and corresponds with the depressions felt on the surface (Phase 3, Fig. 76 and 77). The other set consists of ditches which are generally wider (30-40cm instead of 25-30cm) and actually penetrate the clay by some 4-5cm (Phase 2, Fig. 76 and 78). One ditch, oriented differently than all the others and with an unusual bend, and has been analytically isolated from the others (Phase 1, Fig. 76, Plate 32). It is narrow (20cm) and makes a clear impression in the

clay, being around 6cm deep. Finally, there is a wider and deeper ditch, 80cm wide and 30-35cm deep, running down trench NS2, with which are articulated the ditches assigned to Phase 2. Right at the bottom of its infilling were found one charred sweet potato and small casuarina twigs.

All the ditches have marks from digging sticks, their bottoms being irregular and not flat. The infilling was only topsoil and no dateable material was found. No distinct layers were present in the infill, although lumps of green and yellow volcanic ashes were scattered in the topsoil, especially in the upper part of the site. The top soil thickness averaged 30cm and reached 42cm in the lowest section of the site, where the water table was reached at the southern end of trench NS1.

8-5-2 INTERPRETATION

Because the features found at the surface after grass clearing and prior to excavation perfectly matched the features of Phase 3, it is concluded that this phase saw the latest agricultural activity at MOC. It was clearly a grid-type garden, with the bottom of the plot ditches following the top of the clay. The irregularly spaced small holes found in them, as in the ditches of the other phases, suggest the use of an implement like a man's heavy digging stick and not a broad-bladed wooden spade (e.g. Plates 29 to 31).

There is only one evidence of ditch articulation for Phase 3 (in trench EW2) but because of the discernible surfact evidence it was possible to reconstruct quite accurately the general garden pattern (Fig. 77). One must note that many ditches were totally missing under excavation and the surface evidence was vital for the reconstruction of the pattern. That pattern is one of rectangular plots ranging from 14m^2 to 25m^2 and averaging 18m^2 . It

corresponds to modern sweet potato plots. It seems that all the plots of Phase 3 are part of a single block and that the whole garden extended further in all directions. Figure 77 represents some 616m^2 of gardening. It is probably not enough to decide if the Phase 3 sample excavated at MOC is a section of a communal or an individual garden.

In three parts of MOC there is evidence of ditches of Phase 3 cutting through ditches from Phase 2, showing that the latter is earlier. Assuming that the sections exposed from Phase 2 are from parallel ditches, a hypothetical grid pattern emerges, with ditches regularly spaced (Fig. 78). Compared with Phase 3, these plots are bigger, ranging from 20m^2 to 46m^2 and averaging 33m^2 . But there are two distinct anomalies in this lay-out that one must take into account. The first one concerns the two ditch sections found in trench NS1 which do not have a counterpart in any other trench. They are illustrated with dotted lines. If they belong to the same lay-out, plots would be smaller, ranging from 9m^2 to 22m^2 and averaging 17m^2 . But one of these two ditches is clearly cut by a ditch from the same Phase 2. This supports the theory that they do not belong to the same garden lay-out. Because of formal similarities, however, they are included in Phase 2 and are interpreted as being the remains of an earlier garden lay-out from the same agricultural phase. This could illustrate successive gardening activities interrupted by fallow periods or it could be an example of the "plot displacement" practice. Whatever was the function of these dotted ditches, they are less visible in the clay and it is probable that other ditches from the particular lay-out are archaeologically missing.

The second anomaly in the plot arrangement comes from the bigger ditch found in Trench NS2. It is interpreted as a block

boundary ditch because of the articulation of other ditches with it, its formal differences and the discontinued offsetting of plot ditches on either side of it (see section 7-6-1 for ethnographic similarities). Boundary plots generally do not follow the plot size trend; they can be bigger or, as for Phase 2, smaller. So to find the average size of plots, one has to exclude these boundary plots. By doing this for Fig. 78, one has plots ranging from 30 m^2 to 46 m^2 , averaging 39 m^2 . These figures are clearly very different from those found of the later Phase 3. They suggest that there was a grid pattern agricultural phase, or the grid pattern was followed by another, also of the grid type, but with smaller plots. In the case of MOC, when one superimposes the lay-out of these two phases, one appreciates the extent of the discontinuity suggested by their plot size measurements. It denies the possibility of a short fallow period between them or a plot displacement practice creating Phase 3 out of Phase 2.

I am inclined to believe in a long temporal gap between Phases 2 and 3, long enough that people did not know the previous grid pattern and dug the new one on a different line, Phase 2 ditches coming more directly down the slope using more the natural declivity. Nevertheless, this temporal gap cannot be of more than a few generations because of the evidence that sweet potato was already cultivated during Phase 2 (assuming sweet potato only arrived within the last 300 years). Recently I have changed my views about the arrival of sweet potato in New Guinea, pushing it back to perhaps as early as 1200 years ago (Gorecki 1982b). Unfortunately I have not kept the sweet potato from MOC for C_{14} dating.

It is not known what crops were grown in MOC during Phase 2. Using ethnographic analogies, plot sizes suggest a mixed crop garden similar to those for banana and sugar cane cultivation. But the most convincing evidence is the garden lay-out itself, which

indicates the cultivation of a crop similar to sweet potato. One can also add that there is enough evidence to say that communal gardens existed during Phase 2, with the block division rather similar to the one found ethnographically.

It seems that penetration of the clay by ditches was avoided during Phase 3 and, as with all the phases, there is no sign of clay erosion. This is also indicated by the features found in ditches of all three phases and interpreted as being small holes made by digging sticks during the ditch digging. Even light erosion would probably be enough to obliterate these evidences.

The temporal position of the ditch labelled Phase 1 is unclear. It is certainly earlier than Phase 3 but its position relative to Phase 2 is unknown. It could be contemporary, earlier or later. From the evidence of other excavated sites discussed elsewhere, it is strongly suspected that this odd ditch belongs to a much earlier period than Phase 2. Looking only at MOC, I can merely say that the bend in that ditch was deliberately made and that it does not fit formally and spatially with the other ditches found.

8-6 SITE MNI

Site MNI is located in Kukrumdi and part of it has been described in section 6-5. The garden site excavated is located some 30 m due west of the cooking place, on a long, wide platform which could have been a singing ground in prehistoric times.

The site was covered with kunai grass and it was only a matter of days before coffee seedlings would be planted in it. So it was decided to excavate it in order to test various assumptions. On the ground surface the last garden grid pattern was to be seen, abandoned only two years earlier. It was also assumed that there could be the remains of previous garden pattern (and some artefacts),

from prehistoric as well as from historic times, the area having been intensively gardened for the last 15 years. Finally, it was thought that MN1 could be a prehistoric singsing ground and it was an opportunity to check that belief before the land was taken over by a permanent coffee garden, thus forbidding any future excavation.

It was originally planned to excavate a preliminary 104m^2 (Fig. 79) and to extend it if required by the first results. But the woman who owned the land refused, against the wishes of her husband who was one of my workers, even to allow the full opening up of the 104m^2 . We were allowed to dig along the drains of the last, visible garden pattern and to excavate inside the garden plots but with their perimeter left untouched. She wanted to have "strong" plots and was also afraid that we would dig into the clay, whose addition to the garden would impoverish the soil; this was a fear voiced by many owners of excavated sites. Following her request, only 66m^2 were excavated by a method which is not recommended.

8-6-1 RESULTS

The latest garden at MN1 was clearly to be seen; it was a grid pattern with small blocks of around 9m^2 each. Numerous sweet potato vines were still growing and small tubers were collected for lunch. The N-S ditches terminated suddenly and one metre further north and just on the edge of the site platform was a deeper and wider ditch running E-W, a boundary ditch. This system is called Phase 3 (Fig. 79).

The excavation method of directly following all surface features is not the best and it is possible that the men dug slightly deeper than they were. Nevertheless, except for the

boundary ditch, they all penetrated the clay by less than one centimetre and it is possible that without the help of the surface indications they would have been difficult to locate. These ditches had a width of 30-35cm. The boundary ditch was clearly dug in the clay. It had a depth of 30cm into it and a width of 70-80cm, with a "square" profile. Its bottom, like those of the garden ditches, was smooth and not full of holes. All these ditches were partially filled with loose top soil. Generally over the site topsoil thickness averaged 30-35cm.

Very few finds were made during the excavation of MN1 and all were along the bottom of the boundary ditch: five small cooking stones, two hard lumps of green volcanic ash, one stone file, one quartz crystal and eleven peanut shells. There was also one very thin flat crescent of stone with one whitish side, about which the men laughed very much, saying that somebody tried to make a fake "kina" shell, the most valuable item in prehistoric times. At the present nothing more can be said about this suspected "forgery".

Other sets of ditches found in MN1, also with flat, smooth bottoms, were, on the evidence of their articulations and crossings, classified into Phase 2 and Phase 1. They penetrated the clay to a depth of 3-5cm and had a width of about 25cm. These ditches extended northward and were cut by the later boundary ditch of Phase 3. Their infilling was compact topsoil.

Finally, three post holes were found (in squares E4, D7 and D9) together with a bean-shaped feature located in D7. These features are interpreted as being made by stakes to tie pigs, one of them making a wallow, as it is done today during ceremonial exchanges.

8-6-2 INTERPRETATION

It was said by the men that various sweet potato and peanut gardens, interrupted by short fallows, had been made at MN1 since recolonisation and that the boundary ditch had been dug as a settlement boundary prior to the first gardening activity. Some of the archaeological finds can be interpreted as belonging to Phase 3, which would indicate a modern sweet potato-type garden.

If the local information about the boundary ditch is correct, one has to assume that Phases 2 and 1 are prehistoric because of their ditches extend northwards across it. Ditches from Phase 2 have only two articulations and if no ditches are missing, garden plots of that phase could have been bigger than those of Phase 3: from the single piece of excavated evidence, plots could have been 6.4m along one side, 4m^2 if square.

The two ditches of Phase 1 are clearly the earliest found at MN1. They are parallel, 4m apart. The lack of transverse ditches articulating with them suggests a linear garden pattern, but it is possible that they are missing archaeologically. There are not enough data to be conclusive. In the field, some hypotheses were advanced, the most favoured one being that Phases 1 and 2 were contemporary, the two patterns resulting from the "plot displacement" practice. This is a question left open at this stage.

While it is obvious that ditches of Phase 3 were made by steel spades, the formal aspect of the ditches of the other phases, if prehistoric, suggests the use of wooden spades rather than digging sticks. Of the very few artefacts recovered, it is almost certain that the stone file and the quartz crystal do not belong to Phase 3 but to an earlier phase; they were probably uncovered during the making of the Phase 3 garden and then discarded in the

boundary ditch. The green volcanic ash lumps found were not stratified and could have been similarly discarded in that ditch. The cooking stones and the fake "kina" shell could be Phase 3 or earlier. The peanut peels are certainly contemporary with Phase 3.

8-7 SITE MNX

Site MNX was excavated just after site MN1 and a proper excavation technique could be adopted as distinct from the situation at MN1. The female owner of MNX did not make any opposition to the excavation and was, on the contrary, happy that we were turning the soil for her. At MNX the method was classic, with the use of a one metre recording grid.

MNX is in Kukrumdi, in the middle of the same big communal garden in which the Muk (section 7-6-2) and Rea-Nung (section 7-6-3) plots were recorded. The block in which MNX is located belongs to Mandi, who said that it was about time to recultivate it after some 18 months fallow. It was covered mostly with kunai, with little pitpit and a few small regrowth shrubs. Intensive cultivation of sweet potato, peanuts, corn and beans, interrupted by short fallow periods, had been carried on for more than 15 years at MNX. "Plot displacement" was said to have taken place numerous times.

The assumption made prior to the MNX excavation was that a mass of ditches would be found: not only from prehistoric phases but also from the successive ditch arrangements made in historic times. Some 117m² were cleared (roughly half the size of Mandi's block), over which a grid was laid and 34m² excavated (Fig. 80).

8-7-1 RESULTS

After grass clearing, the last grid-type garden pattern was

to be seen on the surface and sweet potato vines were still growing, with tubers harvested for lunch (18 months after abandonment!). That last garden was recorded as observed, with plots around 13 m² each and ditches some 30 cm wide (Phase 2, Fig. 80).

The excavation of MNX showed immediately that the last garden ditches were totally invisible on the clay surface; archaeologically they are completely missing. Only one set of articulated ditches was found and since it was obvious that it did not belong to the historic phase, it was labelled Phase 1. This garden was set at such an angle with the one of Phase 2 that they cannot be contemporary; furthermore, and a decisive proof, ditches from Phase 1 were, by the evidence of numerous small holes in their base, made by a digging stick and not by a steel or wooden spade (Plates 29 and 30). Whatever intensity of gardening and "plot displacement" took place during the last 15 years, nothing was left to testify to them.

Ditches of Phase 1 were dug rather deeply into the clay, with a formal distinction between those running N-S and those running E-W. The E-W ditches were deeper and wider than the others, the latter abruptly dropping into the former. One of the N-S ditches even failed to be registered in the clay surface in B5. One garden plot emerged from the system and, like those of Phase 2, was 13 m² in area. One post hole was found on the edge of a ditch in B1 (13 cm by 11 cm at the top and 13 cm deep).

The levels taken at MNX (Fig. 81) reflect the penetration of the clay by ditches of Phase 1, as well as showing the rather flat surface of the site. They also reveal an uneven topsoil, due to the gardening of Phase 2, having a thickness of usually more than 30 cm. The depth difference between the N-S and E-W ditches of Phase 1 is clear, with the additional information that E-W ditches tend to have depressions where N-S ditches drop into them, while

N-S ditches are deeper towards their ends than in their middle sections. Outside one small lump of green volcanic ash, nothing else was found.

8-7-2 INTERPRETATION

If the garden plot of Phase 1 is a representative one (and it is suspected to be so), one can say that, together with the garden lay-out, it is a grid-type garden very similar to Phase 2. Two major differences may nevertheless be noted: firstly, all Phase 1 ditches are made with digging sticks and, secondly, they penetrate into the clay surface.

It is probable that the southern E-W ditch was not only a plot ditch but also a block boundary ditch. This function is suggested, as in some ethnographic examples, by its greater depth and by the fact that the N-S tributary plot ditches dropping into it are not on the same line on either side of it. Furthermore, the lone post hole found could be interpreted as a block boundary marker. Its location in the ditch is consistent; ethnographically, ceremonial cordylines are often planted at each corner of a block in a communal garden. This would mean that at the time of Phase 1 communal gardens existed and block divisions within them followed the ethnographic models.

Following other ethnographic examples, it is probable that during Phase 1, a wet-tolerant tuber such as taro was planted in the bottom of E-W ditches, while N-S ditches functioned solely to raise the garden plots for the cultivation of a crop such as yam, sweet potato and pat. E-W ditches seem to have been deliberately dug in the clay, favouring water retention, while this is not so evident for N-S ditches, emphasised by the discontinuity in B5. If all the above interpretations are correct,

Phase 1 cannot be of great antiquity and could well post-date introduction of the sweet potato.

8-8 SITE MNY

The whole of Kukrumdi hamlet is surrounded by a ditch and fence enclosure to protect human activities from destruction by pigs. Along its western limits, this enclosure is still located on dryland, leaving for pigs a narrow stretch of dry ground before reaching the swamp. It is in this stretch, in pig land, that the last two garden sites were excavated (MNY and MNZ). The area has never been gardened since recolonisation and it is covered with pitpit and tall shrubs. It was said that it might eventually be gardened and that the first stage could be a mixed crop garden before the area was put down to coffee trees. Some 81m^2 were excavated in MNY, in the way shown in Fig. 82.

Prior to site excavation, it was assumed that except for some recent pig disturbances, apparently not penetrating below the topsoil, all features found would be prehistoric. There might also have been a ditch and fence enclosure located within the site to separate swampland from dryland and garden land from pig land. Consequently prehistoric garden features similar to those found by Golson in the Kuk swamp as well as to those found during my dryland excavations were also to be expected. There could be prehistoric features made by pigs and, if there was at any time a small rise in the water table level, the whole site MNY could have been under wet conditions. Finally, it was assumed that discoveries made at MNX would be duplicated here, being on the same platform and only 43m away eastwards.

8-8-1 RESULTS

One can consider site MNY as having four sections: the western trench including squares 1 to 13, the eastern trench with squares 16 to 23, and the two small extensions labelled 14/15 and 24/25. The water table level of the swamp was reached west of squares A4 to D4 and this level rose appreciably overnight and after rain. On the other hand, unit 16-23 was dry, rain water being trapped in the features excavated. The modern ditch and fence enclosure was located 18m east of unit 24/25.

Unit 16-23 was excavated first and, as Fig. 82 shows, various features were found. The topsoil was 25-30 cm thick and the clay surface was uneven, disturbed not only by human and pig activities but also by tree roots. Three tankis were growing. When uprooted, they were found to have been come from deep regular holes, penetrating the clay by 50cm, and looking just like postholes. Their top lengths ranged from 11 to 18cm and their top widths from 8 to 12cm. In the clay, two other such holes were found, in which roots of rotten tankis remained. These five features were set in a line. In E20/F20 one distinct feature was attributed to pig activity (very much like a contemporary pig wallow).

Ditch-like features were also found and divided into two phases (Fig. 82). Stratigraphically, Phase 1 was the earlier and the less distinct, constituted by a discontinuous elongated feature centered in D18, and by two other features obliterated by Phase 2 ditches except where a double cutting was observed from A22 to C21 and from A17 to C16. One of these cuttings was shallower than the other elongated one from D18. In contrast, Phase 2 was dug deep in the clay, from 30 to 40 cm into it. It cut through the features attributed to Phase 1 and had a very distinct formal shape, being "square" in cross-section. All the ditches, of both phases,

from units 16-23 showed signs of being made by a digging stick and not by a spade-like implement.

The three ditches of Phase 1 showed some parallelism and were set 2.5m and 2 m apart. Those of Phase 2 were also parallel and set 4m apart. It was decided to test further the parallelism of Phase 2 by opening two extensions. The one to the east (units 24/25) was successful in finding a ditch similar in shape and orientation. The extension to the west (units 14/15) recovered a ditch crossing, the ditches being similar in shape to those of Phase 2 but not exactly parallel with them.

The portable finds made in this unit 14/15 were limited and consisted of six small cooking stones and one chert scraper with acute angle edge; none was associated with a feature.

The excavation of units 1-13 gave similar results. Another series of four aligned holes were found (in C11 and B11), like those mentioned previously in connection with tankis; two of them indeed contained tankis roots; their depth was also substantial, up to 40cm; top lengths at the ranged from 15-20cm, width from 10-13cm, with one exception which was 28cm by 20cm. Features attributed to pigs were also recorded (in D7/D8, A7, B12). Ditch-like features found had formal similarities with those found in the previous units.

Two distinct ditches from units 1-13 were attributed to Phase 2. The one found in C13 and D12 was similar to those from units 16-23. The other ditch was very different. It was a very big one, deep, wide, impressive. It was one meter wide, one meter deep, and steep sided, "square" in profile. Its bottom was covered with a 15cm thick layer of green volcanic ash. No signs of fencing or spoil buildup were found on either side.

Ditches attributed to Phase 1 were more irregularly spaced,

and in some parts were recut. As in the previous units, they were narrower (20cm) and shallower (5-8cm into the clay) than those from the later phase. This Phase 1 system seemed to be extending in the swamp, as evidenced by C2. The topsoil thickened westward, where it became more than 40cm. All the ditches from units 1-13 were made with digging sticks. The clay was generally yellowish, but turned greyish towards the swamp.

Only 15 small cooking stones and little charcoal were found scattered in units 1-13, on the clay surface and not in features.

8-8-2 INTERPRETATION

Although Phase 2 ditches have strong similarities with those of Phase 1 in site MNX, it was not possible to relate them conclusively. There is not enough evidence in MNY to reconstruct a garden pattern for Phase 2 there. The big ditch located in A5/C2 is interpreted as a major boundary ditch, similar to the ethnographic ones dividing gardened land from pig land.

From the MNY evidence, one cannot tell on which side of that ditch the gardened land was, nor where the spoil from its making had gone. I suspect that a fence was aligned along that major ditch but not a single evidence of it was found; it is probably a missing feature. Nothing suggests a relationship between that ditch and the others from Phase 2, but on the evidences of site MNZ, one can say that they were contemporary and belonged to a single phase.

The deposition of green volcanic ash in the bottom of the major ditch suggests that it was fully operating at the time of the ash fall, but it is unknown if it was still in use later. I suspect that the two lines of cordylines formed some sort of fence

and boundary marker, and they are probably recent and postdate Phase 2. They certainly are not associated with that phase because of the lack of parallelism. If these cordylines were bordering a garden, one can only say that all evidences of it are missing.

The pig disturbances are not from the period of recolonisation. They belong to an earlier phase, but it is impossible to correlate them not only with the gardening phases recorded but also with each other. The feature interpreted as a pig wallow, located in B12, seems to postdate Phase 1 and to predate the line of cordylines.

The gardening pattern of Phase 1 is unclear but it seems to be less organised than Phase 2 and more connected with the swamp. The various recuttings found in these ditches (in units 1-13) could illustrate an absence of the plot displacement practice. In units 16-23, the cutting of Phase 2 features along Phase 1 features suggests that when the garden of Phase 2 was made, people followed the orientation of ditches from the previous phase. The earlier system could have been found accidentally but have been deliberately utilised, the infilling of the ditches being softer and moister than the clay itself.

Artefact remains, as with the previous sites excavated, were very poor, but MNY produced the first tool to be recovered, a chert scraper, which unfortunately cannot be associated with a precise phase.

8-9 SITE MNZ

The last garden site excavated is in exactly similar geographical and environmental situation to MNY located some 120m south of it. Site MNZ is also in Kukrumdi, in pig land, between

the swamp and the modern ditch and fence enclosure. Some 262m² were excavated in the pattern illustrated in Fig. 83. The same assumptions were made for MNZ as for MNY and it was expected that the finds from MNZ would be confirmatory to those from MNY.

8-9-1 RESULTS

The water table level was reached in trenches EW1 and EW2 (Fig. 83). Various ditch-like features were found, some articulated and others not. They all had holes in them, suggesting they had been made with digging sticks (Plate 31). All the ditches, with three exceptions, were very shallow and difficult to find. A note made by my assistant F. D. Bulbeck, while recording MNZ, when we almost missed one ditch, illustrates the difficulties encountered: "very shallow drain, so shallow that the excavator cannot be blamed for missing it - gets smaller eastwards to almost invisible, mainly visible by digging stick marks in clay."

Two of the three ditches dug well into the clay are parallel: the most northerly EW ditch which was fully exposed, and the central EW ditch, cutting trench EW4, which was only partially exposed. These are some 25-30cm into the clay, "square" in section, 40cm wide and receive shallow tributary ditches. In numerous places these two ditches had a thin layer of green volcanic ash deposited in the bottom.

The third deep ditch is much bigger. It runs NS and is located almost at the intersection of trenches EW2 and NS2, extending into the western extension made from trench NS2. This big ditch was one meter wide, around one meter deep and had rather straight walls. It had a 10-15cm thick deposit of green volcanic ash in its bottom. An interesting feature associated with this big ditch was the apparent tunneling of the deep central EW ditch

in its articulation with it. In addition all along the eastern side of the big N-S ditch was recorded a lens up to 18 cm thick of hard clay separated from the basal clay by a layer of soil and with topsoil above it. This clay is interpreted as upcast.

Another odd feature recorded was a curving ditch, very shallow, located at the western end of trench EW2. Levelling showed that it did not terminate in the ditch to the south of it, but, on the contrary, started there, getting progressively deeper away from it. Except for one chert scraper with steep edge angle and few cooking stones found in the topsoil, all other finds were made in the ditches: one medium cooking stone, five small cooking stones, three casuarina twigs, nine unworked quartz flakes, three unworked chert flakes and another chert scraper with steep edge angle.

8-9-2 INTERPRETATION

It is possible that, as for site MNY, a wet crop such as taro was cultivated in the two deeper parallel ditches found in MNZ. They receive very shallow tributary ditches which would probably not have been used for taro cultivation but served to provide spoil for the garden beds. The spatial irregularity of the shallow ditches suggests that there could have been some plot displacement practiced centring on the "taro ditches" which were fixed. Some of these shallow ditches could well be missing from the archaeological record.

A tentative reconstruction of the field system (Fig. 84) does not show a clear pattern and many interpretations can be made. One of them is that plot size was around 25 m^2 , but one cannot at all be conclusive.

The big ditch found along the swamp margin and the clay upcast

on its eastern bank indicate a major boundary function of a ditch and fence system, even though no post holes from a fence have been found. It also indicates that at the time the big ditch was operating, gardened lands were located eastwards and pig land westwards, in the swamp. The tunnelled articulation shows that this technique (still present nowadays) was already known at the time of the green volcanic ash fall (Blong, 1982). The deposition of that ash in the major ditch and in the two other deep ditches indicates that the system was fully operating when the volcanic eruption occurred and that it was left unmaintained afterwards; otherwise at least the two parallel garden ditches would have been cleaned (thus suggesting an abandonment).

The features recorded at the western end of trench EW2 are located west of the major ditch and, if contemporary with it, are in pig land. This is almost impossible, unless there was another major boundary ditch at that part of the swamp margin, as suggested in Fig. 84. The curving ditch is difficult to interpret, especially since no ethnographic example was observed. It could be a run off channel during heavy rains or the whole articulated system could be from another agricultural phase than the rest of MNZ.

Finally, there is a very strong resemblance between MNZ and MNY and the big boundary ditches found in each are very probably one and the same ditch running along the swamp margin. One cannot resist the temptation of seeing a strong analogy in land management between the prehistoric phase and the modern one located a mere 20m eastwards, both of them having a ditch and fence enclosure system (and indeed almost parallel!) dividing pig land from gardened land.

If this is a correct interpretation, it means that there is

a continuity in gardening and land management behaviour at Kuk for more than 300 radiocarbon years.

8-10 CONCLUSIONS ON DRY LAND CULTIVATION

The first point immediately noticeable in the 1,124.5 m² of excavations described above is the quantity of artefactual remains: very few cooking stones were found, not a single wooden implement and only three chert scrapers (along the swamp in MNY and MNZ). This was a frustrating aspect of the entire excavation programme. Any analysis of the technological equipment of these prehistoric gardeners remains at the moment totally at the theoretical level, as it does generally for agricultural sites in the New Guinea Highlands (Golson 1977b, Powell et al 1975, Gorecki 1978, Steensberg 1980).

A second point is that most of the phases of cultivation recognised by Golson (1977a) in the swamp have no counterpart on the adjacent dry land. Here my own work suggests a division of agricultural activities into four phases.

My earliest phase, Phase 1, has no describable pattern and is represented only at sites MOC and MNY (Figs. 76, 82). Both are bordering swamps. From the MNY evidence, it seems that Phase 1 extended into the swamp and probably belonged more to swamp than to dryland agriculture. Even if MOC is on a dryland hill slope, Phase 1 was not found further inland, on other hill flanks or on dry flatlands. With the present data, it cannot be dated.

The second phase could be represented by a grid pattern with big blocks, say above 25 m². It could be represented at sites MOA, MOB, MOC and possibly at MNY and MNZ. At the time Phase 2 was operating, big communal gardens existed and inside them block divisions and plot arrangements followed a mechanism similar

to the one encountered today. It is possible that taro was cultivated in block boundary ditches and cultivation on the adjacent ground was intensified by raising garden beds by means of spoil from shallow plot ditches articulated with the taro ditches and through the practice of plot displacement.

Sweet potato was already cultivated during Phase 2 (MOC) and it is possible that part of the system was abandoned after the volcanic ash fall 300 years B.P. (MNZ). This would imply that sweet potato was present in the Highlands prior to 1700 A.D.

The third phase is characterised by a reduction of the garden plot size, say below 20 m². It is represented at sites MOA, MOB, MOC, MNX, probably at MNU, MNV, MNI, and possibly at MNY and MNZ. Cultivation during Phase 3 does not seem to have differed very much from the previous phase, but was not restricted to flat or gently sloping dry lands. It was also practiced on the steep slopes from Ep Ridge (MNU, MNV).

In flat sections, it seems that taro was also cultivated in ditches deliberately dug to penetrate the clay in order to retain water, while on steep or gentle slopes the primary function of ditches was to raise garden beds. Land management through communal gardens and block allocations was present during this phase. The time gap between Phases 2 and 3 at MOC could not be insignificant, since the evidence there suggests at the very least re-establishment of gardening after a period of long fallowing. It is probable that the very beginnings of Phase 3 were contemporary with Phase 2 and progressively replaced it. It is possible that Phase 3 started some time around 250 B.P. (Golson 1977a) and extended until the general abandonment of flatlands prior to A.D. 1900 (Gorecki 1979b).

The fourth and last phase starts with the recolonisation of Kuk after A.D. 1960, and is the ethnographic one. It is characterised by a grid pattern indentical to that of Phase 3, but where a linear garden pattern could be in the process of replacing the grid one. Phase 4 also has taro ditches, but restricted to natural depressions, and these also are immutable. Plot drains to raise garden beds are articulated with them. Very poor archaeological evidence is left from this contemporary phase.

There has been no mention of mixed crop gardens in this archaeological section. It is probable that, as with the ethnographic cases of Phase 4, such gardens were restricted to the moistest areas and thus were not excavated. This remains an open question and further excavations of swamp margins could investigate this possibility.

The features that we know are missing are numerous and from all four phases. This makes one wonder how much more has been totally lost for the archaeologists, which we do not know about. This could explain the lack of evidence in my work for the early agricultural phases found in the Kuk swamp. The almost total lack of erosion on the dry land rules this out as an explanation. It is possible that gardening activities were already at a very early time concentrated in the swamps, the surrounding drylands being less gardened and frequented mainly for such things as timber supply and hunting.

CHAPTER 9

CONCLUSION

I must stress that most of the discussion which follows concerns the Kuk drylands. It was shown in this thesis that wetlands and swamplands at Kuk have a geomorphological history quite distinct from the one related to drylands: deposition and accumulation of sediments do not occur in drylands. This fact makes the archaeological record of the drylands more difficult to interpret because of the relatively poor stratigraphy. By contrast, wetlands and swamplands may be easier to interpret because of the presence of such stratigraphy. Furthermore, organic remains may preserve better in those wetter areas.

It has been observed at Kuk that wetlands and swamplands activities concern basically those related to gardening and pig raising. Gardens may be more stable in terms of their grid-pattern ditches not being shifted each time they are replanted as is the case for those located in drylands. Potentially, gardens in wet areas may leave an archaeological visibility related to a single operational phase which could be sealed off (if abandoned) by subsequent sediment deposition. Overall, the combination of specific human activities and specific geomorphic histories may result in an archaeological visibility of human activities quite different between wet and dry areas. This problem was not tackled in my research; my emphasis was put deliberately and almost exclusively on dryland activities.

Most of the excavated sites have been presented in detail (chapters 4,6,8) together with their related ethnographic observations (chapters 3,5,7) and a long-term view of all these activities (chapter 2). The next step is to ask how all this can contribute to the field of archaeology. To answer the question, general trends in site formation

processes at Kuk must be defined and their applicability to the interpretation of some specific prehistoric data must be assessed.

9-1 FEATURE REMAINS

It has been argued throughout this thesis that the ethnographic activities carried out at Kuk have resulted in the making of a large number of features. On the basis of the ethnographic observations alone, I think the whole area of Kuk was the subject of severe disturbance over the last 20 years. At South Kuk, gardening activities have resulted in the moving of at least 121,500 cubic metres of sub-soil (basal clay). Regarding the top soil alone, found only on drylands, a minimum of 267,000 cubic metres was turned over at least once (89 ha cultivated with an average top soil thickness of 30 cm). Thousands of post holes, ladder holes, planting holes, ditches, etc. were dug. What is the potential archaeological signature of all this intensive farming activity? If it was not for major and boundary ditches which were dug into the basal clay, one would have found only an occasional post hole or part of a shallow ditch. Most of the evidence left from the contemporary agricultural system may not be sufficient to allow its accurate interpretation.

The gardening system is self-destructive archaeologically, but also destructive of most of the earlier activities which may have occurred in the area. Because of this, it appears that on dry land the top soil is of little archaeological significance. Artefacts found within it can be from any period between now and 30,000 years ago, while features which may be found within this top soil would, most likely, belong only to the very last activity which has occurred at that location.

In contrast, the sub-soil should be more informative. Features dug into it are not as easily destroyed as those found within the top

soil. It has been demonstrated here that all features which have been dug in the basal clay should be preserved. The problem then resided in their identification and interpretation. It appears to me that any isolated feature loses most of its meaning; it is a collection of features, an "arrangement", which can be tentatively interpreted. It is basically this poor evidence which should lead us to a site interpretation which may or may not be similar to the ethnographic activity which made it.

Regarding housing and cooking activities, it appears that the archaeological visibility of these follows trends similar to those noted for gardening activities. Houses should leave some evidence in the basal clay of things such as a surrounding ditch, at least one central post hole, a hearth and perhaps an oven. Following a modular principle detailed in chapter 3, it appears possible to interpret such sites quite accurately. Similar comments can be made for cooking sites because ovens are almost always dug in the basal clay and have large numbers of stones clustered around them. All these features carry messages that should enable the archaeologist to offer a reliable interpretation.

While the discussion above concerns exclusively dug-in features, it must be stressed here that driven-in features, made during all activities and in much greater numbers than those dug-in, seem to quickly drop out of the archaeological record. Smaller ones tend to disappear faster. However, within a time perspective more related to archaeology, all these driven-in features have no chance of survival. In rare cases some have been recovered from sites more than five years old; these should only be considered as unexpected bonuses for the site interpretation.

To summarize this section on features, a limited number of points must be emphasized regarding site formation processes and archaeological visibility in the Kuk drylands.

- i) An analytical distinction at the ethnographic level must be made between dug-in and driven-in features. Correlations between sets of dug-in features, and between these and driven-in ones must be defined.
- ii) An analytical distinction at the archaeological level must be made between top soil and basal clay. Features found in the top soil may represent only the very last activity at a location; those found in the basal clay may represent all activities at a location, from its earliest to latest occupation.
- iii) Cultural processes are the main agencies for destroying the archaeological evidence from the top soil.
- iv) Natural processes are the main agencies for destroying the archaeological evidence from driven-in features.
- v) Natural and cultural processes operating at a site are of greatest importance during the first five years following its abandonment. It appears impossible to segregate the part played by either cultural or natural processes. They are operating simultaneously, resulting in a specific archaeological visibility of remains at each site. The archaeological visibility beyond this critical five year period appears to remain stable; it may be little affected by further processes.

9-2 ARTEFACT REMAINS

Artefacts found within the Kuk drylands, given the simple stratigraphy of the area (top soil/basal clay) and because of the peculiar history of the area over the last 20 years, must be divided into two broad categories: organic and inorganic.

Organic remains (food refuse, wooden implements, building material)

have been noted particularly at housing and cooking sites. Those that were test excavated suggest that organic remains follow a trend identical to the one described for driven-in features: most of them have disappeared from the archaeological record after a period of five years following site abandonment. Despite the rapid decay rate (natural process), undoubtedly accelerated by intensive recycling activities (e.g. firewood from building material; cultural process) and scavenging by dogs and pigs (natural process), it appears that organic remains may best survive, thus entering the archaeological record, when located inside features which are dug into the basal clay. The most likely features where such preservation can occur are hearths and earth ovens.

The inventory of crops cultivated at Kuk may never be fully identified in the archaeological record, either at their consumption locations or in the fields. Not only do these crops tend to decay rapidly but they are also floral species which are not suited for providing pollen records (Powell 1982). Very few crops (and pandanus seeds are a good example of this) are well preserved, and because of this the remains may give a biased picture of diets and staples.

Organic remains could have a better preservation rate in swamplands, but here again this rate could be very low (total destruction) when dealing with a time scale more appropriate to archaeological material: extremely limited organic finds have been made throughout swamplands which are more than 2,000 years old (Golson and Hughes 1977).

Inorganic remains, basically the stone component of the society concerned, in contrast preserve very well within the Kuk drylands. Ethnographic observations suggest that these remains should be found in particularly high densities around house site limits (chapter 3; discard locations). Some artefacts are directly associated with specific activities (e.g. cooking stones in and around earth ovens).

Archaeological tests carried out on these recent sites suggest that in fact few inorganic remains should be found at the precise location of activity except for those related to cooking. The emphasis here should be on discard locations rather than activity areas. These discard locations were not archaeologically tested in the Kuk drylands. But taking into account the data provided by chapters 2 to 8, it appears that no artefact found within the top soil is in situ, and that most of those found on the surface of the basal clay are also within a disturbed context. Given the stratigraphic context, one must again emphasize that all cultural material (organic and inorganic) found in the Kuk drylands will most likely be within a disturbed deposit, only 30 cm thick which may have been the subject of cultural activity and artefact deposition lasting more than 30,000 years.

As with the previous section, a few points must be stressed regarding artefact remains from the Kuk drylands:

- i) Natural processes appear to operate particularly on organic remains to such an extent that within a period of five years these processes result in total obliteration in terms of archaeological visibility.
- ii) Cultural processes have a repetitive impact on inorganic artefacts to such an extent that only those produced during the last activity should be considered as being in situ.
- iii) Only inorganic remains found as a component of a structure (e.g. within a pile of cooking stones or stones aligning a hearth) should be considered as being undisturbed.
- iv) Despite these disturbance processes, high (or unusual) concentrations of artefacts carry a meaningful archaeological message; they are part of a specific activity subsequently disturbed at that location.
- v) All artefacts (organic and inorganic) found in a feature belong to and are contemporary with that feature.

vi) Organic remains may be found inside features dug into the basal clay.

9-3 NATURE/CULTURE AND FEATURE/ARTEFACT

To sum up site formation processes operating in this specific ethnoarchaeological context, the critical emphasis must be put on the nature of the stratigraphy of the Kuk drylands and the economy of their occupants. It is a landscape comparable to one of the theoretical models proposed by Foley (1981:Fig.6.14,b, p.178), with "low surface area: sediment volume ratio" which should result in a high archaeological visibility because of this lack of sedimentation. This in itself makes disturbance and destruction processes highly active.

The extent of natural and cultural processes at work within the Kuk environment are such that the archaeological visibility of remains left from known activities is low. One is dealing essentially with an archaeology based on a destroyed and disturbed series of remains. This could be a common phenomenon in archaeology, and has been noted elsewhere (e.g. Gould 1980; Isaac 1981).

Gould noted that in a campsite used by Australian Aborigines, remains left from various activities were, five months later, partially destroyed or mixed with remains from previous activities (Gould 1980:24-8). In the face of this, Gould takes essentially a negative attitude by not comparing and contrasting patterns which were noted during the occupation of Tika-Tika (or general trends in use of campsites) with those emerging five months later.

In contrast, Isaac takes a somewhat more positive approach to the problem and "sieves" through his archaeological evidence (Isaac 1981). Isaac tries to see if patterns within an archaeological site or within sites from a region can be associated and differentiated in an attempt to present a logical archaeological sequence (1981:140-2).

Although Isaac is dealing with early hominid sites from East Africa, he demonstrates that, unlike Gould, one can extract valuable data from sites which appear to be disturbed or have various occupations or in which only a small percentage of debris has been preserved.

Other scholars have also attempted to tackle the archaeological data with a positive frame of mind (cf. contributions in Hodder et al. 1981). I have tried to follow a similar positive approach at Kuk. The major procedural lessons from this research are as follows:

i) It does not make much sense to have only ethnographic observations (sometimes labelled "ethnoarchaeological") at hand; these must be complemented by their archaeological counterparts, i.e. excavations, to ensure that site formation processes operating between a site in use and its archaeological manifestation are better understood. There appears to be enormous differences between the visibility of activities of an occupied site and the visibility of remains at the same site shortly after its abandonment. The archaeological data essentially reflect the type of evidence one would expect from an abandoned site rather than an occupied one.

It is when these site formation processes are defined and quantified that correlations, i.e. analogies and perhaps anomalies (cf. Gould 1980: my understanding of the latter is that in essence they are also analogies) between archaeological visibility and ethnographic "reality" can be best established.

The destruction of data at Kuk appears to stabilize within the first five years following site abandonment, a period within which most of the "damage" has been done. After that critical period has lapsed, not much happens to the visibility of the site; it has reached a level of archaeological visibility which provides the most reliable link between ethnographic and prehistoric data. It is true that new

activities such as gardening, housing and cooking may further destroy or modify the existing evidence, but overall these are relatively minor. It appears to be almost impossible to quantify the part played by either cultural or natural processes. They are operating simultaneously, resulting in a specific level of archaeological visibility at each site. It is this evidence (from sites more than five years old) which concerns me as an archaeologist.

ii) In a sedimentary and environmental situation similar to the one found at Kuk, attention should be directed to the archaeological evidence of the sub-soil, i.e. basal clay, to segregate ethnoarchaeological data from any earlier evidence of human activity that could be found. If this is possible, then this may also be attempted with prehistoric manifestations which are visible at that location but which do not belong to the ethnographic present. Various sequential events may then be possibly defined, quantified and compared.

iii) Another important point emerging from this study concerns the "stabilisation" of processes at a site within a five year period. Because of this short time-span, it could be irrelevant to segregate natural from cultural processes; the attention could be focussed on the correlations existing between an ethnographic activity and its archaeological manifestation left after such a critical period. It is possible that for other site types in other environments, a similar short period of intensive processes followed by a stabilisation occurs. This appears to be the case for trampling activities in rock shelters (Courtin and Villa 1982). It may thus be of great interest to find out in other ethnoarchaeological studies if such rapid stabilisation is noted; if there is one, then there is no doubt that future work should be oriented towards the nature of the archaeological data left from various activities in various conditions after such a stabilisation has occurred.

However, it is probable that in some areas, a stabilisation will occur only after a substantial period of time. Sometimes the archaeological manifestation of an activity may not stabilize at all, or may only gradually change (cf. Evans and Limbrey 1974). With further research, it should be possible to define the general conditions which have created a particular archaeological situation.

iv) Ethnoarchaeology at Kuk: housing activities. Analysis of a limited number of features found in the basal clay is critical. Correlations between central post holes, hearths and ovens should lead towards a modular principle which may define the house type. Indirectly, this finding will have numerous connotations concerning the socioeconomy of its occupants.

v) Ethnoarchaeology at Kuk: cooking activities. These appear to leave the best archaeological visibility. Features tend to be dug deeply into the basal clay and a high concentration (thus significant) of a specific class of artefacts (cooking stones) may be directly associated with these features. A petrological and conjoint analysis of these stones may further lead to clusters of ovens for sites which were occupied at various times. Cooking is also the activity where organic remains may be preserved and identified.

vi) Ethnoarchaeology at Kuk: gardening activities. While it was anticipated that this activity would be the one for which archaeological visibility would be the highest, it appears that it is that one which is the least preserved in the archaeological record. While major ditches are those which have the most frequent visibility, activities related to crops (plot and block ditches, planting holes) and the crops themselves do not, in general enter the archaeological record. It is as if we had a complicated frame around something invisible. It is obvious that there was or there should have been something inside that frame but one is at a loss to discover it.

Gardening activity is also the one which generated most of the disturbance and destruction processes in the Kuk landscape. Cultural and natural processes derived from gardening activities are not only internal (affecting the contemporary activity) but also external to them (affecting earlier activities of any kind). The test excavations carried out at Kuk have produced other unexpected results.

Prehistoric phases 2 and 3 of dryland cultivation (chapter 8) strongly suggest that their archaeological visibility is by far greater than the one for phase 4 (the contemporary system). The socio-economic mechanisms operating during phases 2 and 3 may have been entirely similar to the present-day one (chapter 7). If this is correct, one is then confronted with an unexpected issue in Kuk's gardening ethnoarchaeology: the ethnographic present does not explain most of the archaeological present but does explain part of the archaeological past!

9-4 LOCAL PREHISTORY

The most recent publications on the archaeological evidence from Kuk itself are by Golson (1981a,b,c). In these, Golson reinforces the general belief that the agricultural history at Kuk started at around 9,000 BP on both drylands and swamplands. While swamplands were occasionally abandoned for reasons still unclear, there was an "uninterrupted history of cultivation on adjacent dryland" (1981a:57). Crops such as taro (by 6,000 BP) and sweet potato (by 300 BP) were probably the successive staple crops on which the Kuk systems were based. The six defined phases of swamp cultivation show some kind of "evolution" from one to another leading finally to Phase 7 (Golson 1981a:57) which is the one presented in this thesis.

Concerning Kuk itself, Golson looks at the landscape from a swamp viewpoint, thus giving an important role to the surrounding drylands for

the development of the Kuk agricultural history. I have an opposite position: I am sitting on top of these drylands with their agricultural history freshly dug up, and I see the Kuk swamp down there (and its known archaeological evidence), together with swamps nearby (see Map 1) as being the major centre of the local agricultural history. It is my belief that the data presented in this thesis are important and useful for the interpretation of the local prehistory. It is to this prehistory that I now turn my attention. Golson's data can only be analysed through his own interpretation of the evidence because of the lack of published details. Some general comments can nevertheless be made, at the hypothetical and potential levels, regarding housing and gardening activities.

The major concern here is to use my material to offer alternative or complementary interpretations of the prehistoric agricultural phases found in the Kuk swamp. It is most likely that Golson's Phases 5 (400-250 BP) and 6 (250-100 BP) are the equivalent of respectively my drylands Phases 2 and 3. These earlier dryland systems appear to have operated similarly to those of the ethnographic present. Consequently I am fully confident in applying ethnoarchaeological analogies derived from this research to data related to agricultural systems within the last 400 years.

These systems suggest that for both dryland and swampland there is a need for agricultural fields to accommodate wet crops, to the extent that "wetland" ditches are reclaimed from flat drylands. The grid pattern of garden beds evolves from "large" plots to smaller plots throughout the area, probably using an individual and collective social organization similar to the one in use nowadays.

While I cannot comment much on detailed swamp evidence since Golson's publications concern essentially a "macroanalysis" of the evidence,

aerial photographs of the same swamp give an overall impression of the prehistoric ditching network being a familiar one. The similarities between the present system (as seen from the air) and the prehistoric network at locations where it is the clearest on aerial photographs (and assumed to be from the last 400 years; P. Hughes, pers.comm.) are striking. It is as if one is dealing with a single system, at least in operational terms.

From a ground level viewpoint, it is inferred that here again the prehistoric swamp agricultural systems of Phases 5 and 6 are similar to the contemporary dryland system (Golson 1977a:627). It certainly is so for my prehistoric dryland Phases 2 and 3. There are strong indications that communal gardens existed at the beginning of my Phase 2, within which individual blocks were allocated and made in a manner similar to the one in use nowadays. Even the "plot shifting" process is present. The only major difference is that the prehistoric ditches, even the smallest ones such as plot ditches, were deliberately dug into the basal clay; this is now restricted to boundary and major ditches.

What is unclear for these prehistoric dryland systems is the reason for which their ditches suddenly began to be dug into the subsoil. There is no indication in the sites I have excavated of a gradual process leading to this. It is an abrupt one, which is most likely to be recent since there is no evidence of an anticipated "messy" network of such ditches which would indicate some antiquity.

One interpretation for this change is that by around 400 BP swamplands were almost entirely under cultivation (as suggested by aerial photographs) and became insufficient to sustain the population. People had to extend similar field systems into drylands for both wet and dry crops. It was an optimum use of the land where both raised beds and ditch bottoms were cultivated. Such intensity of land use is not reached by contemporary societies in the Wahgi.

It is possible that mechanisms leading to such situations have their roots back some 4,000 years ago. From Golson's publications, it appears that an orderly network of ditches start to develop from Phase 3 (4,000-2,500 BP) through Phase 4 (2,000-1,200 BP) and up to Phase 6. There is a gradual tightening of the ditching network, resulting in a gradual intensification. If this is so, there are strong continuities from one "phase" to another. And if the above is correct, then it is legitimate to use ethnoarchaeological analogies to offer an interpretation of agricultural processes and developments occurring in the Wahgi swamplands during the last 4,000 years.

It thus seems unlikely that drylands around these swamps have played a major role in the local agricultural history. This is for two basic reasons. One is that there is simply no evidence at Kuk of sophisticated early agricultural systems which match those found in adjacent swamps, and this is despite the drylands being cleared of forest and becoming grasslands more than 5,000 years ago (Powell 1982). Similar conclusions regarding the lesser importance of drylands have been reached for the Middle Wahgi (Harris and Hughes 1978:444). The second indication that a development primarily occurred within swamplands is the nature of these field systems. They seem to be specialised for wet lands, and changes from one phase to another appear to be the result of trials attempted within these wet lands: these can hardly be the result of new agricultural techniques being development in drylands.

It is with Golson's Phases 1 (9,000 BP) and 2 (6,000-5,500 BP) that I fell less confident in applying ethnoarchaeological analogies. The agricultural systems operating during these periods appear to be somewhat different to those in any of the subsequent phases. Nevertheless the network of major ditches so far known (e.g. Golson and Hughes 1977) certainly appears to be overall closer to the one found later at Kuk than to the one found, in say, the Baliem valley (Heider 1970). It is as if

the basic agricultural frame at Kuk remains always similar, but the picture inside that frame is changed from time to time. If this is so, I believe that analogies can be applied to the data to gain an insight which could be different to Golson's one. I hope thus to contribute to a better understanding of the extent of the differences between Golson's Phases 2 and 3.

The origin of agriculture and crops cultivated in the New Guinea Highlands are not the subject of this thesis. But it is important to note that cultigens of New Guinea origin are plentiful and many could be staples. Apart from the possibility of taro (Spriggs 1981, 1982; White and O'Connell 1982), there is sugar cane, banana, yam, pitpit, breadfruit and sago. The nature and distribution of present-day staples throughout Papua New Guinea (Swadling 1981) points towards a particularly complex situation, perhaps even suggesting various origins for numerous staples.

There are some recent hypotheses which may soon result in a revised New Guinea agricultural history in which the emphasis will be put on local developments rather than external influences (Spriggs 1981; Rhoads 1982; White and O'Connell 1982; Gorecki 1979d, 1982b,c). If some of these developments are internal to the Highlands, then some of the above hypotheses may be supported by data presented in this thesis.

Concerning other aspects of agricultural activity, the tools required for all these gardening activities are limited to simple wooden implements such as digging sticks and spades (Powell 1974; Golson 1977b; Gorecki 1978; Steensberg 1980). Based essentially on ethnographic observations and personal experience with the basal clay, digging sticks would have been the typical multi-purpose tools in both drylands and swamplands. Wooden spades in contrast would have been restricted to "soft" activities such as in swamplands and activities related to the top soil in drylands. I do not think that such spades could have been used to dig the basal clay.

Supported by the archaeological evidence presented here, I suspect all dryland ditches dug into the basal clay were made by men's heavy digging sticks (e.g. Plates 1 and 2). Archaeological evidence also suggests that some ditches surrounding prehistoric house platforms (and by analogy all plot ditches) which were dug into the basal clay by digging sticks may have had their base smoothened by rough spade work; these spades were certainly not used to dig the basal clay itself. Overall, in the New Guinea Highlands one of the simplest tool technologies was (and still is) used for a particularly complex and sophisticated mode of subsistence.

Data related to prehistoric houses found in the Kuk swamp are limited and have been discussed in chapters 4 and 6. These houses have no great antiquity and it appears that ethnoarchaeological analogies are a useful tool in the interpretation of their remains. What emerges from this analysis is that there could be no change at all over the last 300 years in house type and probably in behaviour related to house activities. What remains to be found are house sites related to the earlier agricultural phases at Kuk to see, analyse and quantify similarities and differences which may exist between them and the present data.

Overall, it appears that ethnoarchaeological data presented in this thesis can be useful for the interpretation of most prehistoric data available so far from the Kuk swamp. It is my conviction that analogies must be used to explore various possible explanations regarding the origin and development of activities which have occurred at Kuk.

9-5 WIDER PREHISTORIC CONTEXT

The next question one may ask of the Kuk data is their usefulness within a wider prehistoric context. Two avenues can be discussed here, one for agricultural evidence and one for archaeological sites in general,

including hunter-gatherer sites. I shall develop this section from a particular to a general discussion.

Concerning agricultural sites, it must be pointed out here that if the assumption of 9,000 years of dryland cultivation at Kuk is correct, then the direct evidence for it is nil. This could be the result of a swidden system operating in the drylands, which by its nature has left no evidence except vegetation clearance. What is important to note is that the vegetation change at Kuk, from primary forest to grassland, occurred more than 5,000 years ago (Powell 1982). Such change would suggest a more repetitive use of the land to other tropical areas which today are still under a slash-and-burn regime (e.g. Clarke 1971; Dornstreich 1974; Morren 1975; Rhoads 1980). If nothing archaeologically related to agricultural activities can be found in open sites used repetitively (e.g. Kuk dry grasslands), then one should expect to find even less (if this was possible) in areas which have been used less frequently (zones still forested).

Furthermore, human history at Kuk is at least 30,000 years old and in no way could people have found natural shelters in the area (beside a small rock shelter in Kuning still untested but possibly of a younger age). This implies that people camped in the open, for which little or no evidence has been found so far. Because of this, each discrete indication that humans were present at an early date at Kuk should be considered seriously (cf. Golson and Hughes 1977).

Added to this problem of finding early open campsites is the evidence that house sites themselves are the subject of important natural and cultural destructive processes. Despite this, there are indications in the Eastern Highlands that some houses fairly similar to those found today could be as early as 18,000 BP (Watson and Cole 1977). Such evidence points towards an early knowledge of and need for proper houses

as opposed to shelters or simple windbreaks.

I am not suggesting here that people were swidden farmers living in houses by 30,000 BP, rather I am pointing towards the scanty evidence one will get from Highlands open sites because of the peculiarly destructive site formation processes operating. Kosipe (26,000 BP, White *et al.* 1970), NFX (?18,000 BP, Watson and Cole 1977), Kuk (30,000 BP, Golson and Hughes 1977) and Wanlek (15,000 BP, Bulmer 1977) are all open sites which managed to provide evidence of Pleistocene occupation of the Highlands. These sites may indicate that despite the altitude and mild to cold temperature, a well established early and widespread man-environment relationship existed. If most sites have been destroyed by archaeological formation processes, then one should perhaps rely even more heavily on glimpses that can be extracted from some pollen records to better comprehend the discovery of the Highlands and their development presumably from hunter-gatherer economies to swidden and intensive farming communities.

No particular tools are required for agricultural activities and assumptions based on some specific tools may be misleading. In New Guinea one is dealing essentially with a wooden technology which tends not to survive in the archaeological record. Waisted blades found in New Guinea have often been interpreted as being related to agricultural activities (e.g. Golson 1977b; Bulmer 1977). Since these implements have been dated to at least 26,000 BP (e.g. White *et al.* 1970), it is more likely that they are not agricultural markers; they could possibly be simply related to the exploitation of forested environments. Other artefacts such as geometric microliths, pottery and grinding implements have also been noted as being indicators of plant domestication and agriculture (e.g. Childe 1956; Clark 1980). Against this, it must be said that in the Wahgi there is one of the oldest agricultural systems in the world which never required the use of any of these three types of

artefacts. In contrast, Australian Aborigines never had agriculture but did use over millenia artefacts such as geometric microliths and grinding implements.

This is one aspect of my research which could be of some use for archaeological interpretations in a wider context; to promote the search for evidence which may indicate agriculture or house building as being perhaps developed earlier than thought. Another point, also related to agriculture, would be to offer alternative interpretations regarding prehistoric agricultural systems which may share some similarities with the Kuk material (e.g. Central and South American highlands and lowlands). I wish to discuss here only one of them, namely wetland systems found in the Mesoamerican lowlands.

While these systems have received attention for sometime (e.g. Denevan 1963; Parsons and Denevan 1967; Smith et al. 1968; Denevan and Turner 1974), it is only recently that detailed archaeological investigations have been carried out (cf. contributions in Journal of Belizean Affairs 1977; Harrison and Turner 1978; Flannery 1979). As with the Kuk swamp, the Mesoamerican data can only be analysed through the various interpretations offered of it because of the lack of published details.

Seen from the air at least, these Mesoamerican raised fields certainly appear to be as diversified as those found today in the island of New Guinea, with striking similarities between the two regions in some cases. Because of this, it is possible that some of the mechanisms which made them are common. But it is in the comparison of local hydrologies and the social content of their interpretations that major differences between New Guinea and Mesoamerica emerge.

Currently it is the hydrology of the systems that is principally under scrutiny (e.g. Harrison and Turner 1978), and it appears that most of these prehistoric field systems in Mesoamerica may be seasonal only,

operating according to the rise and fall of rivers to which the systems are directly attached. If this is the case, then one is dealing with perhaps twice (two seasons per year) as many systems per unit area as in New Guinea (no seasonal hydrological fluctuations as such in the New Guinea Highlands). One question which arises concerning this double system relates to the possibility that there could also be twice as many house sites and other loci of prehistoric activities (if most of the population goes elsewhere in dry seasons).

Prehistoric population densities in the drainage systems of Mesoamerica have been estimated in several ways, giving results which, while they are difficult to compare with New Guinea, appear higher (cf. Turner 1974, 1976; Rice 1978). Directly associated with this is the question of state organization, central control and religious power which developed in Mesoamerica, but never did in the New Guinea Highlands.

A final major divergence between these two geographical regions concerns the ethnographic data. Unlike New Guinea where present-day wetland systems are widespread and appear to be somewhat similar to the prehistoric ones found within the same areas, there is a peculiar lack of such data in Mesoamerica despite the fact that the prehistoric systems are so far of no great antiquity (less than 3,500 years old; Hammond 1978). It is nevertheless still possible to pursue some specific approaches among contemporary societies to gain further "ethnoarchaeological" insights (e.g. Becquelin 1973; Hayden et al. 1977; Abrams 1977).

The data presented in this thesis may contribute to the understanding of prehistoric agricultural remains found in Mesoamerica in at least two ways. One of them would be to look at the mechanics involved. This could be done by building hypotheses on labour input suspected of being involved for the creation of these fields (e.g. Turner 1981) and the social organization needed for their creation, maintenance and exploitation.

A comparison between present-day systems and prehistoric relics may also give an insight into the nature of crops grown, the cultivation/fallow ratio and the relationship between gardens and other activities (particularly settlements). Finally, the use of data related to site formation processes such as those presented here may give some clues to what is not present in the archaeological record.

I suspect that chapter 2 could be of interest to studies related to wetland cultivation in the tropics. It illustrates that there is no need for a high level of organisation or a complex material technology or a high population density to produce enough archaeological data to lead to the assumption that these exist. More precise studies such as those presented in chapters 3 to 8 may not be directly comparable in their details but should be nevertheless used in terms of the site formation processes involved.

While little is known about these processes in Mesoamerica, it seems that at least some of those described here may be similar while others must be entirely different. For the latter I have in mind processes related to house sites. At Kuk the processes are such that in the long term the archaeological visibility of house sites is low. In contrast, in some areas of Mesoamerica such house sites have high archaeological visibility (Turner, pers.comm.): platforms are used over long periods and show stratified sequences which can be identified. It is as if cultural (certainly) and natural (probably) site formation processes at work for this specific human activity lead to a preservation pattern rather than to one related to total destruction.

The immediate outcome of this pattern would be the finding of "hundreds" of sites over a limited area, occupied over centuries and directly associated with surrounding field systems (themselves perhaps only seasonal). It is this interpretation, the second direct way of using my data, that I turn my attention.

As stated in the introduction of this thesis, "analogies should be tried for sites where it appears they will not work" (p.24). Following this, here is a highly speculative use of analogies from New Guinea ethnoarchaeological data to interpret Mesoamerican prehistoric raised fields. The interpretation itself should not be taken seriously; but its potential for tackling the evidence through an entirely different angle should.

There are about eight ceremonial (singsing) grounds at Kuk, from which there are arrays of tracks leading to settlements and garden areas. The archaeological visibility for all these is known to be low, but just imagine it is high because of a different technology and use of building material (stone instead of decaying wood). The following transformations may happen at Kuk: there are now about eight magnificent temples (.6 temple per km²) at Kuk, from which there are arrays of causeways leading to a large number of stone houses, some in a village-type organization and others scattered in the landscape, all inter-related with a dense network of gardens.

The transformations made here are minimum, in fact they are a simple move from low to high archaeological visibility which results in "archaeological" evidence more related to Mesoamerica than New Guinea. One interpretation of this new Kuk picture could be that one is confronted with a statelike society where religious powers are at their apogee; in and around the temples live a large population of farmers, perhaps even slaves, intensively working to sustain the elite living in these temples (instead of pigs). Twenty years of present-day Kuk activities do give this picture, thus 9,000 years of farming at Kuk should give a picture for which even Wittfogel and Boserup would be at a loss to explain.

Of course this is a very exaggerated interpretation, perhaps it is even a "Von Danikian" approach to the evidence, but there is certainly a

major point of concern here. This is that there was a "Maya Collapse" in Mesoamerica, and such "collapse" almost occurred in the New Guinea Highlands (Gorecki 1979b). It happened late enough to be still present in memory at the time of European contact. My great concern here is about what could have been said today about the Kuk systems if no ethnographic observations or oral histories were available.

I have presented here two approaches to the Mesoamerican material. One is positive and uses ethnoarchaeological analogies to support and complement hypotheses derived from the archaeological evidence. The second approach, in contrast, uses ethnoarchaeological analogies to question and challenge these hypotheses (cf. data available in Cowgill 1962, 1975). I am not in a position to apply either of them, but those who control the data are.

The last discussion I wish to take up in this section concerns the use of ethnoarchaeological data in general, including this Kuk material, to early hominid studies. In this I simply want to stress similarities in approach regarding "landscape archaeology" between my macro-level analysis and its results (chapter 2) with those followed by others (e.g. Isaac 1981) for sites a few million years old. The aims are the same, i.e. the finding of a "meaningful" surface scatter of artefacts over a defined area (e.g. maps 21 and 23; cf. also Thomas 1975) as well as areas with no evidence at all (cf. Groube 1981). It is not only the end product which is sought by the archaeologist, i.e. surface scatters of remains, but also the relationships between these remains and the landscape and human activities. It is a relationship between the archaeological visibility of cultural remains and the cultural and natural processes leading to it that one tries to understand (cf. Foley 1981).

It is because site formation processes are best analysed within ethnoarchaeology that the interpretation of early hominid sites may rely

on it (e.g. Binford 1981). It is not only data derived from hunter-gatherer societies which should be used in analogies (e.g. Gould 1980; Yellen 1977; Binford 1978b), but also data related to site formation processes of other societies, including those such as the Kawelka from Kuk.

When discussed exclusively in an archaeological context, site formation processes remain solely the domain of hypotheses and assumptions (e.g. Reid et al. 1975). One is aware that at a site processes must have been multiple and complex, but one can hardly grasp or identify them. What remains as archaeologically visible is to be interpreted, even though it is known that these remains were most likely the subject of unknown transformations (cf. Collins 1975).

Collins realizes that, since there is an irreversible discontinuity in prehistory between the archaeological evidence and the activity which made it, the field of ethnoarchaeology is an important tool to be used to "understand more fully the articulation between behavior and its tangible consequences" (Collins 1975:32). To achieve better results in this, ethnoarchaeological research should not be restricted to hunter-gatherer societies under the pretence that most prehistoric sites are the result of hunting and/or gathering activities. This research should be oriented to human activities in general because all prehistoric sites are a partial reflection of specific but unclear human activities (cf. Schiffer 1972).

9-6 CURRENT STUDIES IN ETHNOARCHAEOLOGY

In this section I wish to compare the methodology followed in my research, and its results, with a selected number of studies central to current ethnoarchaeological practice. Four authors are under review here, namely Yellen, P.J. Watson, Gould and Binford. The method followed by each

of these authors was aimed at answering a specific set of questions, and this aim was fulfilled. What I wish to discuss here are not the questions themselves, but rather questions or problems which were not asked from the data, thus giving me an overall impression that each was an unfinished field project. My discussion concerns essentially the long-term approach to the data and its related cultural and natural site formation processes, i.e. problems in the archaeological visibility of remains.

Generally speaking, anthropologists carry out fieldwork which is of short duration (less than two years) and as such the data collected relate events which occurred during a specific time period. Since there are good, average and bad years in social and economic activities for all humans, it is often unclear to what extent the observations made are normal or unusual for the society concerned.

Sometimes data gathered in the field are extrapolated into a longer perspective, and since the original material is uncertain, then the hypothetical long-term view may be even more unrepresentative (e.g. Rappaport 1977:87-98). This is certainly of importance when discussing problems related to archaeology since the time span covered by the field of ethnoarchaeology is only minute when compared with the prehistoric perspective.

The notion of a long-term approach to the ethnographic material is now emerging from the literature as being a critical parameter to be taken into account in ethnoarchaeological studies. Rather than looking at the data as being "standard" for the society under study, discussions on "good" and "bad" periods are now being integrated (e.g. Meehan 1982:90-1, 102-5, 162-6). Repetitive field trips to the same areas, as exemplified by Meehan's research, appear to be full of potential (although not always feasible) and to be of much greater value than speculation on an activity if constantly repeated.

9-6-1 Yellen

Yellen's ethnographic study on a group of hunter-gatherers from the Kalahari desert (1977) has certainly contributed to methodological and theoretical approaches to the treatment of archaeological data. His ethnographic observations are outstanding and the conclusions he reaches should be kept in mind by archaeologists dealing with sites assumed to have been occupied by hunter-gatherers. The data themselves are extensively provided in his monograph (1977), an achievement which must be acknowledged, and are certainly valuable in terms of explanations regarding social structures and spatial organization.

There are nevertheless some points where I am not satisfied with Yellen's data, or his analysis of them, because these do not answer my questions. The first point concerns the long-term approach to the use of campsites. Yellen suggests that the nomadism is seasonal but always dependent on water resources, with the formation of larger groups during the dry season (Yellen 1977:38-9, 64-9; Yellen and Lee 1976:42-6; Lee 1976:84-90). The nomadism described here is not unlike the one discussed in chapter 1 (pp.15-7) and is similar to that observed by Cauneille (1968) among a group of pastoralist nomads.

Both Cauneille and Yellen present a detailed seasonal model which is of prime importance for archaeology, but the question to ask is if this model can be first, visible archaeologically in the short-term, and second, if it withstands a long-term study. The long-term approach to this question appears not to work for Cauneille's model, and possibly for Yellen's model as well. There are too many influential factors affecting the nomadism and settlement pattern of the !Kung to allow them to be archaeologically identifiable when an analytical time-depth is involved.

It is uncommon to find one area which contains resources available exclusively during a specific season. Usually it is a range of resources

which predominates during one season in one area, and other resources within the same area may be available during other seasons or even throughout the year. People may prefer to exploit an area during one specific season, but there is no reason why this cannot happen outside the normal calendar year. As such, campsites and other human activities may occur more frequently during one period of the year in one area, thus leaving behind seasonal cultural markers, but this is certainly not always the case. Because of this, nomads of the Sahara, Alaska, Kalahari or Australian Desert may have a seasonal life-style, but when taking into account factors such as unusual rainfall, rifts between groups and minor climatic changes, it is possible that relics of seasonal activities will, in the long term, overlap with those related to non-seasonal activities within the area. Yellen himself (1977:48) is aware of such problem but does not discuss it.

It is the entire notion of seasonality as interpreted from archaeological evidence which is questioned here. If this cannot be established from ethnographic models because of discrepancies noted over long-term studies, then it might be even harder to define seasonality from prehistoric remains which have been subject not only to destructive processes (Collins 1975) but possibly also to repetitive use for different activities (e.g. Foley 1981:160-1).

The second point which worries me about Yellen's material concerns the campsites themselves, which are said to have a strong tendency to be occupied only once (Yellen 1977:67). I draw attention only to his Camp 1 (1977:147-54+maps). Its mapping involves two known occupations and not one, and also illustrates "old features" presumably from a previous unknown occupation. Furthermore, the families involved "plan to return to this camp" (1977:153). Overall Camp 1 is far from being the result of a single occupation, a point which must be true for most sites occupied by human

beings, particularly in environments where critical resources only occur at a few points in the landscape.

Such details may question the validity of data related to seasonality and settlement patterns not only over a long-term period, but also over a short-term one. The mapping of Camp 1 may not even show all the remains from the activities witnessed by Yellen because the site was the subject of an important natural formation process while being mapped: Yellen left the site for a short period (two days) and one learns, in a rather casual manner, that during his absence Camp 1 was visited and disturbed at least by hyenas "which indicate that the camp has been carefully picked over for edible remains" (1977:149).

This last comment is directly relevant to studies on site formation processes. Camp 1 contains a variety of organic remains such as bones, animal hair, nuts, egg shells, etc. Feature remains are essentially those from huts (post holes) and hearths. It certainly would be of importance to have some indication on which of these may enter the archaeological record and how. I certainly have my doubts about "animal hair" (and "guinea fowl feathers" for that matter) and overall I suspect that archaeological visibility of Camp 1, despite its recurrent occupations, may be extremely low.

A last detail to comment on concerns the evidence of ostrich egg shell fragments at Camp 1. These appear to belong to an earlier occupation, and it is of interest to note that some of these fragments are found inside the hut of Occupation 2. Associated with these "35 ostrich egg shell fragments" are "12 roughly shaped, unfinished ostrich egg shell beads" (p.153; features 131 and 132). I do not know if egg shells, egg shell beads and huts preserve very well, but if they do I would be the first person to indicate that "someone was making beads from ostrich eggs at the entrance of hut 2". Such interpretation would be entirely wrong.

While Camp 1 is valuable data for Yellen to build his model on the economy and social organization of !Kung families, it is of little help in my understanding of ethnoarchaeology. Yellen's work relates to ethnography rather than ethnoarchaeology. As such only the behavioural aspect should be emphasized rather than its archaeology, let alone its ethnoarchaeology. Only a "buckshot" approach, a "hit-or-miss" use of his data (1977:7) should be applied to archaeological material. As such, they are valuable data from which alternative interpretations of a prehistoric site may still be offered. Compared with the approach I have followed in this thesis, Yellen's work appears to be unfinished. This is only partially true, since the campsites described by Yellen still exist in the African bush, and may be excavated one day. Such excavations undoubtedly will become invaluable data on site formation processes among hunter-gatherer economies. It is not only the archaeological visibility which should be defined, but also the "sieving" through the various occupations to see if these can be quantified and defined.

Yellen has followed a methodology comparable to my "Steps 1 and 2", with some consideration for "Step 4". It is when a field study following my "Step 3", together with the long-term view of "Step 1" is done that one may gather important insights into links existing between ethnoarchaeology and archaeology through processes related to archaeological visibility of a known ethnography.

9-6-2 P.J. Watson

Watson's work in Iran is presented in Archaeological ethnography in Western Iran (Watson 1979). Being research in "archaeological ethnography", it should be of relevance to my work at Kuk, particularly since the community concerned is also an agricultural one.

What Watson has done in fact is make an exhaustive study on material culture, economy and social organization. As Yellen does, she provides maximum field data to be used or tested by further studies. The major point I wish to stress is that Watson's study is exclusively ethnographic and as such is similar to Yellen's. Both have probably answered the questions they have asked from the field data, but none are put into an archaeological framework. Watson's focus on "understanding of archaeologically known early farming villages in Southwestern Asia" (1979:7) is based on numerous assumptions, the most important being that "cultural continuity is great" (1979:7) and that archaeological visibility is also great. Neither of these two assumptions has been tested or verified.

The concluding remarks (1979:291-9) are of interest. One learns for instance that at the time of the survey the area was under famine conditions (1979:9), and that crop yields could not be estimated because of a plague caused by insect pests (1979:292). But this is precisely an important parameter to take into account, like a "good year" or other pests such as droughts, epidemics and political upheavals, because these might reflect upon other activities being observed: data on things such as population density, gardened areas, diet and herds size may be directly correlated to these pests.

Data on household space (p.294) are interesting when compared with the Kuk material not only because it is possible that the function can be stated from the form (one of my main interests in this topic) but also because there appears to be, as for Kuk, a marked upper limit in the width of buildings. What is certainly surprising is the low number of data used for this analysis since more are available in the monograph itself (e.g. p.35).

It is when Watson's ethnographic data are compared with the evidence from selected prehistoric sites that two major problems arise. The first one is related to the degree of confidence one must have in applying such analogies. Instead of saying that "cultural continuity is great", it appears that there are major differences between the ethnographic present and prehistoric sites from the same broad geographical region (pp.296-7). This short comparative analysis made with four Near-eastern sites shows that "the archaeological sites considered differ in the organization of once-roofed space from the ethnographic case. In the archaeologically known communities the rooms are markedly smaller" (p.297). This is the only instance where Watson attempts to use her ethnographic material to interpret some selected archaeological evidence, and it is not successful.

This is where Watson's introductory hypotheses fail; they are contradicted by the evidence, probably because the ethnographic material collected is of a superficial kind. As such, analogies from them can still be used, but with a low degree of confidence.

The second problem is complementary to the first one. Watson's data are a "snap-shot" without any time-depth of a specific community at a specific time. As such they have limited ethnoarchaeological value but can still be used under "buckshot" conditions to stress similarities and differences between the present and the past.

While there is no time-depth in Watson's monograph, a lack of long-term approach and data on site formation processes (after all these were not her problems), her data can nevertheless be used as analogies. They will also be used when data on site formation processes and archaeological visibility are sought from the same area. It is only when these become available that, in my view, the work will be related to "archaeological ethnography" and could be used with a higher degree of confidence. Watson's methodological approach followed my "Step 1" only.

9-6-3 Gould

Much has been said by or about Gould's research among Australian hunter-gatherers, and I wish to essentially restrict this review to an evaluation of his ethnographic material (Gould 1968, 1969a, 1969b, 1977, 1980).

Gould's ethnographic observations are available in a scatter of publications, but it appears, to those who know the study area, that he followed only a "loose" research design, resulting in serious "limitations of his observations" (Kimber 1981:15). Gould's material consists of a collection of observations, often casual and unrelated to one another, over a large geographical region; it does not consist of a co-ordinated programme such as the one followed by Yellen. Gould's methodology is reviewed here through his data on two campsites, namely Tika-Tika and Wanampi Well.

Tika-Tika is one of the most reliable water supplies in the region and as such is an attractive campsite for present and past Aboriginal groups. Gould recorded two sets of observations. The first set concerned observations on various occupations of Tika-Tika, in the same way as Yellen, with the major difference being that Gould does not provide many details. Gould noticed numerous site formation processes, without realising their archaeological importance, such as erosion resulting in destruction of any evidence in soft soils, preservation in hard soils, perhaps preservation of post holes (and even posts) from shelter building, and a strong tendency to reuse the campsites (1968:106-7).

The second set of observations concerns the "archaeological implications" of the ethnographic material (e.g. 1980:23-8). Tjapaltjari's marital problems have no place here; it is the archaeological visibility of Tjapaltjari's activities that is the source of my questions. For this, Gould restates some site formation processes observed at Tika-Tika.

Five months after the last occupation of this site, there are important natural processes operating, resulting in major destruction or burial of some artefacts and features. The decay rate appears to be high, vegetation is growing back, most of the lithic material has by now become uninformative about this occupation because of its integration with lithic material from earlier occupations. It appears that in a long-term approach, beside large grinding slabs, the entire site will be "useless" in archaeological terms (Gould 1980:27).

According to Gould, analogies cannot be drawn for Tika-Tika, it is a site of "limited use for archaeology. This use hardly justifies the scholarly and logistical efforts one usually has to put into the study of living archaeology, and it cannot serve as a basis for making such studies if they are to be productive" (Gould 1980:28).

I entirely reject such statement; it is Gould's "efforts" which are hardly justified. Gould's ethnographic observations were restricted to "sketch notes" on a restricted body of data. It appears that only "rough" maps were drawn from observed campsites (e.g. 1980:1; 1977:31,35,36,38, 39,44,45), nothing approaching the type of minute and precise recording and mapping required, similar to those offered by Yellen (1977), O'Connell (1977) or Hayden (1979); there is no time depth to Gould's perspective, and this could have been provided with some limited test excavations (e.g. Hayden 1979). While Gould notices important processes in progress, he does not note, grasp or quantify them. Perhaps these were not the questions he asked of his data, but given the way he tackles his archaeological material (prehistoric rock shelters), it appears that the observation of these processes should have been at the forefront of his research.

I do not wish to discuss his excavations beyond stating that his ethnographic data clearly point towards drastic differences of behaviour

between open campsites (domestic/daily activities) and most rock shelters (ceremonial/specialised activities).

Despite his strong cautionary statements, Gould regularly uses his "anomanalogies" (my word) derived from what appears to be overall ethnographic trends noted in Western Desert campsites. Such trends certainly do not emerge from his description of the ethnographic material. Both the ethnographic and prehistoric "floors" follow unconvincing arguments since their interpretations discard all notion of disturbance process. While Gould noted that at Tika Tika there were site formation processes operating at a rapid rate, resulting in dramatic changes in archaeological visibility of remains only five months after site abandonment, in the analogies used for the interpretation of his prehistoric evidence it appears that these processes are to Gould insignificant: a floor today is similar to a floor 10,000 years old.

In my opinion, Gould's material indicates that after all there could perhaps be some correlation between the present and the past. It also indicates his lack of interest in site formation processes. Trampling is one of them, and an important one in rock shelters, which has not been taken seriously into account by Gould (e.g. Stockton 1973; Rick 1976; Glover 1979; Wood and Johnson 1979; Stern 1980; Courtin and Villa 1982).

Surely if Tika-Tika was difficult to record, Gould should have then tackled Wanampi Well with more care than he did (Gould 1968:109-11). This is an artificial campsite created by Europeans in 1964 only, when a bore was built at a location rich in at least animal resources; lack of water had previously made it a hazardous area for humans. Gould was at Wanampi Well only three years after it was created and as such could have recorded an outstanding body of material, only a few years old, related to human activity at and from a stable base camp (the equivalent to Tika-Tika but

with a dramatically different time-depth). This was not done, or only superficially (1968:110; 1977:31). By mastering some of the processes in operation at Wanampi Well, Gould may have been able to return to Tika-Tika and perhaps put some "order" in the "mess" observed there. With the casual field strategy followed by Gould, only a failure could result when he

... thinks about and actually encounters the linkages between behavior and material residues in the context of living, contemporary human societies... (Gould 1980:x)

Gould's methodology covers only partially my "Step 1" (part of the short-term approach only) before moving straight into analogies with deposits from prehistoric rock shelters. Speculatively, I guess there could be an unexpected high correlation in site formation processes between Kuk and Tika-Tika/Wanampi Well, despite the environmental, cultural and economic differences existing between these two areas. This may be because of strong similarities between the lack of sedimentation in the two regions resulting in an accumulation of cultural debris within a single stratigraphic unit. If this is so, then site formation processes from the Australian Desert region should be defined and compared with the "trends" noted at Kuk; these data, when eventually added to others from elsewhere, may become more generalisable.

9-6-4 Binford

The last research I wish to comment on is that carried out among the Nunamiut Eskimo by Binford (1977, 1978a, 1978b, 1981, 1982). In my view it is real "ethnoarchaeological" research, where field data gathered go beyond mere ethnographic observations. Binford's main concern or emphasis was on faunal remains. What Binford demonstrates is that "trends" can be defined by duplicating observations of one specific activity

performed at various sites and various activities performed at one specific site, resulting in solid arguments in favour of "meaningful" signatures or messages left at all sites (1978a:357).

Binford's field research (1978b) involved short-term and long-term ethnographic observations ("Step 1"), it involved surface survey and recording of remains left by specific activities at specific locations during and after their active life ("Step 2"), it involved the excavation of selected sites having various length of abandonment but used during living memory ("Step 3") and it involved determining the patterning of all these data ("Step 4").

Through this material Binford has looked at extensive cultural and natural site formation processes (1977) resulting in various levels of archaeological visibility of remains. More data are perhaps needed on the long-term implications in support of Binford's observations (cf.1978b: 490-5), it is certainly the question of archaeological visibility of remains at a site having recurrent occupations which is the weakest point in Binford's methodology. Overall his ethnographic excavations appear to have been limited (1978b:428-35) and their results (i.e. archaeological visibility of remains) are yet unpublished.

Having defined "trends" in faunal remains (1978b), Binford then moves to apply analogies derived from his data to interpret the evidence from prehistoric sites (1981). While I find these to be legitimate alternative interpretations of the archaeological evidence, I may question his strong reliance, an almost exclusive one, on his own ethnoarchaeological material at the expense of others which are also of relevance for the study of faunal remains. After all Eskimo behaviour patterns may differ from early African hominids, and Alaskan wolves and dogs may behave differently from African hyenas or lions.

Similarly, Binford raises some general theoretical issues, here again based essentially on his Nunamiut experience (1982). It seems to me that for this Binford could have used the ethnoarchaeological "data bank" so far available in the literature to complement his Eskimo material. One has to try to go beyond a localized or regional case study to a more general problem related to trends in site formation processes and archaeological visibility of remains left from known activities.

Nevertheless, it is obvious that Binford's study should be taken as a guideline for further research in ethnoarchaeology. What emerges from his work is that I probably have been on the right methodological track at Kuk, and that my contribution is a modest one in comparison.

9-7 FUTURE ORIENTATIONS

The field of ethnoarchaeology within the discipline of archaeology started relatively recently. Contributions such as those provided by Yellen, Watson and Gould were pioneering it and as such should be seen as the first to invest into a "data-bank" on ethnographic data (e.g. Couchaux 1980) or ethnoarchaeological data to offer a range of analogies to be used in archaeology. They are the foundation for the development of a more refined and solid theoretical and methodological framework on what ethnoarchaeology should be.

Following Binford's contribution, supported by the thesis presented here, it is now the time to move one step further and integrate into ethnoarchaeology two "compulsory" parameters: the long-term approach and the excavation of the ethnography. It is my belief that by including them in the corpus of any ethnoarchaeological study one must get one step closer in our understanding of prehistoric material. Discovering the archaeological visibility of remains left by known activities is an essential step to understanding the past.

The notion of "stabilization" in site formation processes should be defined through test excavations of recent sites. If this stabilization is noted after a relatively short period (i.e. within sites used during living memory; see comments in Collins 1975:30), it is then more interesting to carry further excavations on sites which have been the subject of such major processual stabilization.

The understanding of site formation processes is undoubtedly important in archaeology. These can be discussed and defined at a theoretical level (e.g. Schiffer 1976; Foley 1981), but are more difficult to discover at a practical level. In contrast, it is the archaeological visibility of remains resulting from these processes which appear to be more readily identified and quantified in practical terms. It is only by gathering data from various environments, economies and regions that the range of archaeological visibilities of various activities can be best understood.

It is by applying a long-term model (bounded by the limits of historical records or living memories) to data derived from ethnographic observations as well as excavations of recent but stabilized sites that we would get slowly closer to some aspects of prehistoric life. Critics will always argue that the timescale involved in such studies is incompatible with the wider prehistoric material; perhaps it is so, but it certainly is more positive to offer new insights in the interpretation of archaeological evidence. Most recent ethnoarchaeological studies or projects follow this new "in-depth" line (e.g. Hayden et al. 1977; Meehan 1982) and their results or aims are certainly of direct relevance to many aspects of archaeology.

It has been stated that -

The opening up of the highlands of Australian New Guinea during and after the Second World War provided the last opportunity to observe neolithic cultivators in their

uncontaminated state and it is tragic that it should have been so largely missed: many anthropological research workers, indeed, went there, but these concentrated mainly on certain aspects of social structure and kinship and neglected to make a complete and balanced study of these last communities of neolithic type. Even so their records of the New Guinea highlands are of value simply because they were made by actual witnesses (Clark 1965:19).

Such comment could be addressed to most anthropological studies carried out anywhere in the world. For New Guinea it concerns one specific ethnography (European contact period) of one specific area (Highlands). We should seek an ethnography of one culture from one area at a given time. It is only with a solid methodological framework that ethnographic problems which could be related to the field of archaeology can be tackled. The diversity of environments, economies and cultures found within Papua New Guinea, even today, makes the region outstanding for such ethnoarchaeological research.

Now that my Kuk research is done, I believe that some of Heider's worries (Heider 1967) could be removed by following a similar ethnoarchaeological research design among the Dani. Overall, my hope is that the work presented here may contribute to the discipline of archaeology; in a particular aspect for the Wahgi Project and in the general aspect for the understanding of site formation processes aimed at the interpretation of past human activities.

BIBLIOGRAPHY

Abbreviations used:

AIAS	Australian Institute of Aboriginal Studies, Canberra
ANU	Australian National University, Canberra
ANZAAS	Australian and New Zealand Association of the Advancement of Science
APAO	Archaeology and Physical Anthropology in Oceania
CNRS	Centre National de la Recherche Scientifique
CSIRO	Commonwealth Scientific and Industrial Research Organisation
DPI	Department of Primary Industry, Port Moresby
IPNGS	Institute of Papua New Guinea Studies, Port Moresby
PNG	Papua New Guinea
PR	Patrol Report (to the Administration in Papua New Guinea)
UPNG	University of Papua New Guinea

Abrams, I.R. 1977. Group net fishing in Northern Belize: Nutritional implications for the Maya lowlands. JOURNAL OF BELIZEAN AFFAIRS 5:64-69.

Allen, F.J. 1970a. Prehistoric agricultural systems in the Wahgi Valley: a further note. MANKIND 7(3):177-183.

_____. 1970b. A collection of flaked stone artefacts from the Western Highlands. RECORDS OF THE PNG MUSEUM 1(1):47-53.

Attenborough, D. 1981. JOURNEYS TO THE PAST. Guildford: Lutterworth Press.

Aufenanger, H. 1961. The use of bones. ANTHROPOS 56:869-882.

Behrensmeyer, A.K. and A.P. Hill 1980. FOSSILS IN THE MAKING. Chicago: University of Chicago Press.

Becquelin, P. 1973. Ethnologie et archeologie dans l'aire maya: analogies et évolution culturelle. JOURNAL DE LA SOCIÉTÉ DES AMERICANISTES, Tome LXII:57-87.

Binford, L.R. 1977. Forty seven trips. In R.V.S. Wright (ed.) STONE TOOLS AS CULTURAL MARKERS. AIAS, pp.24-36.

_____. 1978a. Dimensional analysis of behavior and site structure. AMERICAN ANTIQUITY 43(3):330-361.

_____. 1978b. NUNAMIUT ETHNOARCHAEOLOGY. Academic Press.

_____. 1981. BONES. Academic Press.

- Binford, L.R. 1982. The Archaeology of Place. JOURNAL OF ANTHROPOLOGICAL ARCHAEOLOGY 1(1):5-31.
- Blong, R.J. 1982. THE TIME OF DARKNESS. ANU Press.
- Bordes, F. 1980a. Question de contemporanéité: L'illusion des remontages. BULLETIN DE LA SOCIÉTÉ PRÉHISTORIQUE FRANÇAISE 77(5):132-133.
- . 1980b. Savez-vous remonter les cailloux à la mode de chez nous? BULLETIN DE LA SOCIÉTÉ PRÉHISTORIQUE FRANÇAISE 77(8):232-234.
- Brookfield, H.C. and P. Brown 1963. STRUGGLE FOR LAND. Oxford University Press.
- Brookfield, H.C. and D. Hart 1966. RAINFALL IN THE TROPICAL SOUTHWEST PACIFIC. Geography, Publication G/3, ANU, Canberra.
- Brown, P. and H.C. Brookfield 1959. Chimbu land and society. OCEANIA 30(1):1-75.
- Bulmer, S. 1964. Prehistoric stone implements from the New Guinea Highlands. OCEANIA 34(4):246-268.
- . 1975. Settlement and economy in prehistoric Papua New Guinea. JOURNAL DE LA SOCIÉTÉ DES OcéANISTES 31(46):7-75.
- . 1977. Between the mountains and the plains. In J.H. Winslow (ed.), THE MELANESIAN ENVIRONMENT: CHANGE AND DEVELOPMENT. ANU Press.
- Bulmer, S. and W.C. Clarke 1970. Two stone spear or dagger heads from the Bismarck Mountains, Australian New Guinea. RECORDS OF THE PNG MUSEUM 1(1):42-46.
- Cahen, D. 1980. Question de contemporanéité: L'apport des remontages. BULLETIN DE LA SOCIÉTÉ PRÉHISTORIQUE FRANÇAISE 77(8):230-232.
- Cauneille, A. 1968. LES CHAANBA. Paris: CNRS.
- Chappell, J. 1966. Stone axe factories in the Highlands of East New Guinea. PROCEEDINGS OF THE PREHISTORIC SOCIETY 32(5):96-121.
- Childe, V.G. 1956. PIECING TOGETHER THE PAST. New York: Praeger.
- Christensen, O.A. 1975a. A tanged blade from the New Guinea Highlands. MANKIND 10(1):37-39.
- Clark, J.G.D. 1980. MESOLITHIC PRELUDE. Edinburgh University Press.
- Clark, R.M. 1975. A calibration curve for radiocarbon dates. ANTIQUITY 49:251-266.
- Clarke, W.C. 1966. From extensive to intensive shifting cultivation: a succession from New Guinea. ETHNOLOGY 5:347-359.

- Clarke, W.C. 1971. PLACE AND PEOPLE. ANU Press.
- Clarke, W.C. and J.M. Street 1967. Soil fertility and cultivation practices in New Guinea. THE JOURNAL OF TROPICAL GEOGRAPHY (24):7-11.
- Couchaux, D. 1980. HABITATS NOMADS. Édition Alternative et Parallèles.
- Courtin, J. and P. Villa 1982. Une experience de piétinement. BULLETIN DE LA SOCIÉTÉ PRÉHISTORIQUE FRANÇAISE 79(4):117-123.
- Cowgill, V.M. 1962. An agricultural study of the southern Maya lowlands. AMERICAN ANTHROPOLOGIST 64:273-286.
- Gowgill, G.L. 1975. Population pressure as a non-explanation. In A.C. Swedlund (ed.) POPULATION STUDIES IN ARCHAEOLOGY AND BIOLOGICAL ANTHROPOLOGY: A SYMPOSIUM. Memoirs of the Society for American Archaeology No.30:127-131.
- Denevan, W.M. 1963. Additional comments on the earthworks of Mojos in northeastern Bolivia. AMERICAN ANTIQUITY 28(4):540-545.
- Denevan, W.M. and B.L. Turner II. 1974. Forms, functions and association of raised fields in the Old World Tropics. JOURNAL OF TROPICAL GEOGRAPHY 39:24-33.
- Donaldson, M. and K. Good 1981. The Eastern Highlands, coffee and class. In D. Denoon and C. Snowden (eds), A TIME TO PLANT AND A TIME TO UPROOT. Port Moresby: IPNGS, pp.143-169.
- Dornstreich, M.D. 1974. AN ECOLOGICAL STUDY OF GADIO ENGA (NEW GUINEA) SUBSISTENCE. Ann Arbor, Michigan: University Microfilms.
- Efremov, I.A. 1940. Taphonomy: New branch of palaeontology. PAN AMERICAN GEOLOGY 74(2):81-93.
- Elsmore, R.T. 1945. New Guinea's mountain and swampland dwellers. NATIONAL GEOGRAPHIC 88(6):671-694.
- Family of Man. 1979. MARSHALL CAVENDISH ENCYCLOPEDIA 5(61).
- Flannery, K.V. (ed.) 1979. MAYA SUBSISTENCE. Academic Press.
- Fletcher, R. 1977. Alternatives and Differences. In M. Spriggs (ed.), ARCHAEOLOGY AND ANTHROPOLOGY: AREAS OF MUTUAL INTEREST. Oxford: British Archaeological Reports.
- _____. 1978. Issues in the analysis of settlement space. In D. Green et al. (eds), SOCIAL ORGANISATION AND SETTLEMENT. Oxford: British Archaeological Reports 47, pp.225-240.
- _____. 1981. People and space: a case study on material behaviour. In I. Hodder et al. (eds), pp.97-128.

- Foley, R. 1981. Off-site archaeology: an alternative approach for the short-sited. In I. Hodder et al (eds), pp.157-183.
- Gifford, D.P. 1980. Ethnoarchaeological contributions to the taphonomy of human sites. In A.K. Behrensmeyer and A.P. Hill (eds).
- _____. 1981. Taphonomy and paleoecology: a critical review of archaeology's sister disciplines. In M.B. Schiffer (ed.) ADVANCES IN ARCHAEOLOGICAL METHOD AND THEORY vol.4, pp.365-425. Academic Press.
- Gitlow, A.L. 1966. ECONOMICS OF THE MOUNT HAGEN TRIBES, NEW GUINEA. University of Washington Press.
- Glover, I.C. 1979. The effects of sink action on archaeological deposits in caves: an Indonesian example. WORLD ARCHAEOLOGY 10:302-317.
- Golson, J. 1977a. No room at the top: agricultural intensification in the New Guinea Highlands. In J. Allen et al (eds), SUNDA AND SAHUL, pp.601-638. Academic Press.
- _____. 1977b. Simple tools and complex technology. In R.V.S. Wright (ed.), STONE TOOLS AS CULTURAL MARKERS, pp.154-161. AIAS.
- _____. 1981a. New Guinea agricultural history: a case study. In D. Denoon and C. Snowden (eds), A TIME TO PLANT AND A TIME TO UPROOT. IPNGS, Port Moresby, pp.55-64.
- _____. 1981b. Agriculture in New Guinea: the long view. In D. Denoon and C. Snowden (eds), pp.33-42.
- _____. 1981c. Agricultural technology in New Guinea. In D. Denoon and C. Snowden (eds), pp.43-54.
- Golson, J. and P. Hughes 1977. Ditches before time. HEMISPHERE 21(2):13-21.
- Gorecki, P.P. 1978. Further notes on prehistoric wooden spades from the New Guinea Highlands. TOOLS AND TILLAGE 3(3):185-190.
- _____. 1979a. The Kuk obelisk. ORAL HISTORY 7(4):19-28.
- _____. 1979b. Population growth and abandonment of swamplands: a New Guinea Highlands example. Journal de la Société des Océanistes 63:97-107.
- _____. 1979c. Disposal of human remains in the New Guinea Highlands. APAO 14(2):107-117.
- _____. 1979d. Irrigated taro fields in the Highlands. ORAL HISTORY 7(7):116-120.
- Gorecki, P.P. (ed.) 1981a. EARLY RECORDS OF STONE ARTEFACTS AND THE IMPACT OF STEEL IMPLEMENTS IN THE CENTRAL HIGHLANDS OF PAPUA NEW GUINEA. Oceania Monograph 23.

- Gorecki, P.P. 1982b. Human occupation and agricultural development in the New Guinea Highlands. In C. Pain (ed.), *GEOECOLOGY OF THE PAPUA NEW GUINEA HIGHLANDS* (in press).
- _____. 1982c. Staple and secondary crops throughout the New Guinea Highlands: the proto-historical evidence (in preparation).
- Gould, R.A. 1968. Living archaeology: the Ngatatjara of Western Australia. *SOUTHWESTERN JOURNAL OF ANTHROPOLOGY* 24(2):101-122.
- _____. 1969a. YIWARA. Collins.
- _____. 1969b. Subsistence behaviour among the western desert Aborigines of Australia. *OCEANIA* 39(4):253-274.
- _____. 1977. PUNTUTJARPA ROCKSHELTER AND THE AUSTRALIAN DESERT CULTURE. Anthropological papers of the American Museum of Natural History, vol.54. New York.
- _____. 1980. *LIVING ARCHAEOLOGY*. Cambridge University Press.
- Gould, R.A. and M.B. Schiffer (eds). 1981. *MODERN MATERIAL CULTURE*. Academic Press.
- Griffin, B.F. 1958. PR Hagen 3-57/8.
- Groube, L. 1981. Black holes in British prehistory: the analysis of settlement distributions. In I. Hodder *et al* (eds), *PATTERN OF THE PAST*, pp.185-209.
- Grove, D.S. 1946. PR Hagen 1-45/6.
- Haantjens, H.A. *et al*. 1970. *LANDS OF THE GOROKA-MOUNT HAGEN AREA, TERRITORY OF PAPUA AND NEW GUINEA*. Land Research Series No.27, CSIRO.
- HAC. 1976. *UPPER JIMI VALLEY NUTRITION*. Highlands Agricultural College, Mount Hagen.
- Hammond, G. and N. 1981. Child's play: a distorting factor in archaeological distribution. *AMERICAN ANTIQUITY* 46(3):634-636.
- Hammond, N. 1978. The myth of the Milpa: agricultural expansion in the Maya lowlands. In Harrison and Turner (eds), pp.23-34.
- Harris, E.C. 1977. Hed Mound. A New Guinean house site. Unpublished paper. Prehistory, Research School of Pacific Studies, ANU, Canberra.
- Harris, E.C. and P.J. Hughes, 1978. An early agricultural system at Mugumamp Ridge. *MANKIND* 11(4):437-444.
- Harrison, P.D. and B.L. Turner (eds). 1978. *PRE-HISPANIC MAYA AGRICULTURE*. University of Mexico Press.

- Hayden, B. et al. 1977. The Coxoh ethnoarchaeology project: 1977. Preliminary report. Mimeo, Simon Fraser University.
- Hayden, B. 1979. PALAEO LITHIC REFLECTIONS. AIAS.
- Heider, K.G. 1967. Archaeological assumptions and ethnographical facts: a cautionary tale from New Guinea. SOUTHWESTERN JOURNAL OF ANTHROPOLOGY 23:52-64.
- _____. 1970. THE DUGUM DANI. Viking Fund Publications in Anthropology, No.49.
- Hodder, I. et al. (eds). 1981. PATTERN OF THE PAST. Cambridge University Press.
- Holmes, C.H. 1953. Hidden people of the high valleys. WALKABOUT, August, pp.11-19.
- Hope, J.H. and G.S. Hope 1976. Palaeoenvironments for man in New Guinea. In R.L. Kirk and A.G. Thorne (eds), THE ORIGIN OF THE AUSTRALIANS. AIAS, pp.29-54.
- Hughes, I. 1977. New Guinea Stone age trade. TERRA AUSTRALIS 3. ANU.
- Isaac, G. 1981. Stone age visiting cards: approaches to the study of early land use patterns. In I. Hodder et al. (eds), pp.131-155.
- Journal of Belizean Affairs 1977. SPECIAL ISSUE: THE RIO HONDO PROJECT, No.5. Belize City.
- Julien, M. 1972. Témoins relatifs au feu. In A. Leroi-Gourhan and M. Brézillon (eds), FOUILLES DE PINCEVENT, pp.279-294. CNRS, Paris.
- Kelly, R.C. 1968. Demographic pressure and descent group structure in the New Guinea Highlands. OCEANIA 39(1):36-63.
- Kimber, R.G. 1981. Reflections on Living Archaeology. AUSTRALIAN ARCHAEOLOGY 13:12-15.
- Knuttson, J. 1978. Pig killing ceremony (Bukainge) among the Kunanaku of the Kerowagi sub-province. ORAL HISTORY 6(7):2-33.
- Lampert, R.J. 1972. HAGEN AXES. Mimeo, ANU.
- _____. 1973. Using ethnography to interpret prehistoric houses excavated in the New Guinea Highlands. Unpublished ANZAAS paper.
- Leahy, M.J. 1936. The Central Highlands of New Guinea. GEOGRAPHICAL JOURNAL 87:229-262.
- Leahy, M.J. and Crain, M. 1937. THE LAND THAT TIME FORGOT. London: Hurst and Blackett.
- Lee, R.B. 1976. !Kung spatial organisation. In R.B. Lee and I. De Vore (eds), KALAHARI HUNTER-GATHERERS. Harvard University Press, pp. 74-97.

- Leroi Gourhan, A. 1973a. *L'Homme et la matière*. Albin Michel, Paris.
- _____. 1973b. *Milieu et techniques*. Albin Michel, Paris.
- Linsley, G. 1951. PR Hagen 2-1950/1.
- Logan, K.J. 1977. PR Hagen 1977.
- Luzbetak, L. 1956. Worship of the dead in the Middle Wahgi. *ANTHROPOS* 51:81-96.
- Macewan, J.M. 1978. Subsistence agriculture. *AGRICULTURE IN THE ECONOMY*. DPI, Port Moresby.
- Macilwain, R.I. 1957. PR Minj 1-56.7.
- Marshall, A. 1979. Axially-pitched longhouses from New Guinea and Neolithic Europe. *APAO* 14(2):99-106.
- McAlpine, J.R. 1970. Climate of the Goroka-Mount Hagen area. In H.A. Haantjens *et al.*, pp.66-79.
- McIntosh, R.J. 1974. Archaeology and mud wall decay in a West African village. *WORLD ARCHAEOLOGY* 6:154-171.
- McLaren, A. 1982. Coffee, land and society in the highlands of Papua New Guinea. BA(Hons) thesis, University of Sydney.
- Meehan, B. 1982. SHELL BED TO SHELL MIDDEN. AIAS.
- Modjeska, C.J.N. 1977. Production among the Duna. PhD thesis, ANU.
- Morren, G.E.B. 1975. Settlement strategies and hunting in a New Guinea society. PhD thesis. University Microfilms, Ann Arbor, Michigan.
- O'Connell, J.F. 1977. Room to move: contemporary Alyawara settlement patterns and their implications for Aboriginal housing policy. *MANKIND* 11(2):119-131.
- Parsons, J.J. and W.M. Denevan 1967. Pre-Columbian ridged fields. *SCIENTIFIC AMERICAN* 217(1):92-100.
- Pospisil, L. 1963. KAPAUKU PAPUAN ECONOMY. Yale University publication in Anthropology No.67.
- Powell, J.M. 1970a. The impact of man on the vegetation of the Mt Hagen region, New Guinea. Unpublished PhD thesis, ANU.
- _____. 1970b. The history of agriculture in the New Guinea Highlands. *SEARCH* 1:199-200.
- _____. 1974. A note on wooden gardening implements of the Mt Hagen region, New Guinea. *RECORDS OF THE PNG MUSEUM* 4:21-28.
- _____. 1982. Plant resources and palaeobotanical evidence for plant use in the Papua New Guinea Highlands. *ARCHAEOLOGY IN OCEANIA* 17(1):28-37.

- Powell, J.M. et al. 1975. AGRICULTURAL TRADITIONS OF THE MOUNT HAGEN AREA. UPNG, Department of Geography, Occasional Papers No.12.
- Puleston, D.E. 1977. Experiments in prehistoric raised field agriculture: learning from the past. JOURNAL OF BELIZEAN AFFAIRS 5:36-43.
- Rathje, W.L. 1979. Modern material culture studies. In M.B. Schiffer (ed.), ADVANCES IN ARCHAEOLOGICAL METHOD AND THEORY vol.2, pp.1-38. Academic Press.
- Rhoads, J.W. 1980. Through a glass darkly: present and past land-use systems of Papuan sago palm users. PhD thesis, ANU.
- Rice, D.S. 1978. Population growth and subsistence alternatives in a tropical lacustrine environment. In P.D. Harrison and B.L. Turner (eds), PRE-HISPANIC MAYA AGRICULTURE, pp.35-61.
- Rick, J.W. 1976. Downslope movement and archaeological intrasite spatial analysis. AMERICAN ANTIQUITY 41:133-144.
- Rickwood, F.K. 1954. The geology of the Western Highlands of New Guinea. JOURNAL OF THE GEOLOGICAL SOCIETY IN AUSTRALIA 2:63-82.
- Ross, W.A. 1936. Ethnological notes on Mt Hagen tribes (Mandated Territory of New Guinea). ANTHROPOS 31:341-363.
- Salisbury, R.F. 1962. FROM STONE TO STEEL. Melbourne University Press.
- Schiffer, M.B. 1976. BEHAVIORAL ARCHAEOLOGY. Academic Press.
- Serpenti, L.M. 1956. CULTIVATORS IN THE SWAMPS: SOCIAL STRUCTURE AND HORTICULTURE IN A NEW GUINEAN SOCIETY. Assen: Van Gorcum.
- Sillitoe, P. 1978. Thunder woman's bind. PARADISE, November, pp.11-14. Port Moresby.
- _____. 1979. GIVE AND TAKE. ANU Press.
- Smith, C.T. et al. 1968. Ancient ridged fields in the region of Lake Titicaca. GEOGRAPHICAL JOURNAL 134.
- Spriggs, M.J.T. 1981. Vegetable kingdoms - taro irrigation and Pacific prehistory. PhD thesis, ANU.
- _____. 1982. Taro cropping systems in the Southeast Asian-Pacific Region: archaeological evidence. ARCHAEOLOGY IN OCEANIA 17(1):7-15.
- Steensberg, A. 1980. NEW GUINEA GARDENS. Academic Press.
- Stern, N. 1980. Taphonomy: some observations about its place in archaeology. BA(Hons) thesis, University of Sydney.

- Stockton, E.D. 1973. Shaw's creek shelter: human displacement of artifacts and its significance. *MANKIND* 9:112-117.
- Strathern, A.J. 1971. *THE ROPE OF MOKA*. Cambridge University Press.
- _____. 1972. *ONE FATHER, ONE BLOOD. DESCENT AND GROUP STRUCTURE AMONG THE MELPA PEOPLE*. ANU Press.
- _____. 1979. 'It's his affair': a note on the individual and the group in New Guinea Highlands societies. *CANBERRA ANTHROPOLOGY* 2(1):98-113.
- Strathern, A.M. 1965. Axe types and quarries. *JOURNAL OF THE POLYNESIAN SOCIETY* 74(2):182-191.
- _____. 1969. Stone axes and flake tools: evaluation from two New Guinea Highlands Societies. *PROCEEDINGS OF THE PREHISTORIC SOCIETY* 35:311-329.
- Sutton, D.G. 1971. Cooking methods in prehistoric New Zealand. BA(Hons) thesis, Otago.
- Swadling, P.L. 1973. *THE HUMAN SETTLEMENT OF THE ARONA VALLEY*. PNG Electricity Commission, Port Moresby.
- _____. 1981. *PAPUA NEW GUINEA'S PREHISTORY*. National Museum, Port Moresby.
- Turner, B.L. II. 1974. Prehistoric intensive agriculture in the Maya lowlands. *SCIENCE* 185:118-124.
- _____. 1976. Population density in the classic Maya lowlands: new evidence for old approaches. *GEOGRAPHICAL REVIEW* 66(1):72-82.
- _____. 1981. The meaning of agricultural intensity and its measure in prehistory: two examples from the Maya lowlands. Paper presented at Conference on Prehistoric intensive agriculture in the Tropics, ANU, August.
- Van Noten, F. 1978. *LES CHASSEURS DE MEER*. *Dissertationes Archaeologicae Gandenses*, De Tempel, Brugge.
- Vicedom, G.F. and H. Tischner 1942-8. *DIE MBOWAMB*. 3 vols. Hamburg. Vol.1 translated in 1973 by F.E. Rheinsteint and E. Klestadt, and Vol.2 translated in 1977 by H. Wurm. Copies held at ANU.
- Waddell, E. 1972. *THE MOUND BUILDERS*. University of Washington Press.
- Watson, P.J. 1979. *ARCHAEOLOGICAL ETHNOGRAPHY IN WESTERN IRAN*. Viking Funds Publications in Anthropology No.57.
- Watson, V.D. and J.D. Cole 1977. *PREHISTORY OF THE EASTERN HIGHLANDS OF NEW GUINEA*. University of Washington Press.
- Williams, F.E. 1940. Lake Kutubu. *OCEANIA* 11(2):121-157.

- White, J.P. 1968. Fabricators, outils écaillés or scalar cores?
MANKIND 6(12):658-666.
- _____. 1972. Ol Tumbuna: archaeological excavations in the
eastern central Highlands, Papua New Guinea. TERRA AUSTRALIS
2, ANU.
- _____. 1977a. The Christensen axe collection. Mimeo, ANU.
- _____. 1977b. AXES AND ARÉ: STONE TOOLS OF THE DUNA. 16 mm colour
film. University of California, Extension Media Center.
- White, J.P. et al. 1970. Kosipe: a late pleistocene site in the Papuan
Highlands. PROCEEDINGS OF THE PREHISTORIC SOCIETY 36:152-170.
- White, J.P. and N. Modjeska 1978. Acquirers, users, finders, losers:
the use axe blades make of the Duna. MANKIND 11(3):276-287.
- White, J.P. and J.F. O'Connell 1982. A PREHISTORY OF AUSTRALIA,
NEW GUINEA AND SAHUL. Academic Press.
- Wobst, M.H. 1978. The archaeo-ethnology of hunter-gatherers or the
tyranny of the ethnographic record in archaeology. AMERICAN
ANTIQUITY 48:303-309.
- Wood, W.R. and D.L. Johnson 1979. A survey of disturbance processes in
archaeological site formation. In M.B. Schiffer (ed.), ADVANCES IN
ARCHAEOLOGICAL METHOD AND THEORY 1:315-381. Academic Press.
- Yellen, J.E. 1977. ARCHAEOLOGICAL APPROACHES TO THE PRESENT.
Academic Press.
- Yellen, J.E. and R.B. Lee 1976. The Dobe-/Du/da environment. In
R.B. Lee and I. De Vore (eds), KALAHARI HUNTER-GATHERERS.
Harvard University Press, pp.28-46.

ADDENDUM

- Clark, J.G.D. 1965. Traffic in stone axe and adze blades.
THE ECONOMIC HISTORY REVIEW 18(1):1-28.
- Collins, M.B. 1975. Sources of bias in processual data: an appraisal.
In J.W. Mueller (ed.), SAMPLING IN ARCHAEOLOGY. University of
Arizona Press, pp.26-32.
- Evans, J.G. and S. Limbrey 1974. The experimental earthwork on
Morden Bog, Wareham, Dorset, England: 1963 to 1972.
PROCEEDINGS OF THE PREHISTORIC SOCIETY 40:170-202.
- Rappaport, R.A. 1977. PIGS FOR THE ANCESTORS. Yale University Press.
- Reid, J.J. et al. 1975. Archaeological considerations of intrasite
sampling. In J.W. Mueller (ed.) SAMPLING IN ARCHAEOLOGY.
University of Arizona Press, pp.209-224.
- Rhoads, J.W. 1982. Sagopalm management in Melanesia: an alternative
perspective. ARCHAEOLOGY IN OCEANIA 17(1):20-27.
- Schiffer, M.B. 1972. Archaeological context and systemic context.
AMERICAN ANTIQUITY 37:156-165.
- Thomas, D.H. 1975. Nonsite sampling in archaeology: up the creek without
a site? In J.W. Mueller (ed.), SAMPLING IN ARCHAEOLOGY.
University of Arizona Press, pp.61-81.

The University of Sydney

Copyright in relation to this thesis*

Under the Copyright Act 1968 (several provisions of which are referred to below) this thesis must be used only under the normal conditions of scholarly fair dealing for the purposes of research, criticism or review. In particular no results or conclusions should be extracted from it, nor should it be copied or closely paraphrased in whole or in part without the written consent of the author. Proper written acknowledgement should be made for any assistance obtained from this thesis.

Under Section 35(2) of the Copyright Act 1968, the 'author of a literary, dramatic, musical or artistic work is the owner of any copyright subsisting in the work'. By virtue of Section 32(1), copyright 'subsists in an original literary, dramatic, musical or artistic work that is unpublished' and of which the author was an Australian citizen, an Australian protected person or a person resident in Australia.

The Act, by Section 36(1), provides: 'Subject to this Act, the copyright in a literary, dramatic, musical or artistic work is infringed by a person who, not being the owner of the copyright and without the licence of the owner of the copyright, does in Australia, or authorises the doing in Australia of, any act comprised in the copyright'.

Section 31(1) (a) (i) provides that copyright includes the exclusive right to 'reproduce the work in a material form'. Thus, copyright is infringed by a person who, not being the owner of the copyright and without the licence of the owner of the copyright, reproduces or authorises the reproduction of a work, or of more than a reasonable part of the work, in a material form, unless the reproduction is a 'fair dealing' with the work 'for the purpose of research or study' as further defined in Sections 40 and 41 of the Act.

Kenneth W. Knight
Registrar

* 'Thesis' includes 'treatise', 'disertation' and other similar productions.

ETHNOARCHAEOLOGY AT KUK:
PROBLEMS IN
SITE FORMATION PROCESSES

Pawel Piotr Gorecki

Volume II

This work is a thesis
submitted for the Degree
of Doctor of Philosophy
in the University of Sydney

December 1982

Volume II

The ordering of material in this volume parallels the order in which this material is referred to in Volume I.

MAPS

1	Location of Study Area	page 1
2	Kawelka Territory	2
3	Area Gardened - 1969	5
4	Area Gardened - 1970	6
5	Area Gardened - 1973	7
6	Area Gardened - 1977	8
7	Major Ditches - 1969	9
8	Major Ditches - 1970	10
9	Major Ditches - 1973	11
10	Major Ditches - 1977	12
11	Ditches Added Between 1969 and 1970	13
12	Ditches Added Between 1970 and 1973	14
13	Ditches Added Between 1973 and 1977	15
14	Planilateral Axes	17
15	Lenticular Axes	18
16	Axes with Battered Sides	19
17	Pebble Axes	20
18	Axe Blanks	21
19	Rejuvenation of Cutting Edge	22
20	Flakes from Ground Axes	23
21	Flakes from Blank Axes	24
22	Other Stone Artefacts	25
23	Chert Concentration	26
24	Location of House Sites	61
25	Location of Cooking Sites	106
26	Location of Garden Sites	133

FIGURES

1	Tanged Blade	28
2	Stone Knives	29
3	Stone Tools - Steep Angle Edge	30
4	Other Stone Tools (Chert)	31
5	Glass Artefacts	32
6	House Site Definition	35
7	Pita Homestead	37
8	Ru-Rea Homestead	38
9	Men's Round House	41
10	Plan of Oval Houses	44
11	Plan of Oval Houses	45
12	Plan of Woman and Pig Houses	47
13	Plan of Veranda Houses	49
14	Plan of Menstruation Houses	51
15	Plan of Pig Houses	51
16	Cassowary Trough	52
17	Pita House	56
18	Pita House	58
19	Pita House	59
20	Pita House: Interpretation	60
21	Site MOP	64
22	Site MOG/R	65
23	Site MOH	66
24	Site MOH/R	67
25	Site MOQ	68
26	Site MOQ	69
27	Site MOL	70
28	Site MOL	71

29	Site MOO	77
30	Site MOO/Phase 1	77
31	Site MOO/Phase 2	78
32	Site MOO/Phase 3	78
33	Site MOD	80
34	Site MOD - Reconstruction	81
35	Site MOE	82
36	Site MOE - Reconstruction	82
37	Site MOH/WP	83
38	Stone Pathway	84
39	Site MOF	87
40	Site MOF - Reconstruction	88
41	Sites MOH/M and MOH/T	90
42	Site MOM	91
43	Site MON	91
44	Site MOK	93
45	Site MNT	93
46	Sites MOI and MOJ	95
47	Plan and Section of Ovens	103
48	Two Types of Traditional Cooking	105
49	Site MNG	108
50	Site MNI	110
51	Site MNI: Chronology of Ovens	111
52	Site MNH	113
53	Ru Garden	119
54	Konga Garden	120
55	Korua Garden	122
56	Nigints Garden	123
57	Communal Garden - Theoretical Model	125
58	Muk Garden	126

59	Rea-Nung Garden. Phase 1	127
60	Rea-Nung Garden. Phase 2	128
61	Rea-Nung Garden. Phases 1 and 2	129
62	Swamp Gardens. Phase 1	130
63	Swamp Gardens. Phase 2	131
64	Site MNU/1	137
65	Site MNU/2	138
66	Trenches from MNU/3	139
67	Site MNU/3	140
68	Site MNU/3	141
69	Site MNV/1	142
70	Site MNV/3	143
71	Section of MNV/3	143
72	Site MNV/4	143
73	Ditch/Fence Enclosure	143
74	Site MOA	144
75	Site MOB	145
76	Site MOC	146
77	Site MOC - Phase 3	147
78	Site MOC - Phase 2	148
79	Site MNI	149
80	Site MNX	150
81	Site MNX - Levels	151
82	Site MNY	152
83	Site MNZ	153
84	Site MNZ - Interpretation	154

TABLES

1	South Kuk - Land Use	16
2	Surface Collection - Chert Artefacts	27
2a	House Site Limits	36
3	Ru-Rea Homestead	39
4	Evolution of House Types	40
5	Houses Measurements: Men's Round and Menstruation	42
6	Dimensions of Oval Houses	43
7	Dimensions of Woman and Pig Houses	46
8	Dimensions of Veranda and Kiap Houses	48
9	Dimensions of Other Buildings	50
10	Length and Width of All House Types	53
11	Number of Rooms According to House Length	54
12	Correlation between Door/Room and Length/Type of Houses	55
13	Key for Figure 17 (Pita House)	57
14	Site MOP	72
15	Site MOG/R	73
16	Site MOH/R	74
17	Site MOQ	75
18	Site MOL	76
19	Site MOO	79
20	Site MOD	85
21	Site MOH/WP	86
22	Site MOF	89
23	Sites MOM and MON	92
24	Site MOK	94
25	Cassowary Cages	96
26	House Site Formation Processes	97
27	Typology of Domestic Ovens	100
28	Typology of Ceremonial Ovens	100

29	Measurements of Ovens Located Inside Houses	101
30	Measurements of Ovens Located Outside Houses	102
31	Case Study on Cooking Stones	104
32	Site MNG	109
33	Site MNI	112
34	Site MNH	114
35	Measurements of Prehistoric Ovens	115
36	Dimensions of Stake Holes	121
37	Size and Number of Plots in Gardens	124
38	Formal Ditches	132
39	Functional Ditches	132
40	Ditch Dimensions - Top Width	132
41	Ditch Dimensions - Depth	132

PLATES

1 and 2	Korua Garden, Kukrumdi. Dryland	Front of Vol. I
3	Dryland, Kukrumdi. Aerial view	3
4	Dryland, Kukrumdi. Aerial view	3
5	Dryland, Kukrumdi. Aerial view	4
6	Swampland, Kuning. Aerial view	4
7	Artificial House Platform. Dryland	33
8	Oval House. Swampland	33
9	Woman and Pig House Burned Down	34
10	New Hearth	34
11	Site MOP. Excavation in Progress	62
12	Site MOH/WP. Stone Pathway	62
13	Site MOH/R. Hearth	63
14	Site MOP. Hearth	63
15	Transfer of Heated Stones to Oven	98
16	Heated Stones Left Unused	98
17	First Stones in Oven	99
18	Last Stones in Oven	99
19	Site MNG, Feature B4-C4	107
20	Site MNG, Feature D3-E3	107
21	New Swamp Garden, Tibi Residue	116
22	New Swamp Garden, Kum	116
23	Ditch and Fence Enclosure, Mup	117
24	Communal Garden, Gengla	117
25	Extension of Major Ditch, Kukrumdi	118
26	New Shifted Grid Pattern, Kum	118
27	Site MNV/1	134
28	Site MOA	134
29	Site MNX	135

30 Site MNX

135

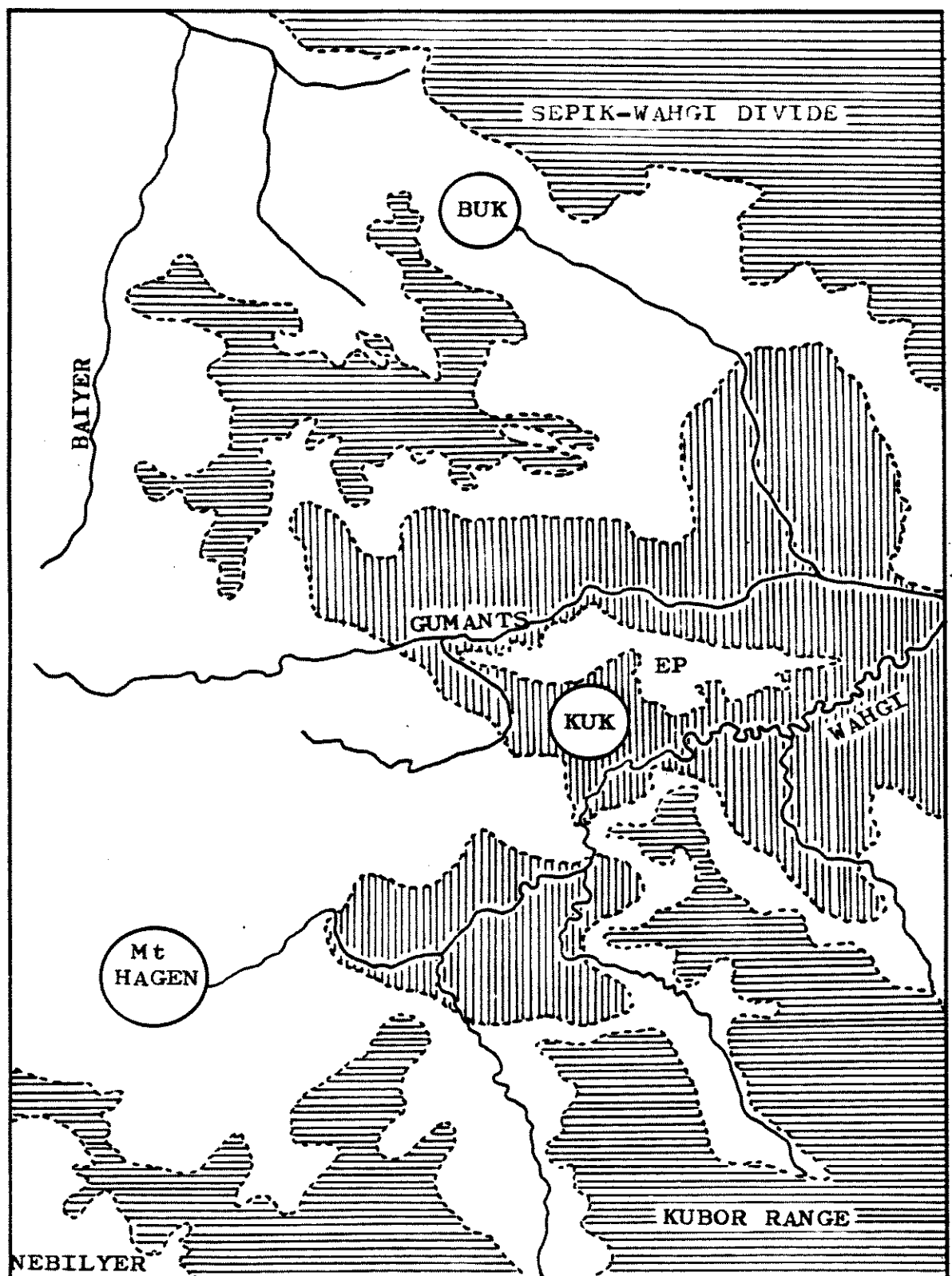
31 Site MNZ

136

32 Site MOC

136

Map 1. Location of Study Area



N

10 km



Forest (Mountains)

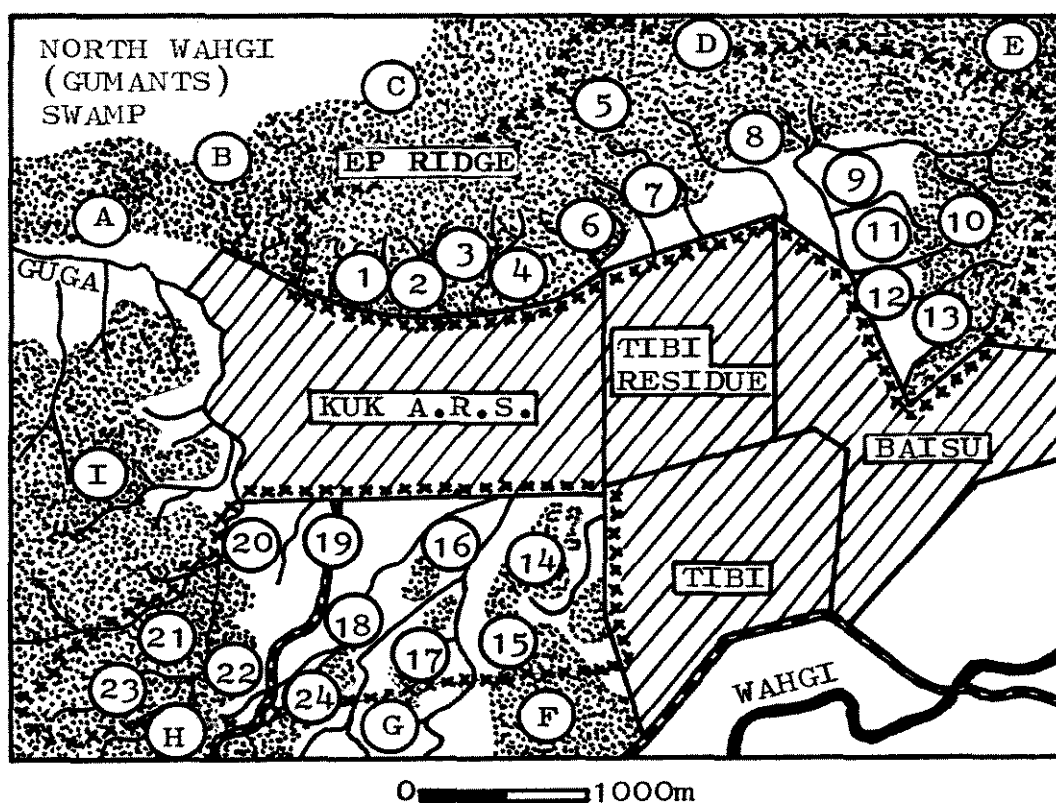


Predominantly Drylands (Hilly Grasslands)



Predominantly Wetlands (Swampy Grasslands)

Map 2 - KAWELKA TERRITORY

SURROUNDING TRIBES

A KIMKE
 B ROGLAGA
 C KUKILKE
 D ELTI
 E MOGE
 F PENAMBI
 G KEME
 H KUKILKE
 I DJIGA

KAWELKA HAMLETS

1 PRKL ROMBIL
 2 PRKL KUNGUNT
 3 PRKL WAIKE
 4 PRKL RAKARONG
 5 RUGWA
 6 PRKL ROBRI
 7 PRKL RANGORONG
 8 AGWAMUNDI
 9 KUNING
 10 KENTUPAI
 11 EMDEL
 12 METEN
 13 KAKLONG
 14 KUKRUMDI
 15 KUM
 16 MUP NENGBANA
 17 MUP
 18 BAGLA
 19 KUK
 20 KUK WAIGA
 21 GENGLA
 22 KENTA
 23 MAPA
 24 ROMBUGL

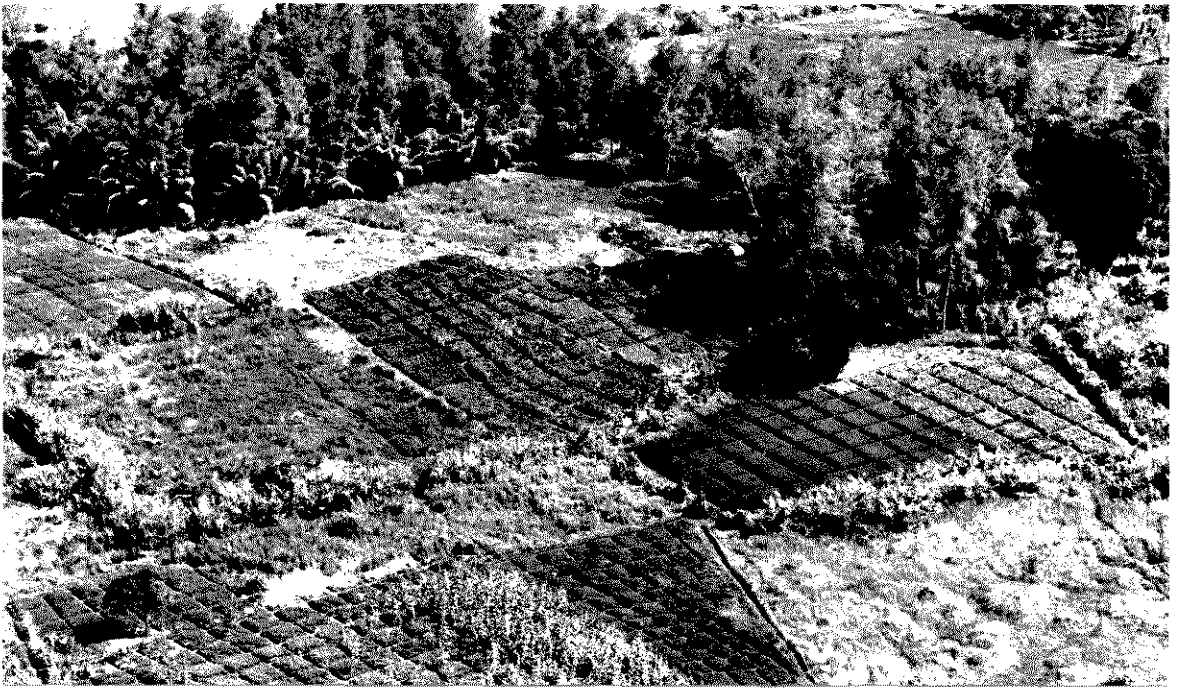


Plate 3. Dryland and wetland gardens, Kukrumdi. Corn and yam in bottom centre, remainder being new and mature sweet potato beds. Swamp bottom right. Coffee blocks under casuarina trees.



Plate 4. Dryland gardens, Kukrumdi. Pat in centre left, corn in $\frac{2}{4}$ left, mixed crops garden in bottom left, remainder sweet potato. Burning kunai after short fallow in centre top.

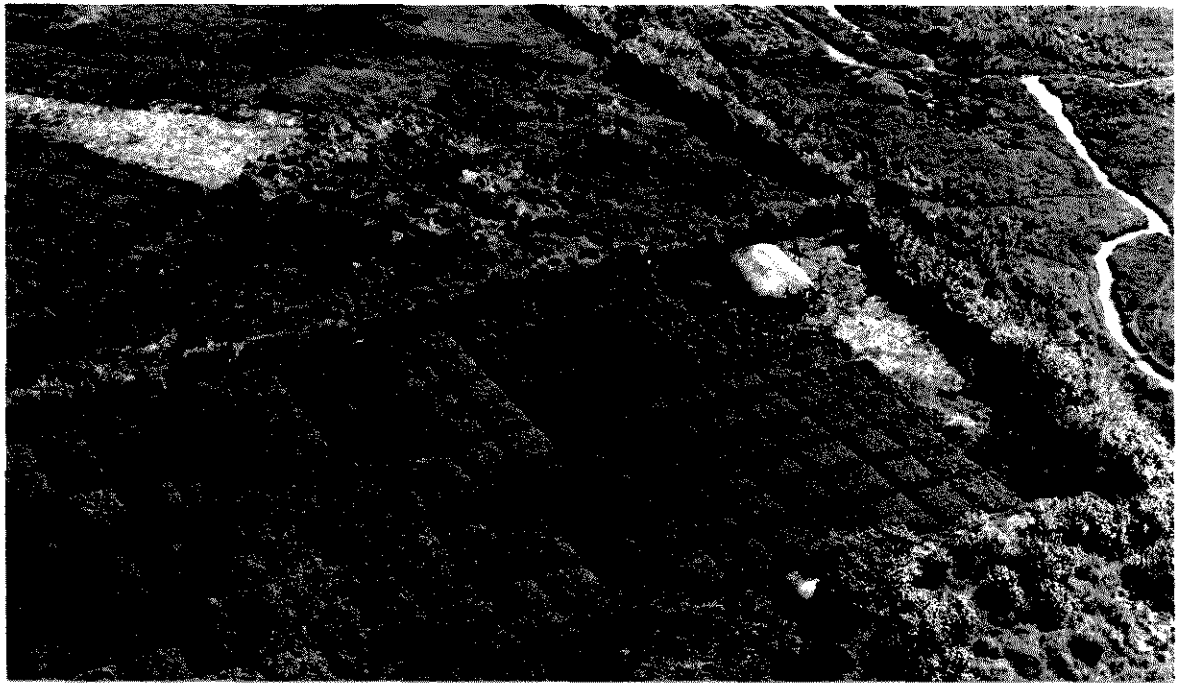
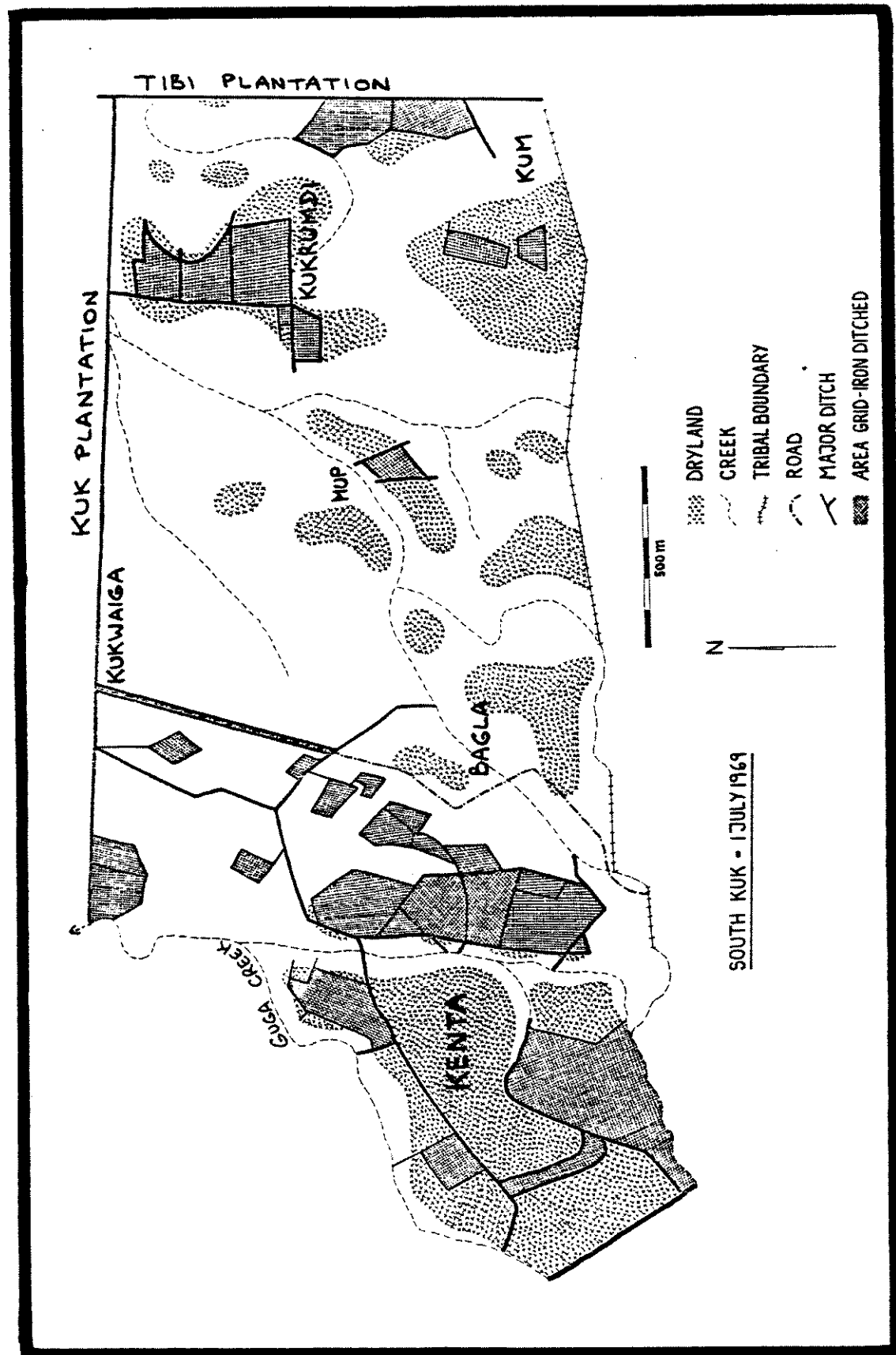


Plate 5. Gor settlement, Kukrumdi. Dryland. Mature sweet potato in left corner where tethered pigs are allowed to destroy the grid pattern. Peanut plots in right. Site MNP in bottom right.

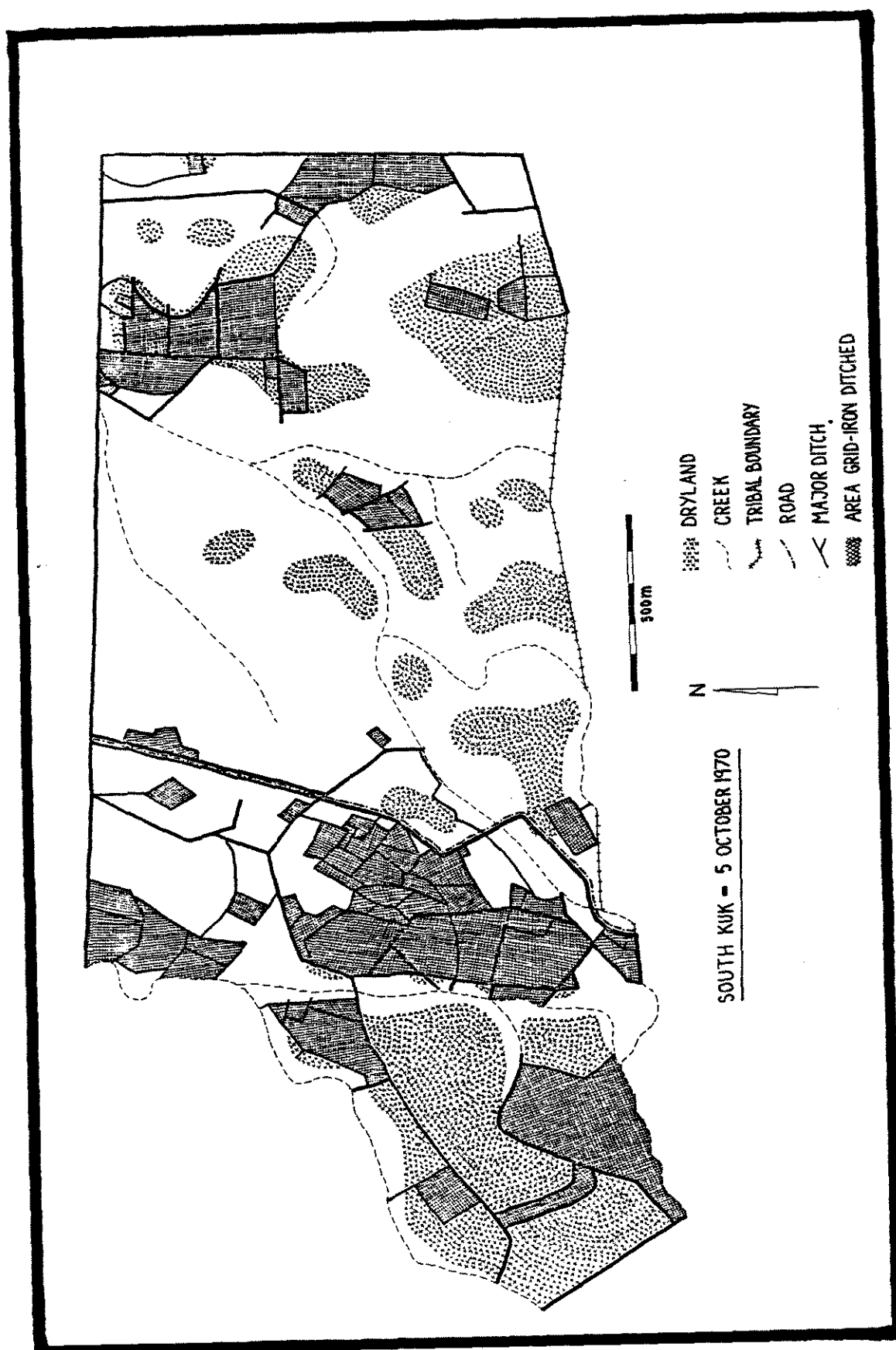


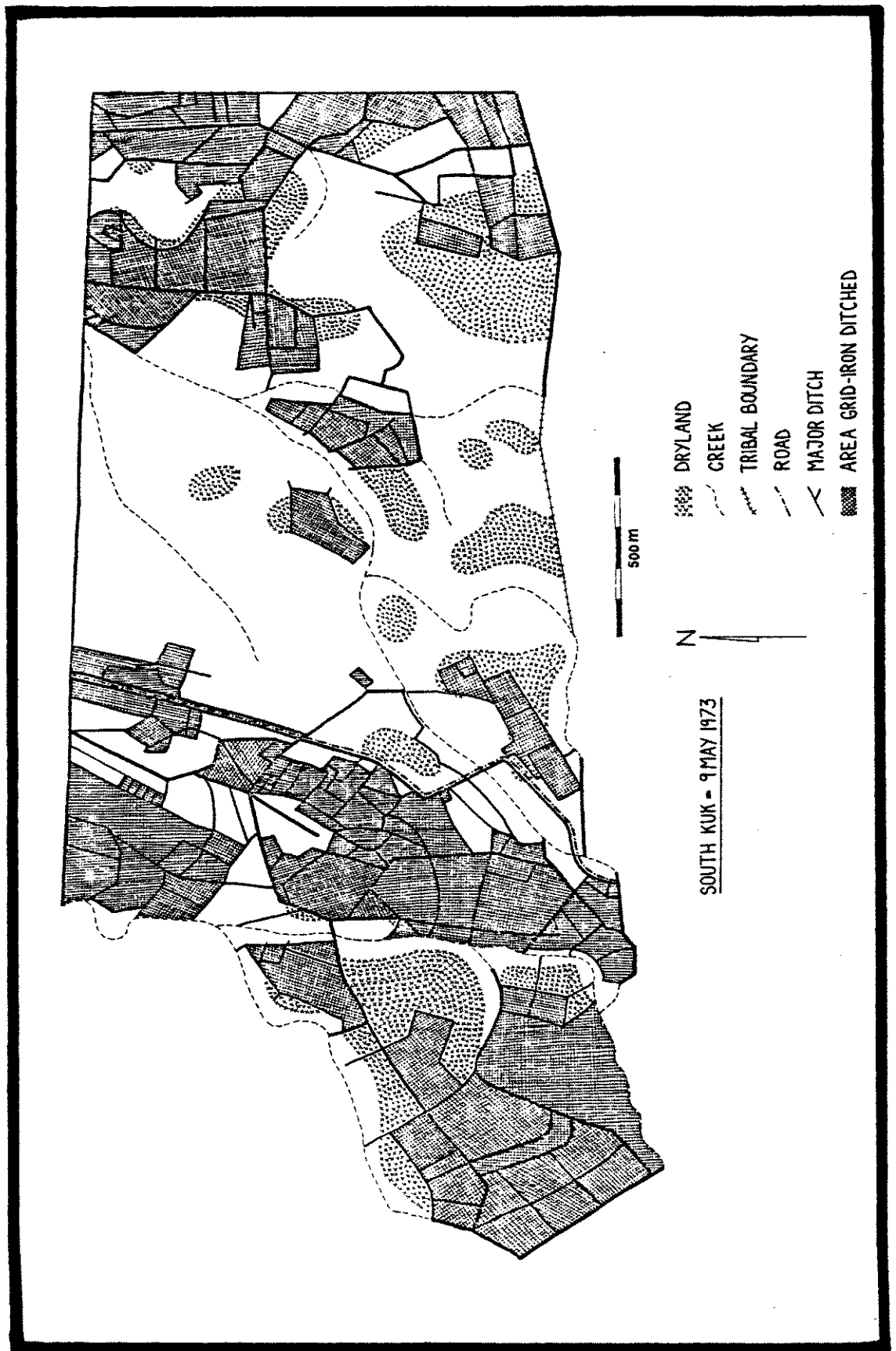
Plate 6. Swamp gardens at Kuning. All sweet potato except mixed crops or coffee under casuarina trees. Pig land between cultivated sections of swamp.

Map 3. Area Gardened - 1969

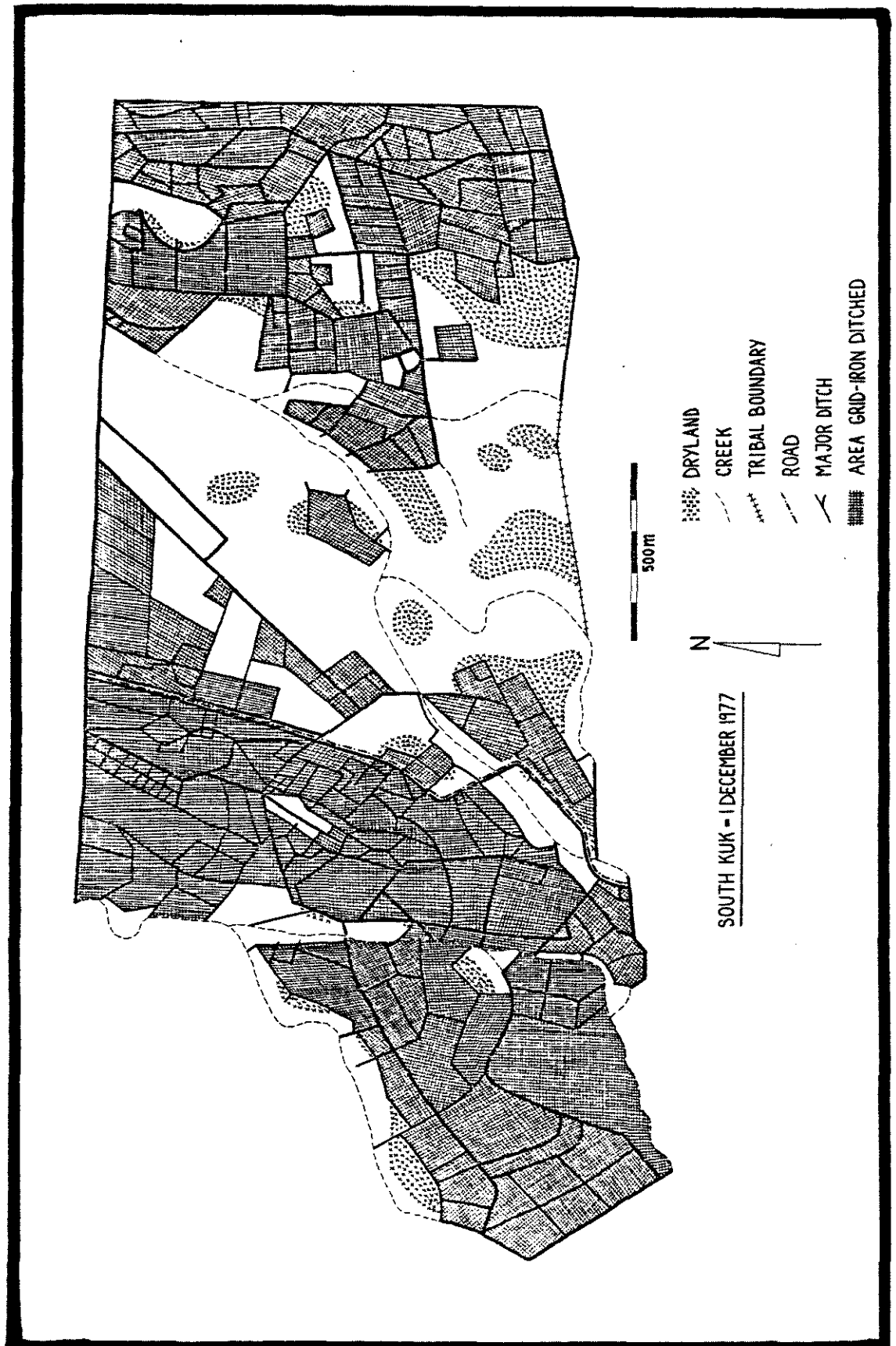


Map 4. Area Gardened - 1970

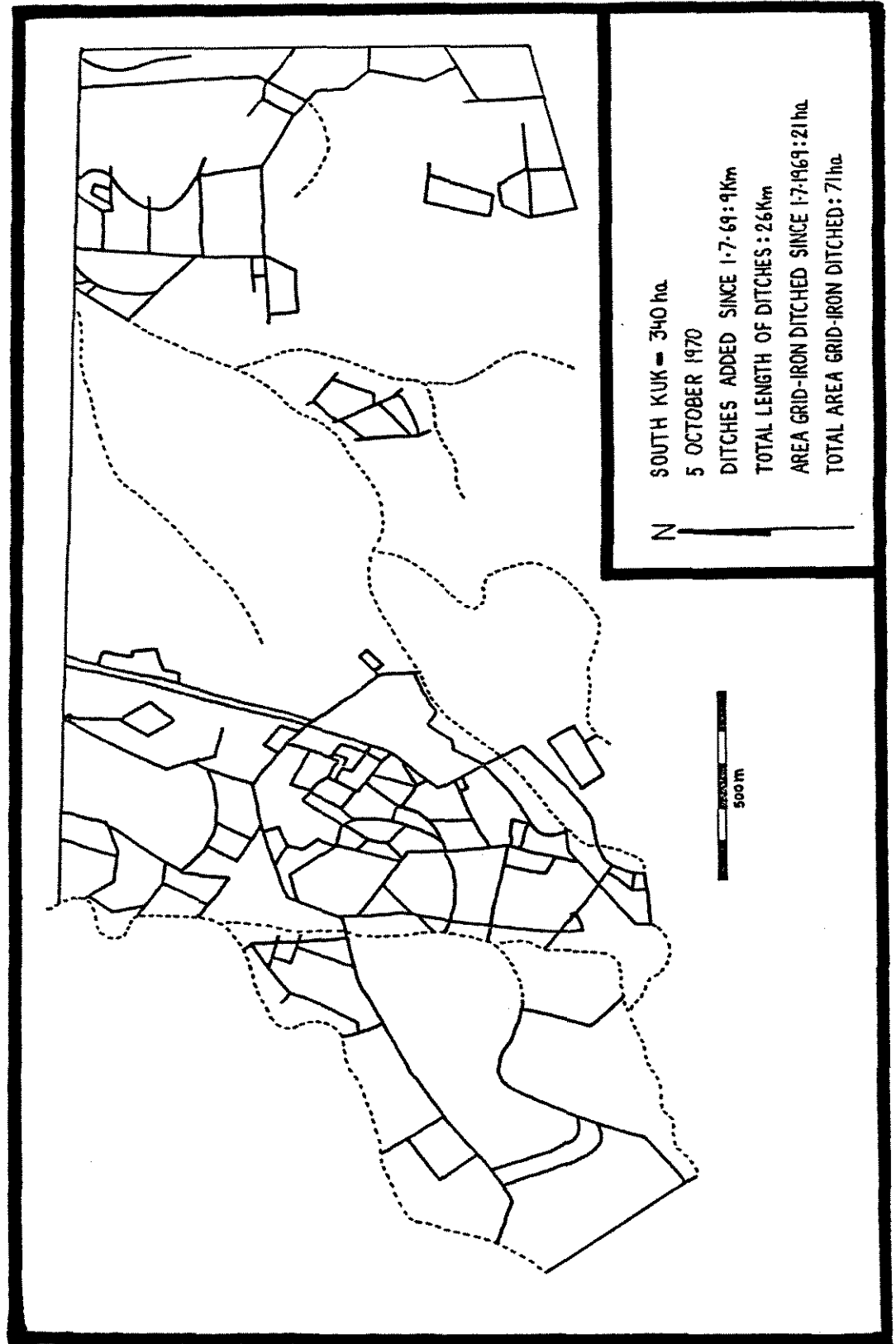




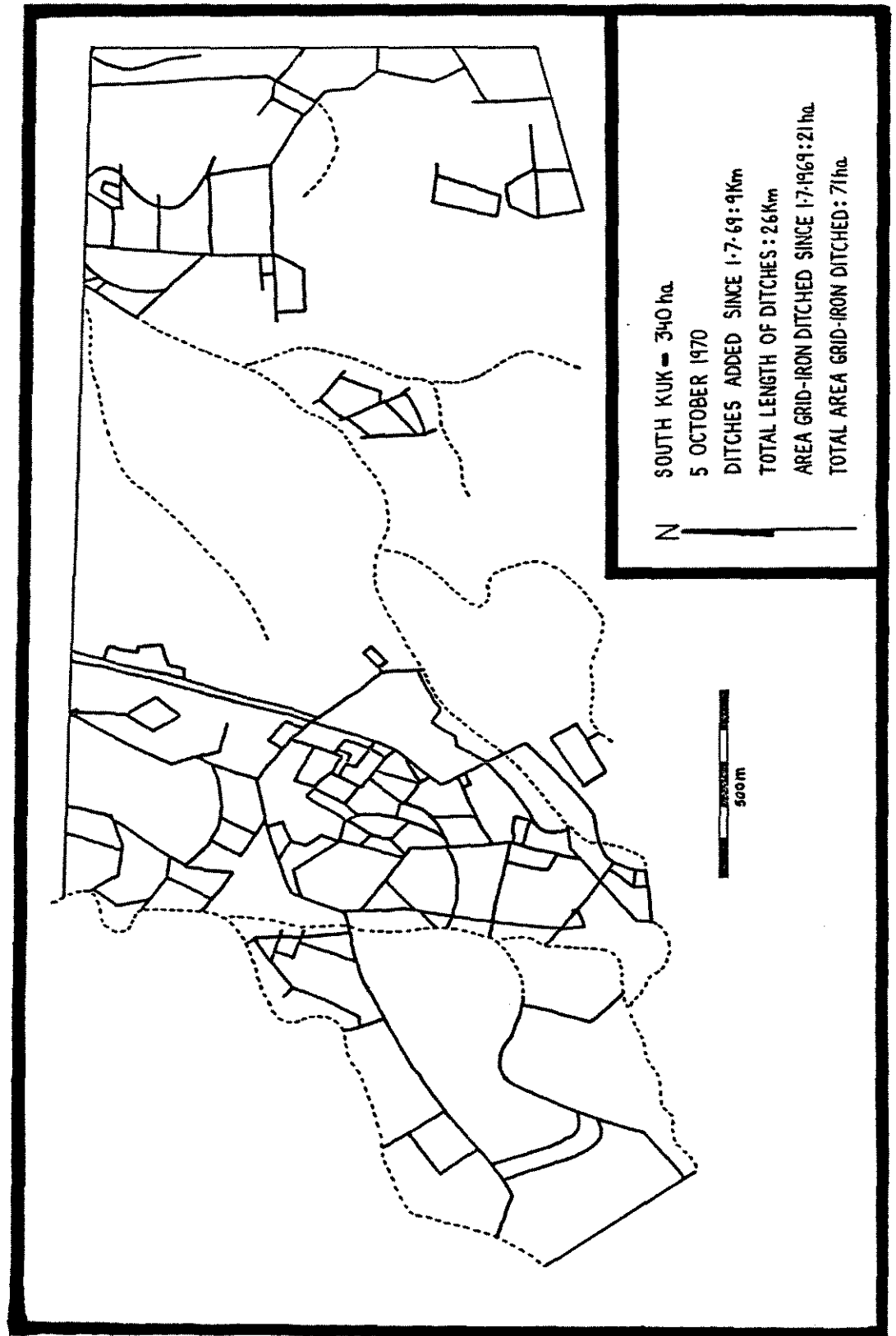
Map 6. Area Gardened - 1977



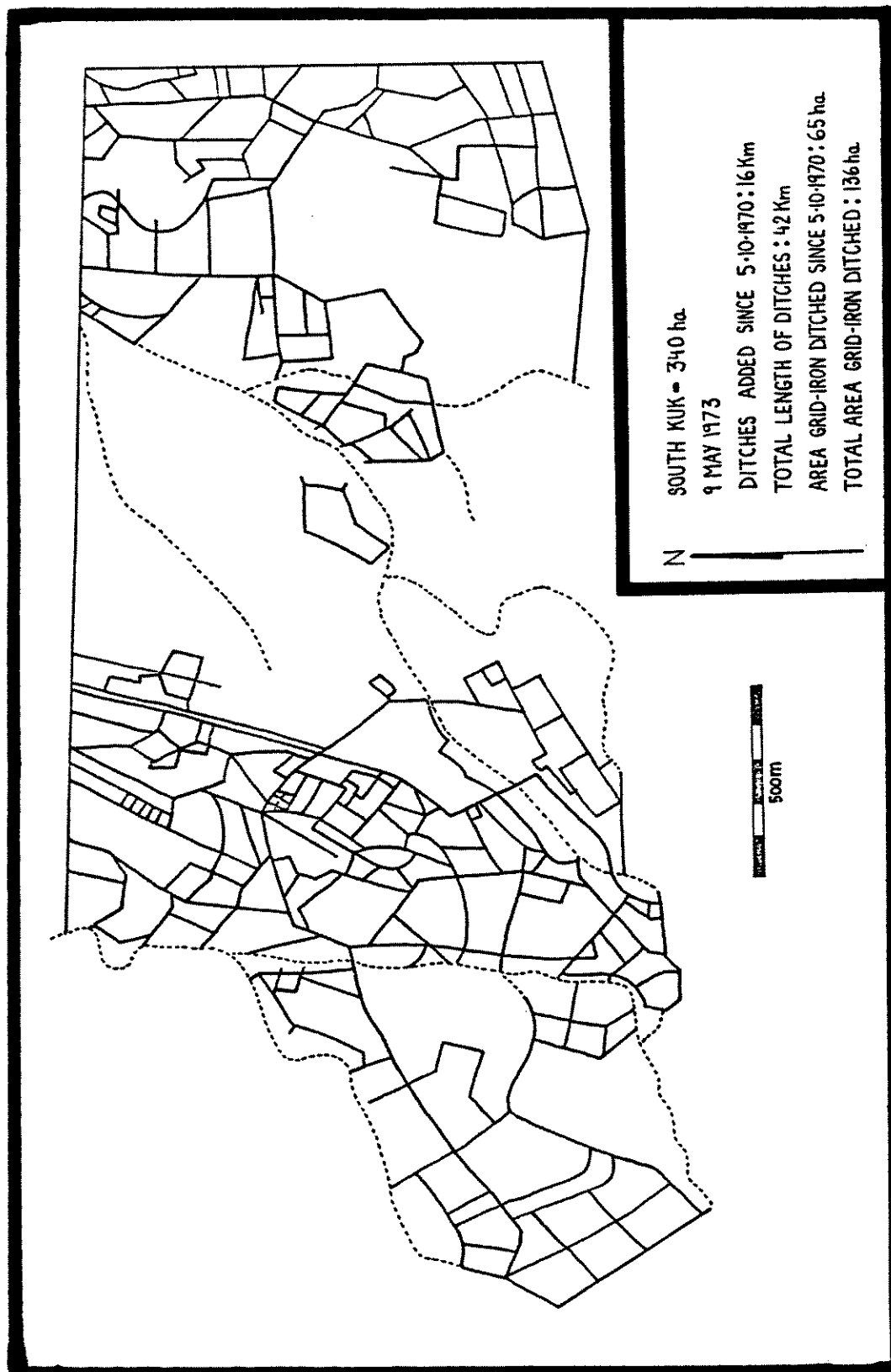
Map 8 . Major Ditches - 1970



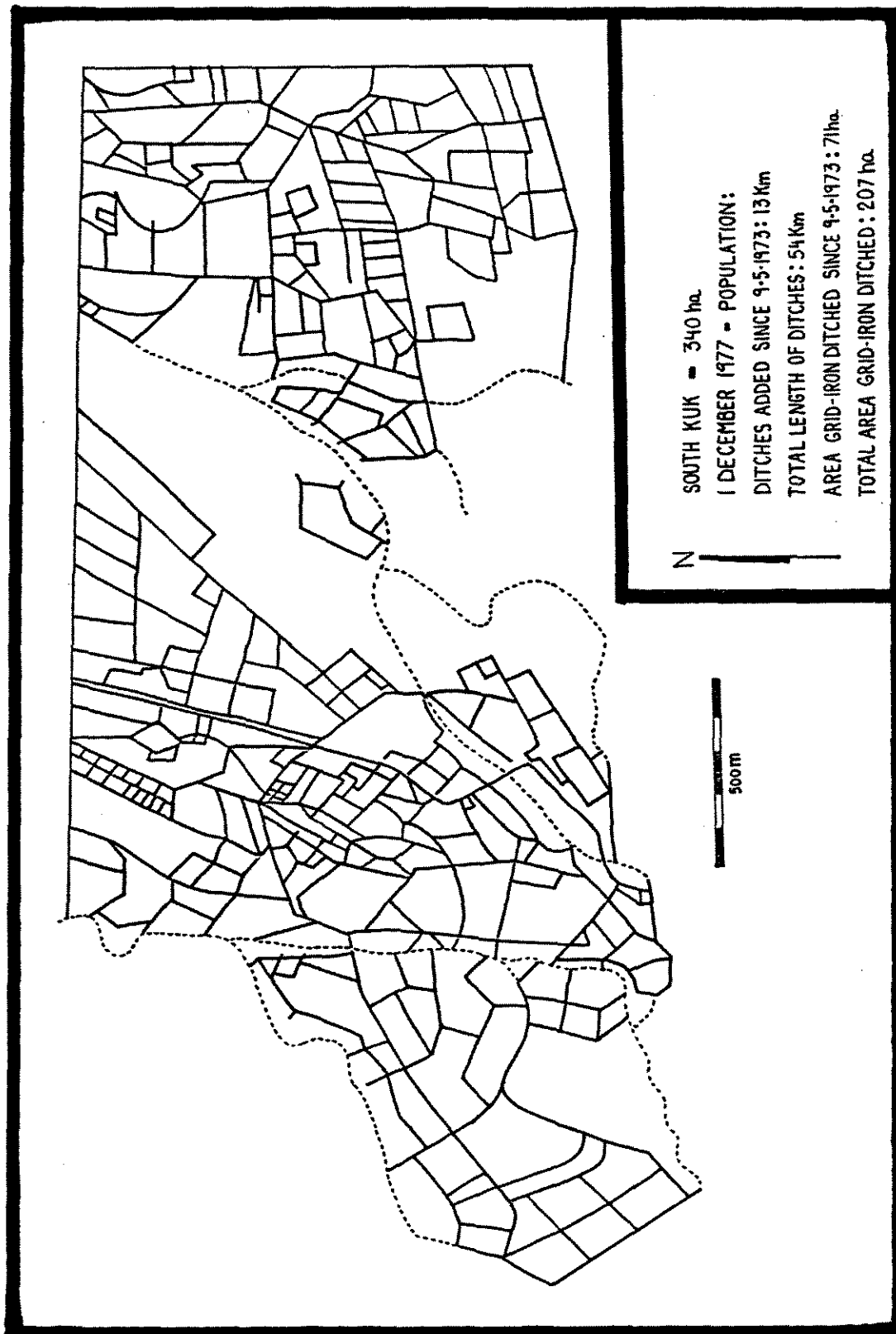
Map 8 . Major Ditches - 1970



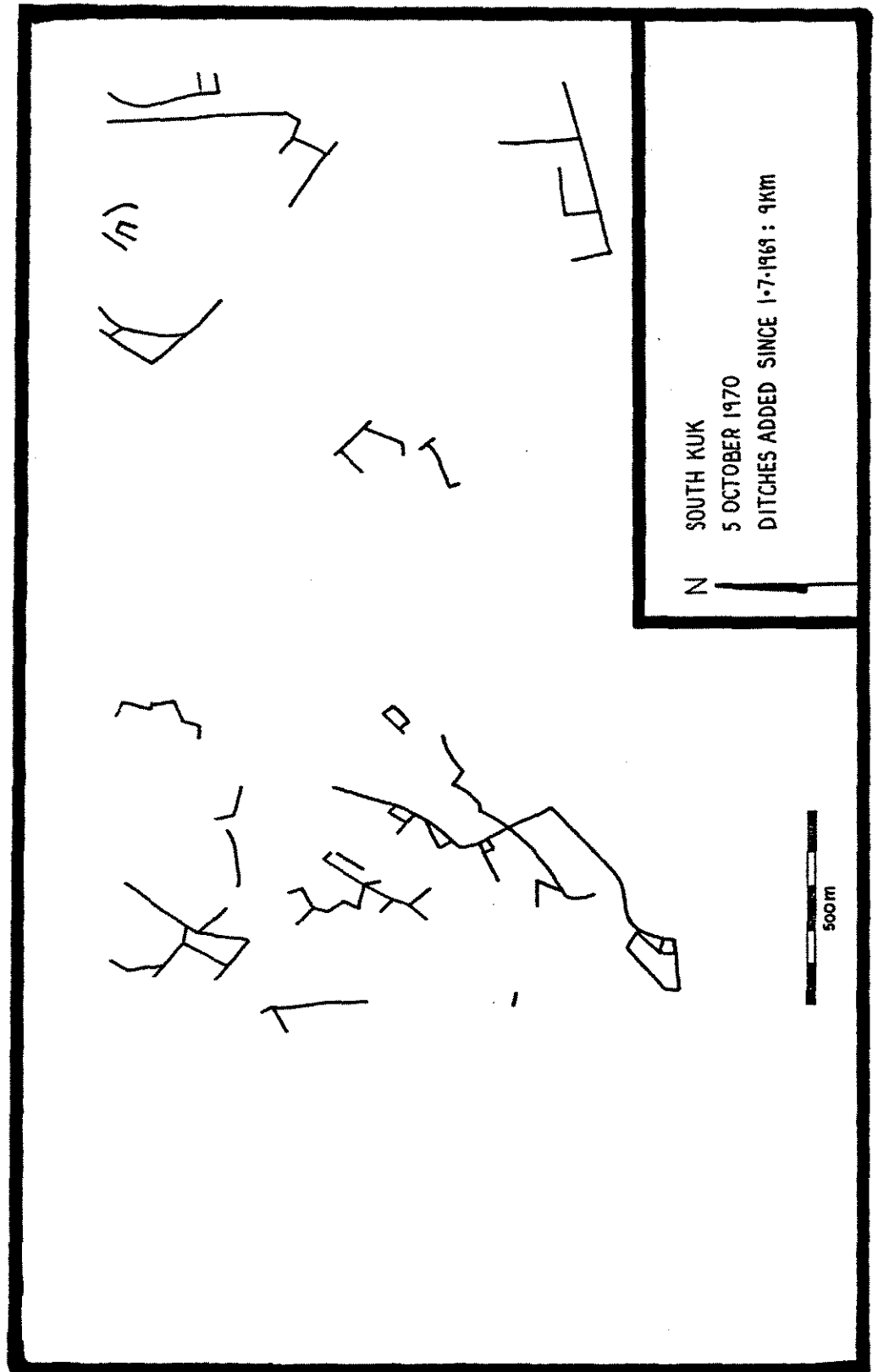
Map 9. Major Ditches - 1973



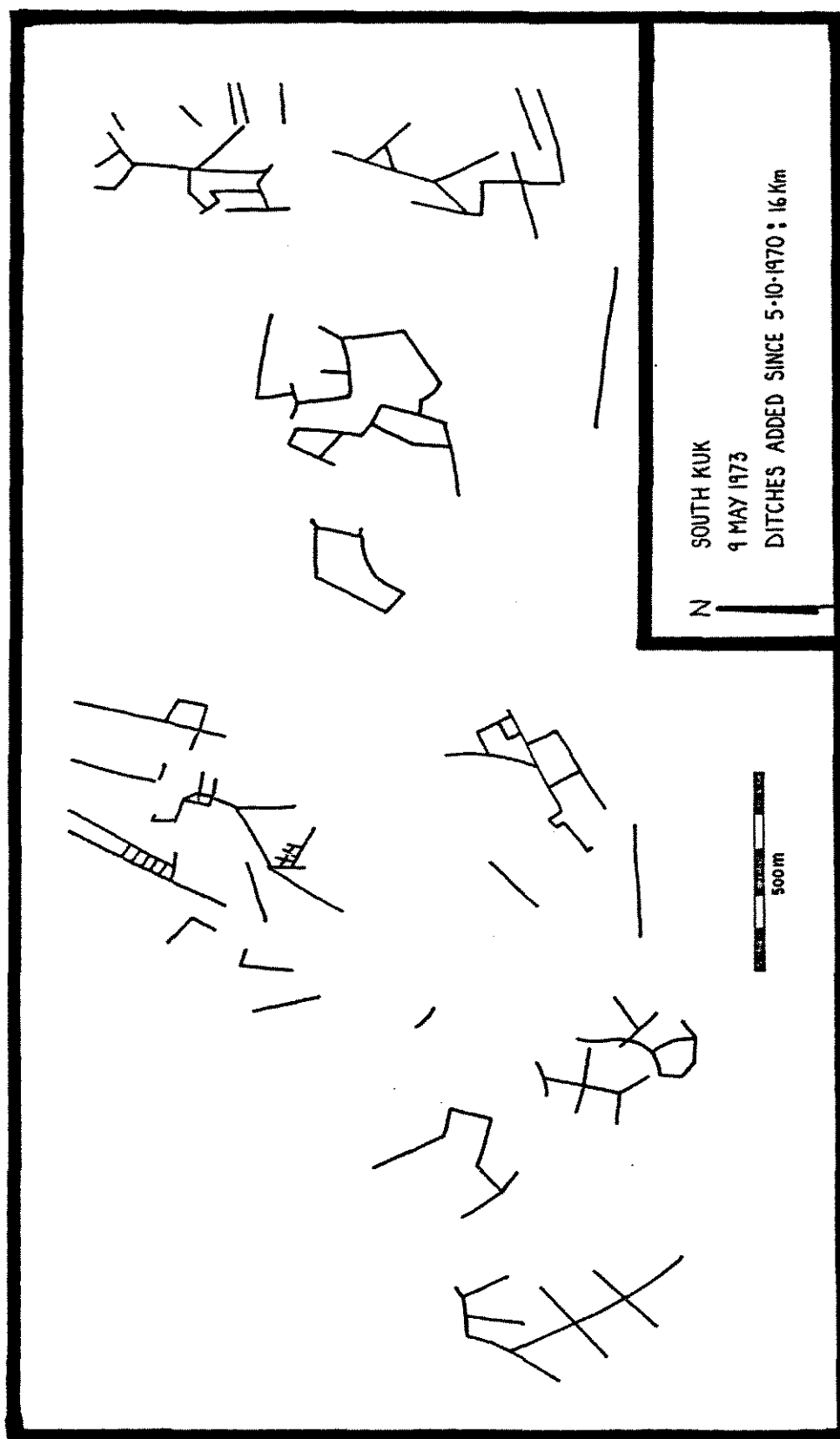
Map 1Q Major Ditches - 1977



Map 11. Ditches Added Between 1969 and 1970



Map 12. Ditches Added Between 1970 and 1973



Map 13. Ditches Added Between 1973 and 1977

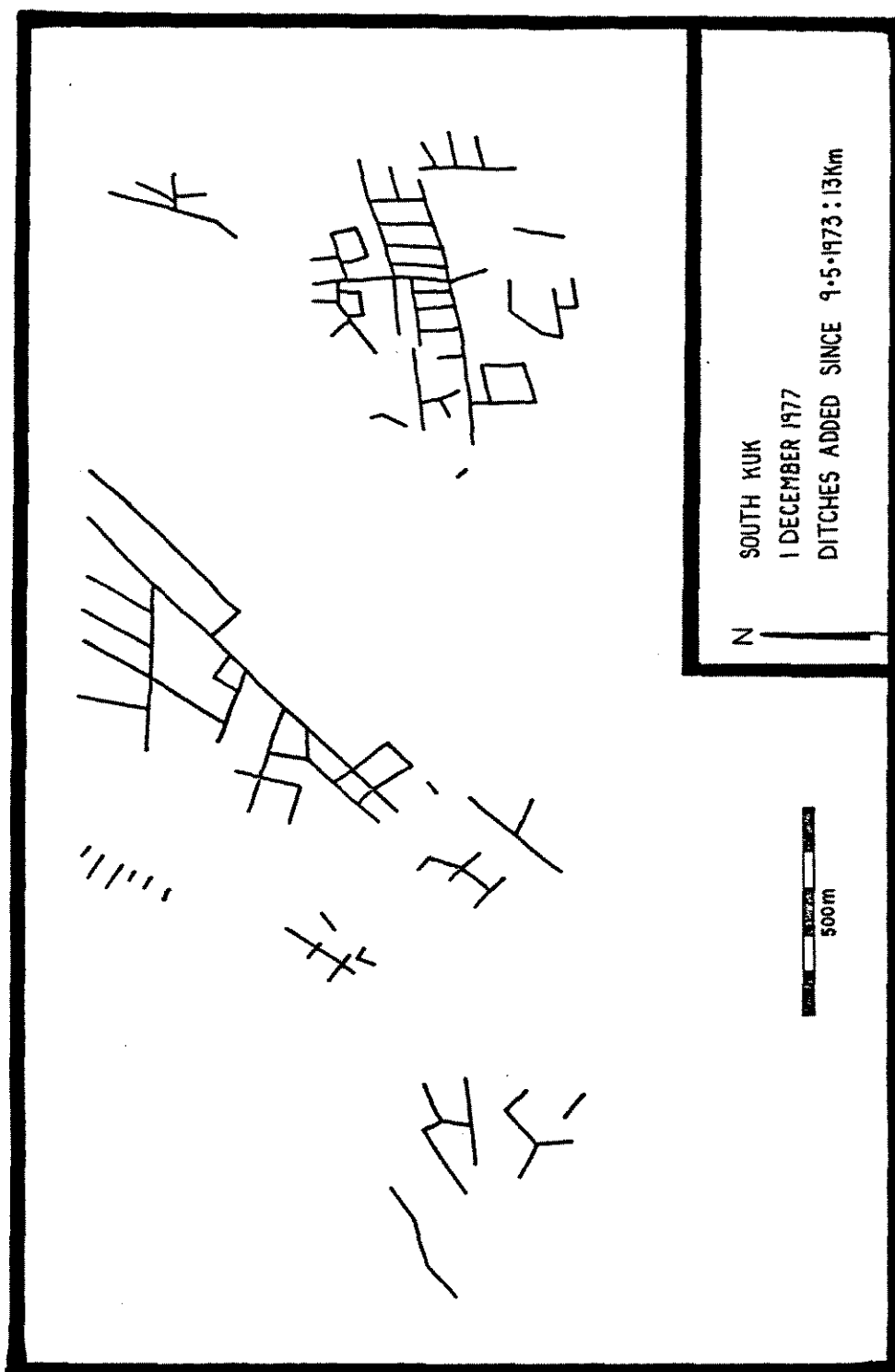
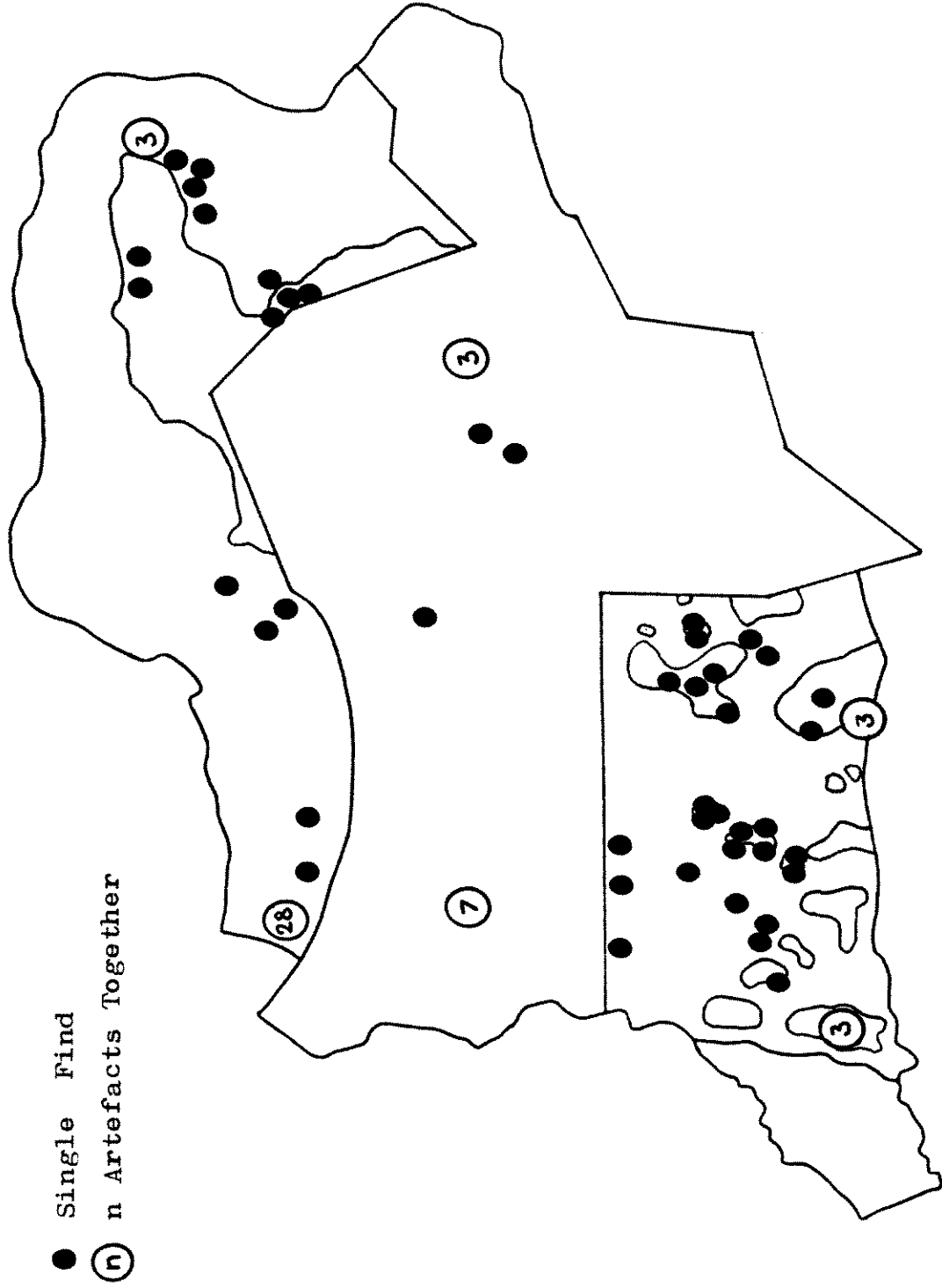


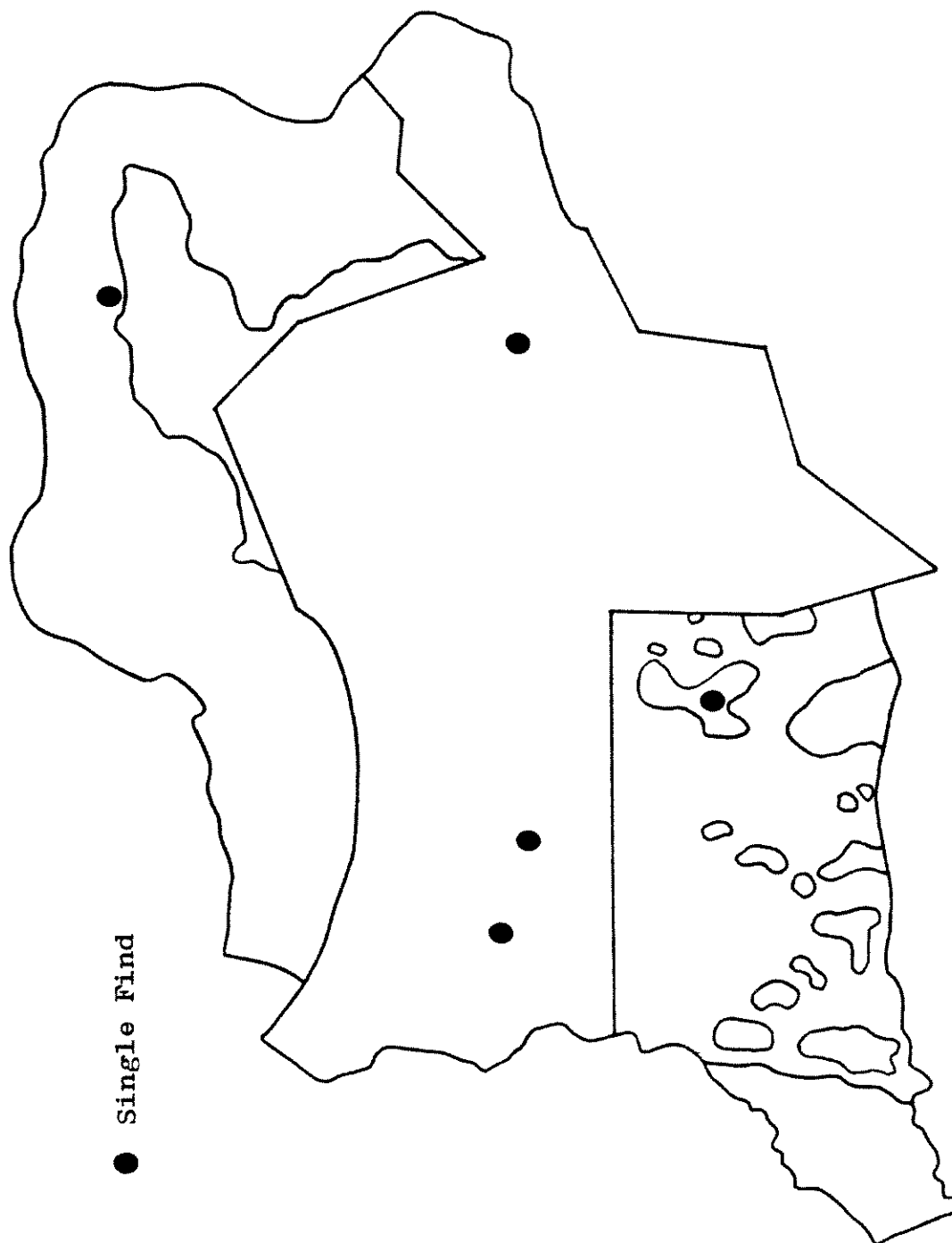
Table 1 - South Kuk: Land Use

South Kuk = 340 ha (320 ha wetland, 110 ha dryland)

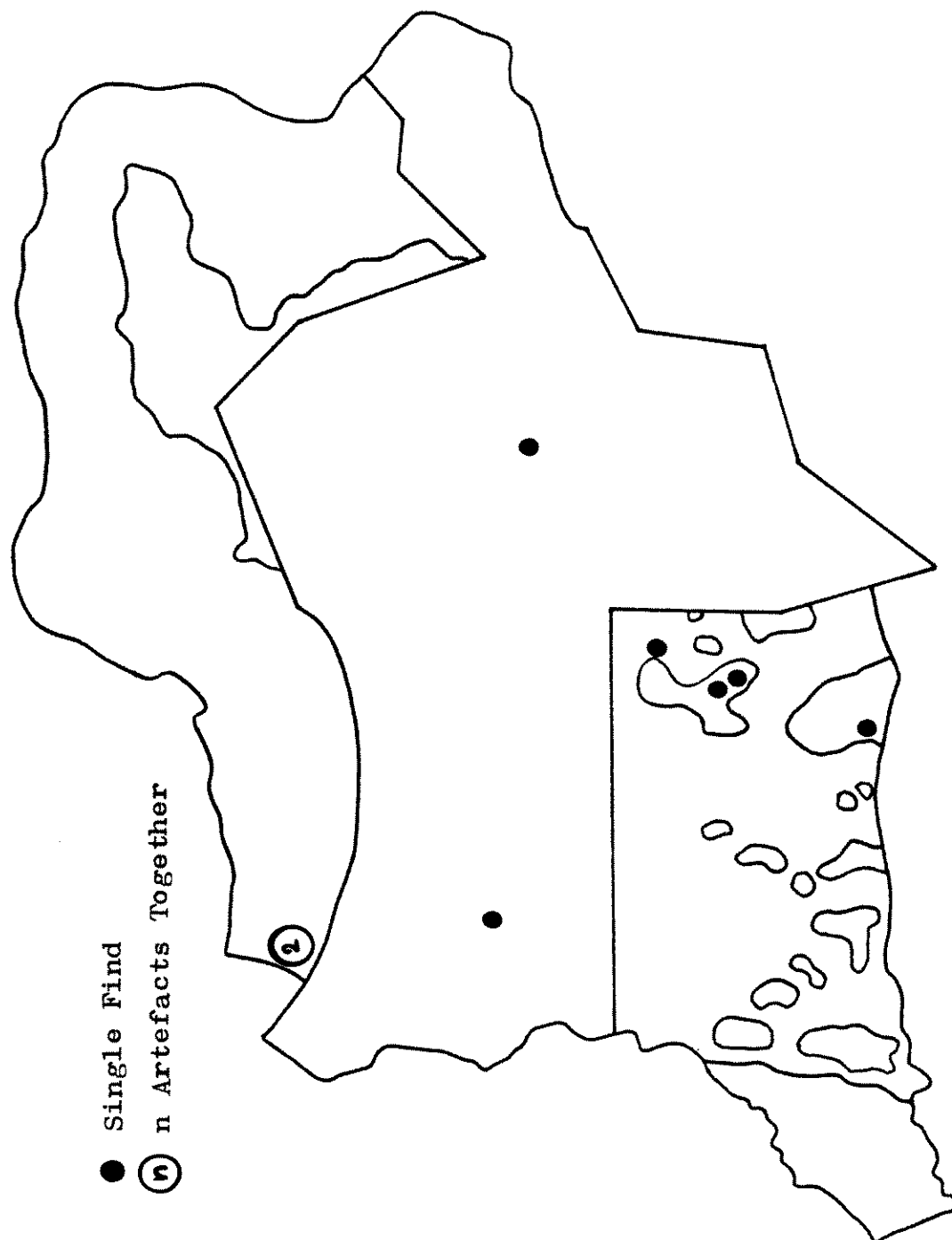
SOUTH KUK		1960	1969	1970	1973	1977
Population		0	170	190	310	571
p/km ²		-	50	56	91	168
New Major Ditches	km	0	17	9	16	13
	m per new head	0	100	450	133	50
Total Major Ditches	km	0	17	26	42	54
	m/head	0	100	137	135	95
New Grid-Iron Ditched Land	ha	0	50	21	65	71
	ha per new head	0	.29	1.05	.54	.27
Total Grid-Iron Ditched Land	ha	0	50	71	136	207
	ha/head	0	.29	.37	.44	.36
New Grid-Iron Ditched Wetland	ha	0	10	14	37	57
	ha per new head	0	.06	.70	.31	.22
Total Grid-Iron Ditched Wetland	ha	0	10	24	61	118
	ha/head	0	.06	.13	.20	.21
New Grid-Iron Ditched Dryland	ha	0	40	7	28	14
	ha per new head	0	.24	.35	.23	.05
Total Grid-Iron Ditched Dryland	ha	0	40	47	75	89
	ha/head	0	.24	.25	.24	.16



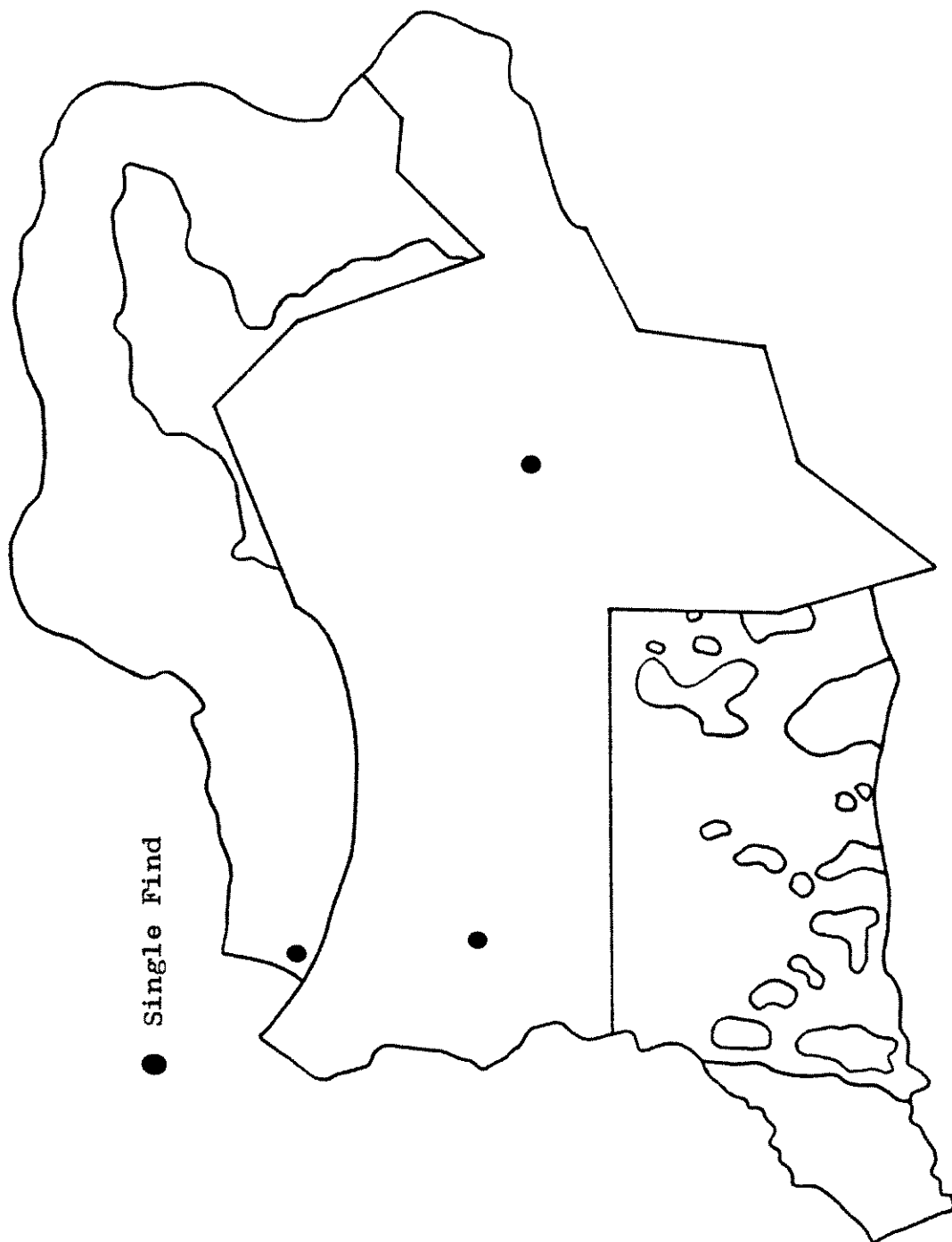
Map 14. Stone Axe or Parts of Stone Axe: Planilateral



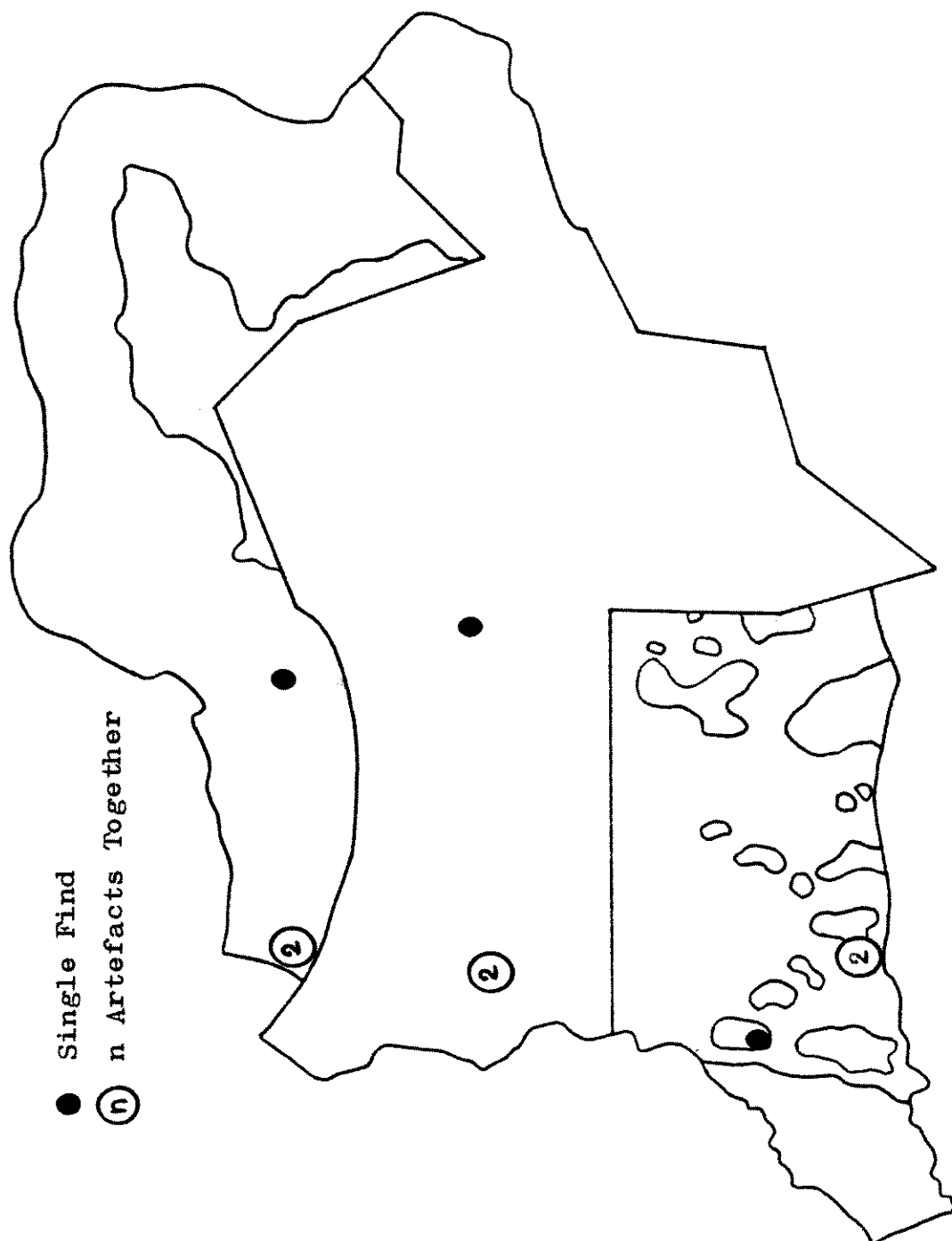
Map 15. Stone Axe or Parts of Stone Axe: Lenticular



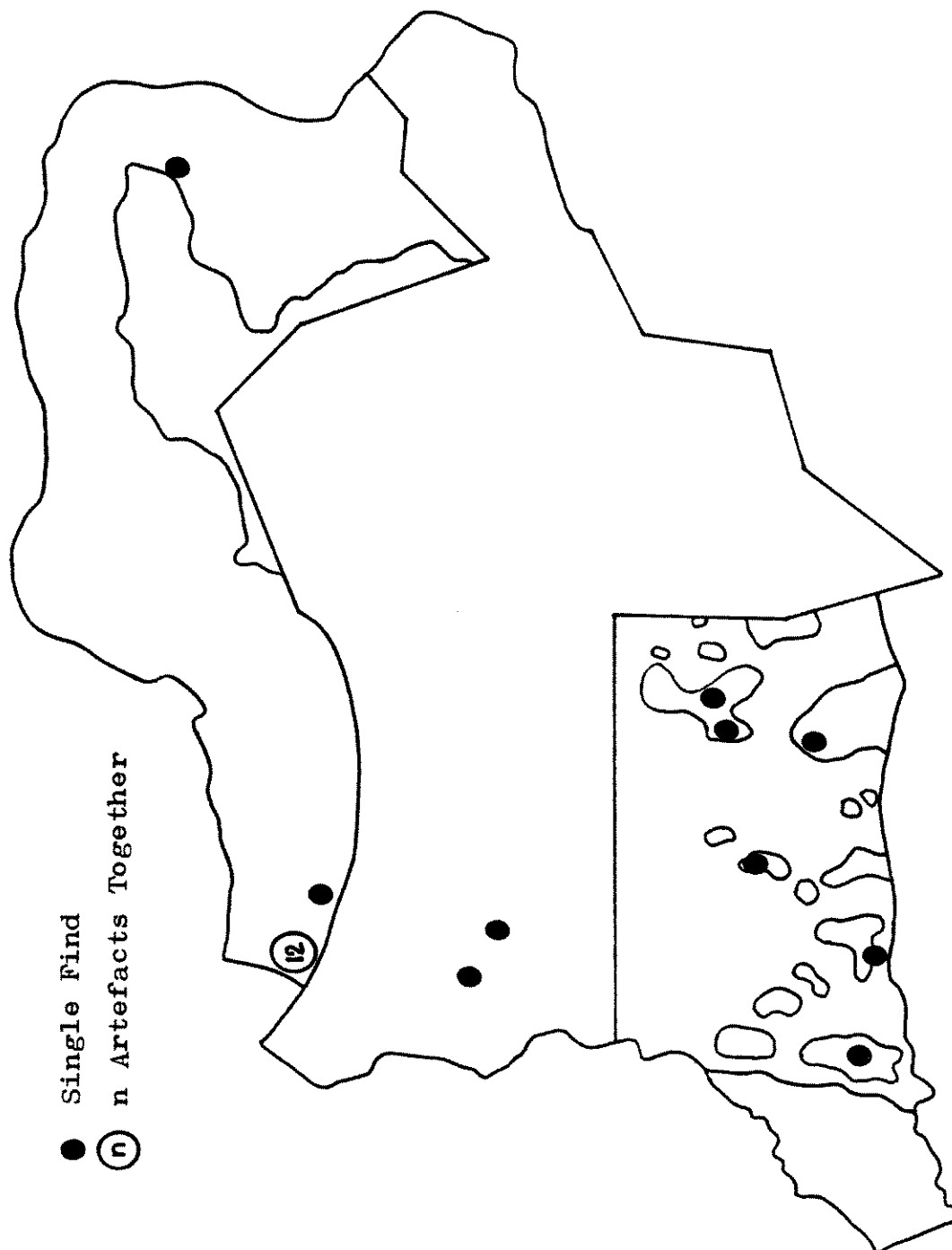
Map 16. Stone Axe or Parts of Stone Axe: Battered Sides



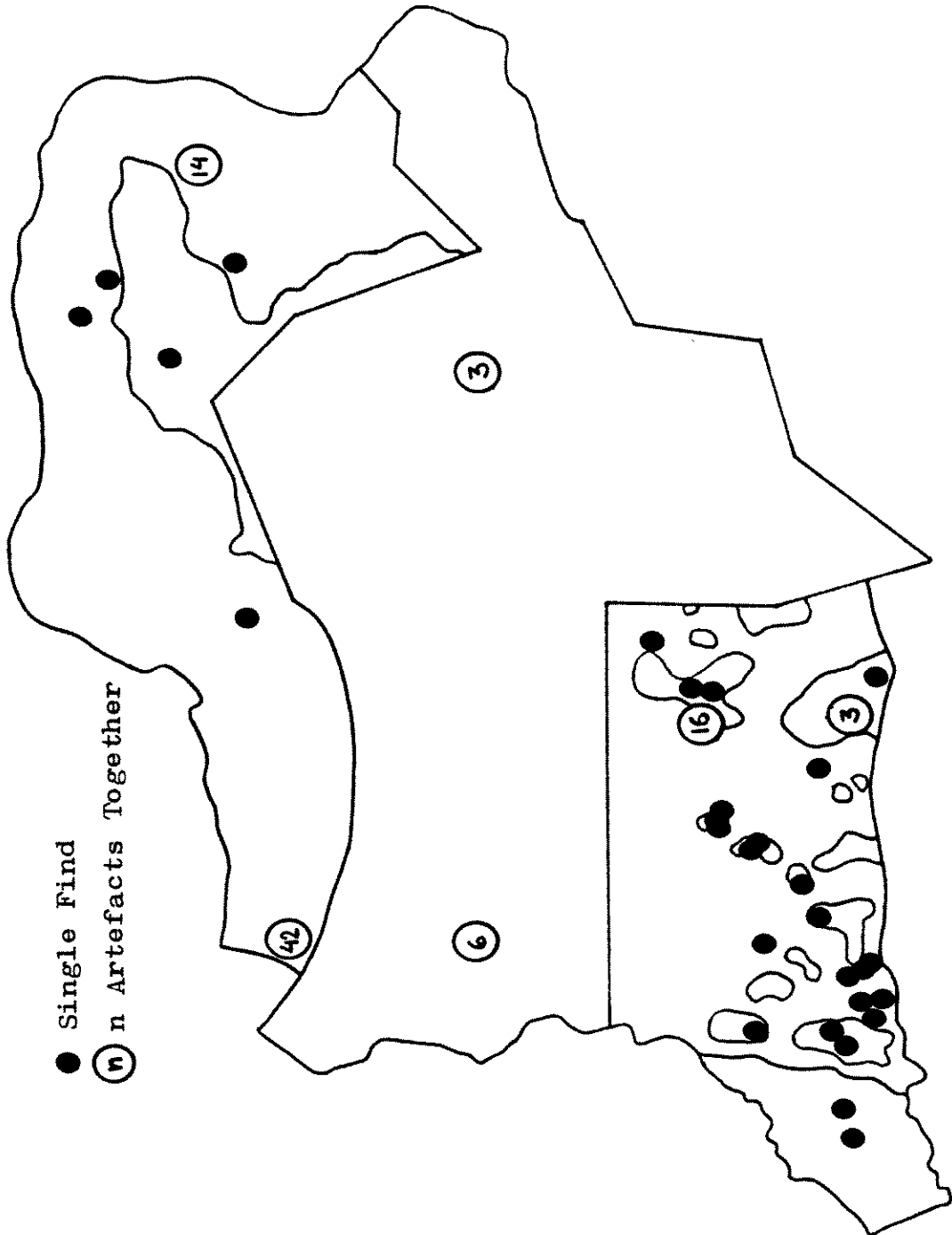
Map 17. Stone Axe or Parts of Stone Axe: Pebble



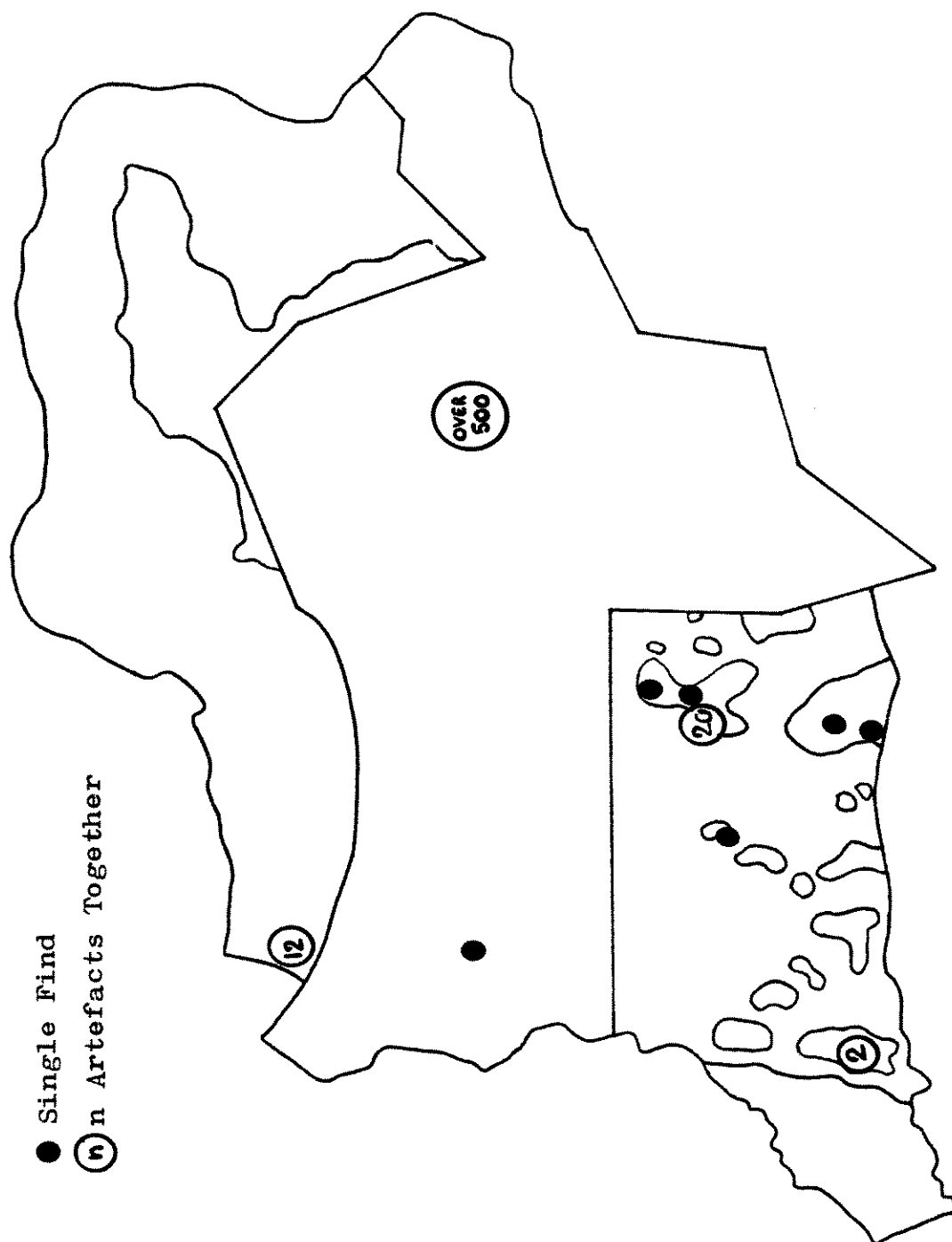
Map 18. Stone Axe Blank



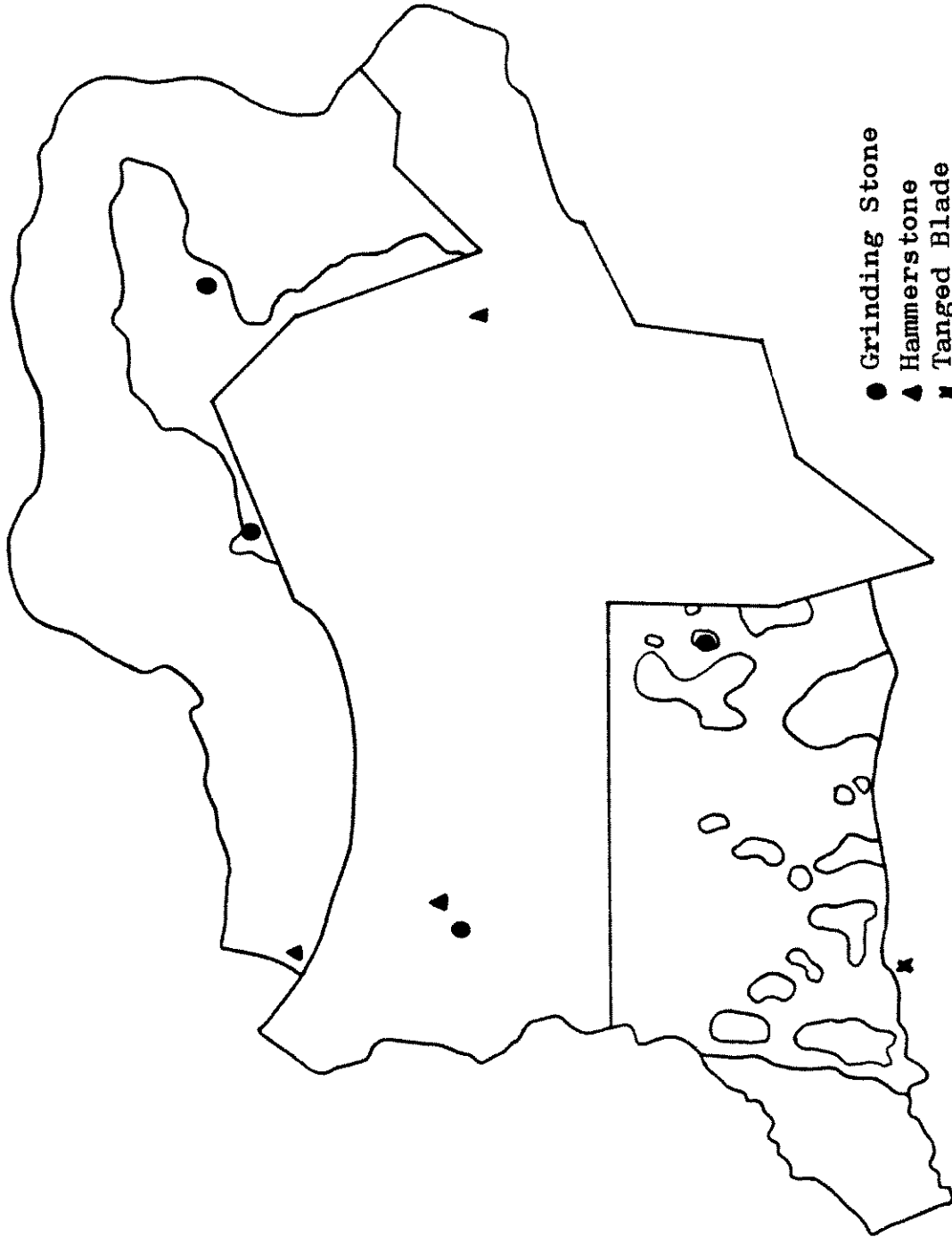
Map 19. Planilateral Stone Axe: Rejuvenation of Cutting Edge



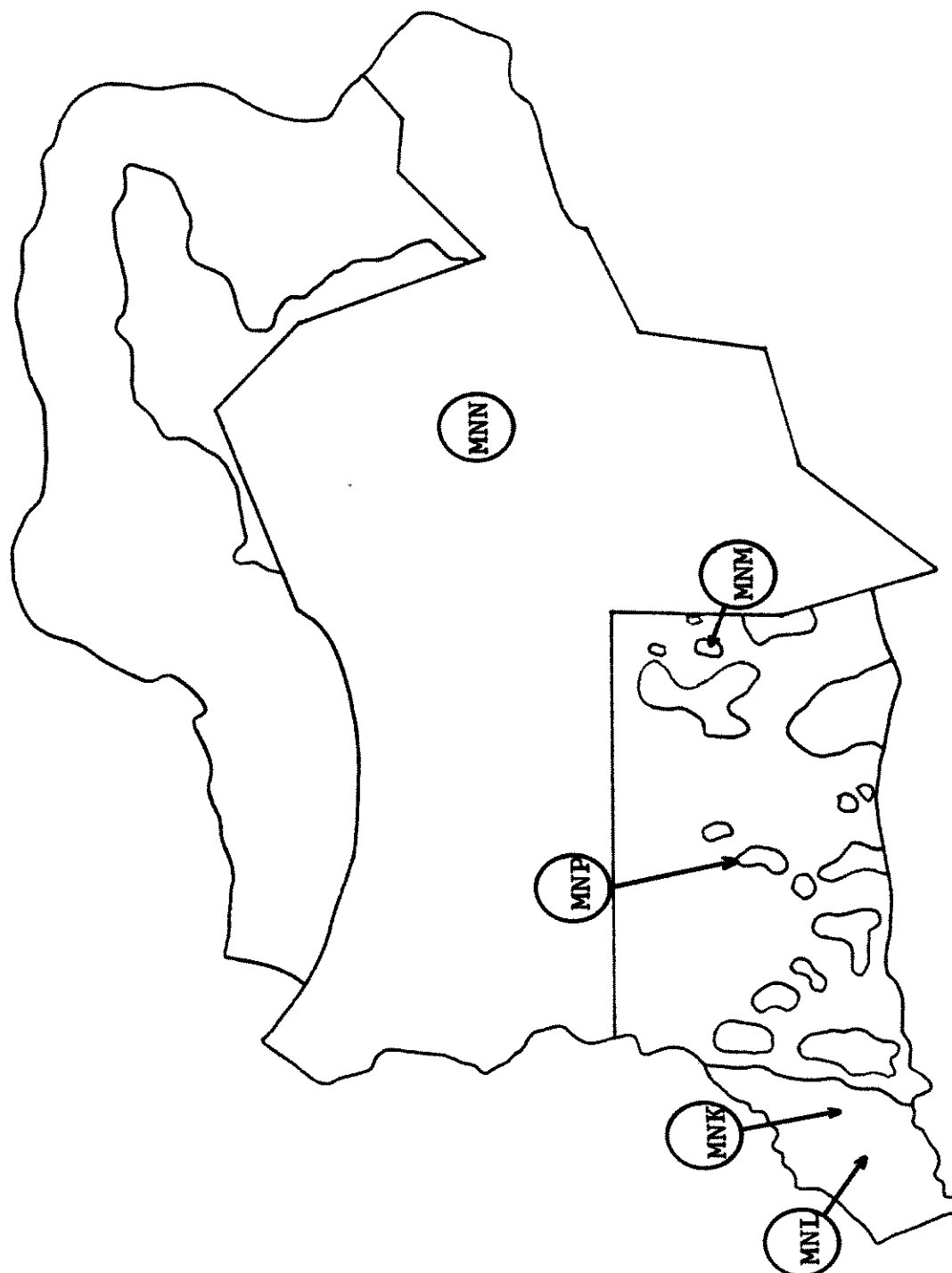
Map 20. Polished Flakes and Chips (off Planilateral)



Map 21. Unpolished Flakes and Chips (Axe Blanks)



Map22. Other Stone Artefacts



Map 23. Chert Concentration

Table 2. Surface Collection - Chert Artefacts

	Total	Waste	Accute Angle Edge	Steep Angle Edge	Scalar Core	Core	Trimming Flake
MNM	236	192	6	23	4	-	11
MNL	199	176	1	19	-	1	2
MNK	157	140	5	12	-	-	-
MNP	90	75	2	10	-	-	3
MNN	132	111	8	9	4	-	-
Others	33	22	-	5	2	-	4
Total	847	716	22	78	10	1	20
%	100	84.5	2.6	9.2	1.2	.1	2.4

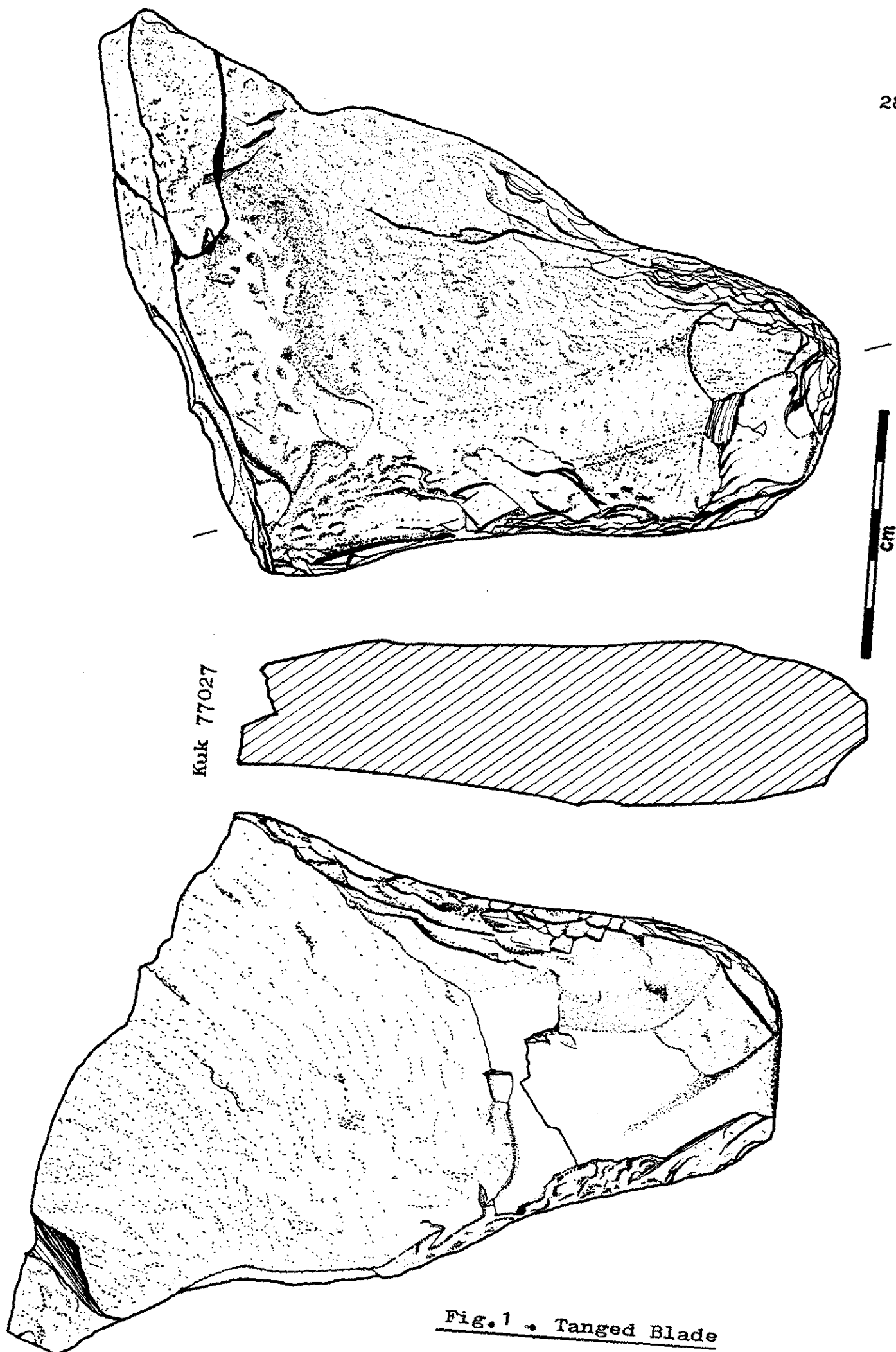
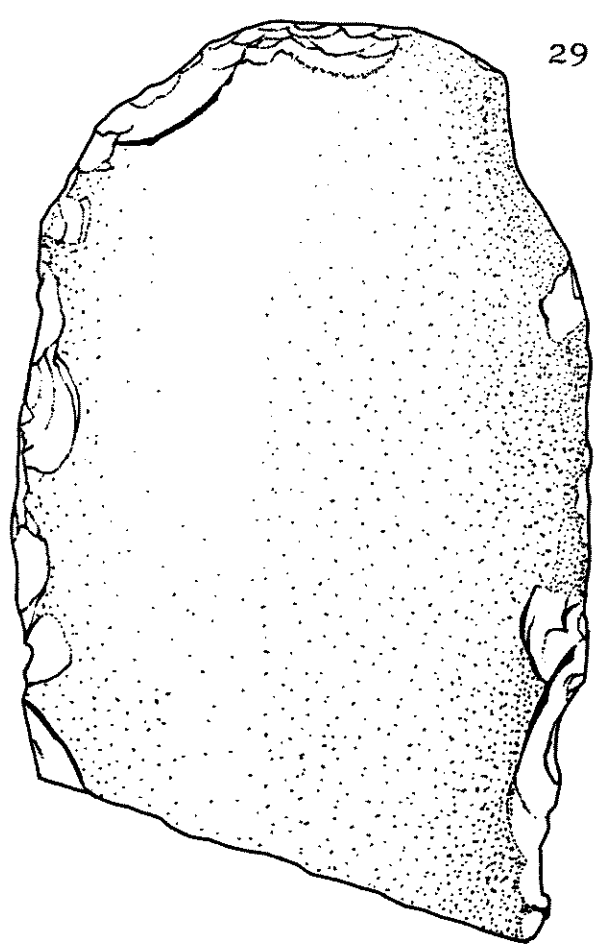
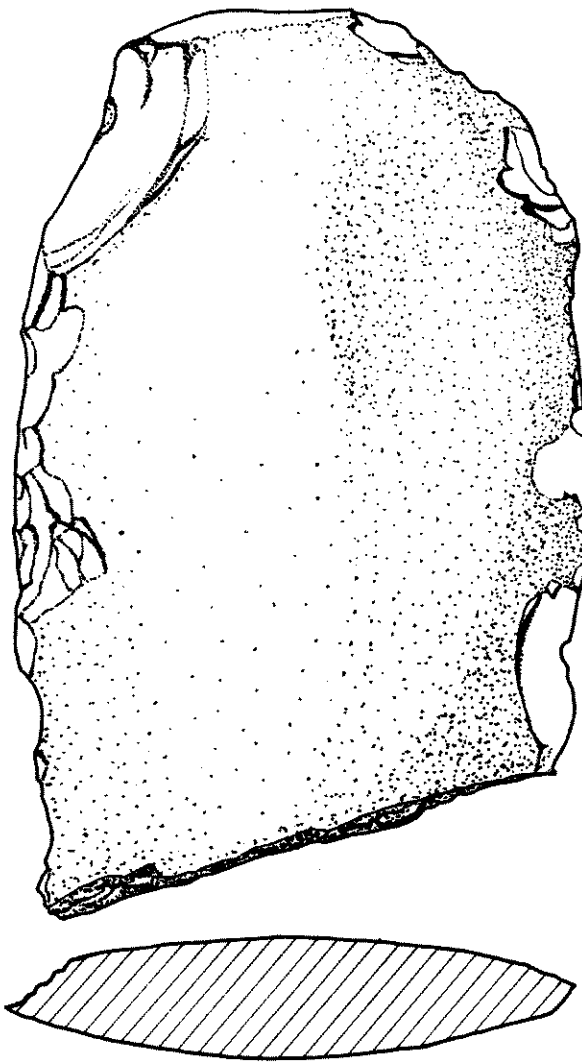


Fig. 1 . Tanged Blade

Kuk 78004


cm

Kuk 77010

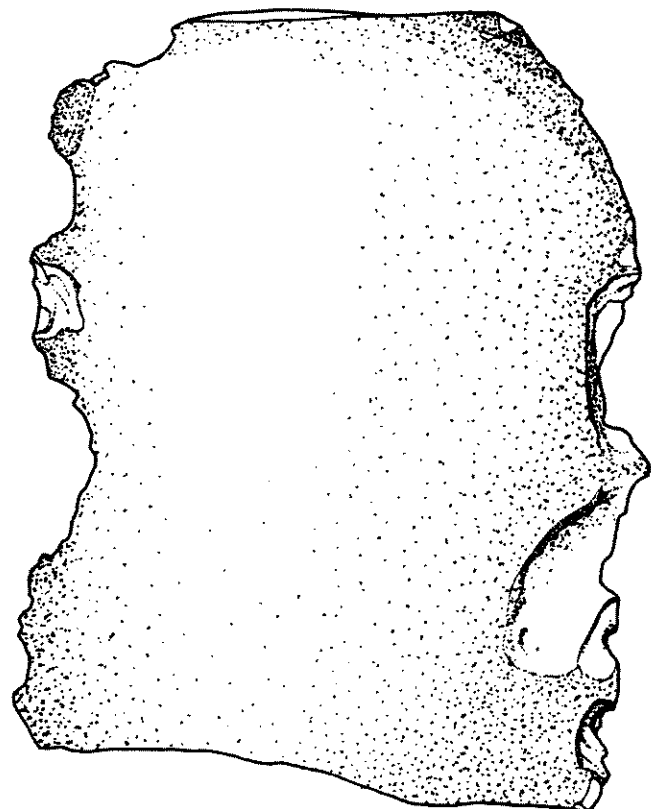
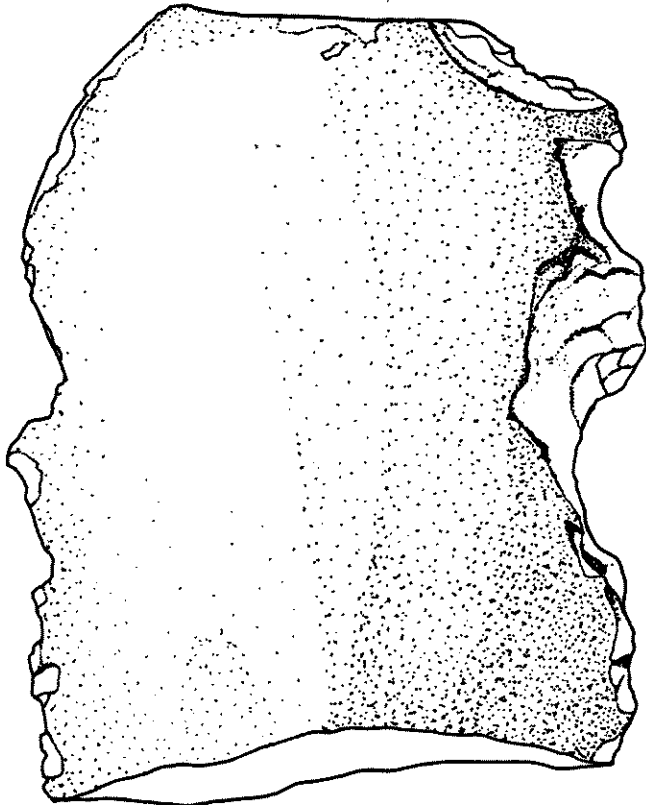
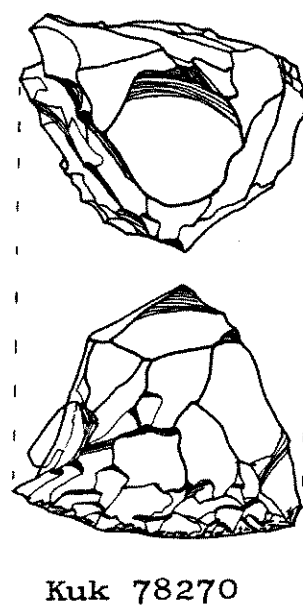
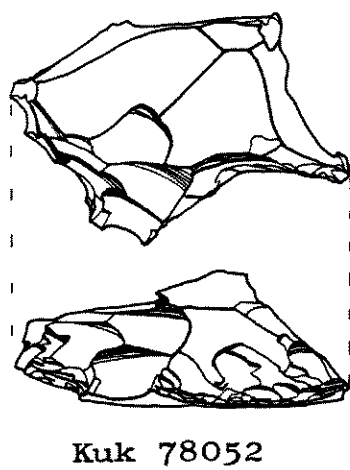
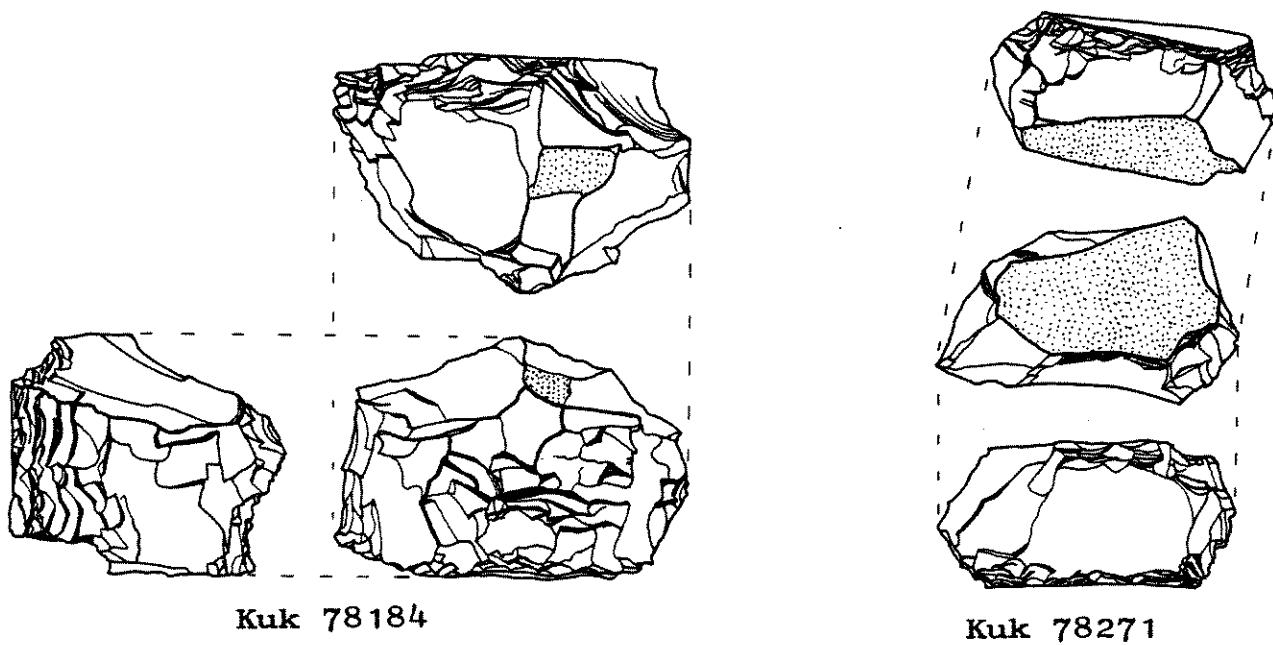


Fig.2 . Stone Knives

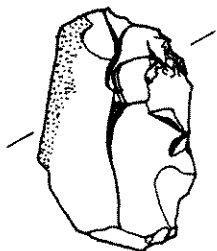
Fig. 3. Stone Tools: Steep Angle Edge



cm

Fig. 4. Other Stone Tools (chert)

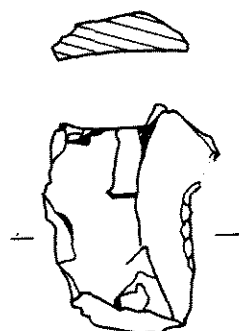
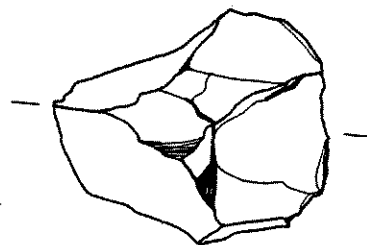
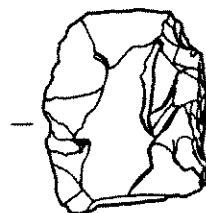
Kuk 77078



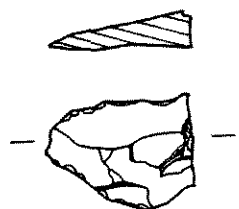
Kuk 77081



Kuk 77054



Kuk 78266



Kuk 78265



Kuk 78268



Kuk 78269

- Kuk 77078: Accute Angle Edge
- Kuk 78266: Accute Angle Edge
- Kuk 78265: Accute Angle Edge
- Kuk 77081: Trimming Flake
- Kuk 77054: Trimming Flake
- Kuk 78268: Scalar Core
- Kuk 78269: Scalar Core

Fig. 5. Glass (Beer Bottle) Artefacts

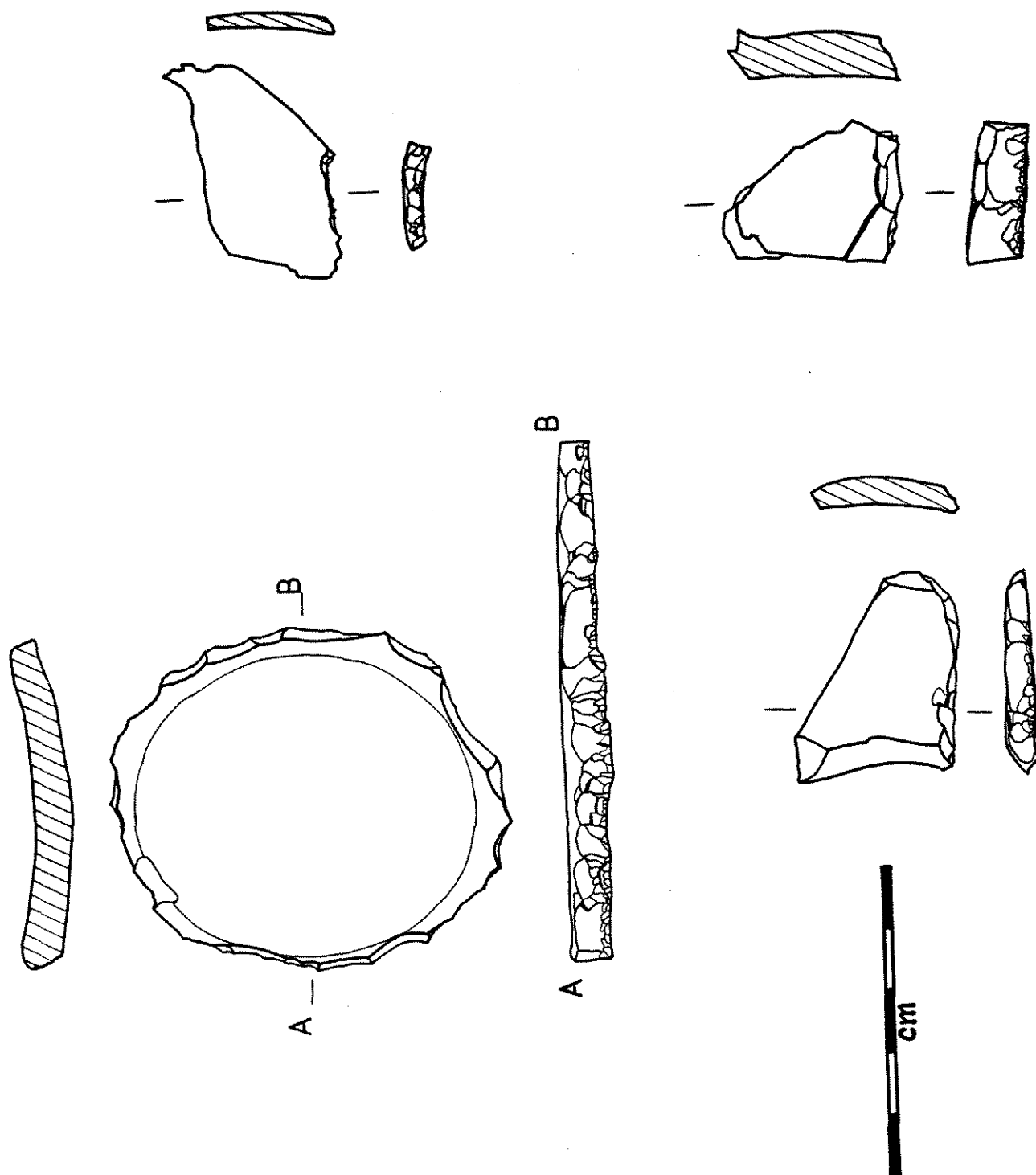




Plate 7. Artificial house platform. Dryland, Kukrumdi.



Plate 8. Oval house, swampland, Kuk. Taro along house wall, mixed crops (kitchen) garden on the left.



Plate 9. Woman and pig house destroyed by fire. Note platform with stone aligned hearth, and surrounding ditch. Dryland, Kum.



Plate 10. New and still unused hearth, circular and made from mixture of ash and water. New and still green leaves laid as carpet around it. Oval house, dryland, Kukrumdi.

Fig. 6 - House Site Definition

(not to scale)

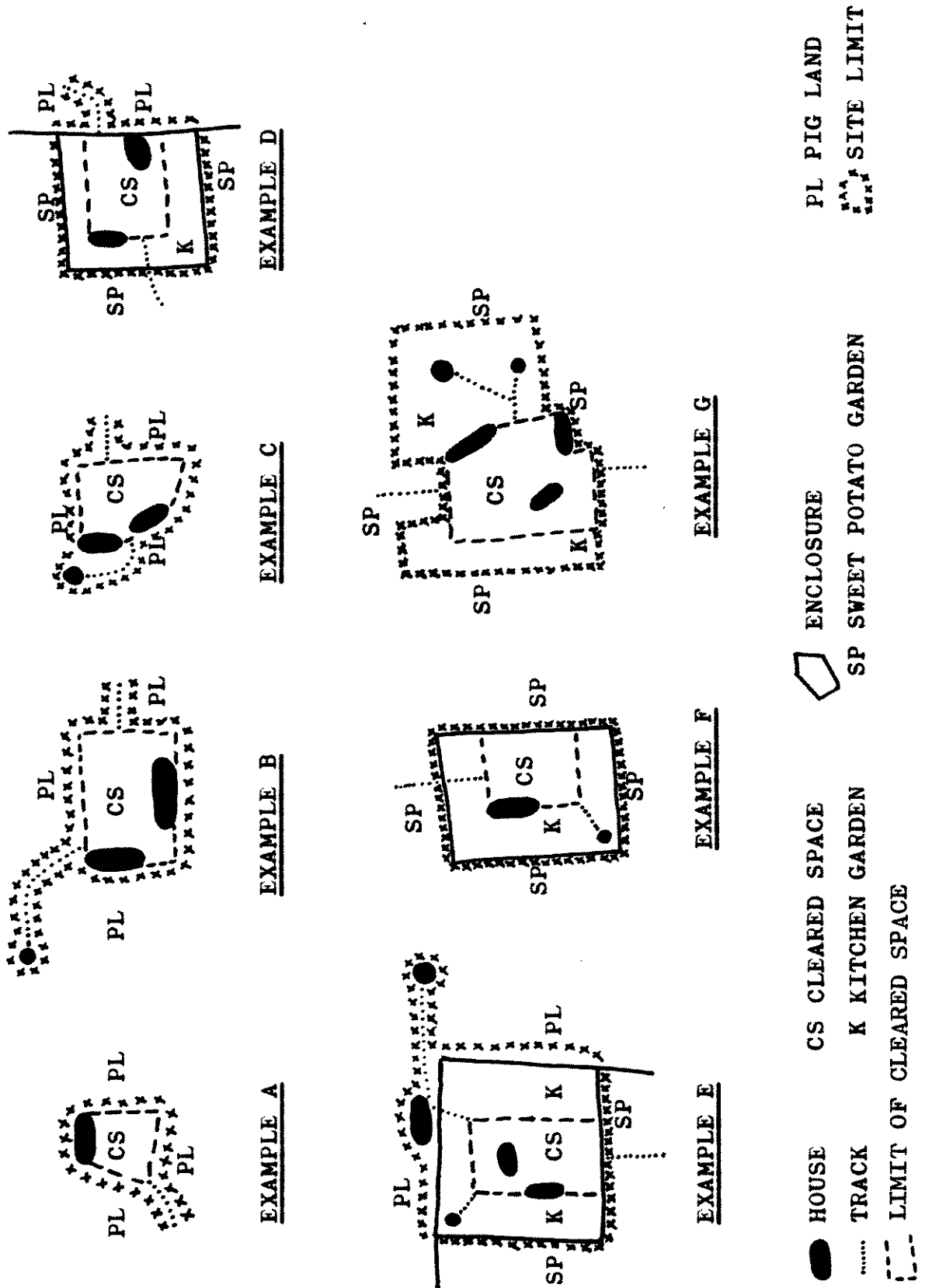
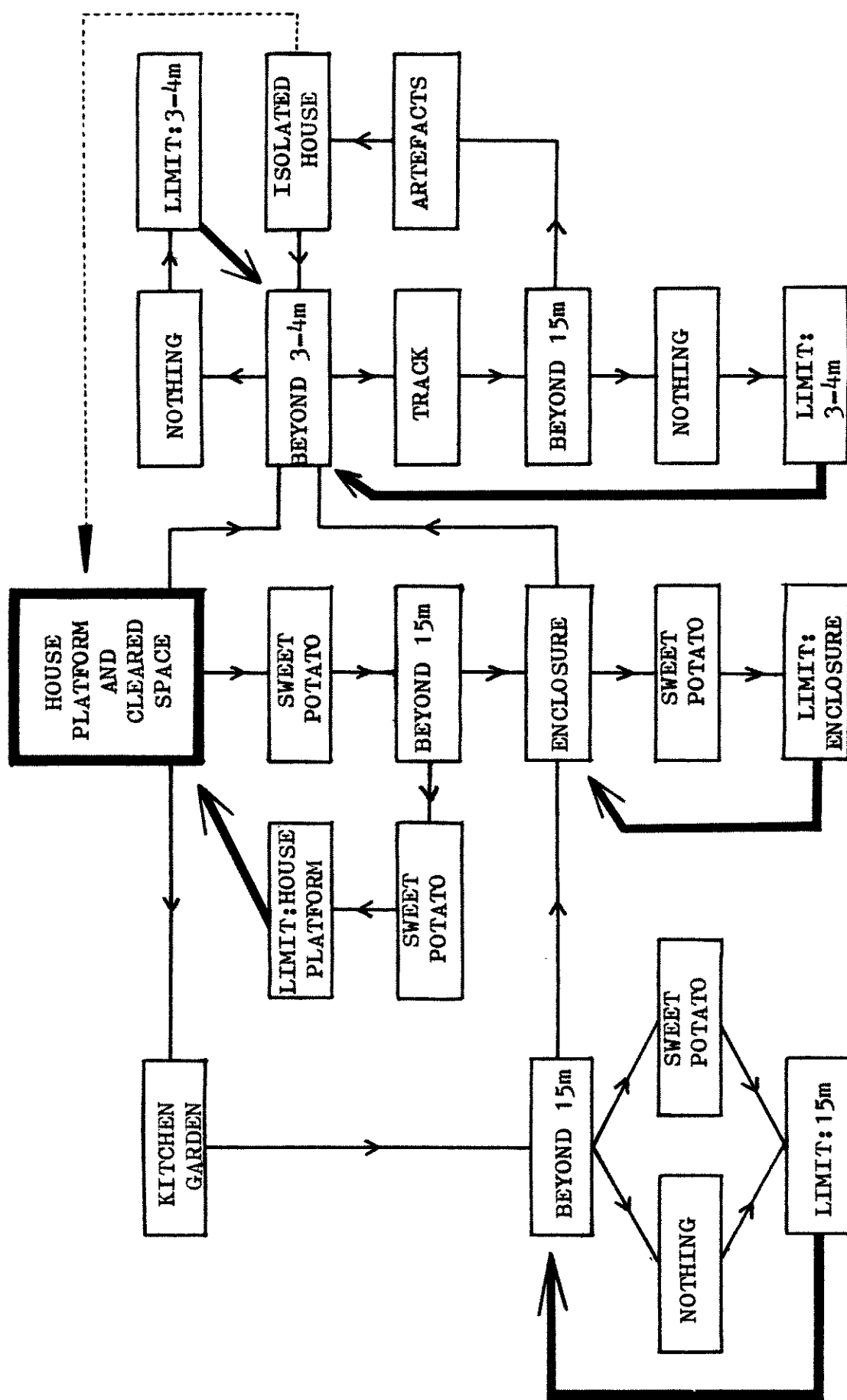


TABLE 2a - HOUSE SITE LIMITS



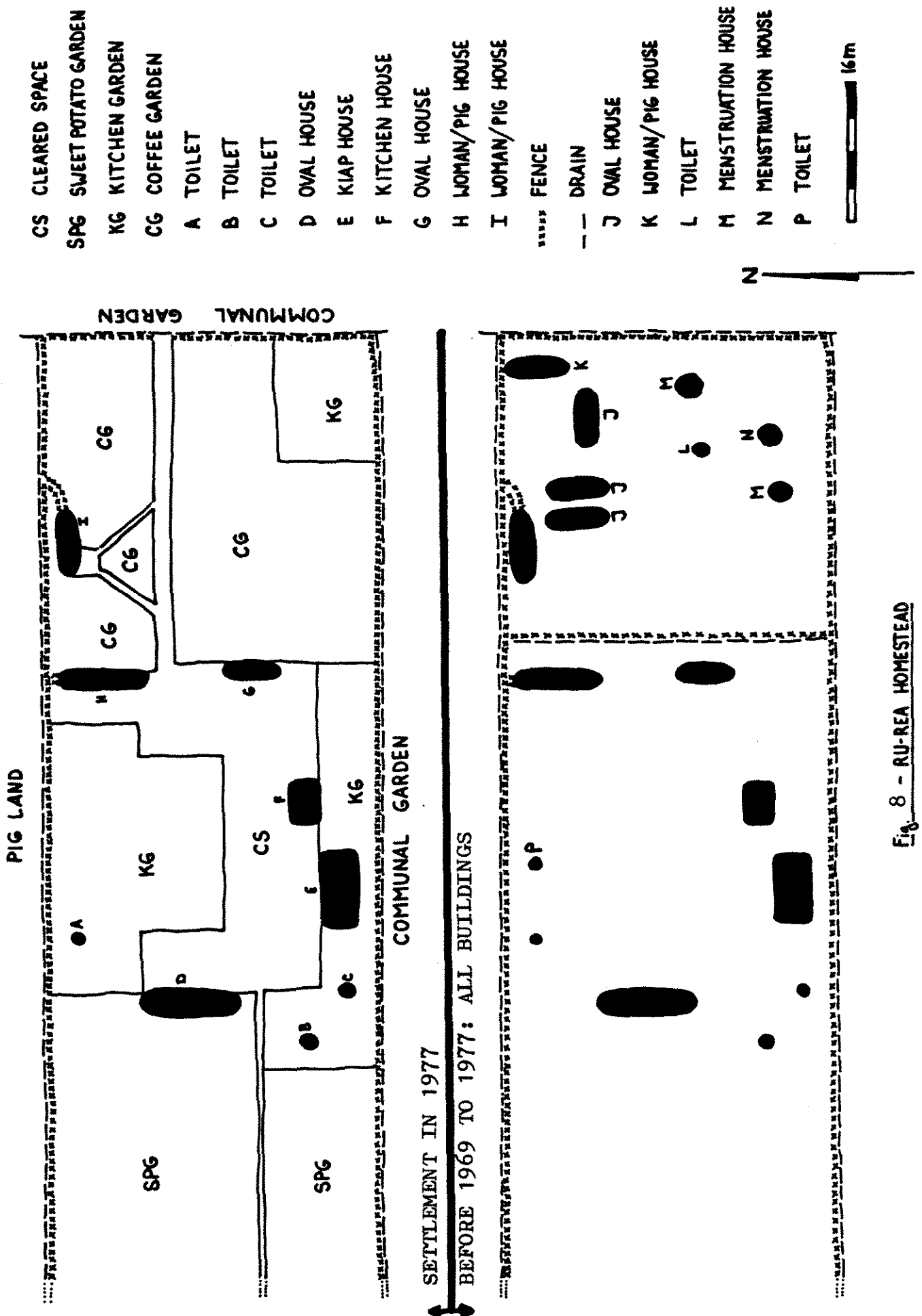


Fig. 8 - RU-REA HOMESTEAD

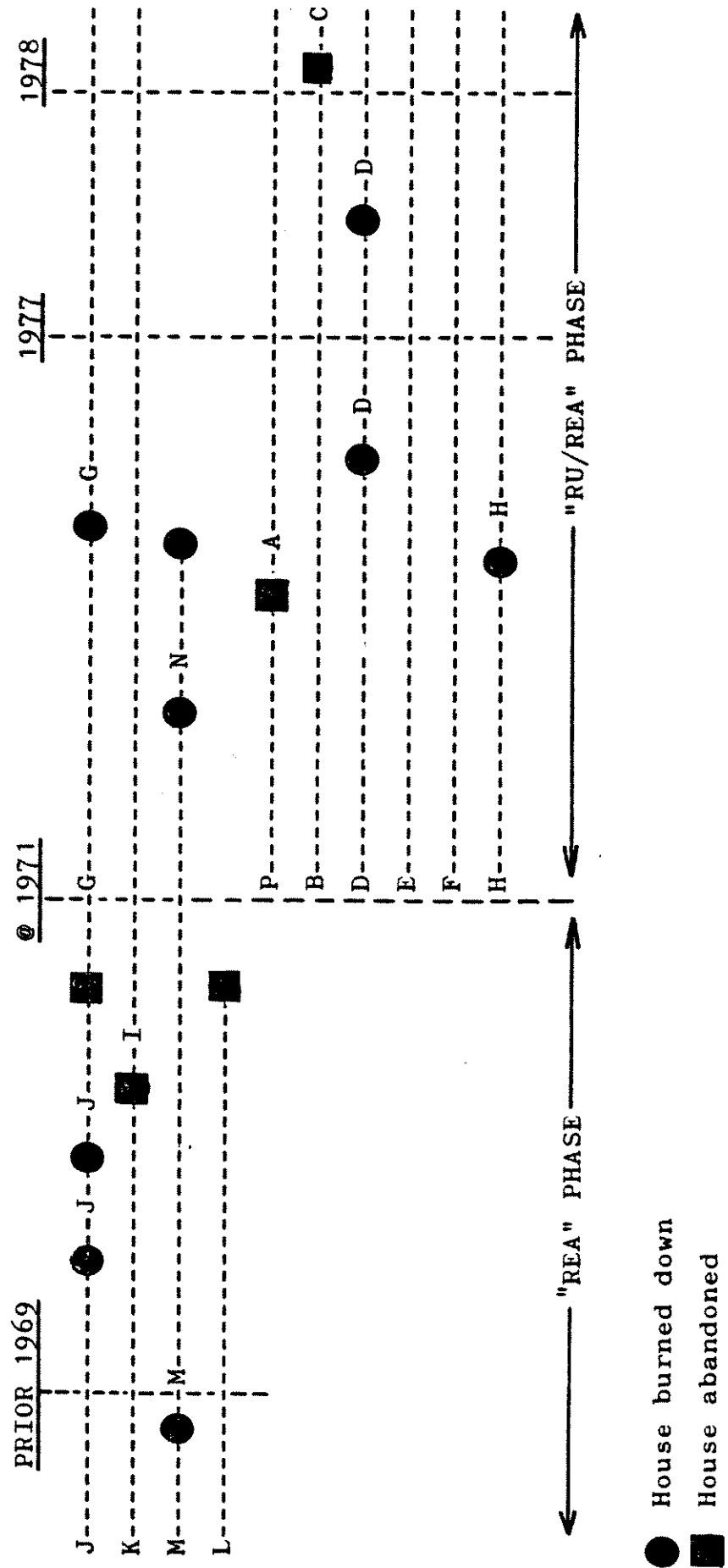


Table 3 - Ru/Rea Homestead

Table 4 : Evolution of House Types

	<u>1933</u>	<u>1977</u>	<u>Future</u>
DOMESTIC BUILDINGS	1: Man Oval -----	X,4,27	
	2: Man Veranda -----	2,4,27 -----	X,4,27
	3: Man Round -----	3,4,27 -----	X,4,27
	4: Family Oval -----	4,27 -----	4,27
	5: Woman Oval -----	5,4,27 -----	X,4,27
	6: Woman Veranda -----	X,4,5,27	
	7: Long Woman and Pig -----	7,4,5,12,27 -----	X,4,12,27
	8: Man Seclusion -----	X	
	9: Menstruation -----	9,4,5,7 -----	X,4,27
	10: Toilet -----	10 -----	10
	11: Stronghold -----	X	
	12: Pig -----	12 -----	12
	13: Cassowary -----	13 -----	13
	14: Marsupial -----	X	
	15: Overnight Hut -----	15,28 -----	X,28
	16: Garden Windbreak -----	16 -----	16
	17: Shooting Hut -----	17 -----	17
CEREMONIAL BUILDINGS		27: Kiap -----	27
		28: Guest -----	28
		29: Trade Store -----	29
		30: Dog -----	30
	18: Man Oval -----	18,19 -----	X,19
	19: Man Round -----	19 -----	19
	20: Guest -----	20 -----	20
	21: Ghost Man -----	X	
	22: Offering-Cemetery -----	X	
	23: Skull Hut -----	X	
	24: Secret Host -----	?	
	25: Ancestral Stones -----	?	
	26: Meat Hut -----	?	

X: Extinct House Type.

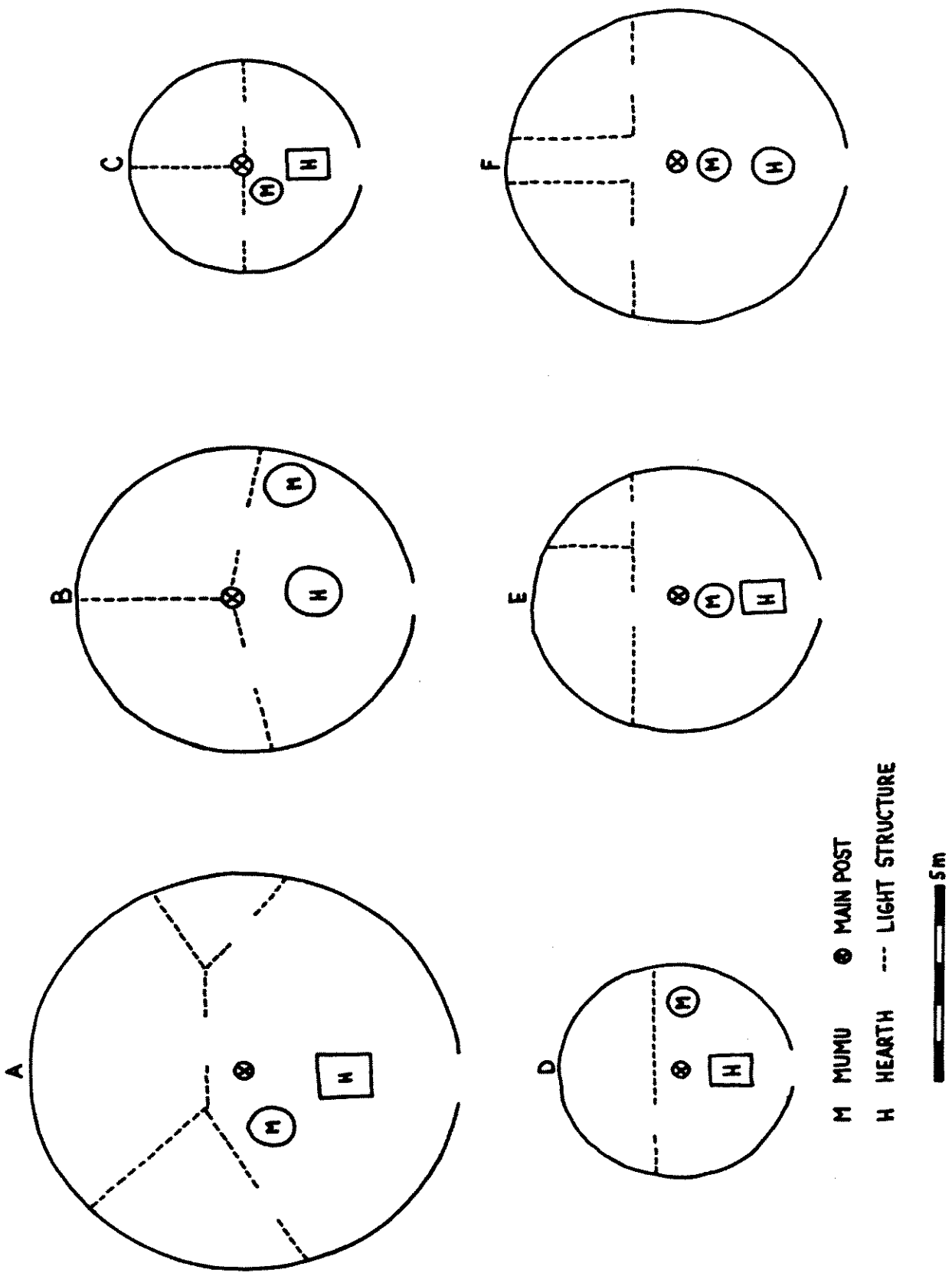


Fig. 9 - MEN'S ROUND HOUSE

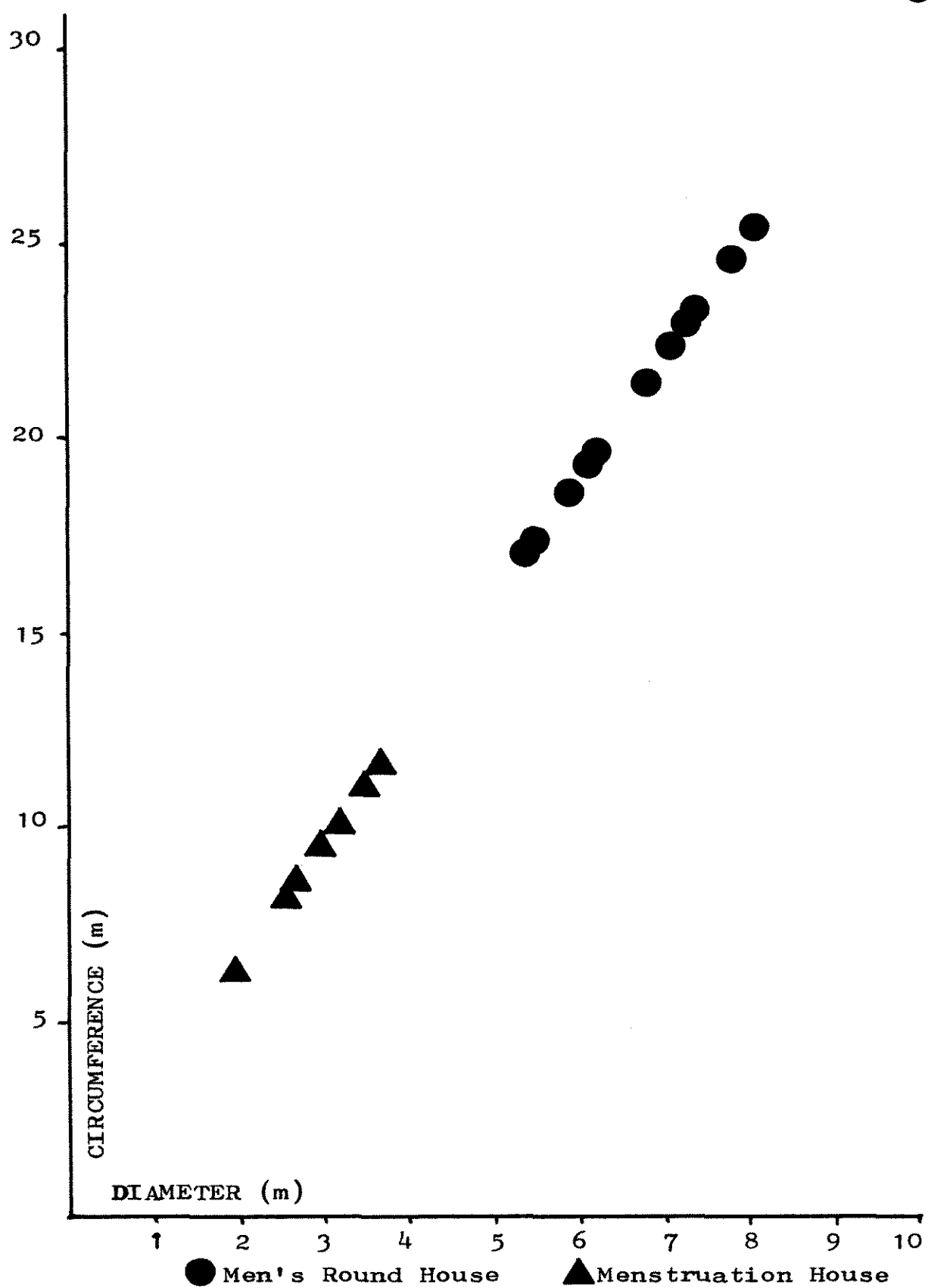


Table 5. Houses Measurements: Men's Round and Menstruation

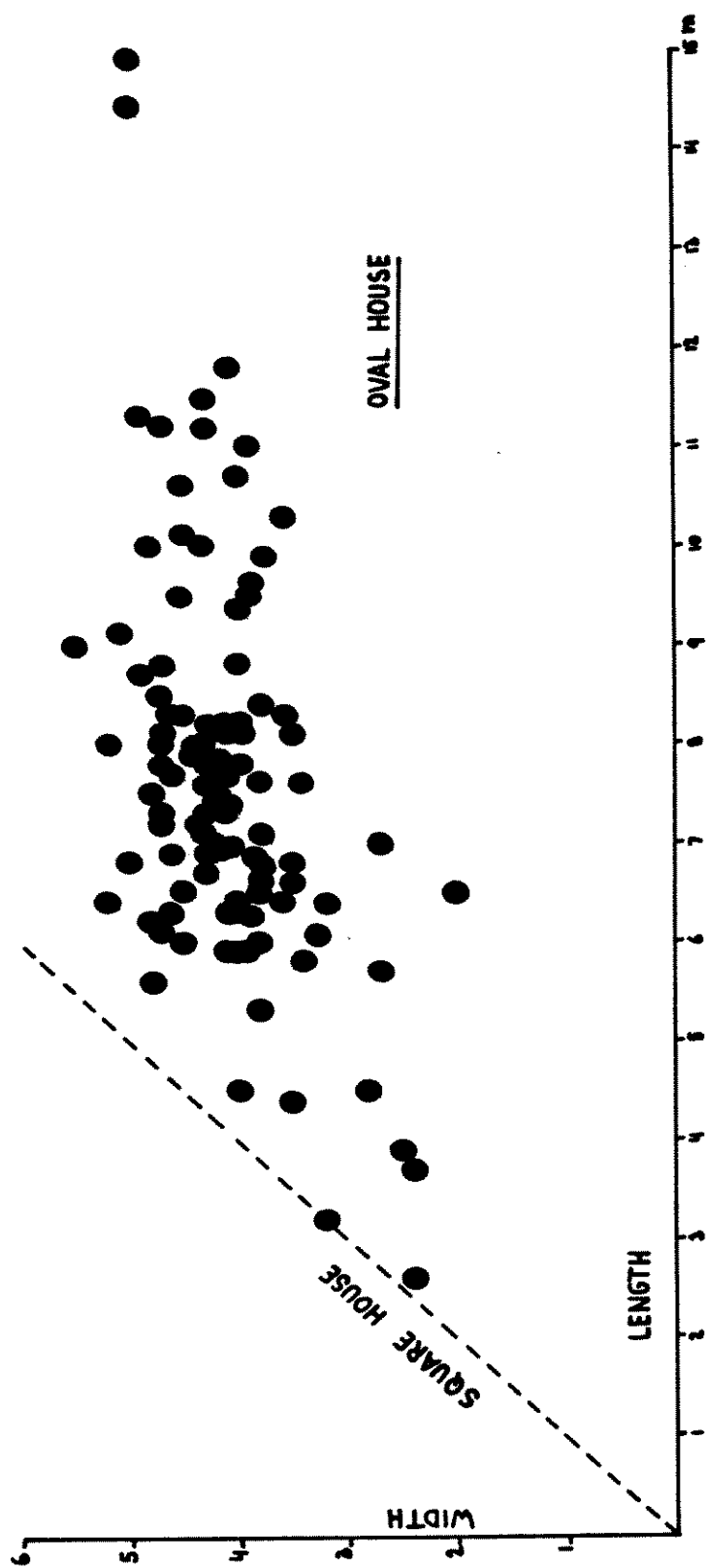


Table 6 . Dimension of Oval Houses

Fig. 10. Plan of Oval Houses

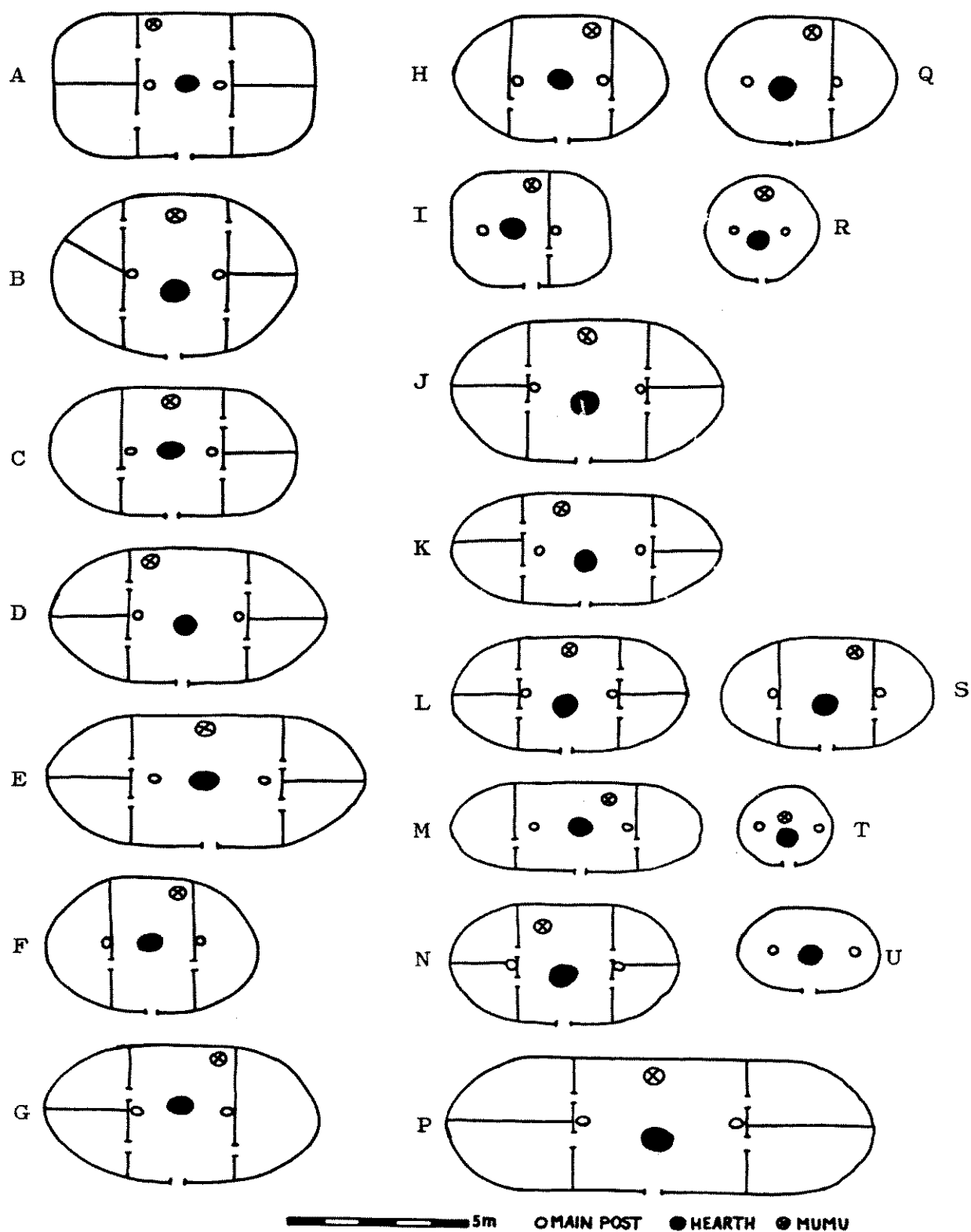
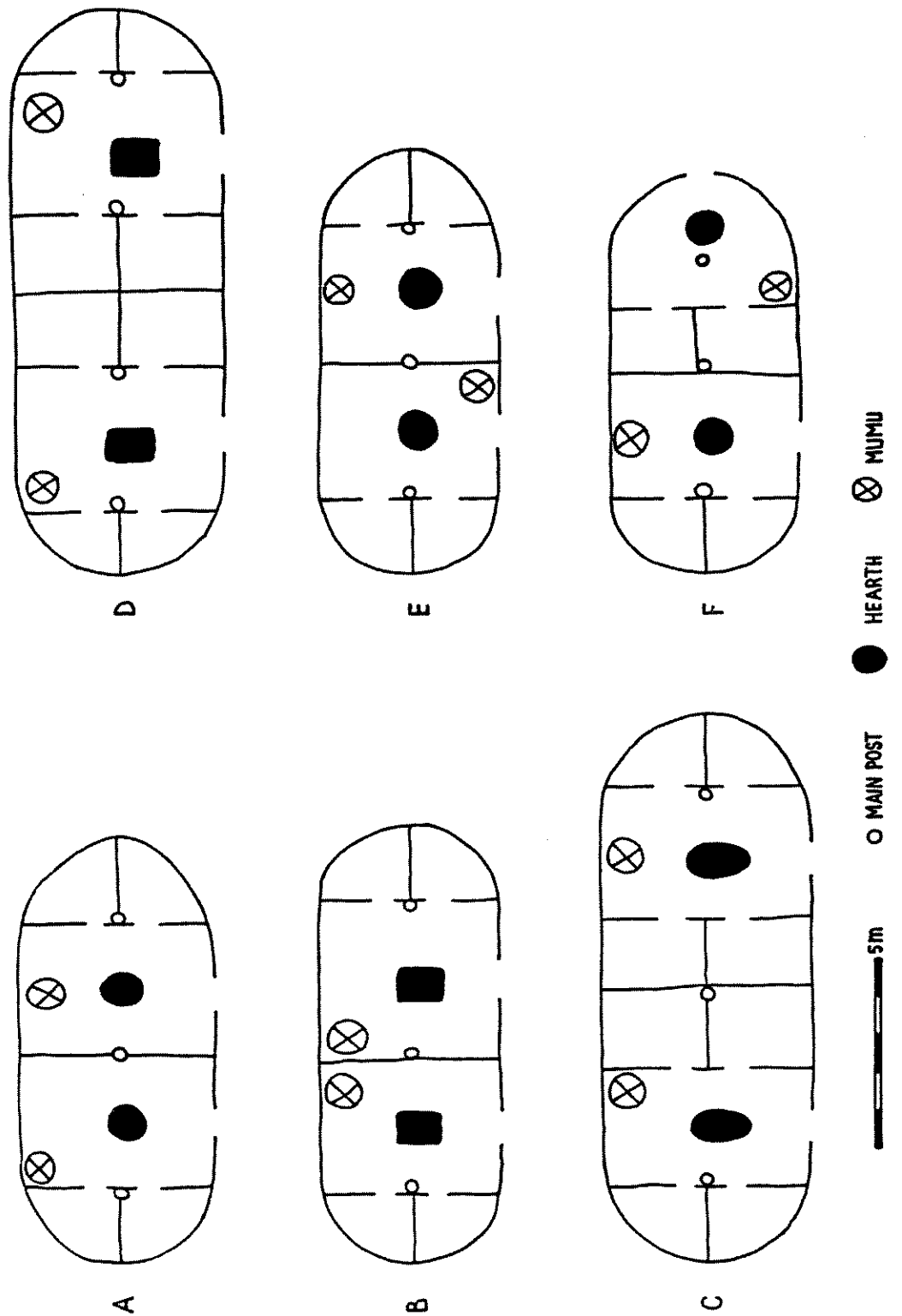


Fig. 11. Plan of Oval Houses



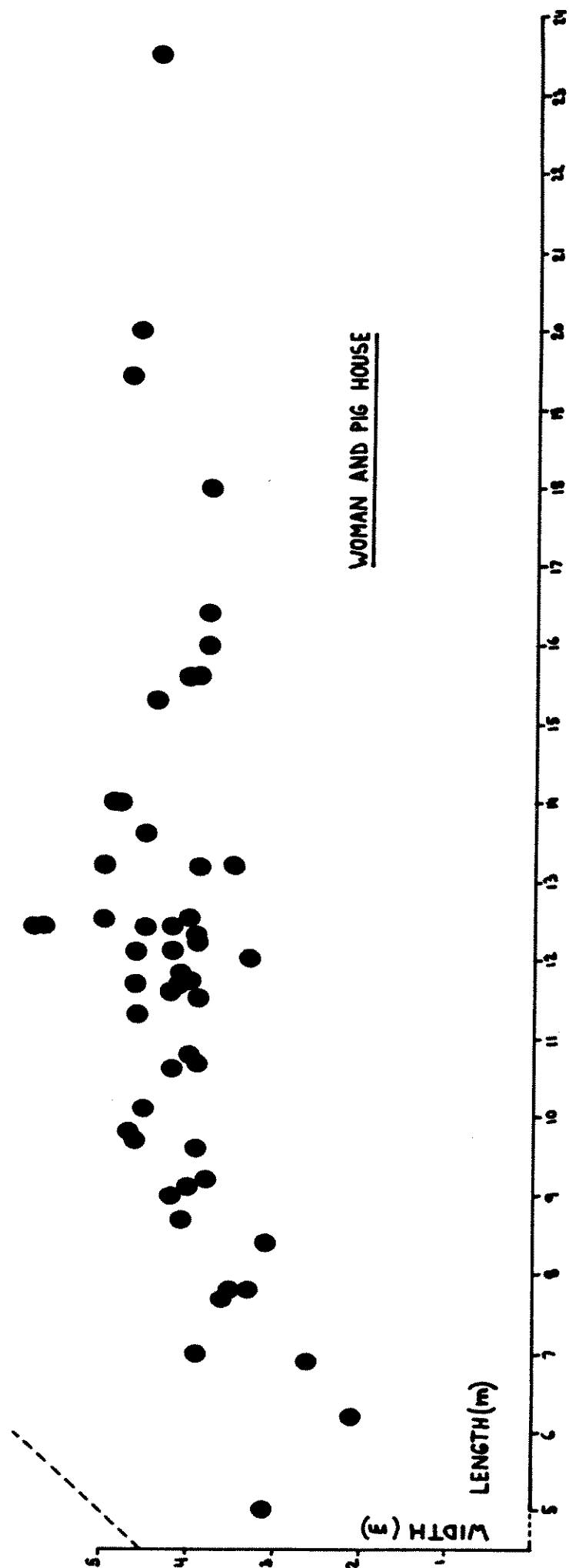


Table 7. Dimension of Woman and Pig Houses

Fig.12. Plan of Woman and Pig Houses

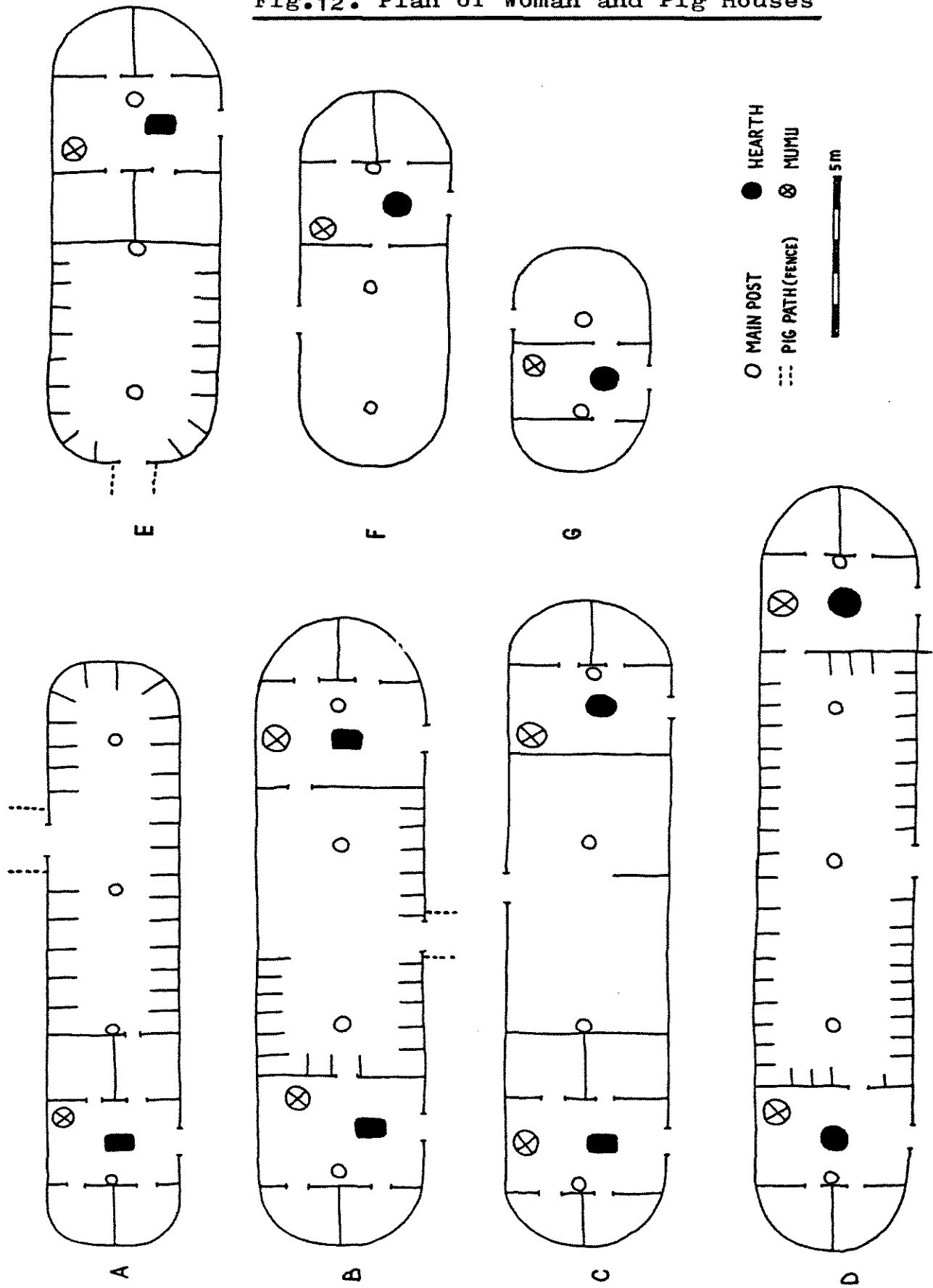


Table 8 . Dimension of Veranda and Kiap Houses

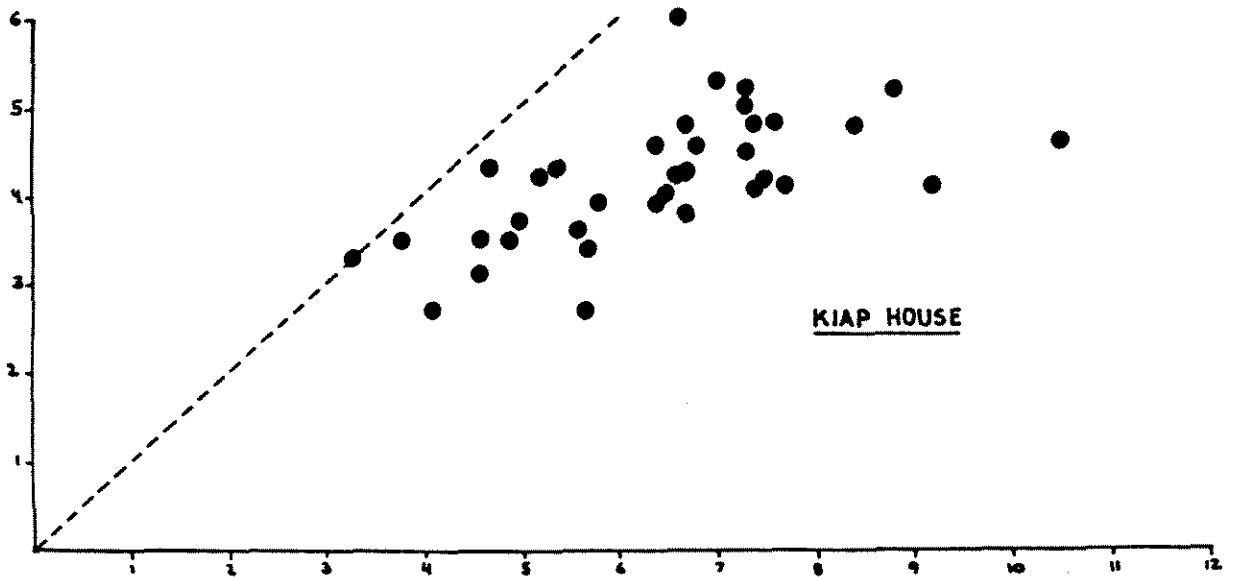
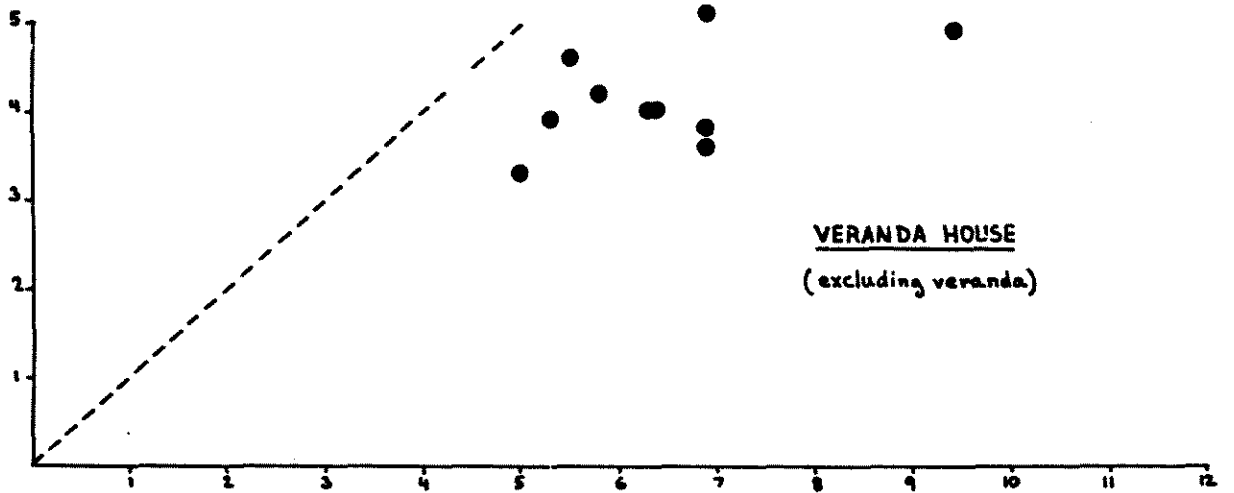
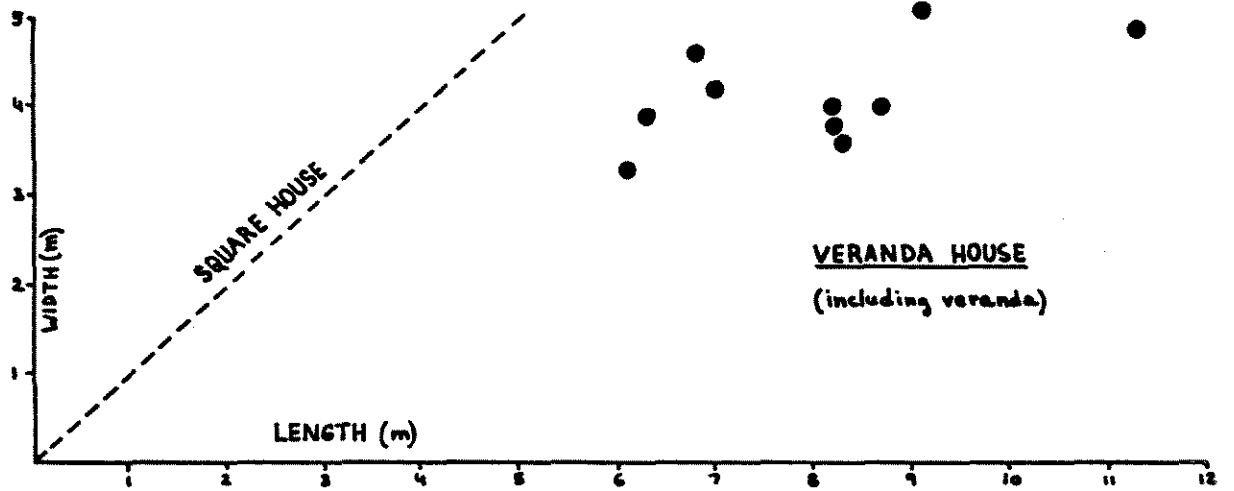


Fig. 13. Plan of Veranda Houses

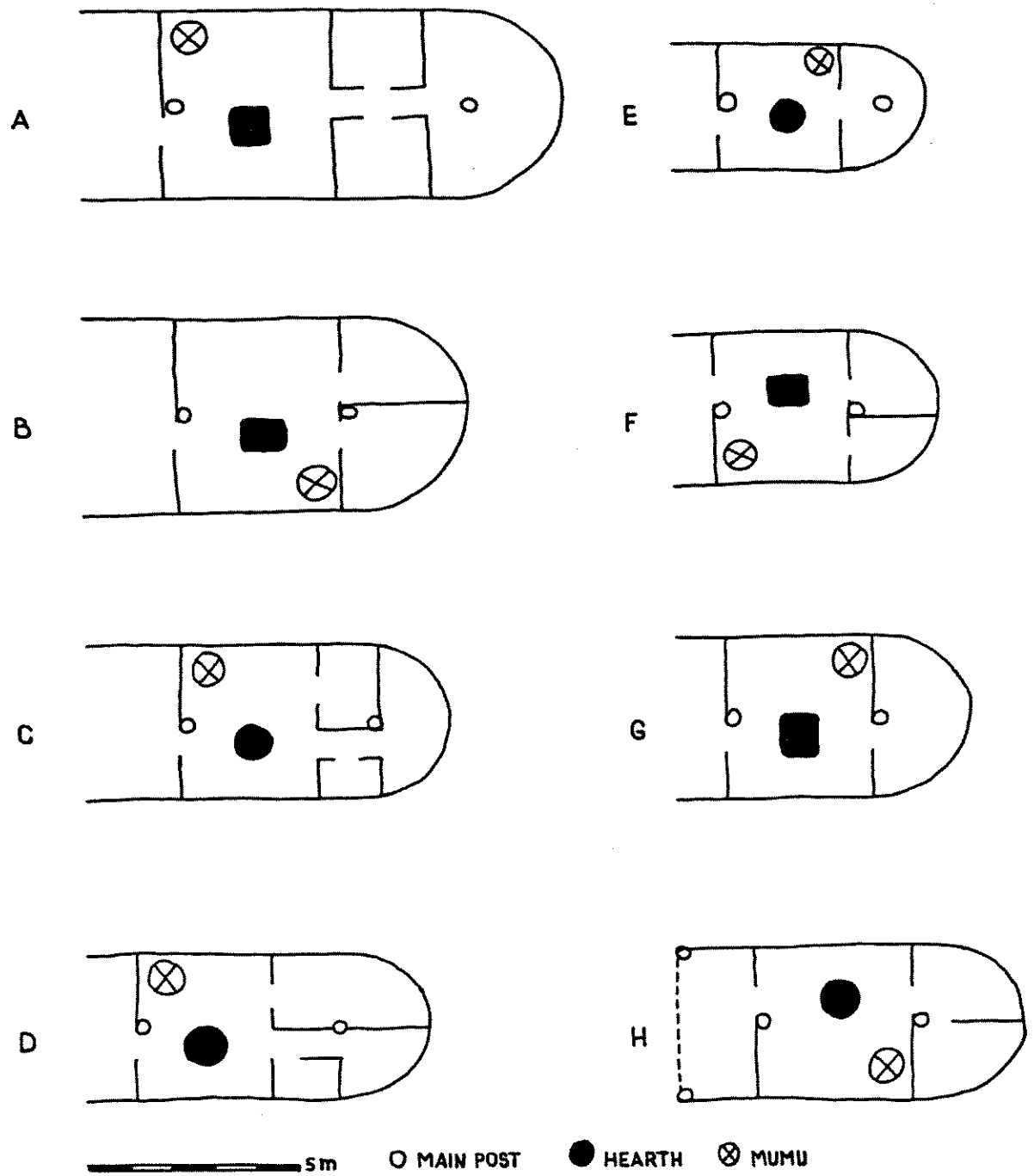


Table 9 . Dimension of Other Buildings

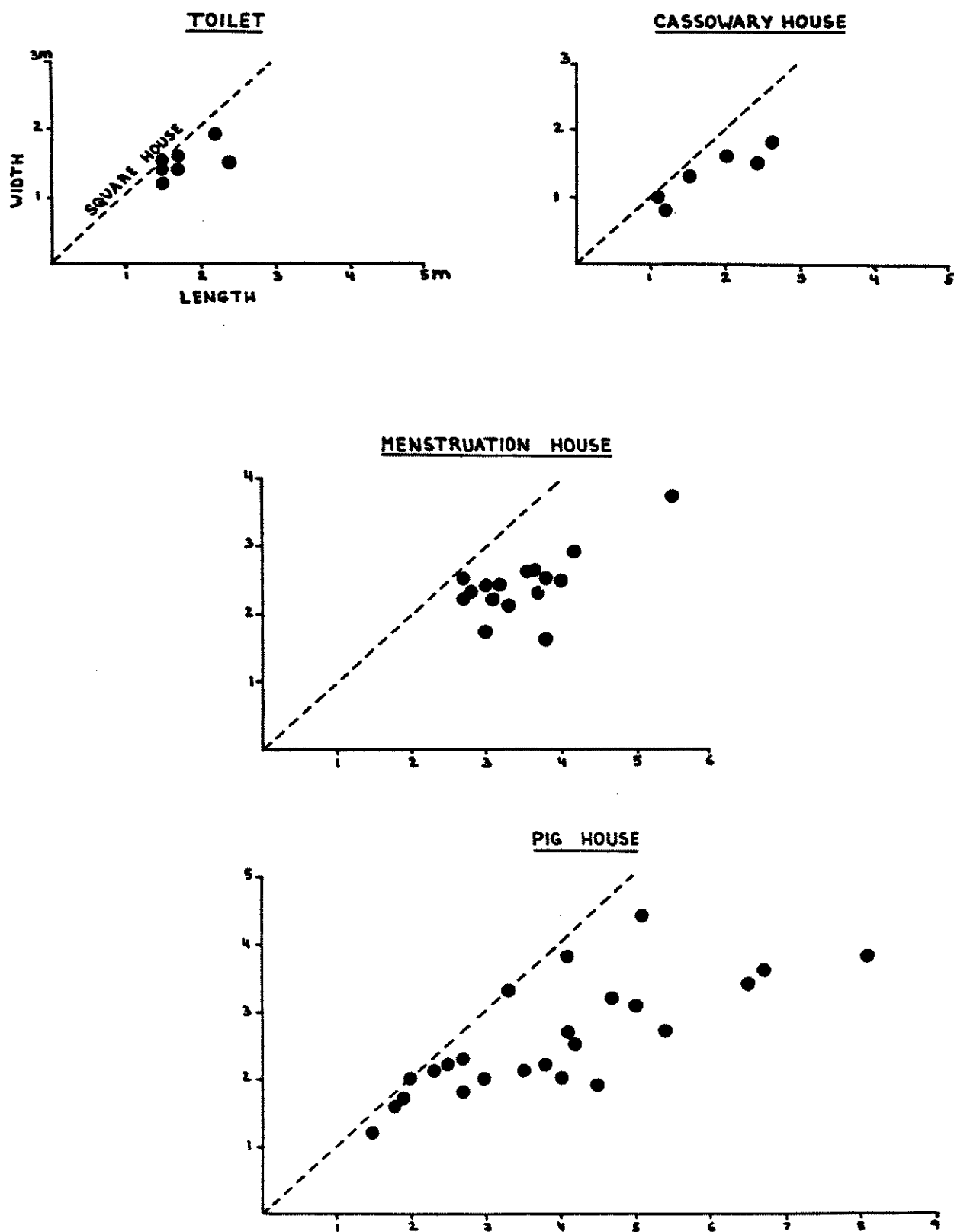
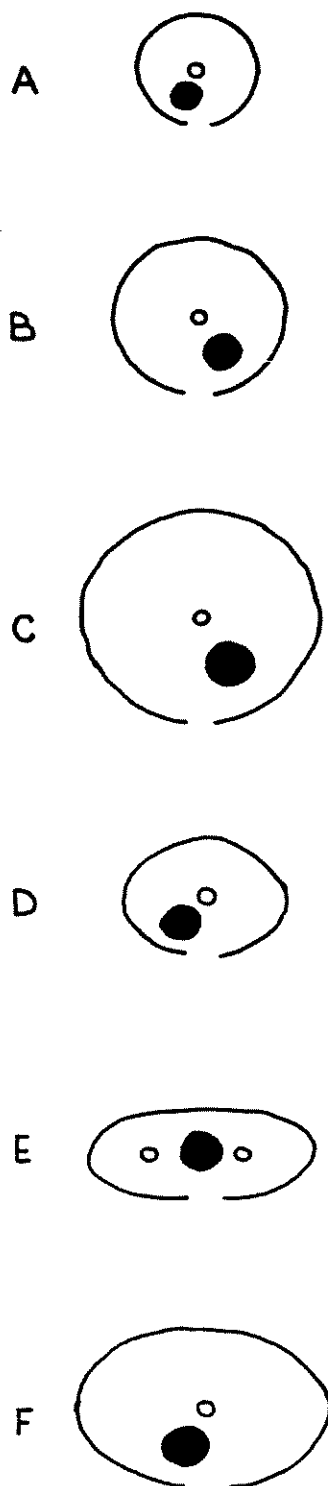
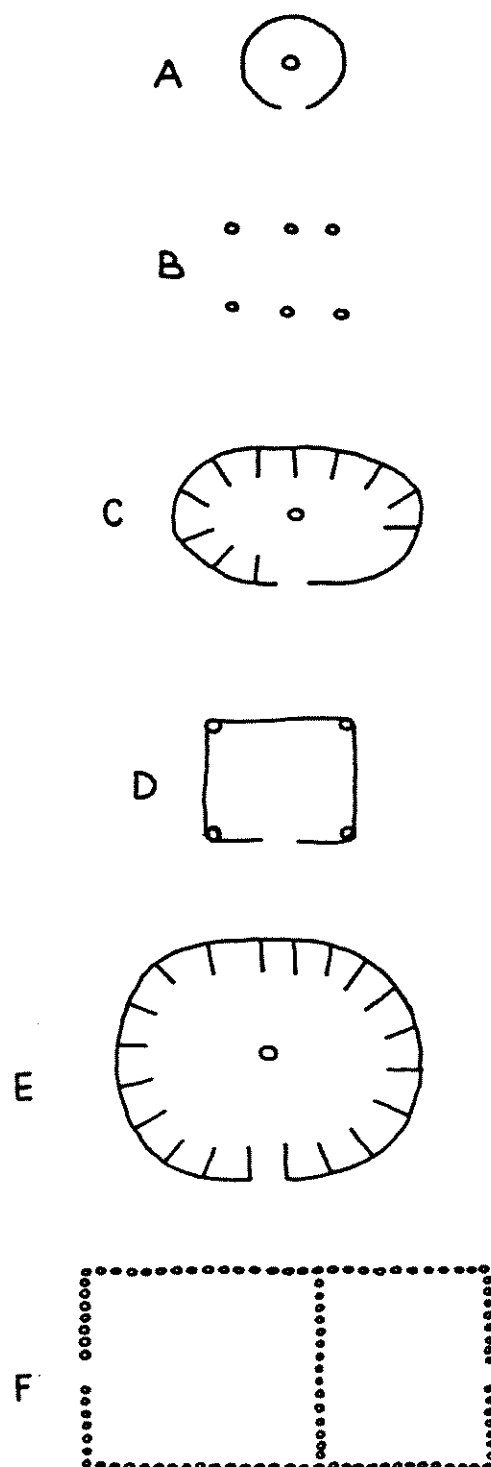


Fig. 14- MENSTRUATION HOUSEFig. 15- PIG HOUSE

○ MAIN POST

● HEARTH

5m

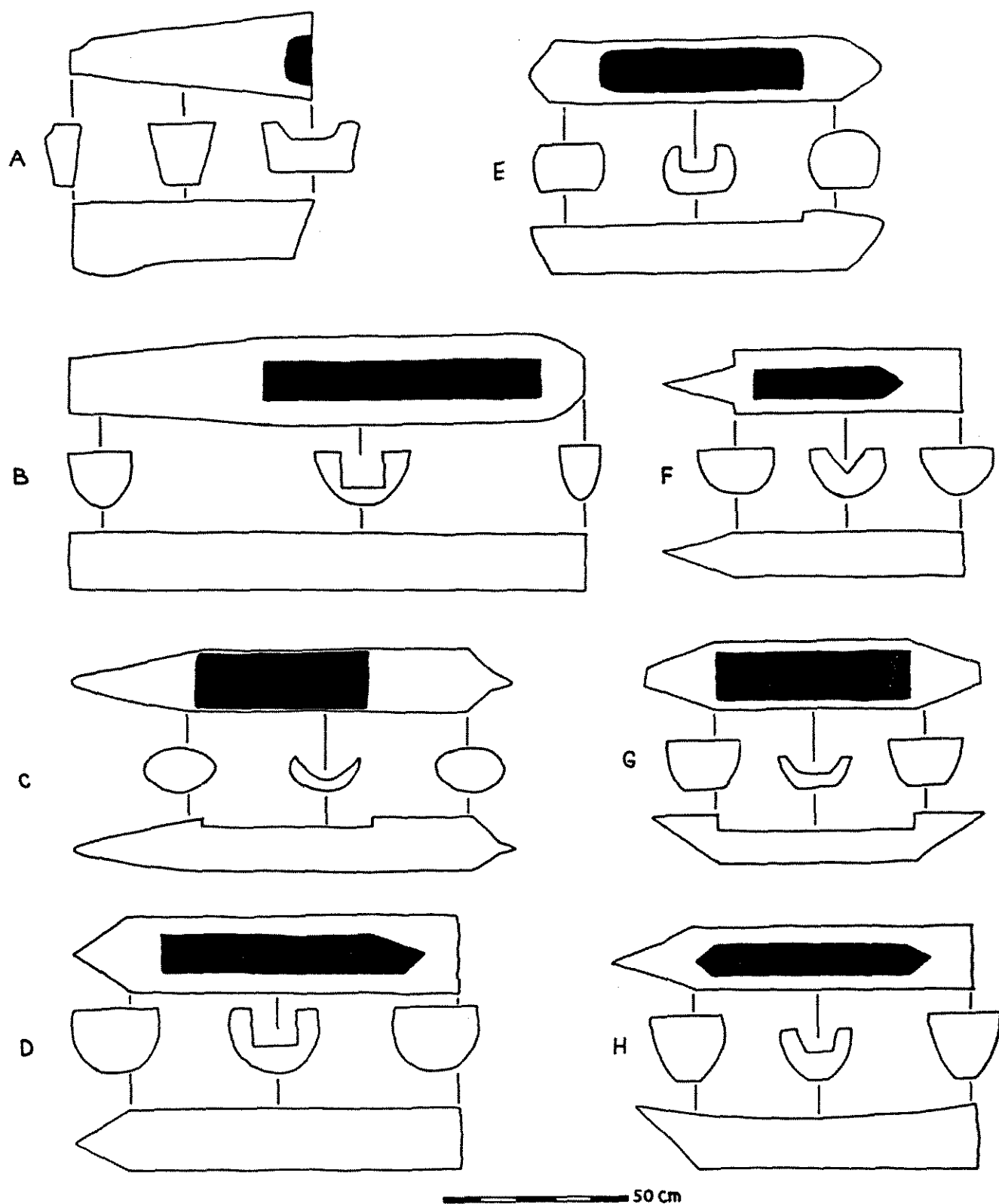


Fig. 16. Cassowary Trough

A CASSOWARY C MENSTRUATION E KIAP G OVAL
B TOILET D PIG F WOMAN AND PIG H VERANDA

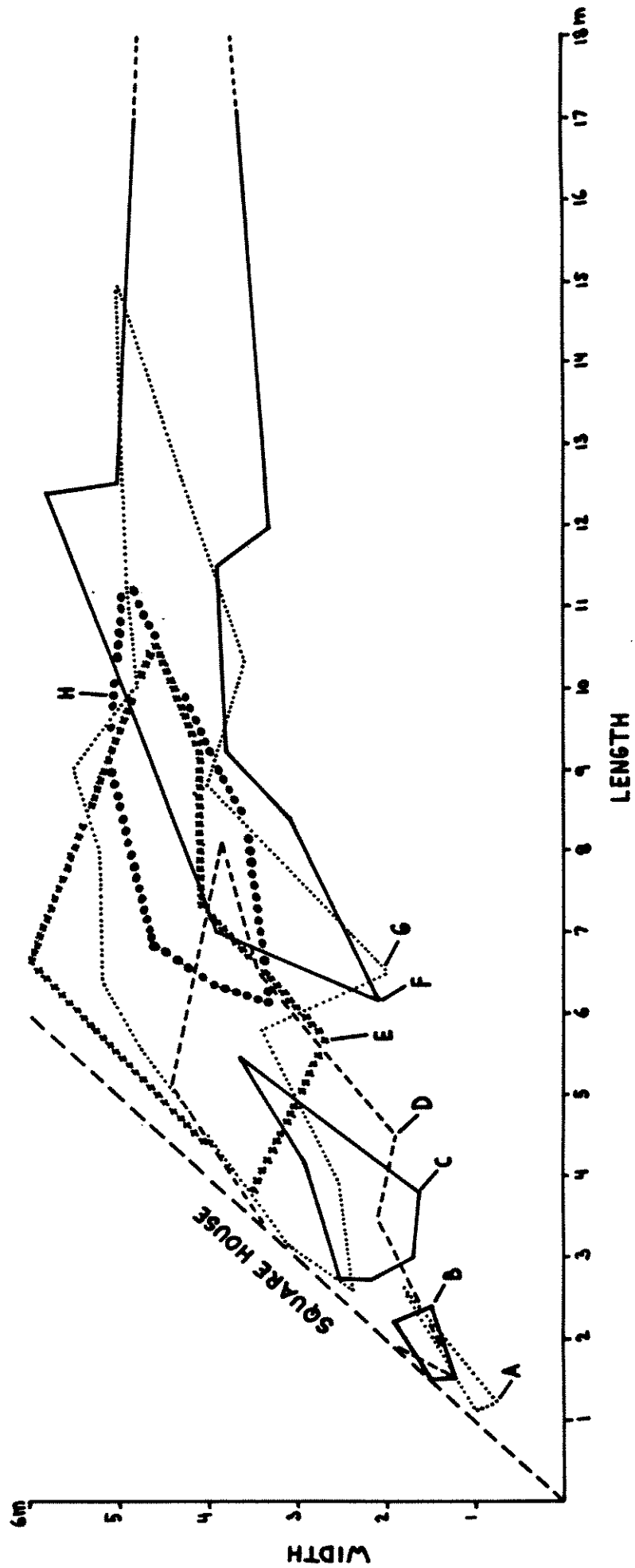
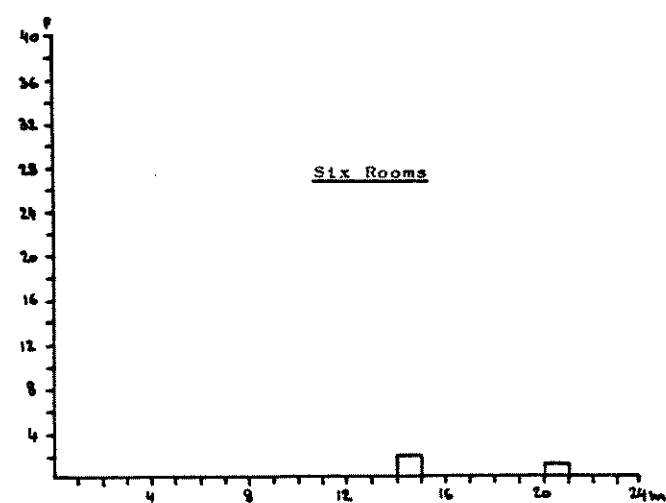
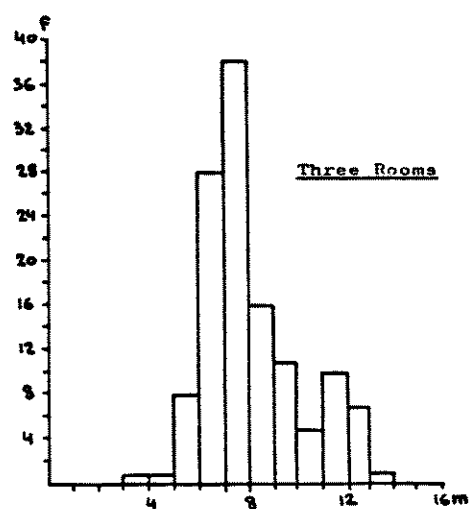
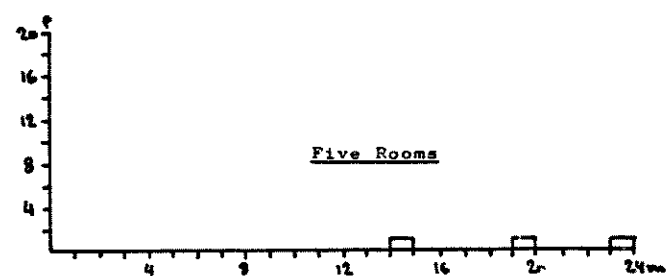
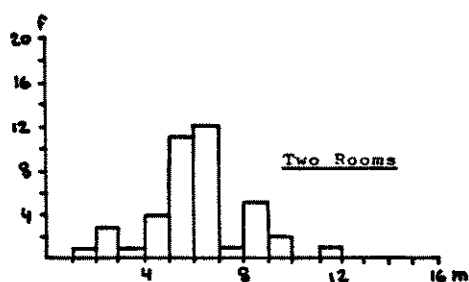
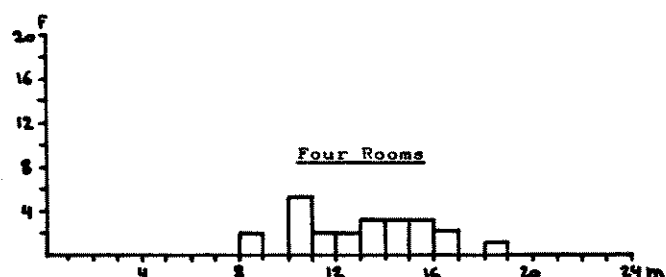
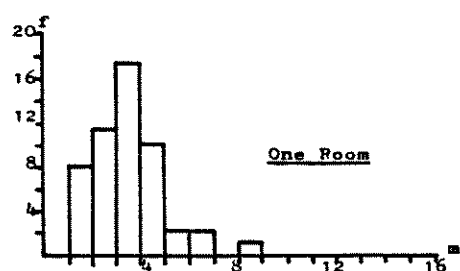


Table 10. Length and Width of all House Types

Table 11. Frequency of Number of Rooms According to House Length



n	DOORS		
	1	2	3
1	A	B	C
2	D	E	F
3	G	H	I
4	J	K	L
5	M	N	P
6	Q	R	S

ROOMS

**Table 12. Correlation Between
DOOR/ROOM and
LENGTH/TYPE of Houses**

LENGTH (m)	TOILET	CASSOWARY	PIG	MENSTRU- ATION	OVAL	KIAP	VERANDA (EXCL.)	VERANDA (INCL.)	WOMAN AND PIG	TOTAL	%
1-2	1A	2A, 1E	2A							9	3.7
2-3	2A	3E	4A, 1B	3A	1A					14	5.7
3-4			3A, 1B	10A	1A, 1D, 1G	2A				19	7.7
4-5			3A, 2B, 1C	1A	1A, 2D	2A, 2D, 1G				15	6.1
5-6			2A	1D	2D, 5G	3D, 3G	4D		1D	21	8.5
6-7			2B		1D, 22G	2D, 5G	5D	3D	1E, 1H	42	17.1
7-8					28G	6G		1D	4H	39	15.8
8-9			1A		1D, 13G, 2K	1G		4D	2H	24	9.8
9-10					4G	1G	1D	1D	6H	13	5.3
10-11					1G, 4K	1K			4H	10	4.1
11-12					4G, 1K			1D	6H, 1K	13	5.3
12-13									7H, 2K	9	3.7
13-14									1H, 3K	4	1.6
14-15					2R				2K, 1P	5	2.0
15-19									6K	6	2.4
19-24									2P, 1S	3	1.2
Total	6	6	22	15	97	29	10	10	51	246	-
%	2.4	2.4	9.0	6.1	39.4	11.8	4.1	4.1	20.7	-	100

Fig. 17. Pita House (see key next page)

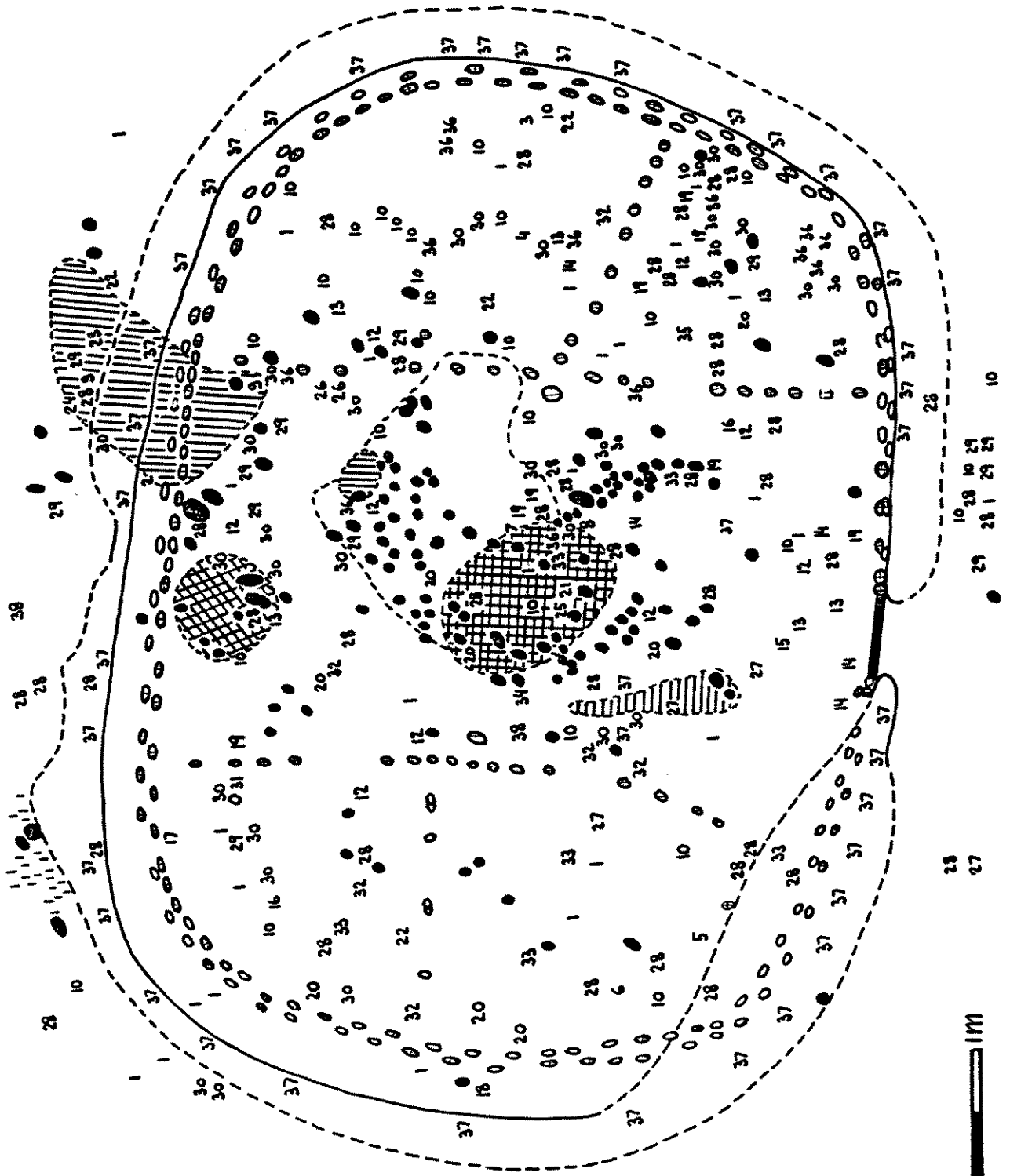
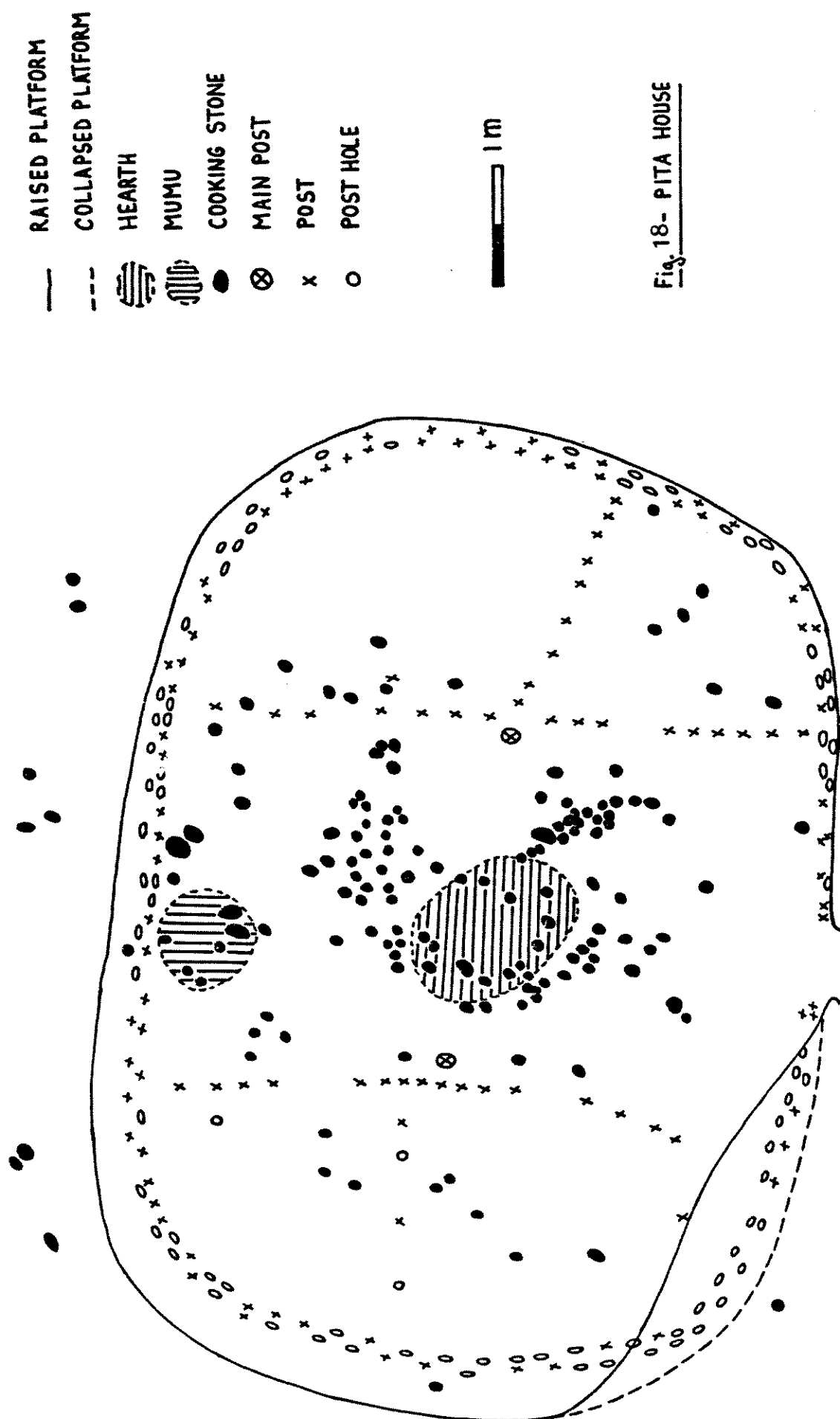
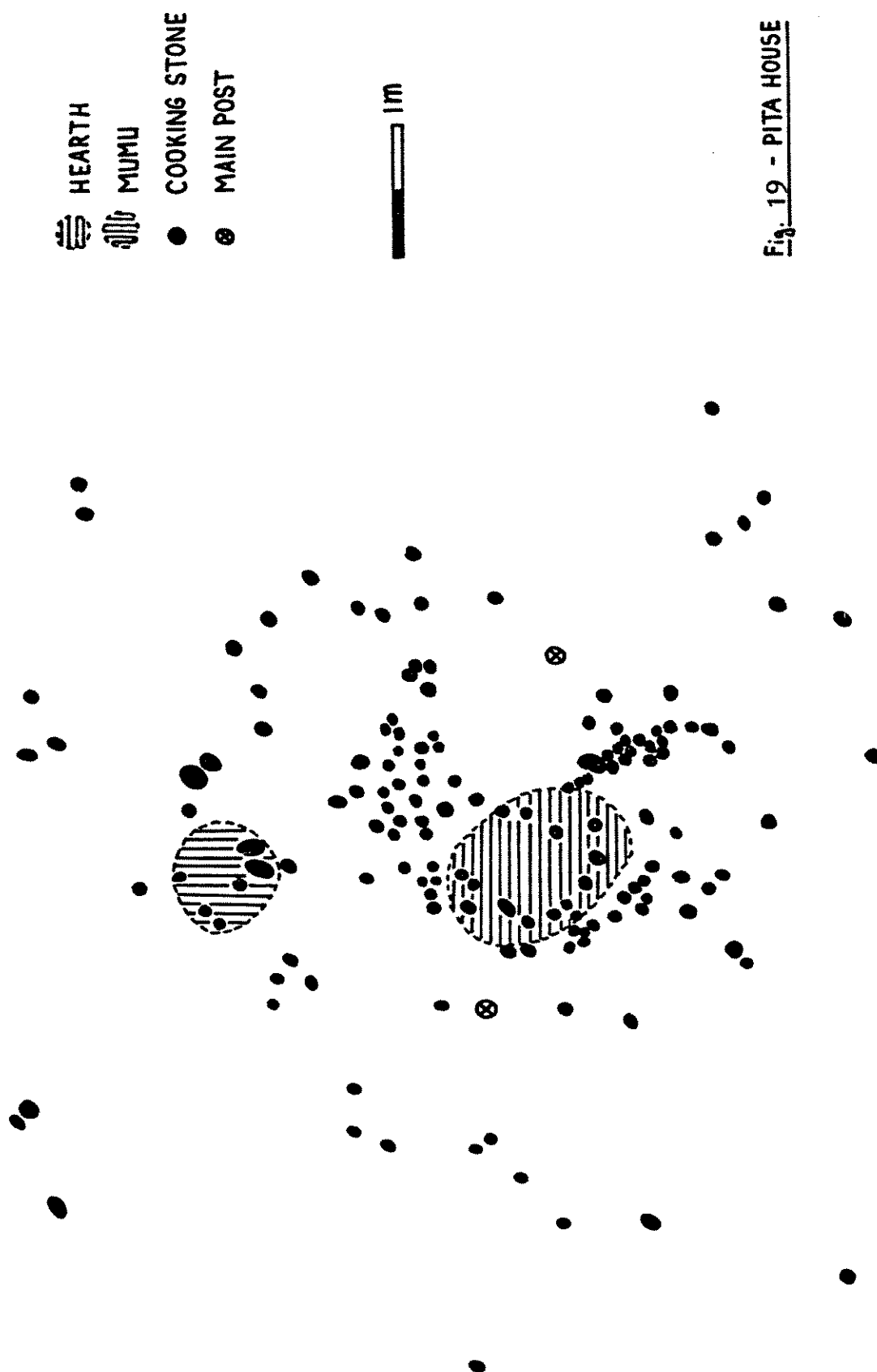


Table 13. Key for Figure (Pita House)

-  House Platform
-  Door Foundation
-  Hearth
-  Mumu
-  Organic Concentration (banana, pitpit, tankis, sugar cane, twigs)
-  Charcoal, Burned Clay
-  Coffee
-  Cooking Stone
-  Post
-  Post Hole
- 1 Glass
- 2 Mirror
- 3 Mice Trap Spring
- 4 Razor Blade
- 5 Lighter
- 6 Watch Bracelet
- 7 Plastic Wallet
- 8 Broach
- 9 Car Tyre
- 10 Beer Bottle Cap
- 11 Tin
- 12 Iron Sheet
- 13 Iron Wire
- 14 Aluminium Foil
- 15 Plastic Sheet
- 16 Plastic Container
- 17 Salt in Plastic Bag
- 18 Beer Box
- 19 Paper
- 20 Laplap (cloth)
- 21 Towel
- 22 Jute

- 23 Painting Pot (for singsing)
- 24 Bird of Paradise Feather (Plastic Bag)
- 25 Rope for Pigs
- 26 Pig Coprolite
- 27 Tankis
- 28 Timber, Firewood
- 29 Sweet Potato
- 30 Corn
- 31 Pat (bean)
- 32 Karuka Nut (Pandanus)
- 33 Betel Nut
- 34 Yam
- 35 Peanut
- 36 Pig Bone
- 37 Planted Tobacco
- 38 Uprooted Tobacco





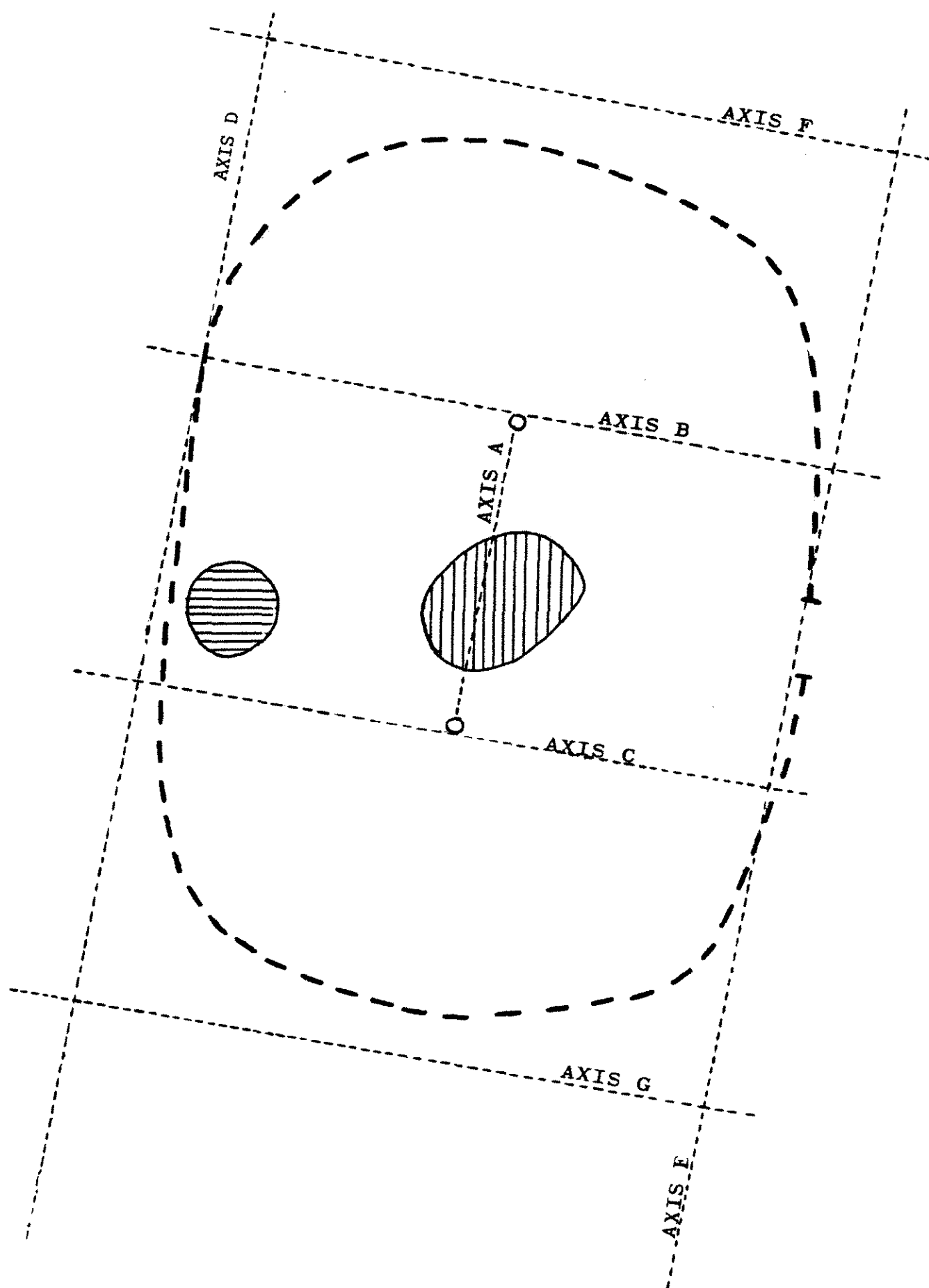
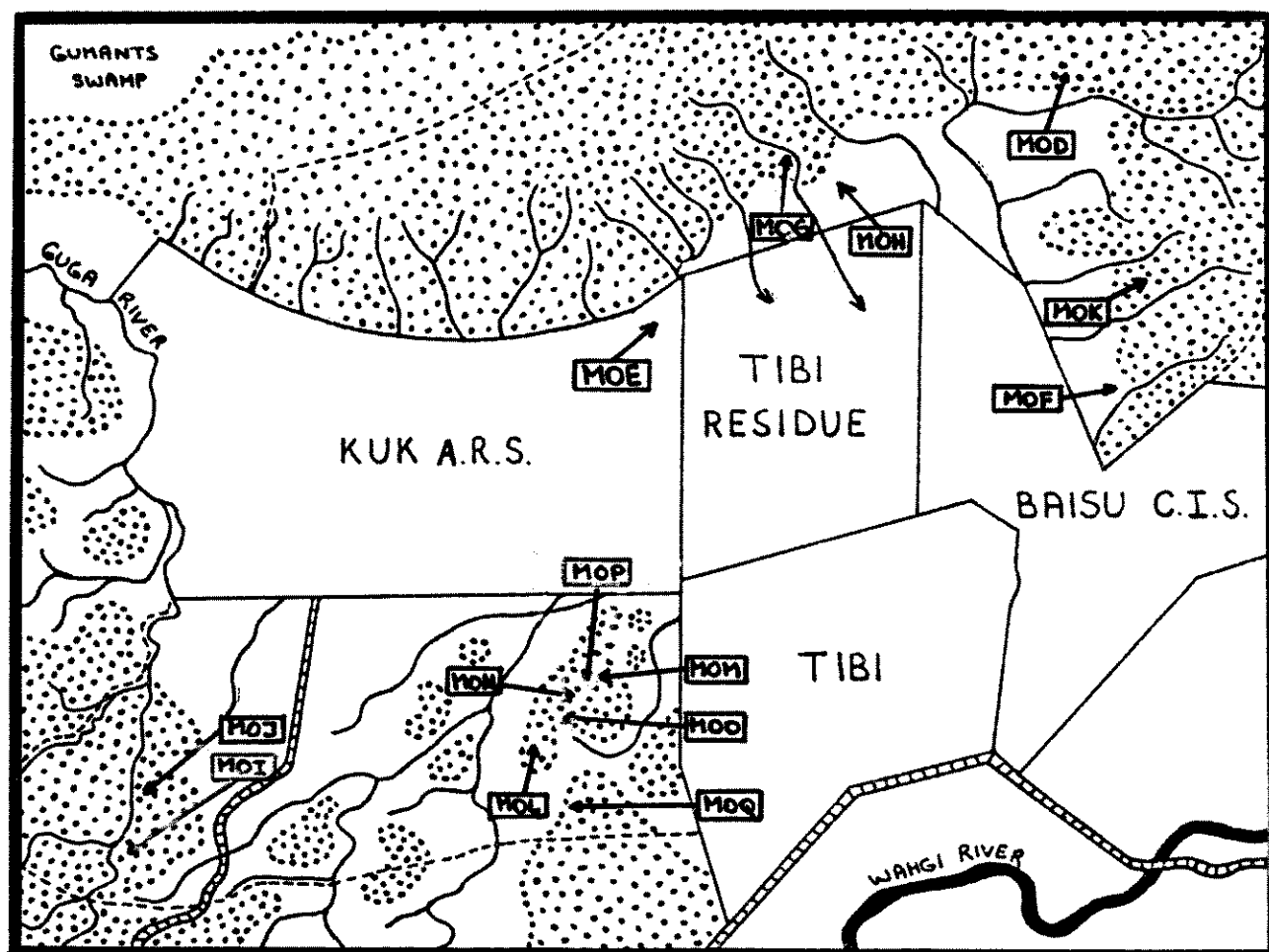


Fig. 20. Pita House: Interpretation

Map 24. Location of House Sites



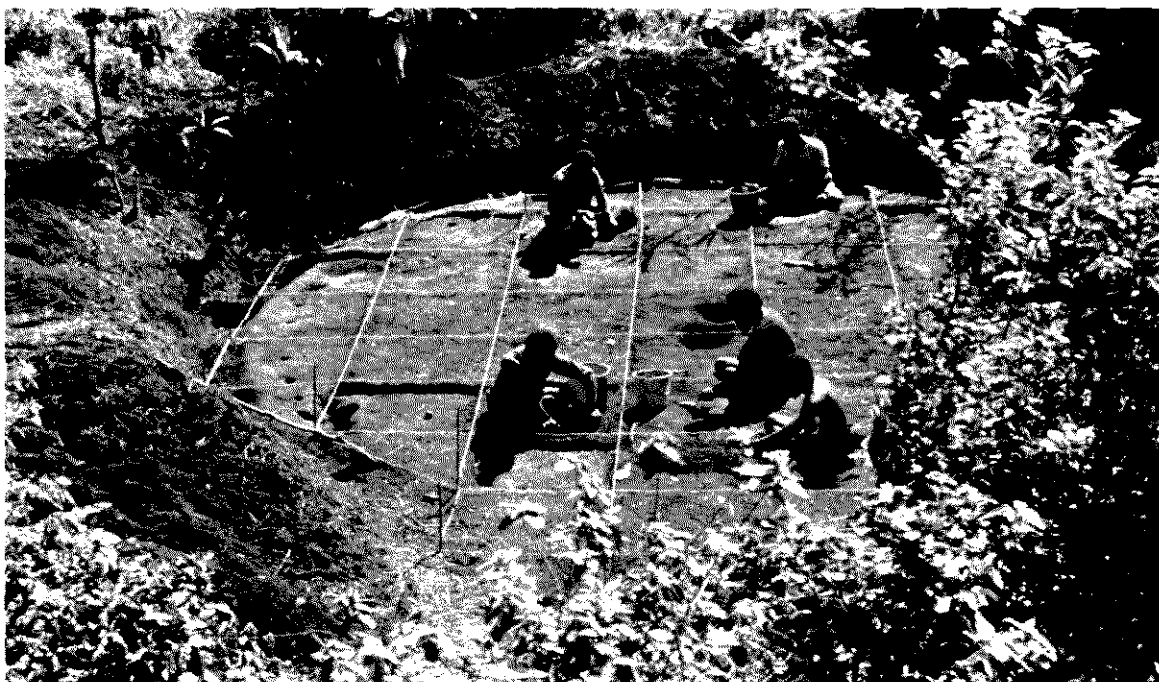


Plate 11. Site MOP. Excavation in progress. Some squares are dug down to basal clay and post holes can be seen on left. Central post hole and oven in centre (square E5), hearth not yet excavated.



Plate 12. Site MOH/WP. Stone pathway for pigs.



Plate 13. Site MOH/R. Hearth. Excavation in progress. Hearth similar to the one seen in Plate 9 (stone aligned and square).



Plate 14. Site MOP. Profile of hearth. Type similar to the one shown in Plate 10 (made with ash and water, circular).

Fig.21- SITE MOP

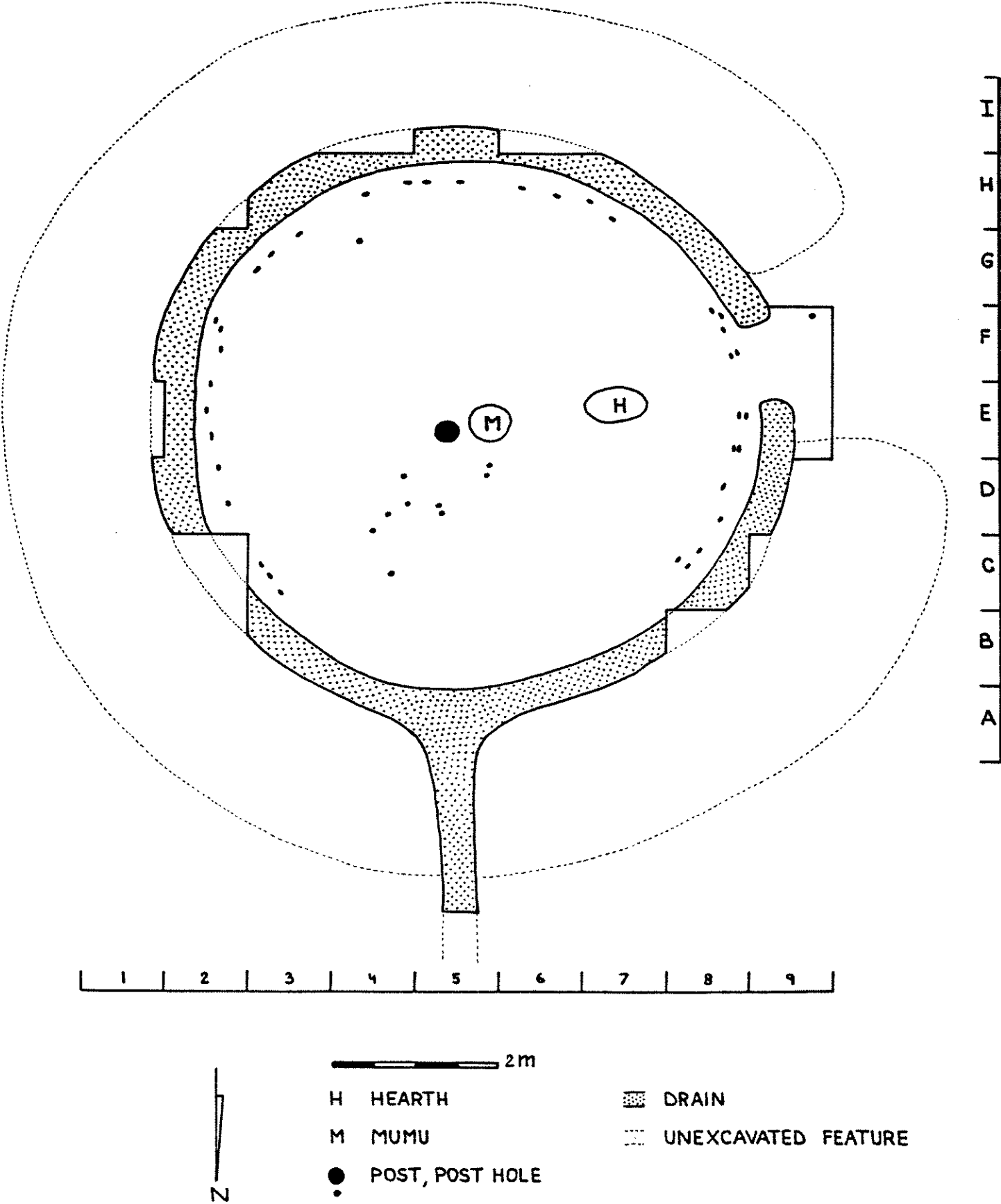


Fig. 22. Site MOG/R

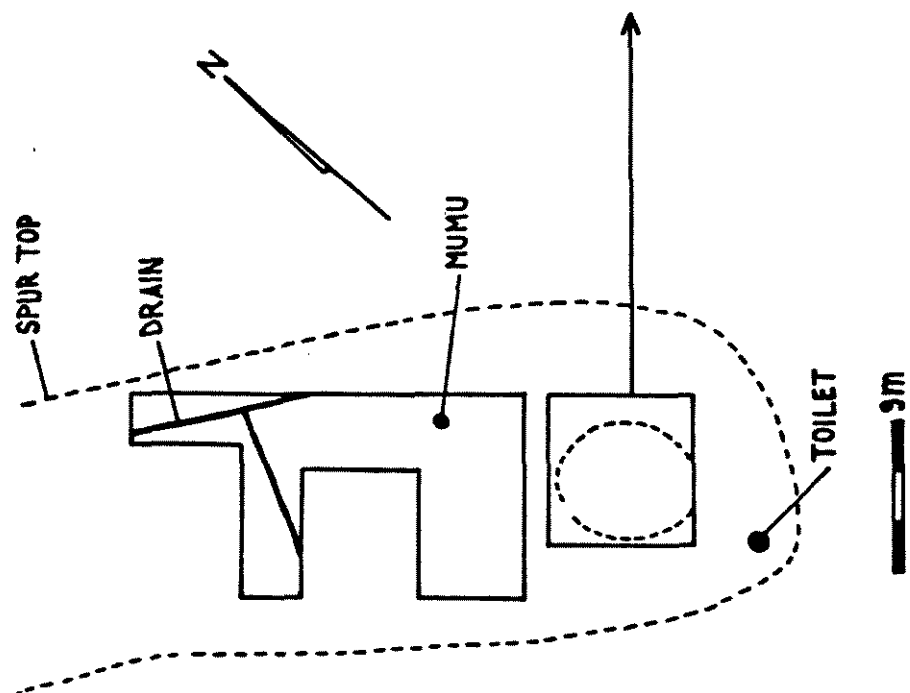
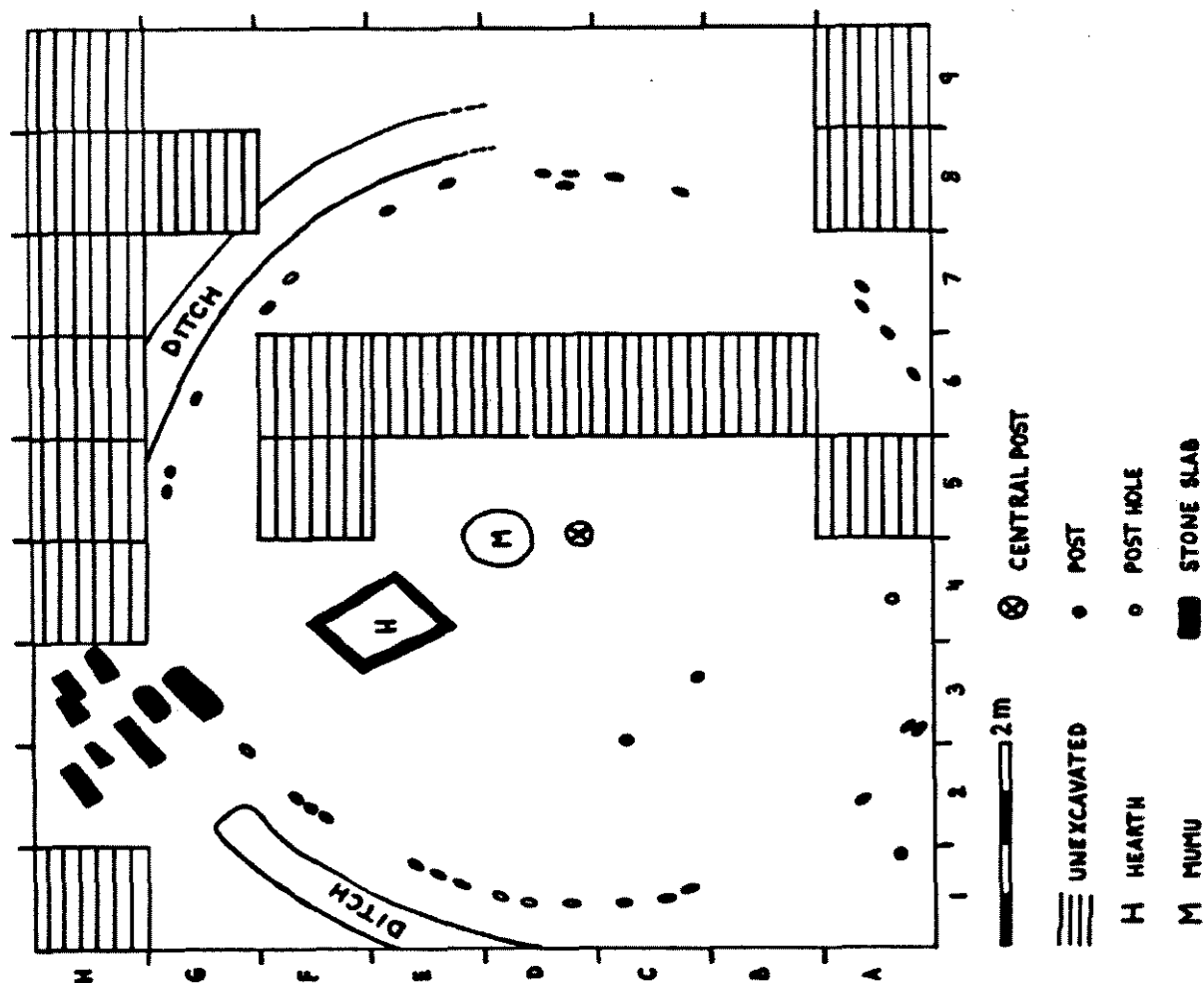
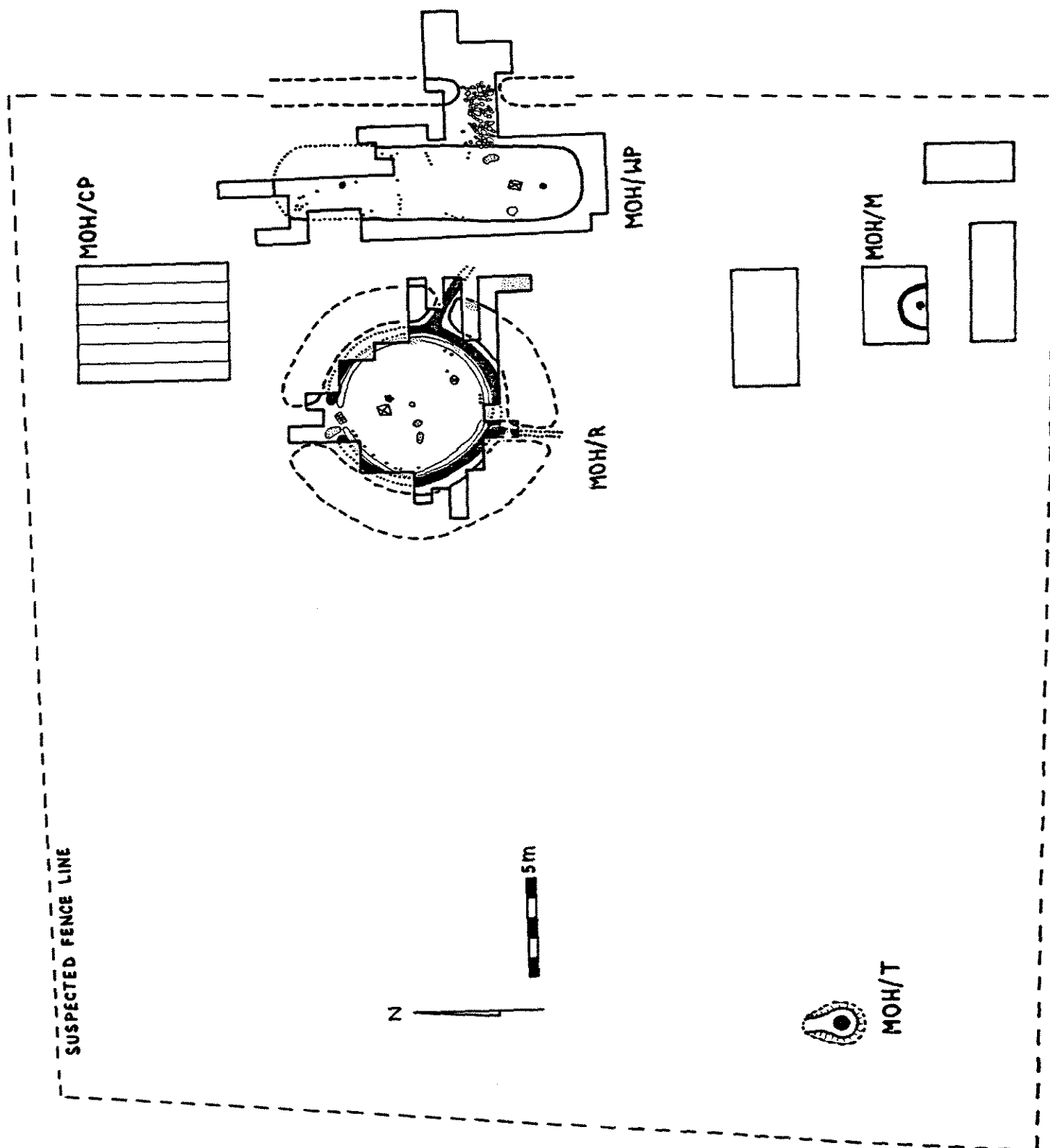


Fig. 23 . Site MOH



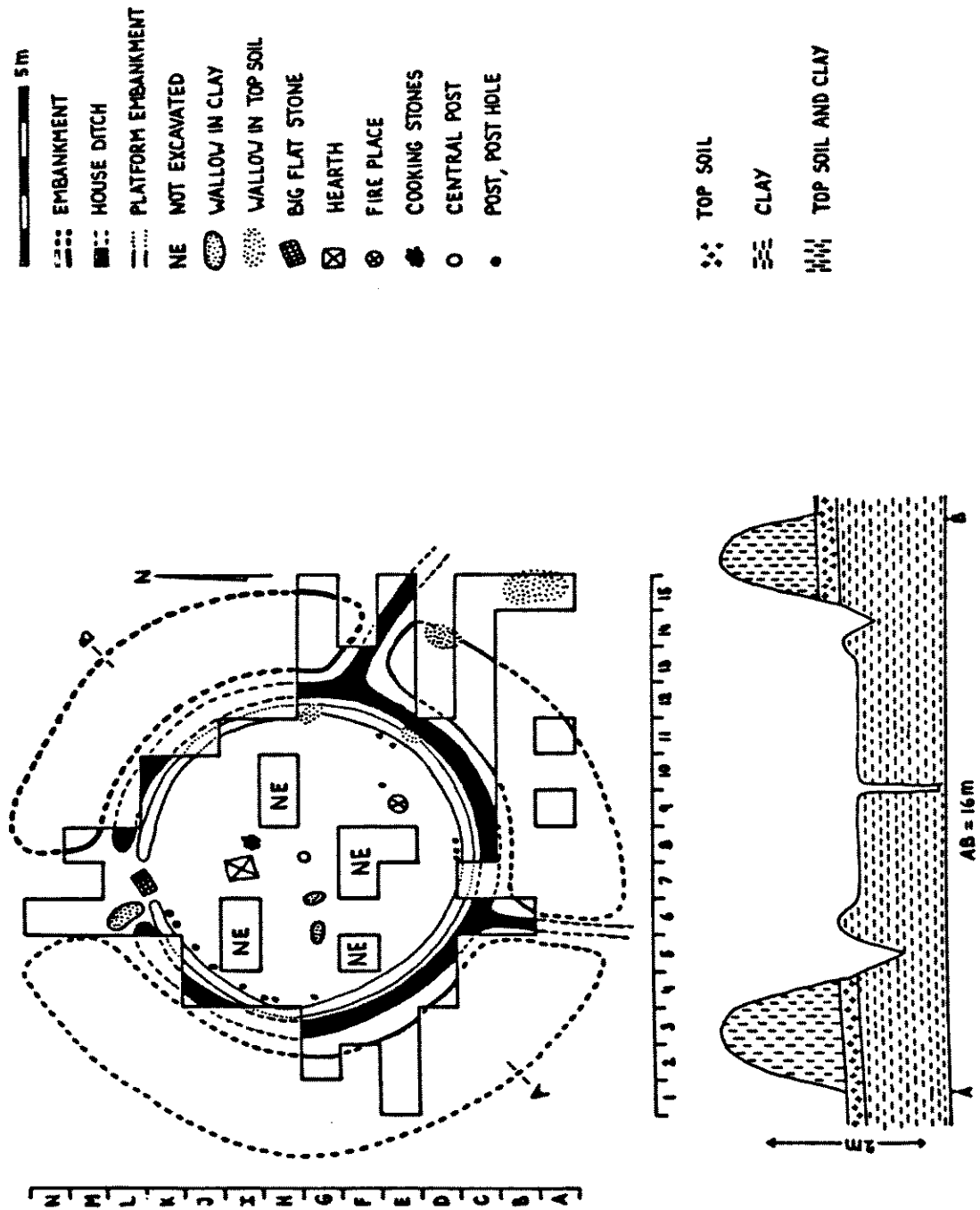
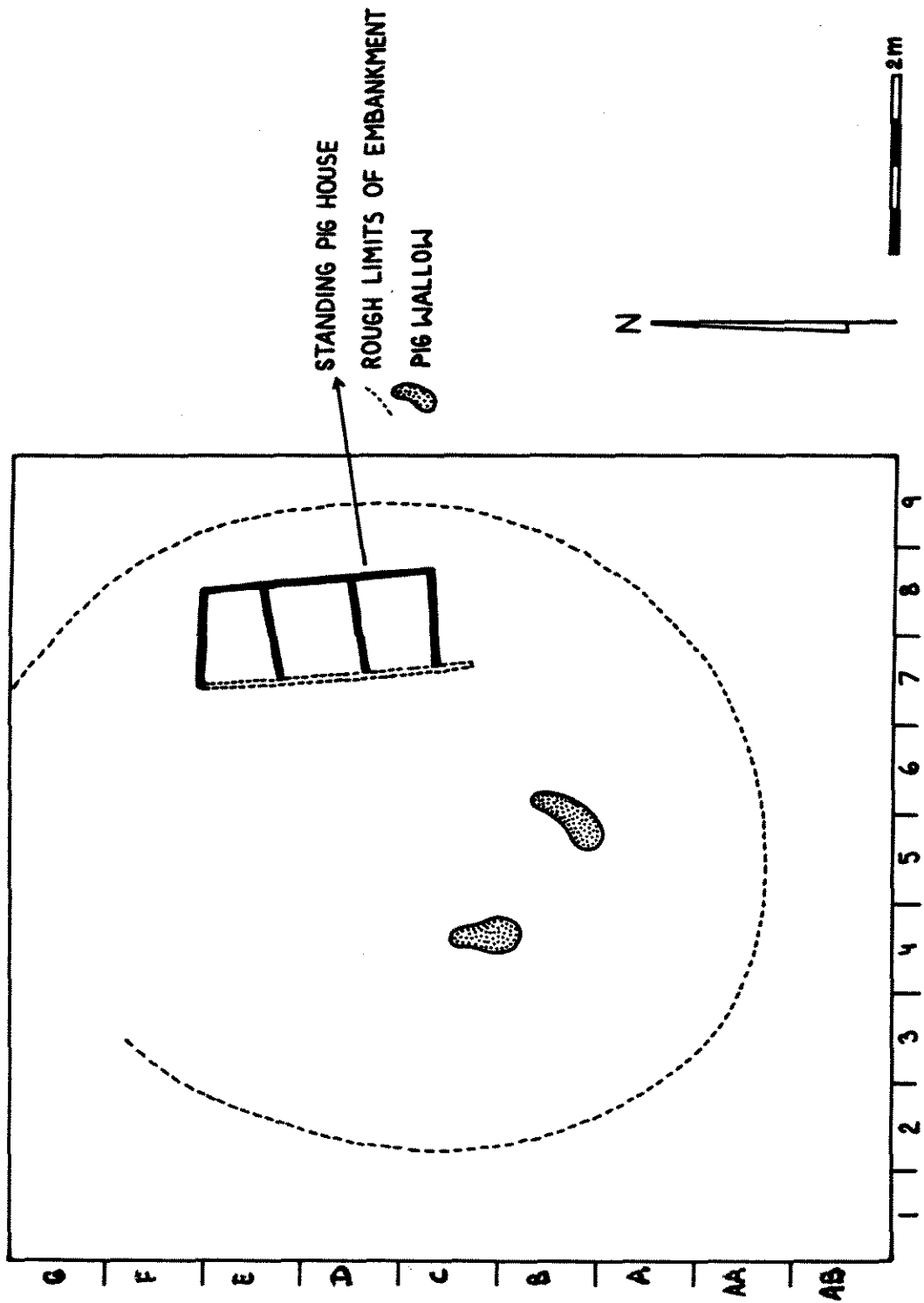


Fig. 24. Site MOH/R

Fig.25. Site MOQ



BASAL YELLOW CLAY

BROWN CLAY-SOIL LAYER

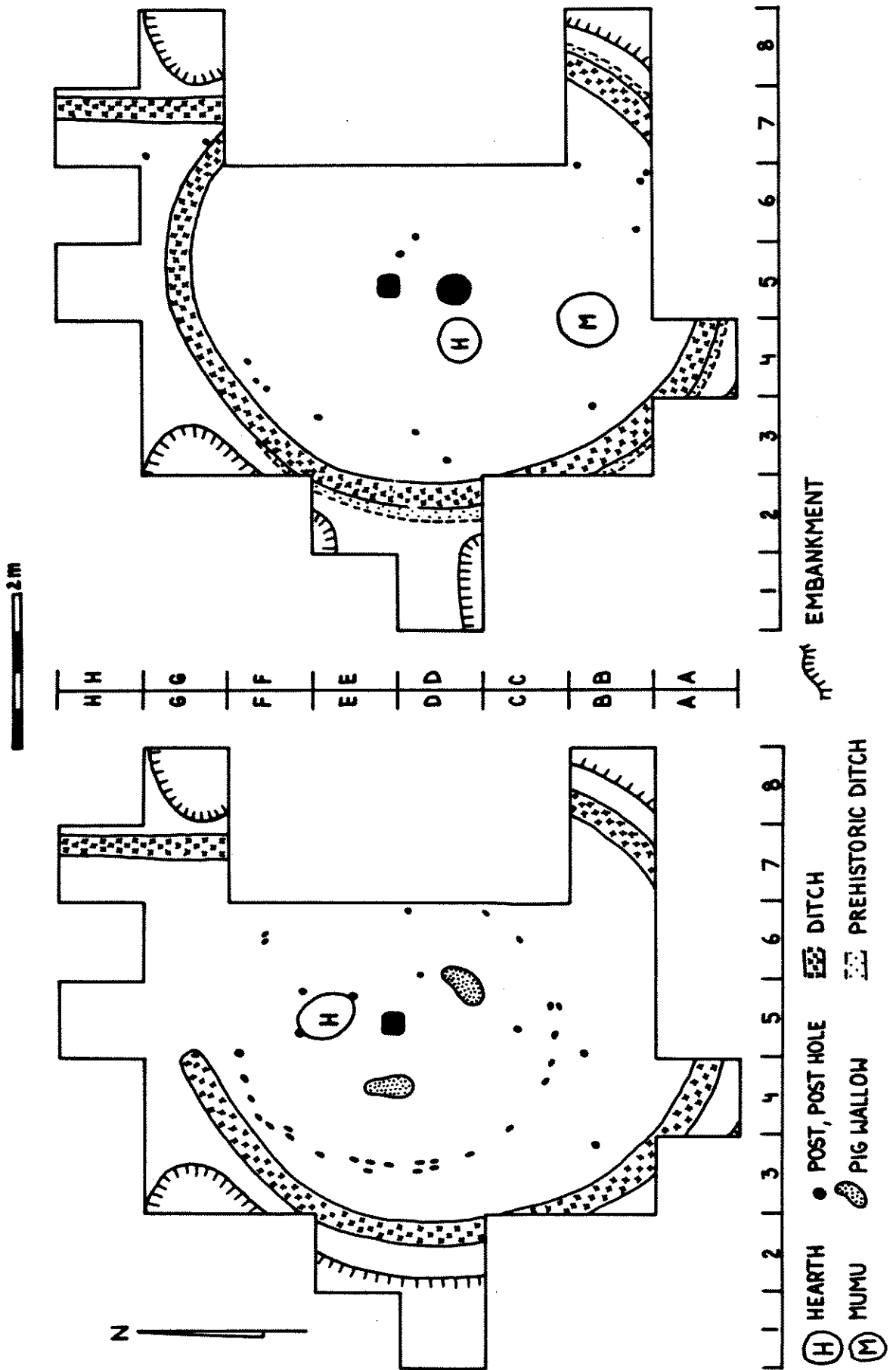


Fig.27. Site MOL

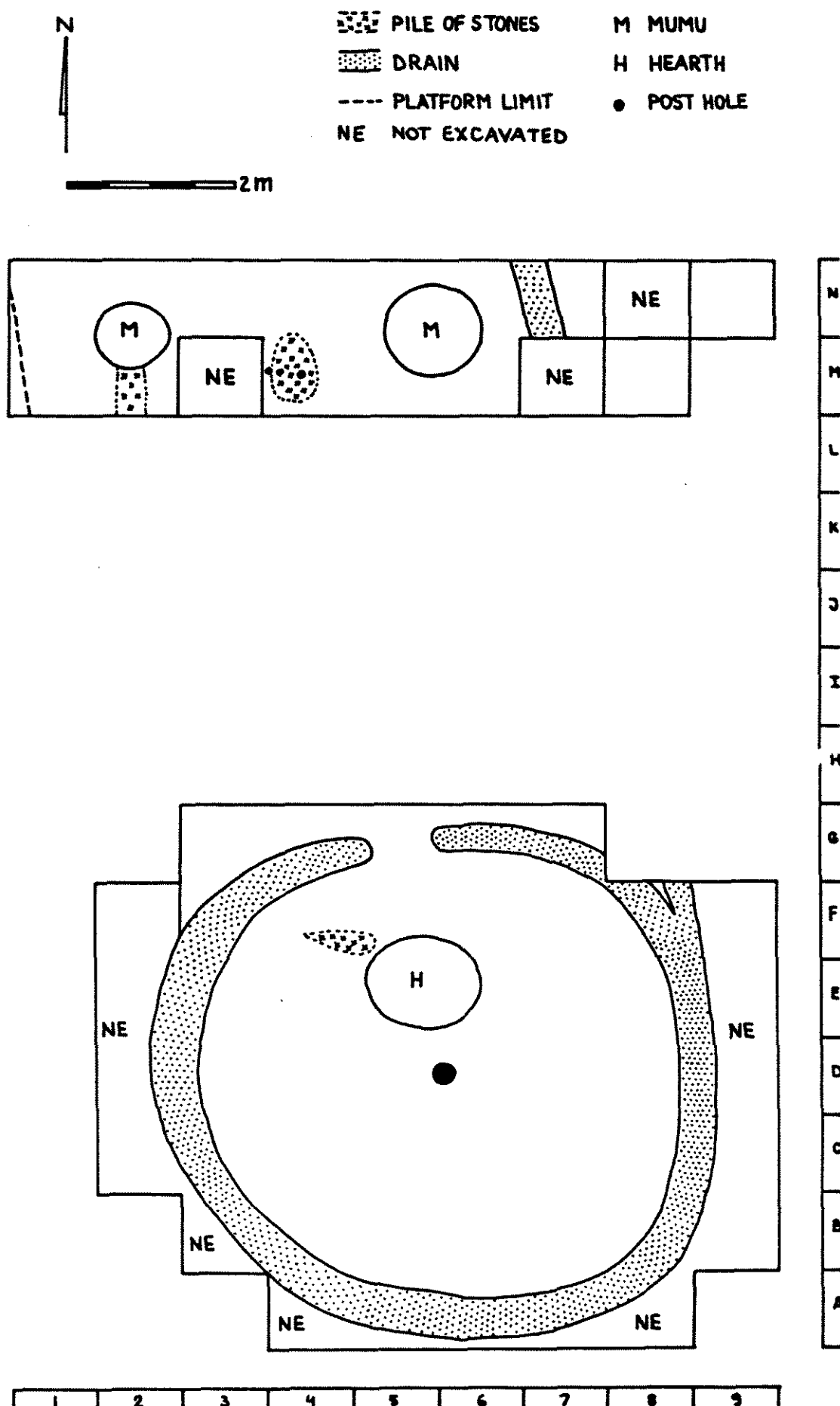


Fig. 28 . Site MOL

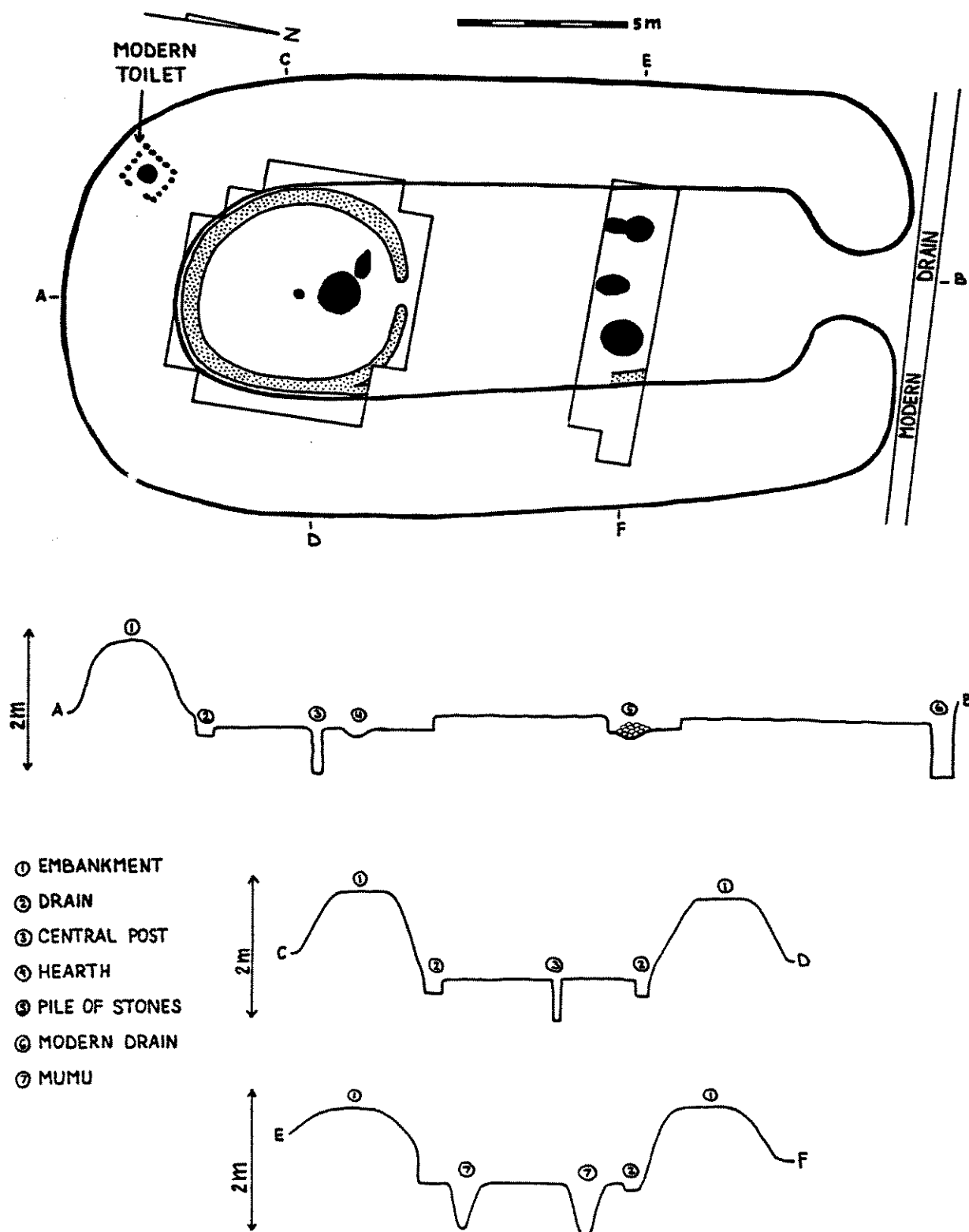


TABLE 14 - Site MOP

		HOUSE PLATFORM	DRAIN	CENTRAL POST	MUMU	HEARTH	TOTAL
COOKING STONE		81	5	4	25		115
UNWORKED QUARTZ		9	2				11
KONGA		1					1
SHELL (RAEM)		3					3
BROKEN KINA SHELL		11					11
STONE FILE		1					1
PIG BONE		34			2	64	100
PIG TOOTH		14	1		1		16
CHIKEN BONE		1					1
PUMPKIN SEED					1	1	2
TRADE STORE BEAD	RED	294	29			14	337
	WHITE	124	10				134
	BLUE	148	17		3	4	172
	YELLOW	5					5
	PINK	36			5		41
	GREEN	4					4
BEER BOTTLE		5					5
GLASS		61			3	4	68
NAIL		23				6	29
PLASTIC		16	4		1		21
OTHER MODERN ITEMS		23	2		1	2	28
TOTAL		894	70	4	42	95	1105

Table 15. Site MOG/R

OUTSIDE HOUSE	TOTAL	86
	NAIL	3
	GLASS	
	TRADE STORE BEAD	
	YELLOW	
	BLUE	
	RED	13
	TREE RESIN (GLUE)	
	QUARTZ	1
	QUARTZ CRYSTAL	1
	CHERT SCRAPER	
	KINA SHELL	
	PIG TOOTH	1
	PIG BONE	67
	COOKING STONE RIVER PEBBLE	
	COOKING STONE EP RIDGE	
INSIDE HOUSE		2
HEARTH		3
MUMU		17
TOTAL		49
		22
		199
		2
		1
		1
		1
		1
		1
		2
		38
		5
		2
		6
		4
		335
		57
		132
		60

Table 16. Site MOH/R

	FIG						KAPUL		HUMAN PREMOLAR	GOURD	COCONUT	SEA SHELL	RESIN	"KONGA"	BEAD					NAIL	WIRE	STEEL FILE	PLASTIC	GLASS	PAPER	TOTAL	
	BONE	JAW	TOOTH	NAIL	TUSK	TOOTH	BONE	TOOTH																			
COOKING STONE	48	1	20	1	1	1	14	1					2			193	27	25	4	4	13	2	1	2	8	1	353
STONE AXE FLAKE/CHIP	4												2	1	1	24	1	3			2				1		1293
OUTSIDE PLATFORM													5			18	1	1									29
BARET														1							2						3
TOTAL	48	1	20	1	1	1	14	1				2	8	2		235	29	29	4	4	15	4	1	2	9	1	1678

Table 17 - Site MOQ

	Black Top Soil	Brown Clay/Soil	Yellow Clay	Mumu B5	Hearth E5	Ditch	Central Post D5	Total
Cooking Stone	28	30	100	54	15	11	5	243
Pig Bone	3		10		56		1	70
Pig Tooth					1			1
Sweet Potato	1							1
Stone Axe Blade			2					2
Quartz					1			1
Red Bead					1			1
Blue Bead			1		2			3
Money Coin		1			1			2
Bottle		9	3	3	4			19
Glass	24		67	6	8			105
Tin		4			5			9
Nail	1	2	4		3			10
Other	6	1	3		2			12
Total	63	47	190	63	99	11	6	479

Table 18. Site MOL

	TOP SOIL		BASAL CLAY		I N F F A T U R E									
	HOUSE PLATFORM	COOKING PLACE	HOUSE PLATFORM	COOKING PLACE	HOUSE DITCH	CENTRAL POST	HEARTH	F4	N2	M2	M4	MN6	N7	TOTAL
COOKING STONE	39	169	166	350	170	51	37	28	51	212	48	99	2	1422
STONE AXE CHIP/FLAKE			4	7	2									13
CHERT SCRAPER			1	1										2
QUARTZ CORE					1									1
WASTE CHERT			1											1
EP STONE			11										14	25
PIG BONE				2	11		25			2				40
PIG TOOTH	2				1									3
PIG NAIL	1													1
RESIN					1									1
BEER BOTTLE	4													4
BEER BOTTLE CAP	1													1
GLASS	50													50
PLASTIC	1		1											2
SINGSING PAINTING POT	1													1
TOTAL	99	169	184	360	186	51	62	28	51	214	48	99	16	1567

Fig. 29. Site M00

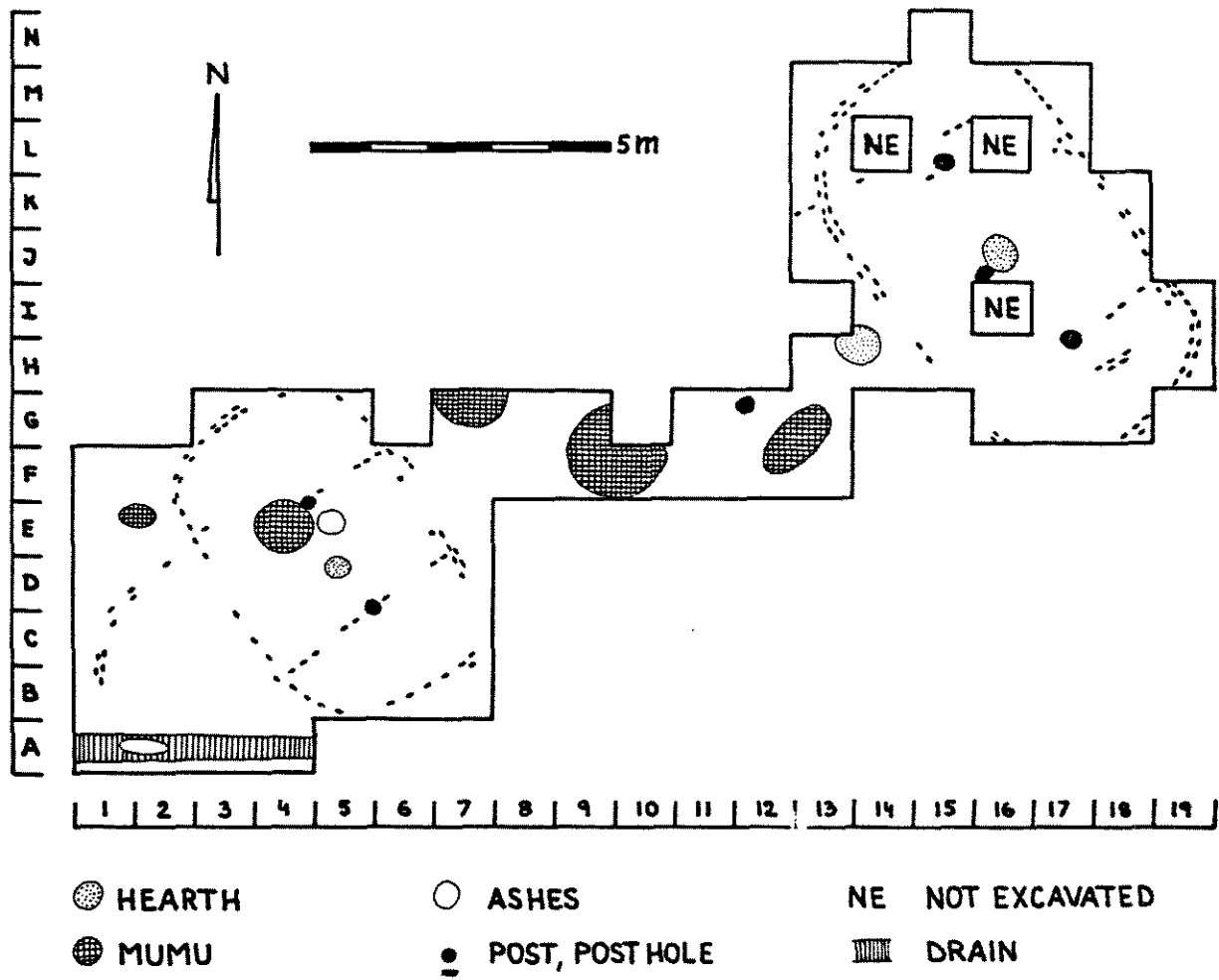


Fig.30 . Site M00/Phase 1

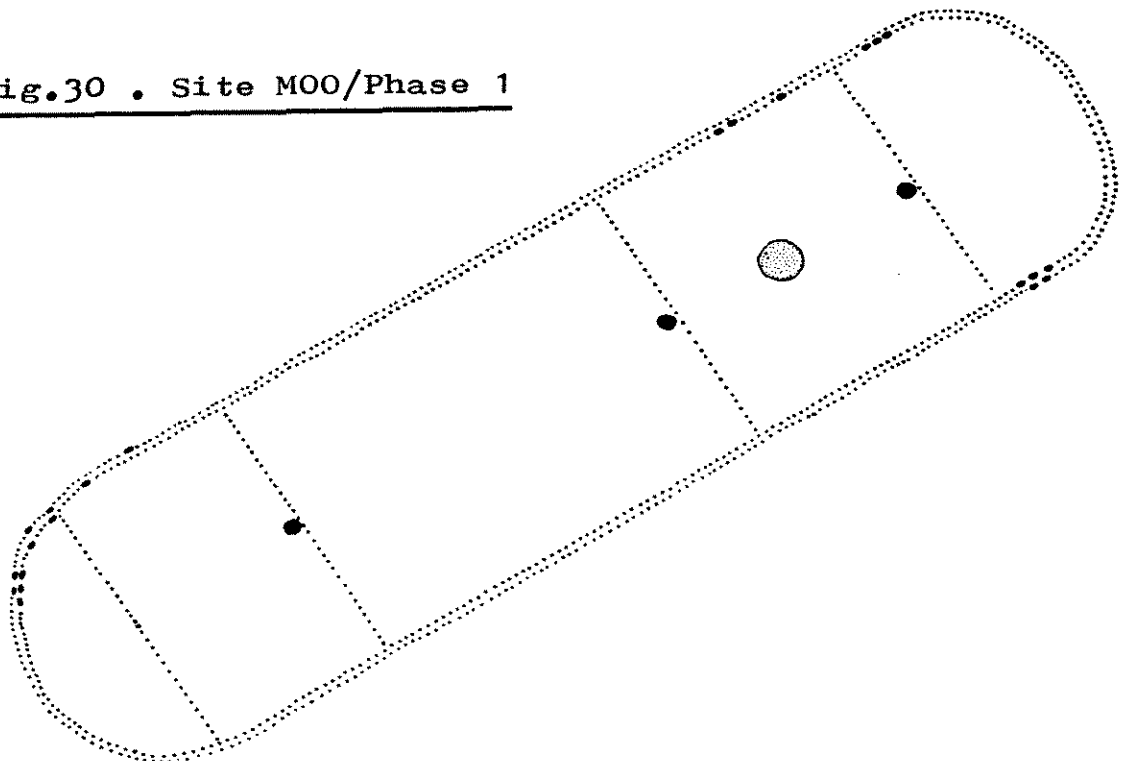


Fig. 31 . Site M00/Phase 2

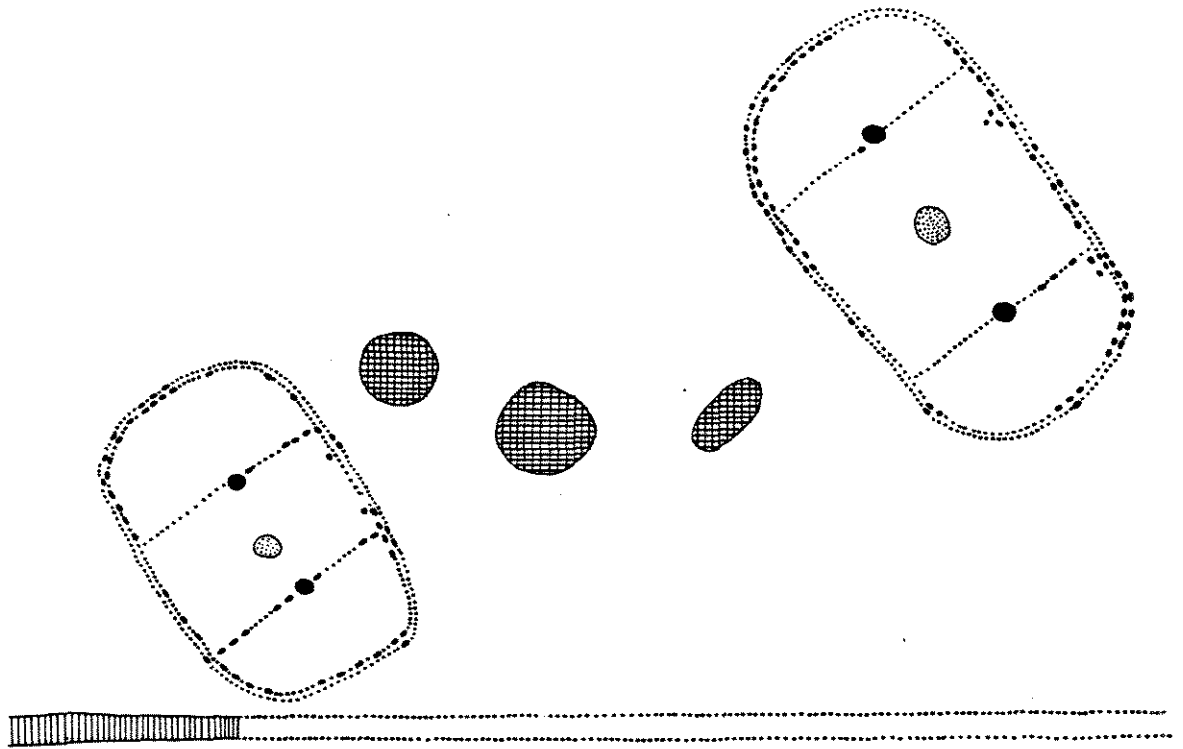


Fig. 32 . Site M00/Phase 3

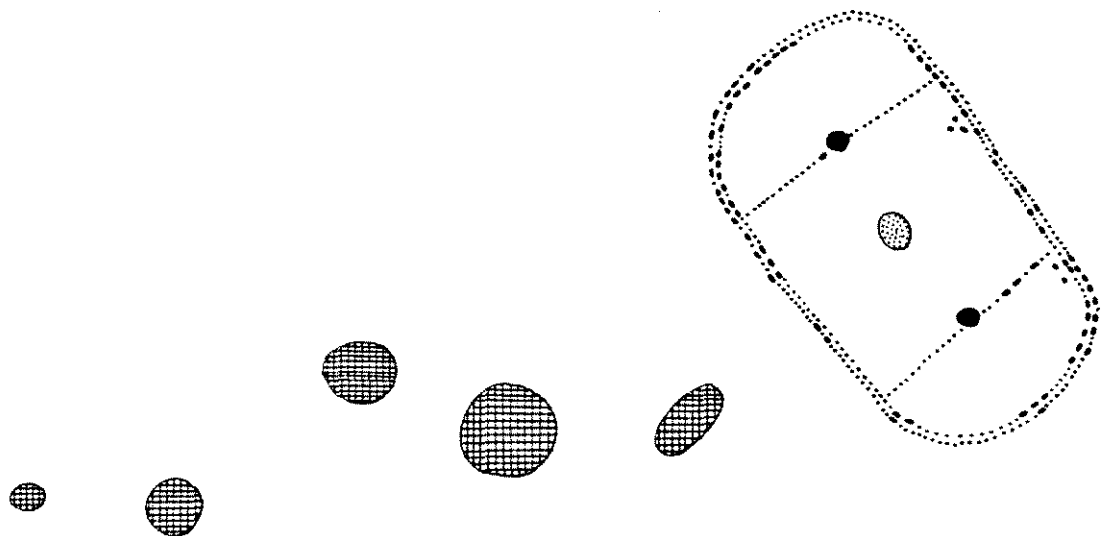


Table 19. Site M00

	WESTERN HOUSE	EASTERN HOUSE	MUMU E1	MUMU E4	MUMU G7	MUMU F9	MUMU F12	HEARTH D5	HEARTH H13	HEARTH J16	TOTAL
cooking stone	424	25	2	12	22	120	150	20		2	926
stone axe blade	1										1
digging stick	1	1									2
pig bone	5	18			1			5	62	27	120
pig tooth	2	11				1				4	18
pig tusk	1	1									2
Kapul bone										1	1
cassowary bone		1									1
chicken bone		2								1	3
sugar cane		1									1
karuka nut		1									1
sweet potato						1				1	2
pumpkin seed	2	4								1	7
peanut	2	8									10
corn	1	1									2
gourd container		1									1
kina shell piece										1	1
konga		1									1
native bead	2										2
money coin	3	8									11
TRADE STONE BEADS	red	22	649					1	14	22	709
	blue	122	120						23	13	278
	yellow	17	33							4	54
	green		22							2	24
	black		2								2
	white	2	15							8	25
nail	14	23					1			3	38
glass	252	302	3	1	86	65	30	5	2	87	897
glass scraper	2	6									8
bottle cap	27	16		1	1	1	1	1		2	61
tin	19	4			1	4	10			1	38
rubber	14	12		1				1		2	29
plastic	2	26		1		1		1		1	32
other modern items	12	24		1		1	2			2	42
TOTAL	928	1721	3	17	113	264	203	14	101	124	2670

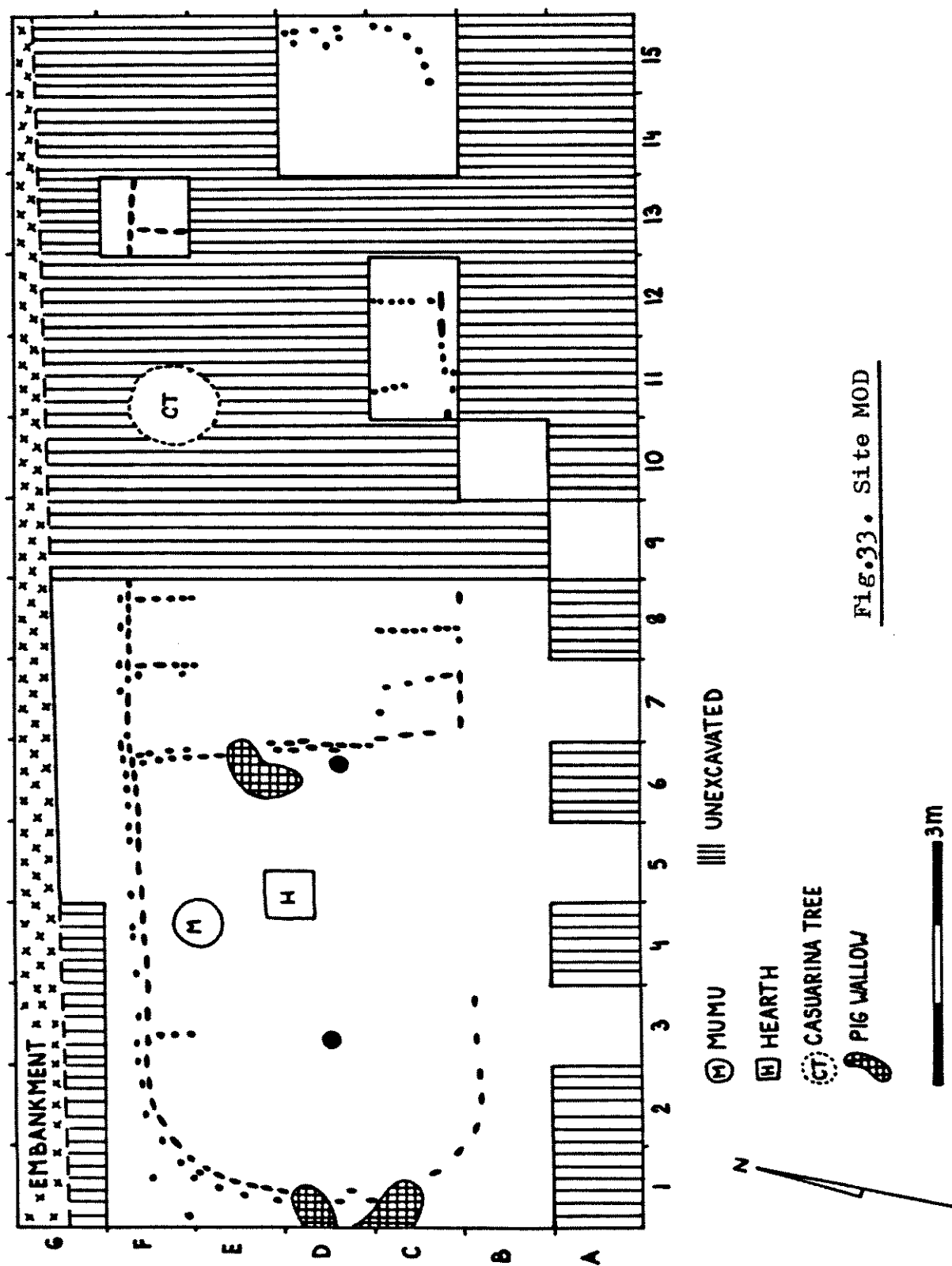


Fig.33. Site MOD

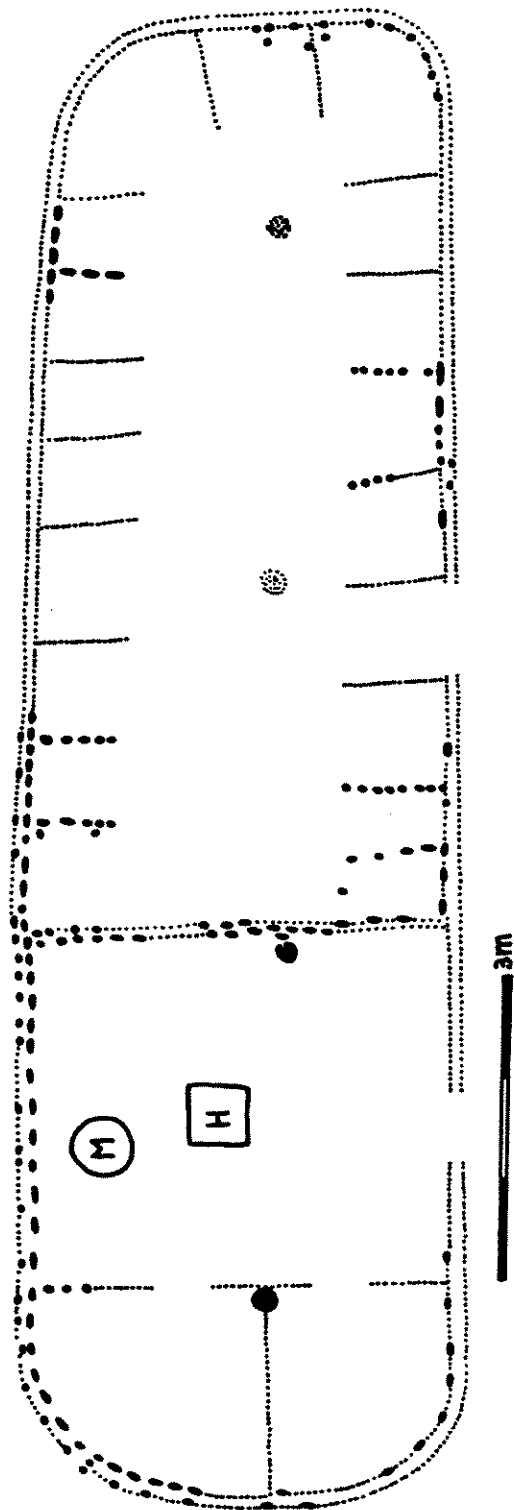


Fig.34. Site MOD - Reconstruction

Fig.35. Site MOE

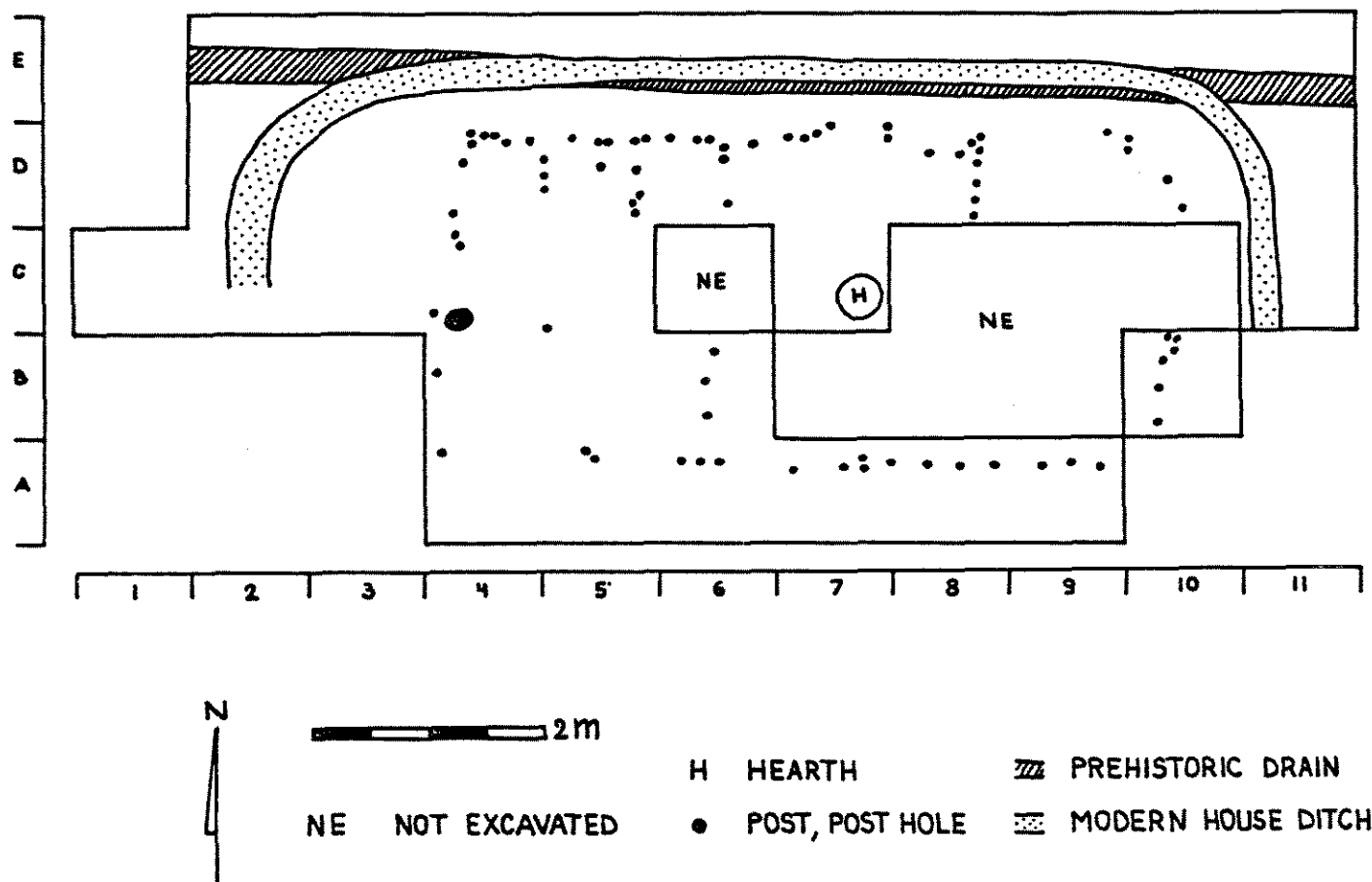


Fig.36. Site MOE - Reconstruction

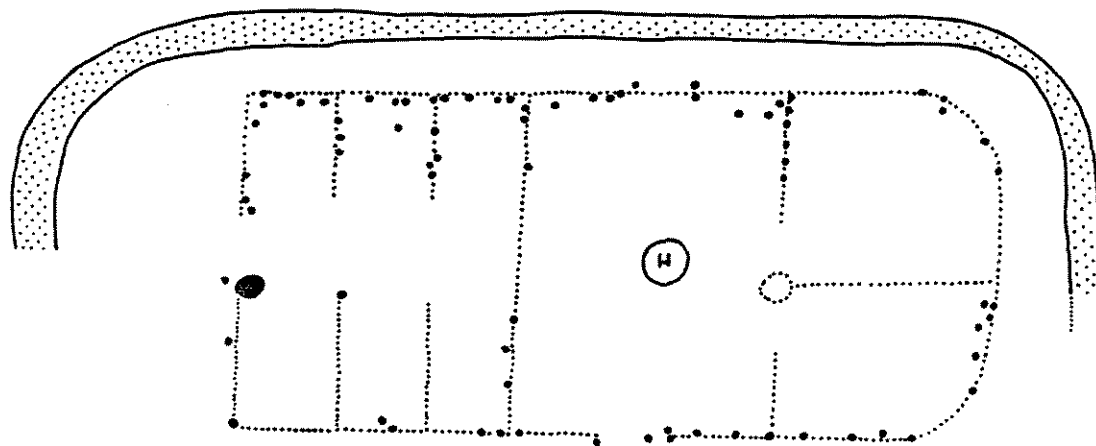
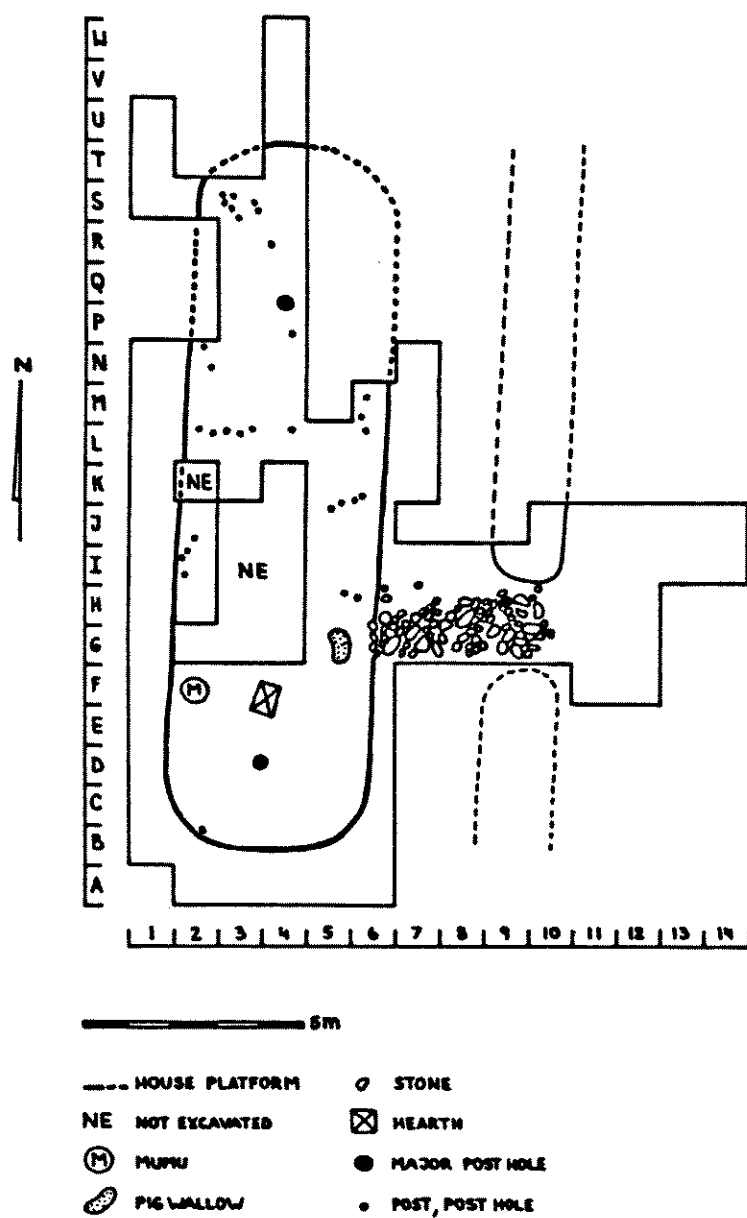


Fig.37. Site MOH/WP



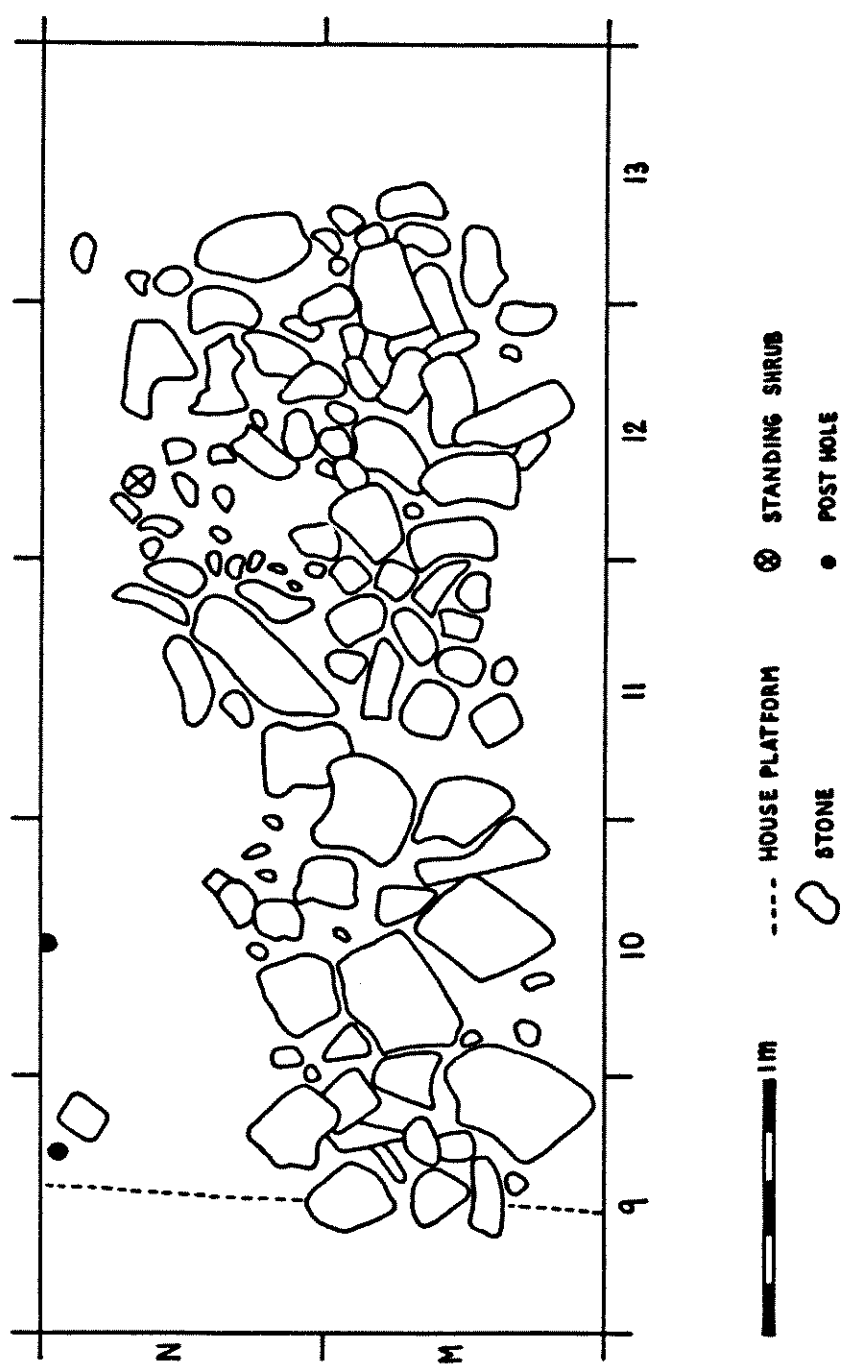


Fig.38. Stone Pathway

	COOKING STONE	PIG BONE	PIG TOOTH	CHARRED SWEET POTATO	STONE AXE CHIP	TRADE STORE BEAD					CLOTH	GLASS	TOTAL
						RED	WHITE	BLUE	YELLOW	GREEN			
NORTHERN SECTION	1	4				9	6	1	1	1			23
CENTRAL SECTION	1		1	1	1	7		1			1		13
SOUTHERN SECTION		1				53	1	4				1	60
HEARTH		82				5						1	88
MUMU	257					1						1	259
TOTAL	259	87	1	1	1	75	7	6	1	1	1	3	443

TABLE 21 - SITE MOH/WP

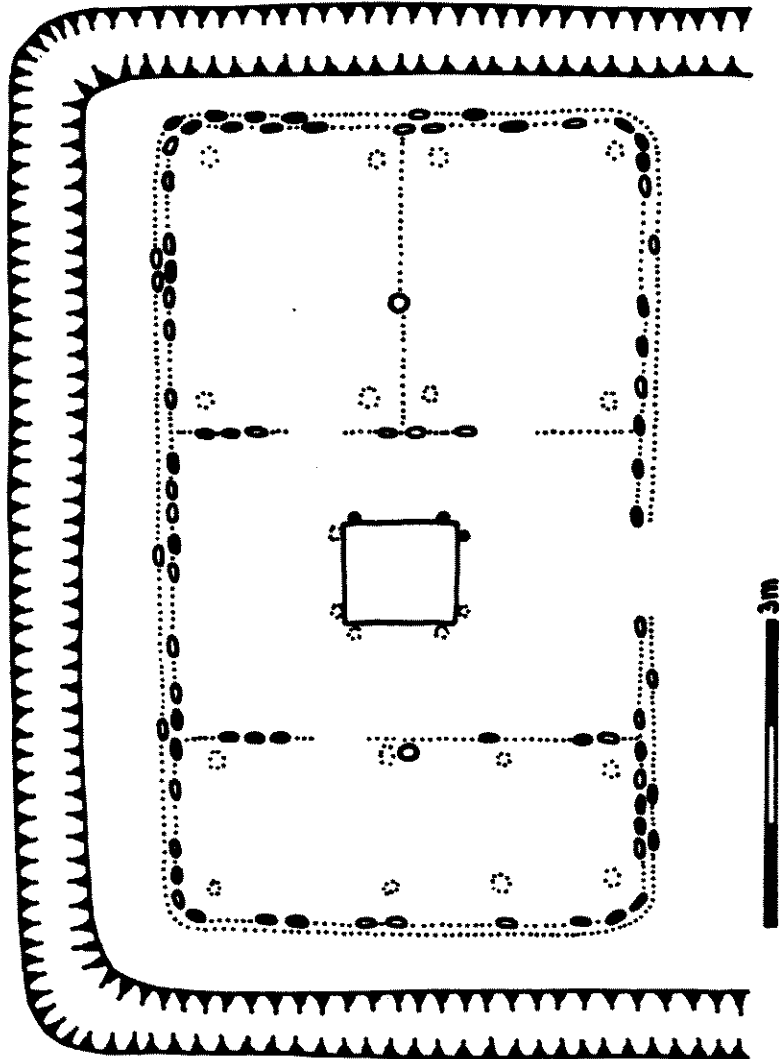


Fig 40- SITE MOF (RECONSTRUCTION)

Table 22. Site MOF

	COOKING STONE	PIG BONE	PIG TOOTH	POSSUM BONE	BIRD'S BEAK	OTHER BONE	S E E D						F O O D									BUSH ROPE	STORE ROPE	CIGARETTE BUTT	PAPER	MATCH	GLASS	BEER CAP	LAPLAP	STORE BEAD	NAIL	OTHER	TOTAL																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																												
							PUMPKIN	BEAN	TOMATO	MARATA	CUCUMBER	UNKNOWN	KARUKA NUT	BETEL NUT	SWEET POTATO	CORN	PEANUT	SUGAR CANE	PINEAPPLE	EGG SHELL	INSECT NEST																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																								</

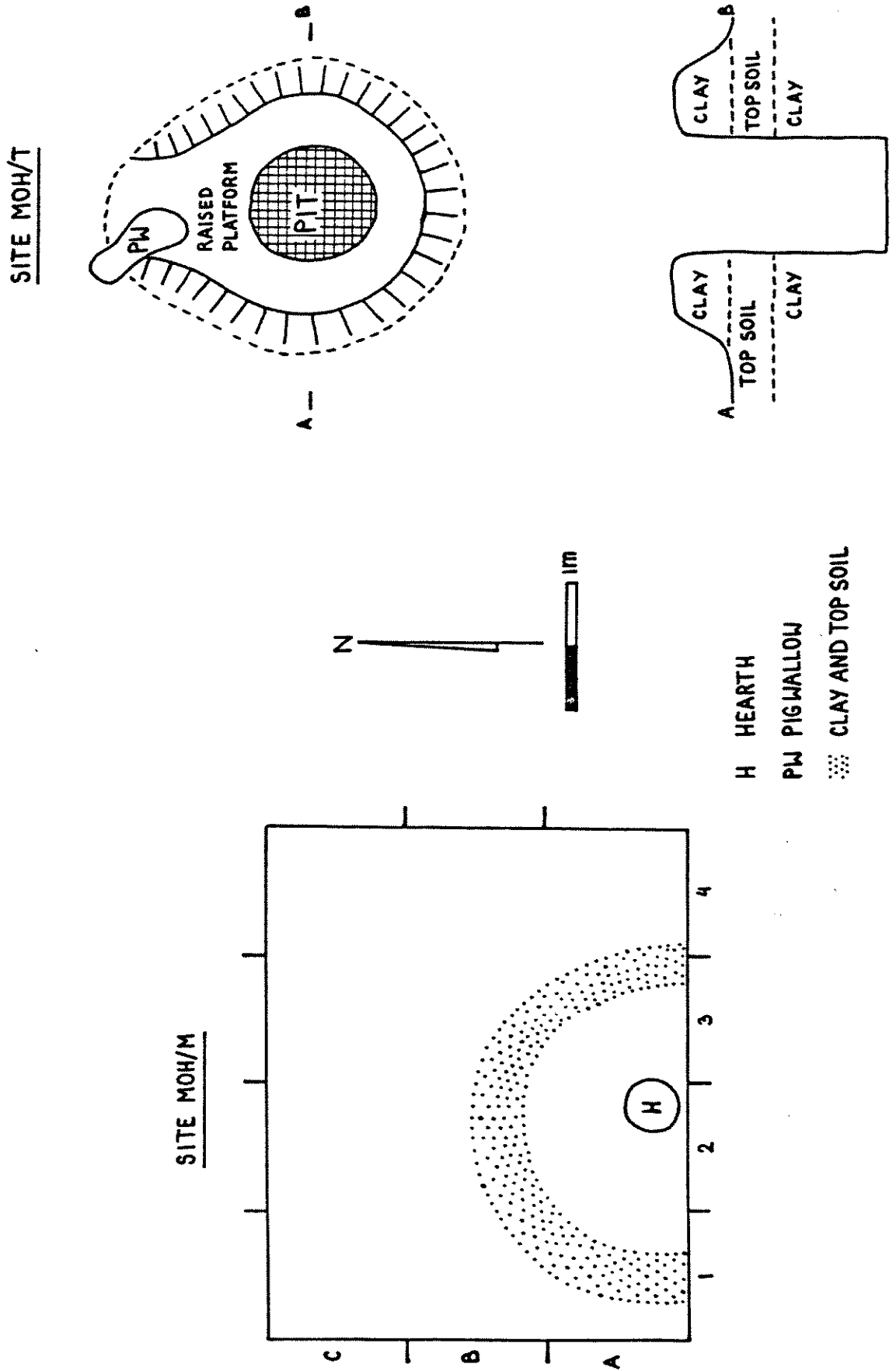


Fig.41. Sites MOH/M and MOH/T

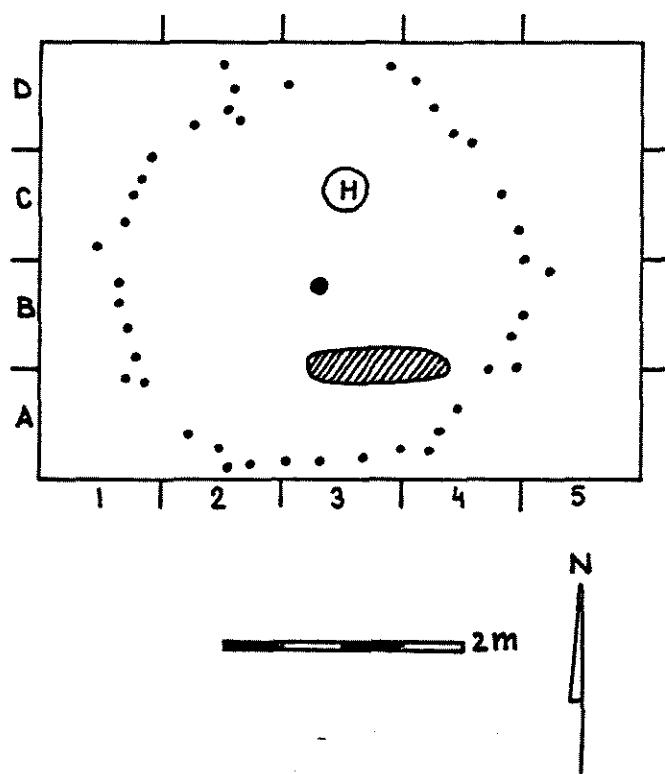


Fig.42. Site MOM

- NE NOT EXCAVATED
- H HEARTH
- M MUMU
- CT CASUARINA TREE
- B BANANA TREE
- POST, POST HOLE
- /// DEPRESSION
- ▨ DRAIN

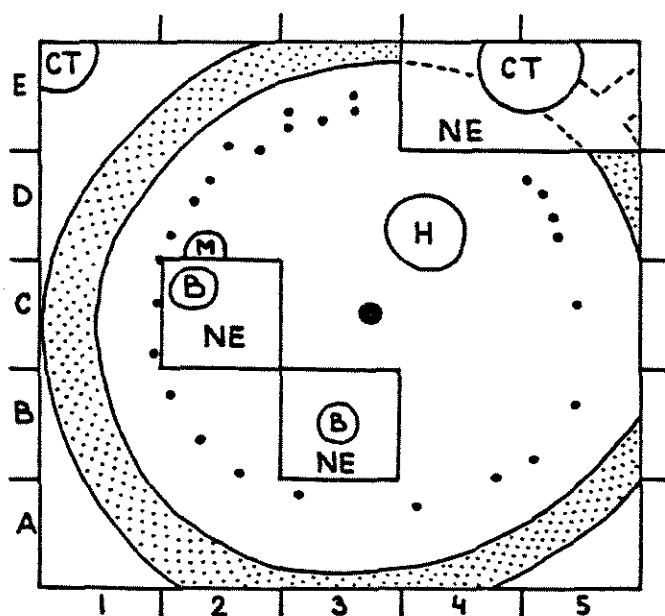


Fig.43. Site MON

SECTION OF HEARTH FROM MON :

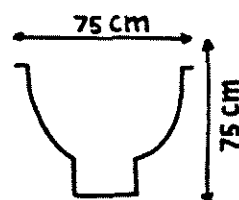
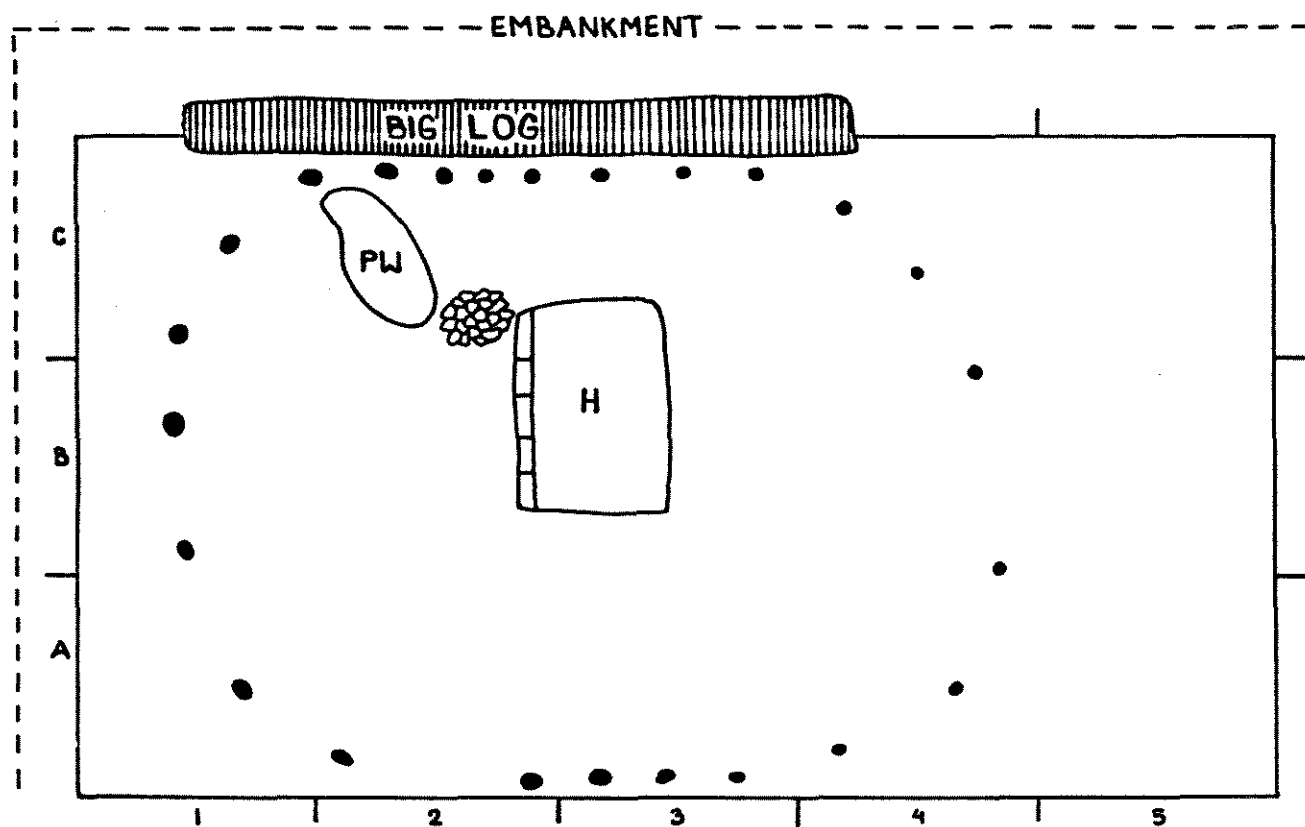
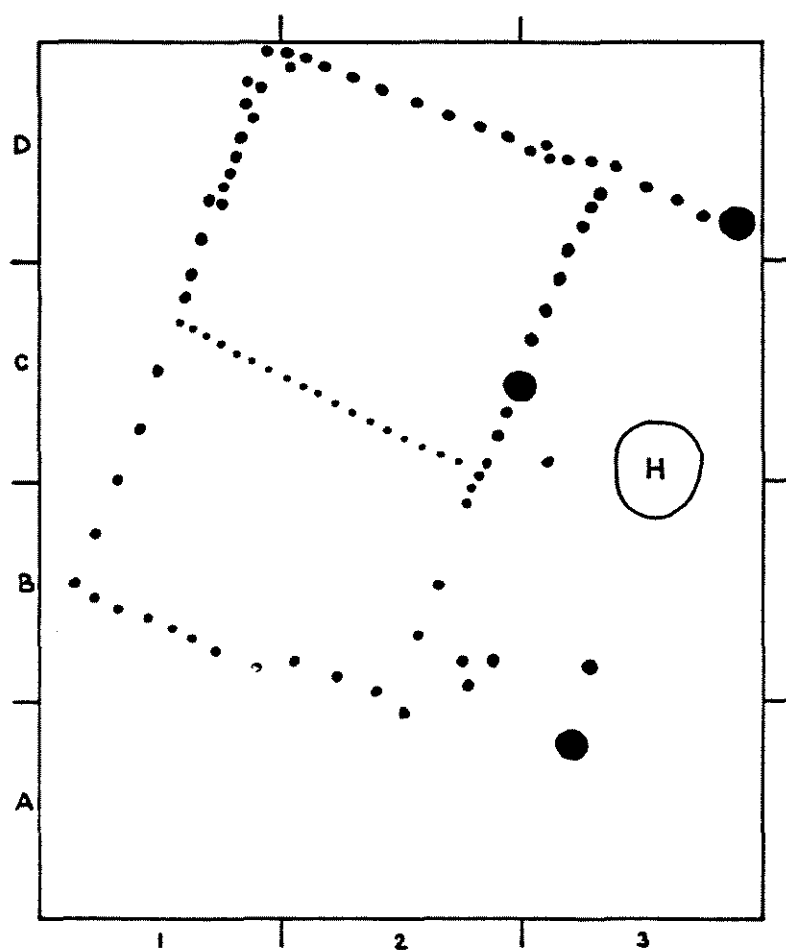


Table 23 - Site MOM and Site MON

	INSIDE STRUCTURE MOM	INSIDE STRUCTURE MON	HEARTH OF MON	TOTAL
COOKING STONE	34	15	4	53
KONGA		1		1
PIG BONE	1			1
SWEET POTATO			1	1
SUGAR CANE		1		1
GLASS		9	1	10
GLASS SCRAPER		1		1
WHITE BEAD	1			1
BLUE BEAD	3	1	1	5
RED BEAD	9	1		10
NAIL	1			1
TIN		2		2
WIRE		1		1
BOTTLE CAP		3		3
PLASTIC	1	2		3
FOIL PAPER		3	1	4
CLOTH		1		1
BUTTON		2		2
TOTAL	50	43	8	101

Fig.44 . Site MOK

1m



H HEARTH

PW PIG WALLOW

● POST, POST HOLE

PILE OF STONES

Fig.45 . Site MNT

Table 24 - Site MOK

	SHELTER	HEARTH	TOTAL
COOKING STONE	14		14
PILE OF COOKING STONES	15		15
PIG BONE		2	2
KARUKA NUT	1		1
EGG SHELL	2		2
BUSH ROPE	2	1	3
MONEY COIN-1975	1		1
BEAD	1		1
NAIL		4	4
METAL WIRE	10	1	11
NYLON STRING	4	3	7
PLASTIC	23		23
RUBBER	2	3	5
LEATHER	2	3	5
GLASS	2		2
FOIL PAPER	42	3	45
BUTTON	1		1
BAND-AID	2		2
CLOTH	1	1	2
TOTAL	125	21	146

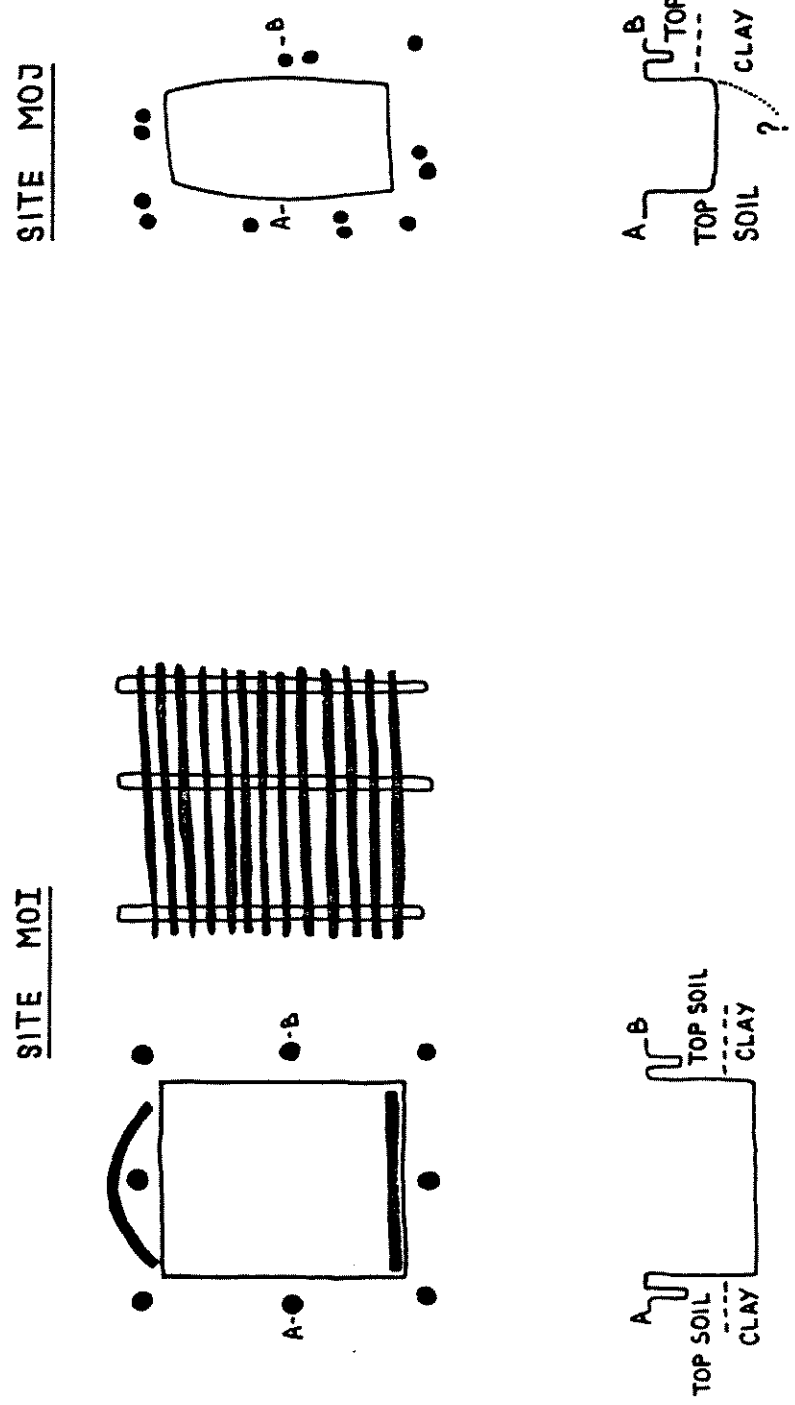


Fig.46. Sites MOI and MOJ (Cassowary Cages)

	COOKING STONE	CHARCOAL	PIG BONE	PIG TUSK	CASSOWARY FEATHER	CAGE POST	NATIVE BEAD	BLUE BEAD	GLASS	TIN	PLASTIC	TOTAL
HK1	3	2	1	1		3	1		14			25
HK2	1	2			1				1	1	1	7
MOI		2			2				14	4		22
MOJ	5					8		4	13	1		31
TOTAL	9	6	1	1	3	11	1	4	42	6	1	85

TABLE 25 - CASSOWARY CAGES

Table 26. House Site Formation Processes

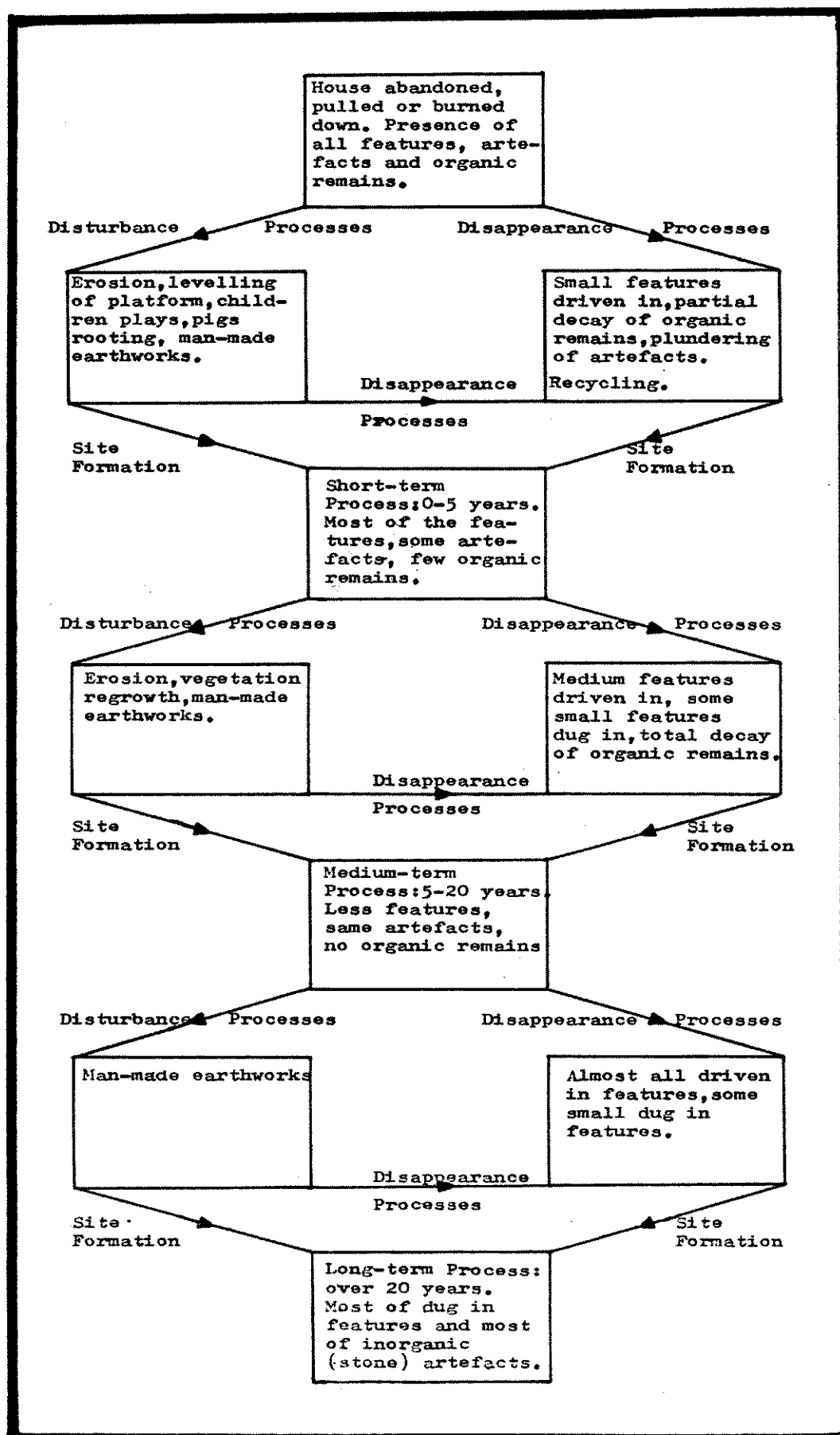




Plate 15. Hot stones transfered to oven (background) with tongs. Dryland, Kuk.



Plate 16. Small heated stones left unused. Dryland, Kenta.



Plate 17. First stones in oven. Dryland, Bagla.



Plate 18. Last stones in same oven as for Plate 17.

Table 27. Typology of Domestic Ovens

D O M E S T I C								
PLAN	CIRCULAR				OVAL			
DEPTH	DEEP		SHALLOW		DEEP		SHALLOW	
SECTION	Vertic	Slope	Vertic	Slope	Vertic	Slope	Vertic	Slope
CODE	DCDV	DCDS	DCSV	DCSS	DODV	DODS	DOSV	DOSS

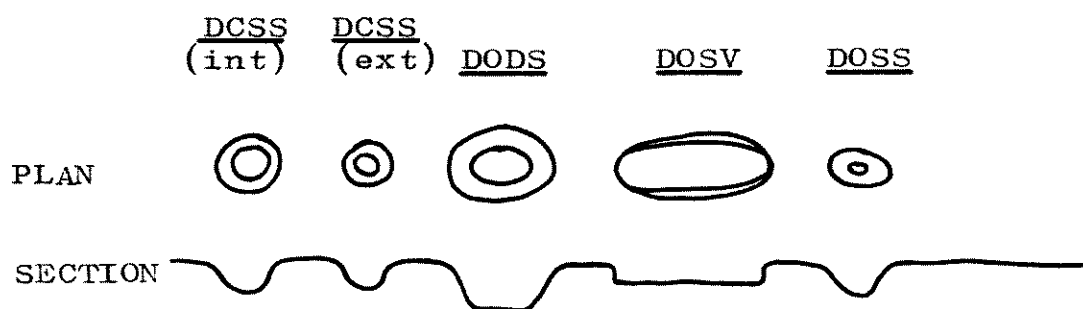
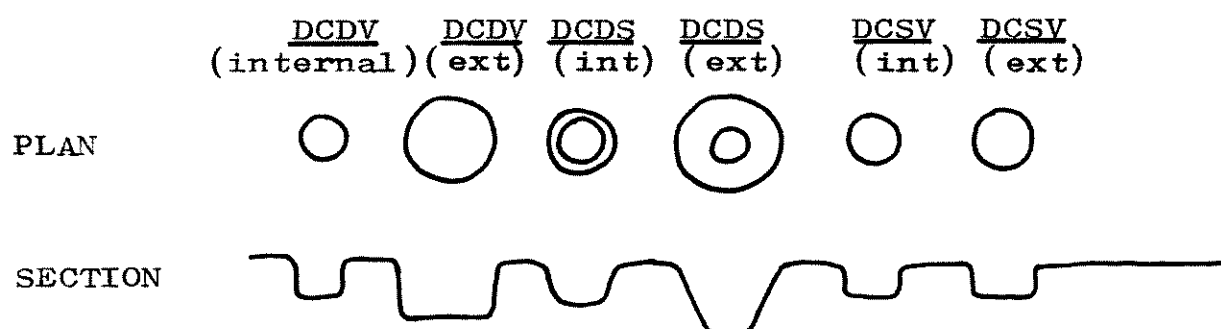
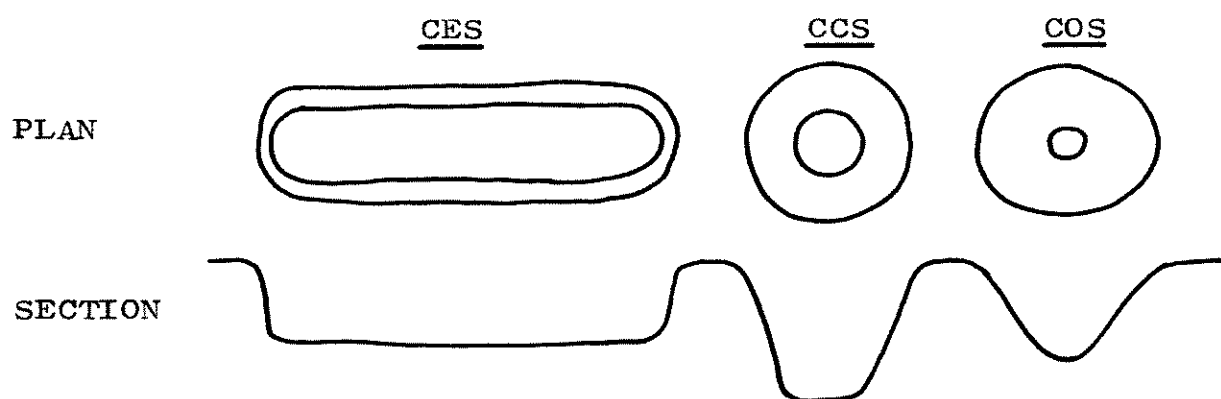
Table 28. Typology of Ceremonial Ovens

C E R E M O N I A L						
Plan	Elliptical		Circular		Oval	
Section	Vertic	Slope	Vertic	Slope	Vertic	Slope
Code	CEV	CES	CCV	CCS	COV	COS

Table 30. Measurements of Ovens Located Outside Houses

FUNCTION	DOMESTIC								CEREMONIAL					
	DCDV	DCDS	DCSV	DCSS	DODV	DODS	DOSV	DOSS	CEV	CES	CCV	CCS	COV	COS
NUMBER RECORDED	3	25	3	20	0	2	1	2	0	4	0	11	0	3
TOP	65-85	72-100	45-55	24-50		74-100 x 60-75	120 x 50	50 x 30		230-500 x 60-150		110-177		140-160 x 110-140
BOTTOM	65-85	22-40	45-55	15-31		43-50 x 38-40	120 x 45	23 x 14		200-480 x 20-90		28-70		20-30
DEPTH	45-60	47-75	25-30	16-33		40-44	17	30		65-78		97-137		70-105
TOP	75	84	50	37		87x67	120x50	50x30		330x100		137		150x130
BOTTOM	75	29	50	22		47x39	120x45	23x14		310x65		54		27
DEPTH	52	64	28	25		42	17	30		73		122		85

Fig. 47 . Plan and Section of Ovens

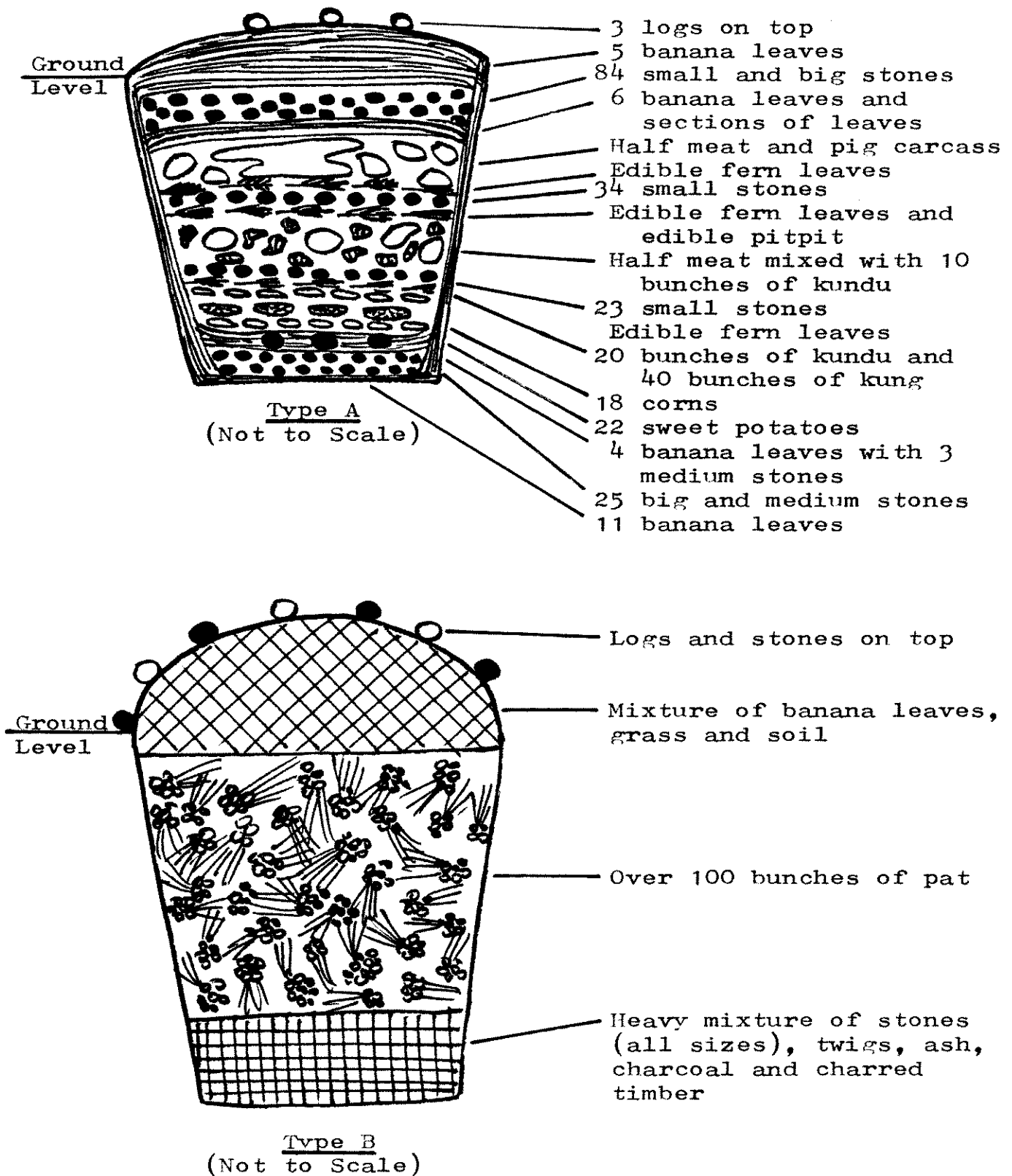


100cm

Table 31. Case Study on Cooking Stones.

Date	NUMBER OF COOKING STONES			
	Large	Medium	Small	Discarded
June		101		
July		37	90	
August	79	80	163	
September	44	127	109	288
October	85	60	86	448
November	54	42	70	926
November	78	70	110	1054

Fig.48 Two Types of Traditional Cooking



Map 25. Location of Cooking Sites

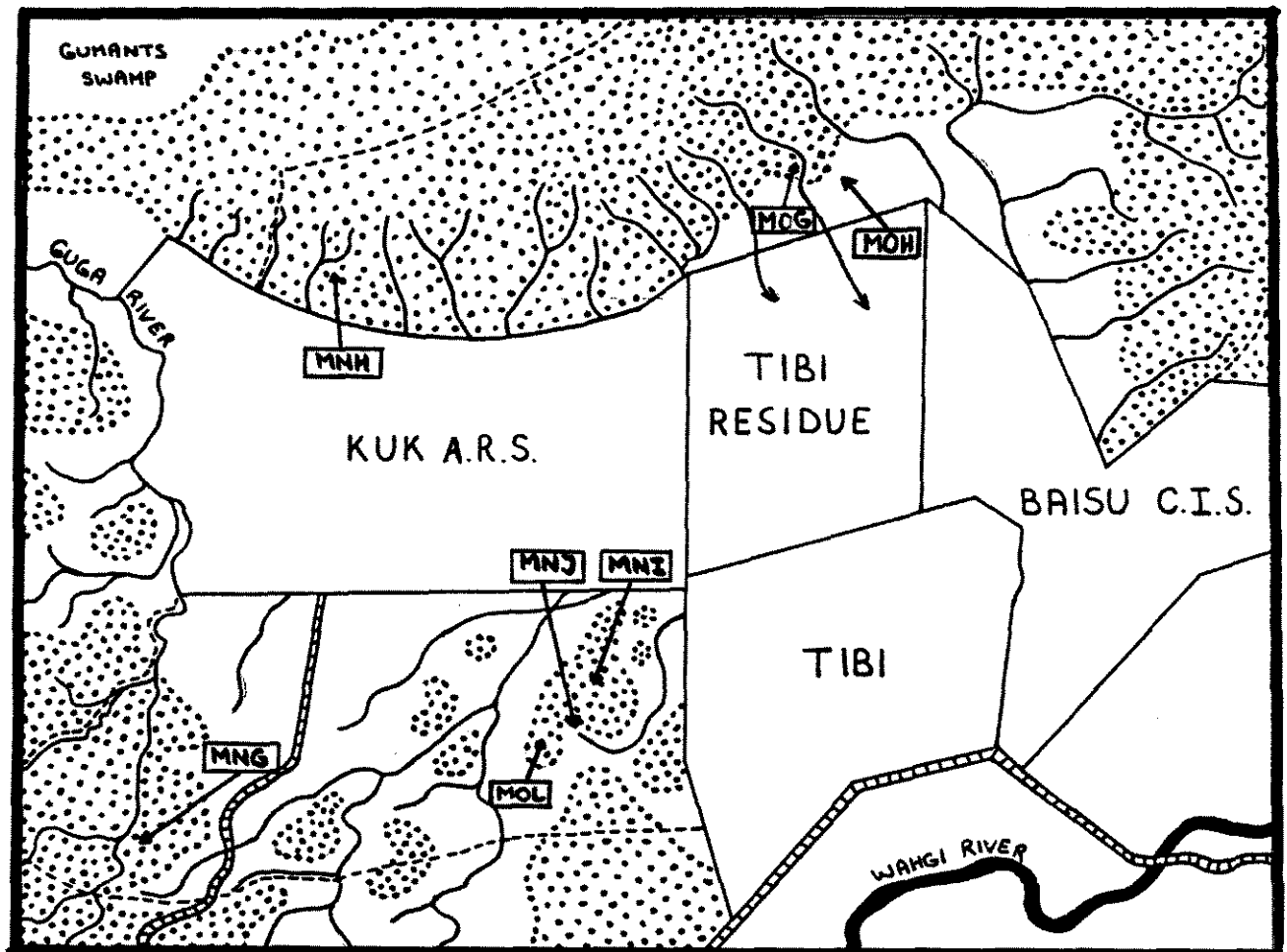




Plate 19. Site MNG. Piles of large, medium and small cooking stones excavated from feature B4-C4.



Plate 20. Site MNG. Profile of feature D3-E3.

Fig. 49 . Site MNG

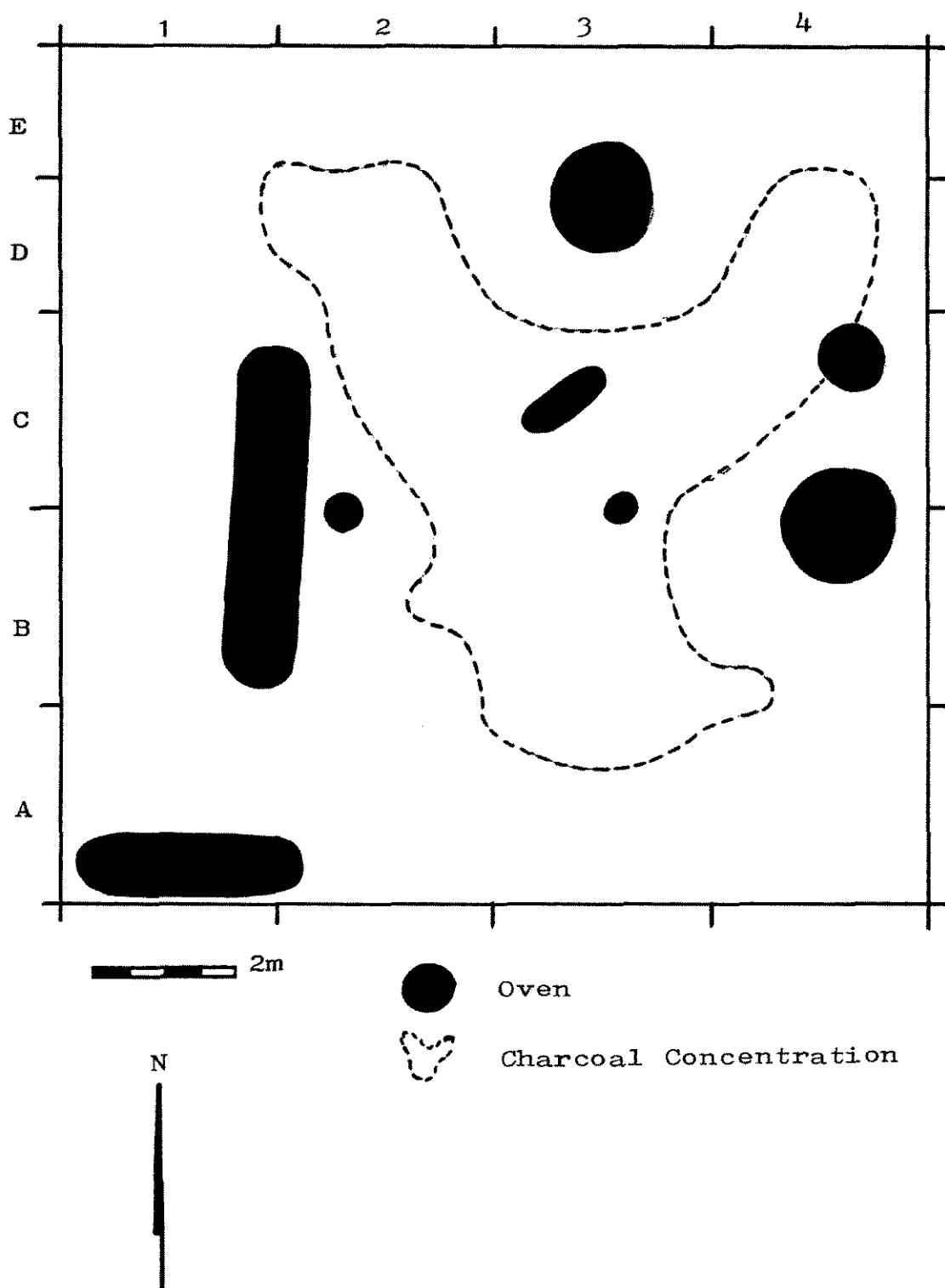


Table 32. Finds from Site MNG

FEATURE	COOKING STONES				BANANA LEAF	SWEET POTATO	CORN	KUMU	HALF COCONUT	PIG FAT	PIG BONE	COW BONE	GOURD	BAMBOO KNIFE	KINA SHELL	CHERT FRAGMENT	HOUSE POST	GLASS	BEER BOTTLE	BEER BOTTLE CAP	RUBBER	PLASTIC	SILVER FOIL	POCKET KNIFE	NAIL	TIN CAN	TOTAL
	BIG	MEDIUM	SMALL																								
SURFACE			65						1					1			1	5				1					74
A1-A2	44	86	158								5			1				4	1								299
B1-C1		65	52								13					1		5				1					137
B2-C2																											0
B3-C3	2	52	19	yes				yes																			73
B4-C4	156	158	165	yes						yes																	479
C3																											0
C4	2	150	299																								451
D3-E3	1	79	133		yes	yes	yes				14	4	1		2			54			5			3	1	5	304
TOTAL	205	590	891	yes	yes	yes	yes	yes	1	yes	32	4	1	2	2	1	1	68	1	5	5	1	1	3	1	5	1817

Fig. 50. Site MNI

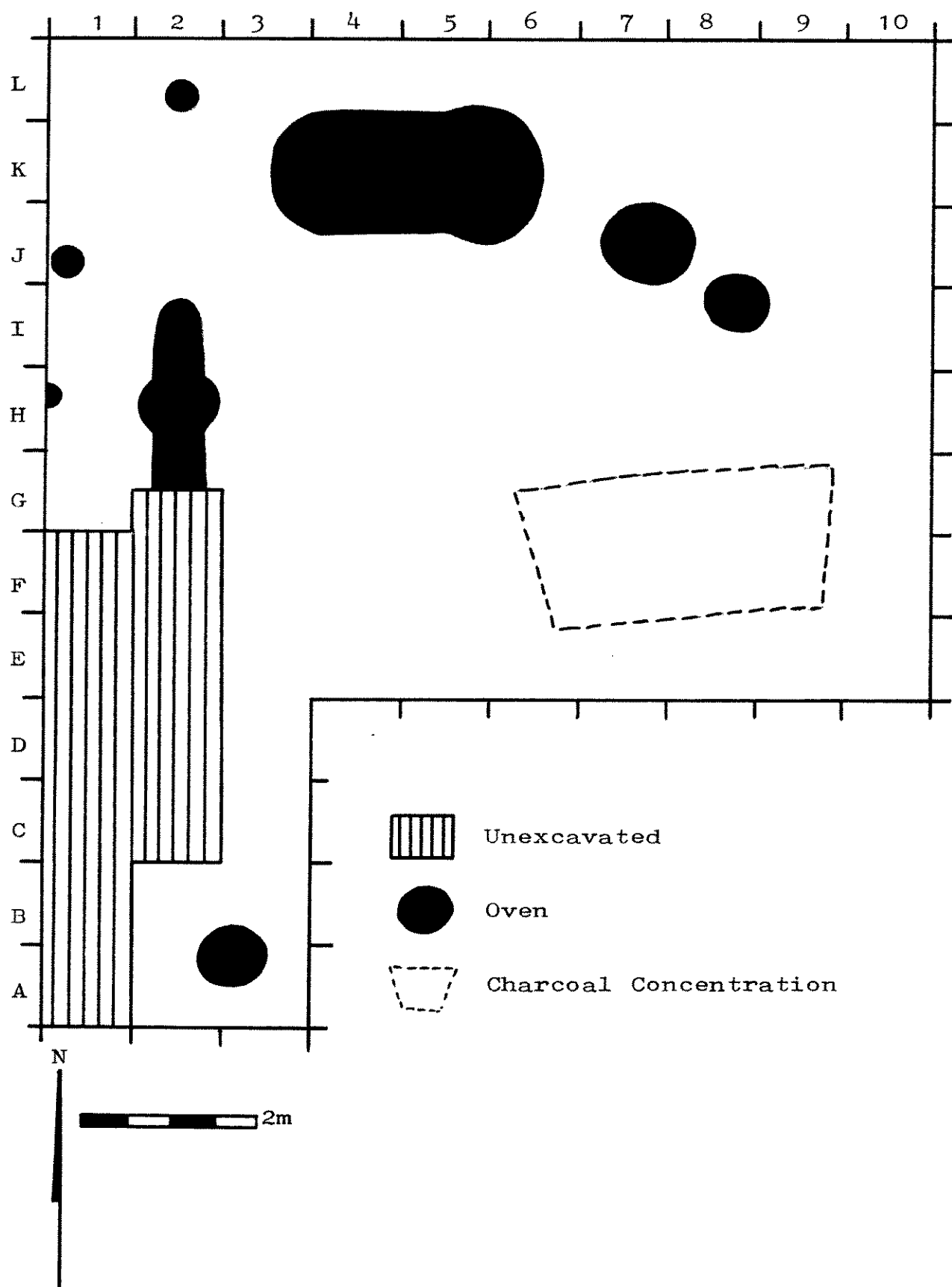
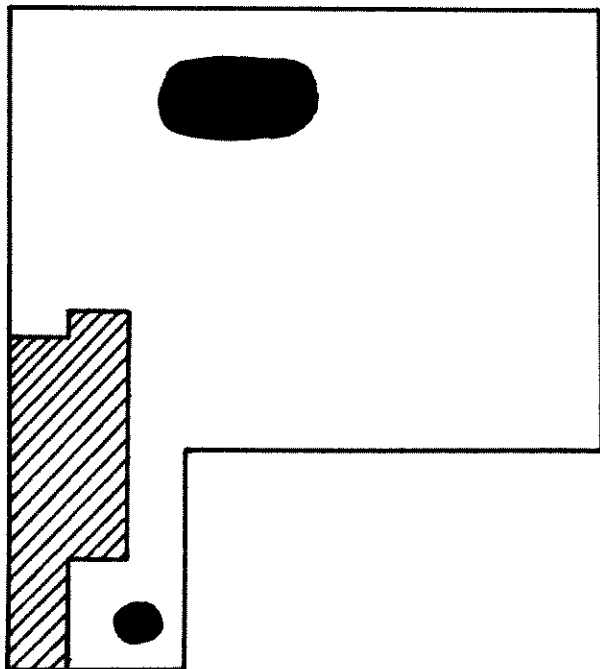
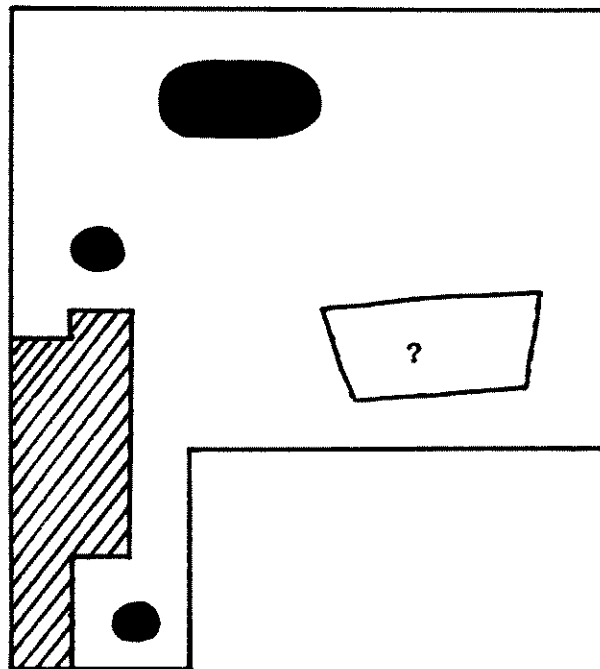


Fig. 51 Site MNI: Chronology of Ovens

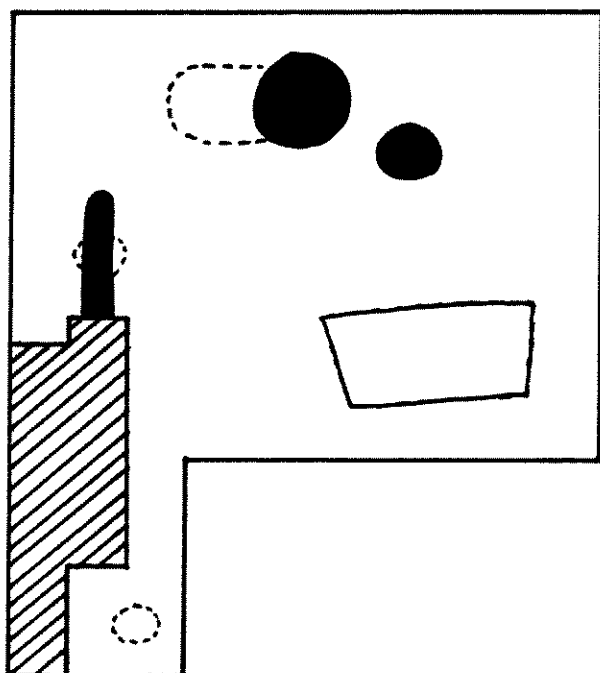
Phase 1



Phase 2



Phase 3



Phase 4

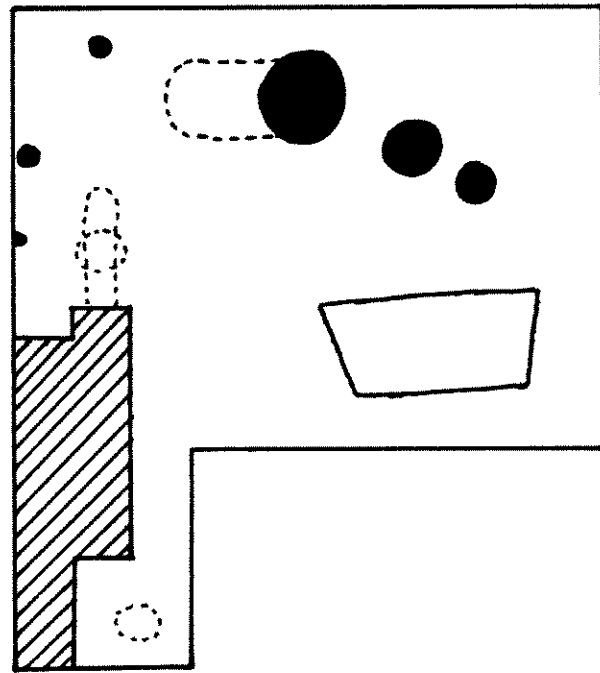


Table 33. Finds from MNI

Feature	COOKING STONES			KUMU	PIG BONE	BAMBOO KNIFE	CHERT FRAGMENT	TIN CAN	TOTAL
	BIG	MEDIUM	SMALL						
J7	95	330	181			2			608
I8			35	Yes					35
A3			2					1	3
H2			6		2		1		9
K5		65	65		11		2		143
H1			18						18
J1			52						52
L2		6	53						59
TOTAL	95	401	412	Yes	13	2	3	1	927

Fig. 52. Site MNH

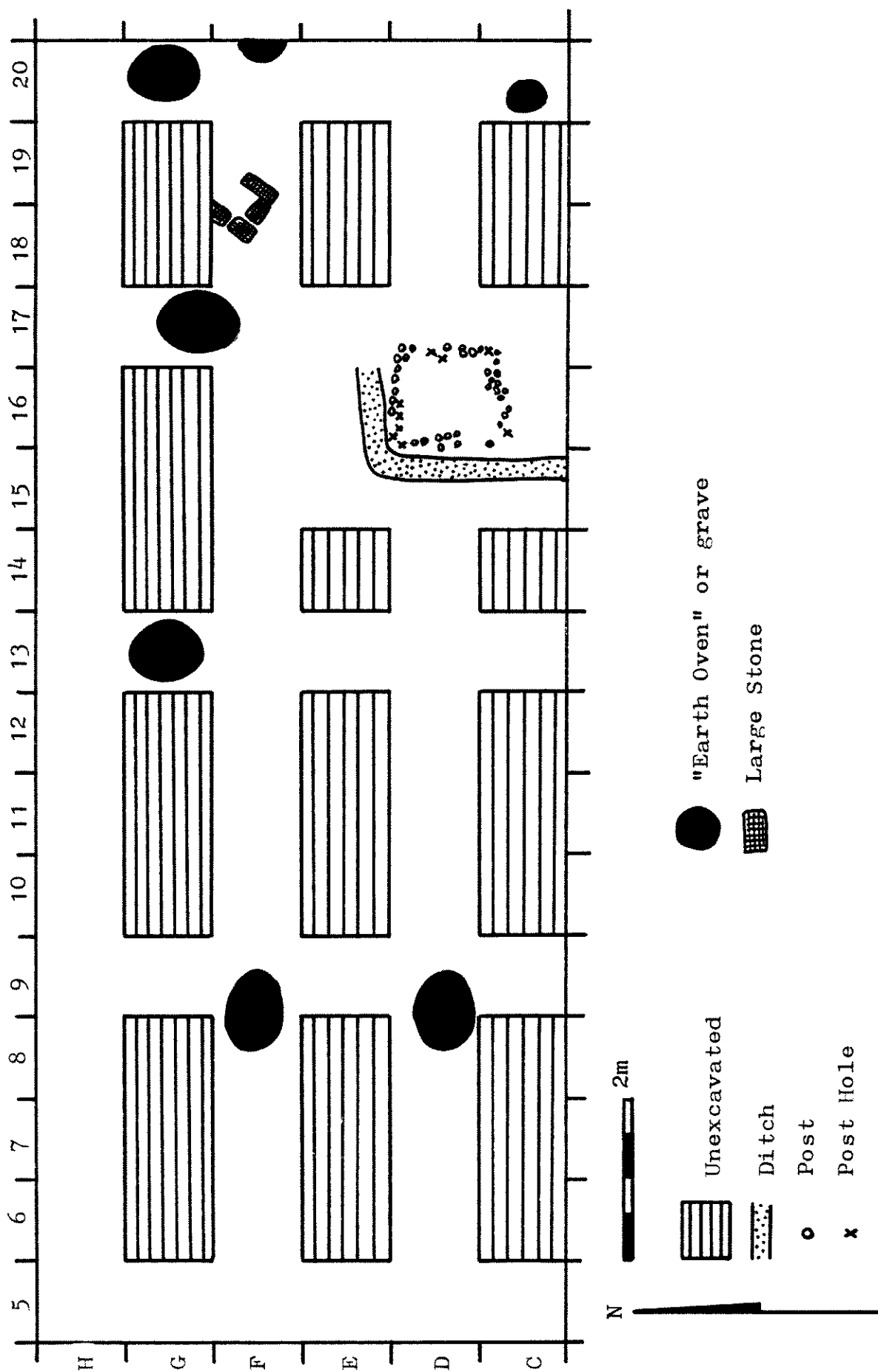


Table 34 . Finds from Site MNH

	SURFACE	D8-9	C20	F20	F8-9	TOTAL
SMALL COOKING STONE	68	70	21		320	479
STONE AXE CHIP/FLAKE	2	1		1		4
PIG TOOTH	1	3			5	9
PIG BONE			1		4	5
HUMAN LONG BONE				2		2
HUMAN SKULL					1	1
TOTAL	71	74	22	3	330	500

Table 35 . Measurements of Prehistoric Ovens

FUNCTION		D O M E S T I C							
TYPE		DCDV	DCDS	DCSV	DCSS	DODV	DODS	DOSV	DOSS
DATA FROM HARRIS 1977	NUMBER RECORDED	0	8	0	20	0	0	0	2
	AVERAGE (in cm)	TOP	65		40				70x60
		BOTTOM	30		20				50x40
		DEPTH	40		25				20
DATA FROM WATSON AND COLE 1977	NUMBER RECORDED	4	0	0	7	0	0	0	2
	RANGE (in cm)	TOP	52-88		40-87				38x16 and 72x60
		DEPTH	61-89		20				?
	AVERAGE (in cm)	TOP	66		62				?
		DEPTH	72		20				?



Plate 21. Starting a new communal garden in the swamp. All spoil dug from major ditch thrown on side to be gardenized. Manioc to be planted on top of this embankment. Pig land on right side. Tibi Residue.



Plate 22. Same as for Plate 21, but pitpit will be planted here. Kum.

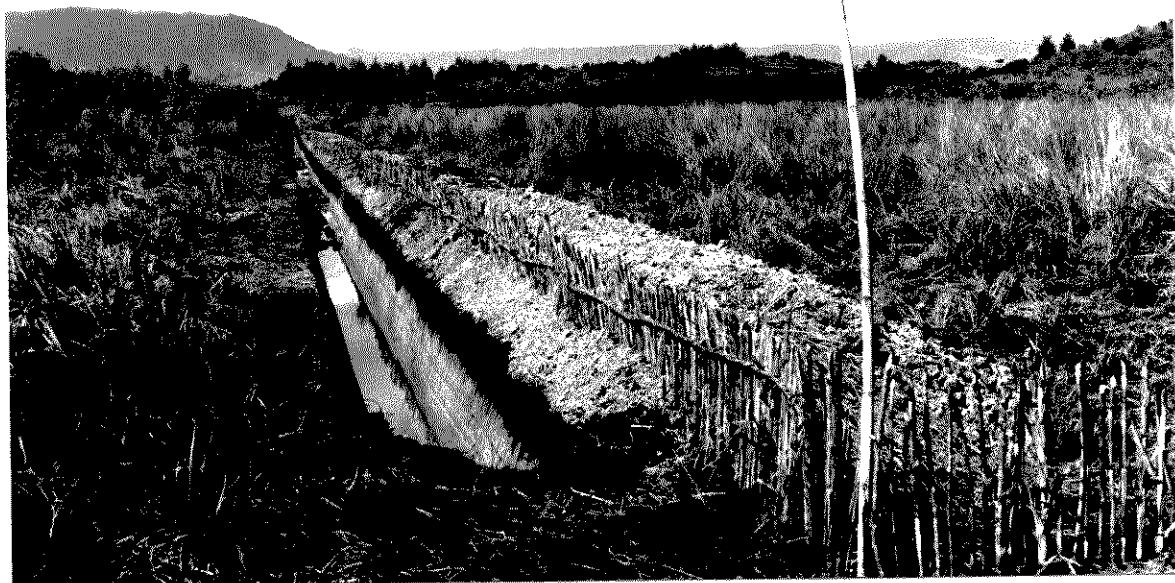


Plate 23. New (but still unfinished) ditch and fence (made with pitpit) enclosing future communal garden in the swamp. Dryland on the left is used as pig land. Mup.

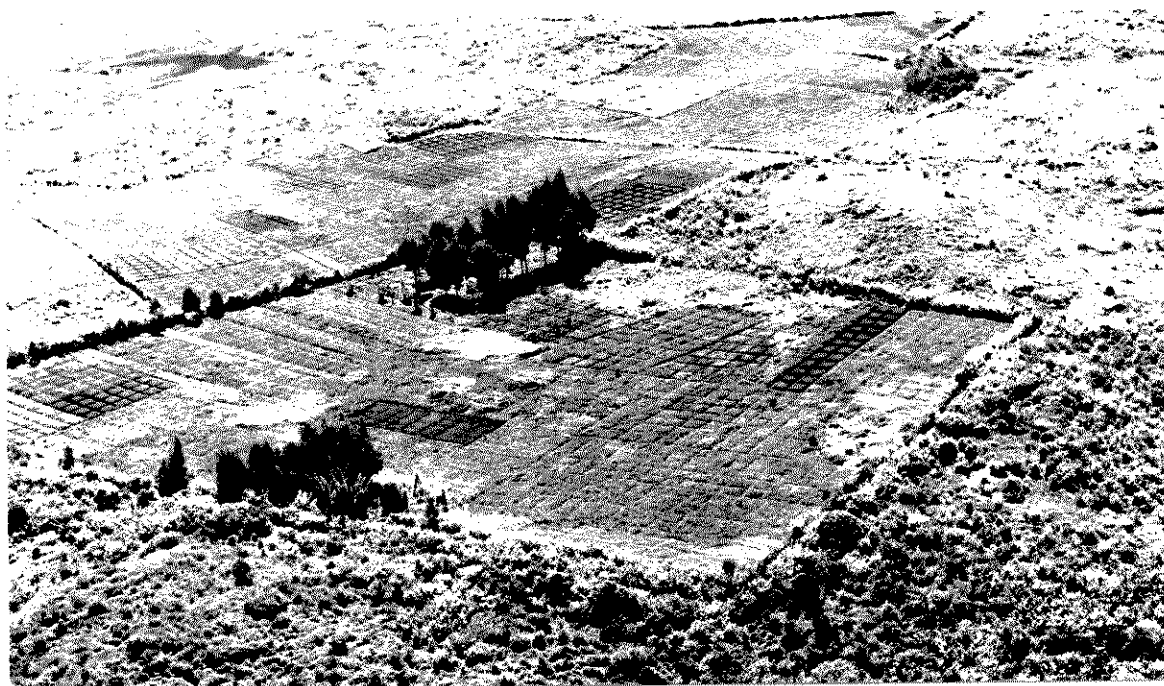


Plate 24. Large communal garden, mostly for sweet potato. Four successive extensions can be seen. Dryland, Gengla.



Plate 25. Extension of major ditch for new individual garden. Swamp on left, dryland on right. Kukrumdi.

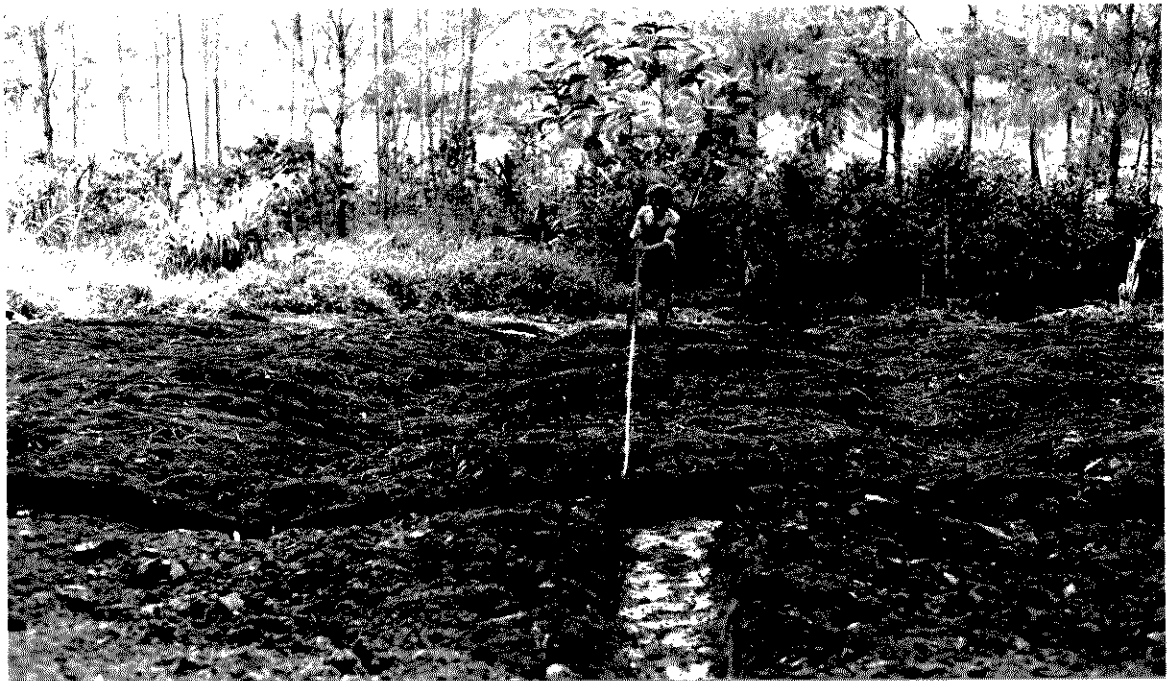


Plate 26. Making a new grid, slightly shifted from the previous one, for sweet potato garden. No fallow period between the two phases. Dryland, Kum.

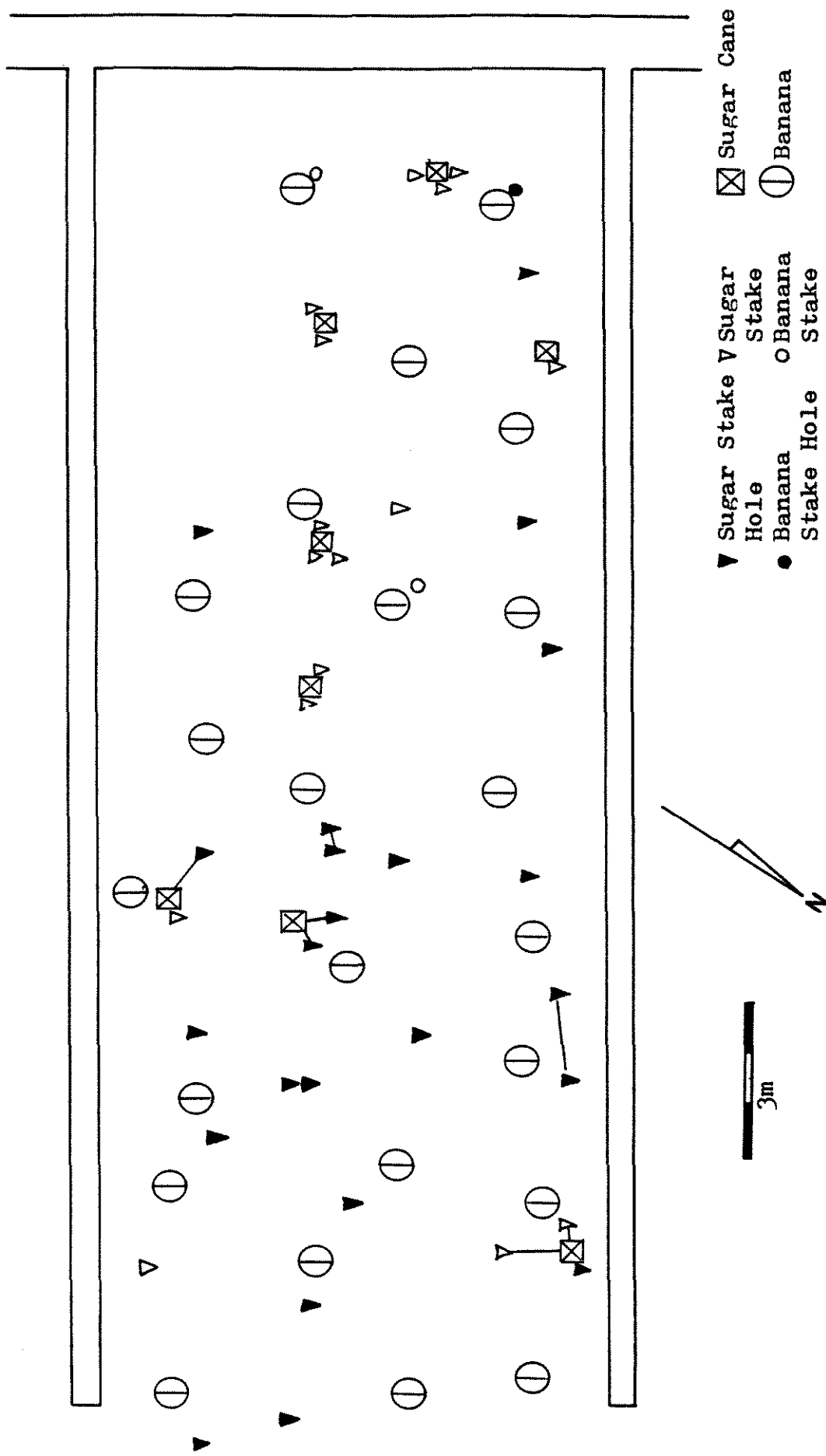
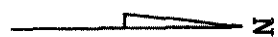
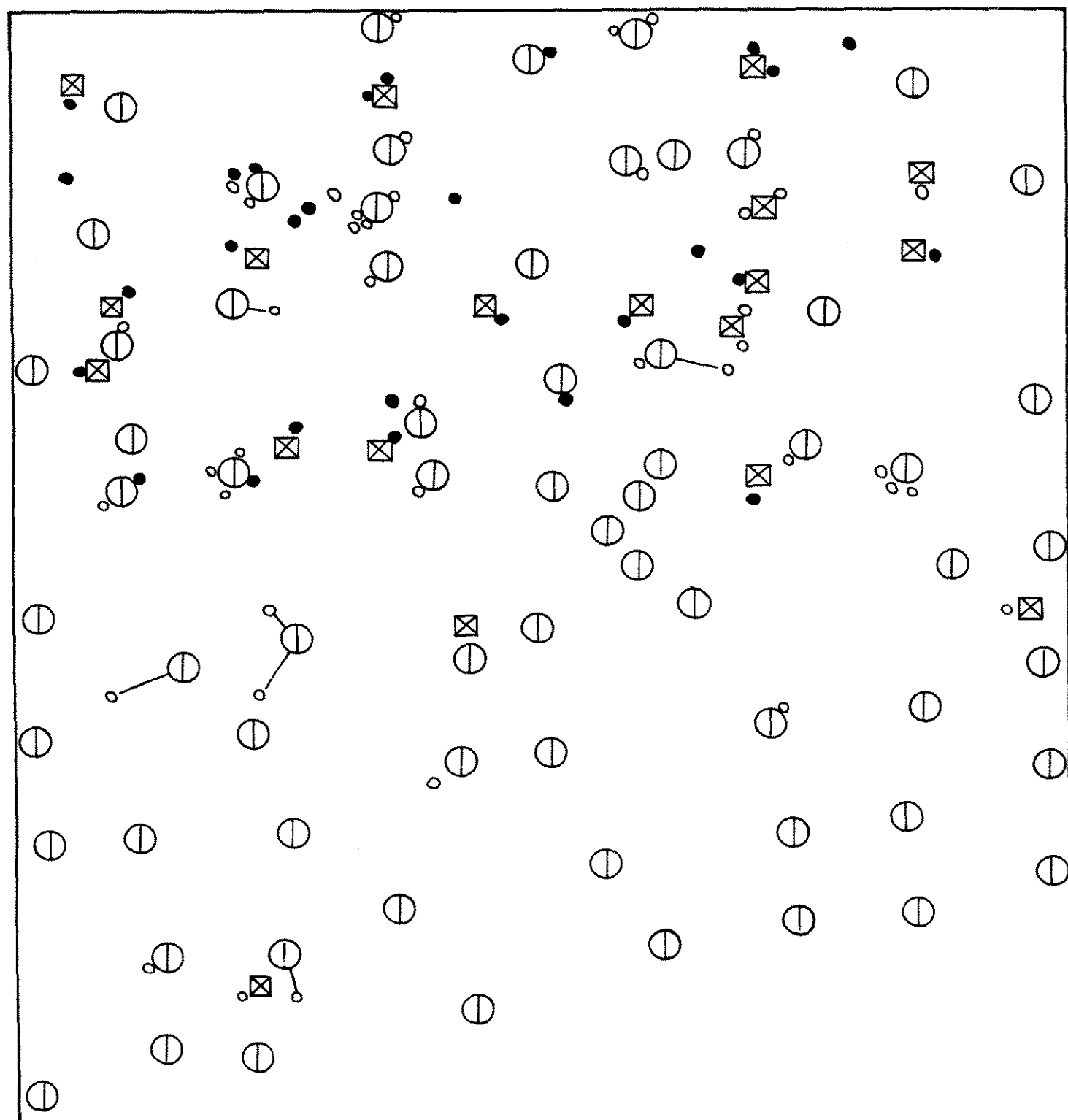


Fig.53. Ru Garden

Fig.54. Konga Garden

3m

○ Stake

● Stake Hole

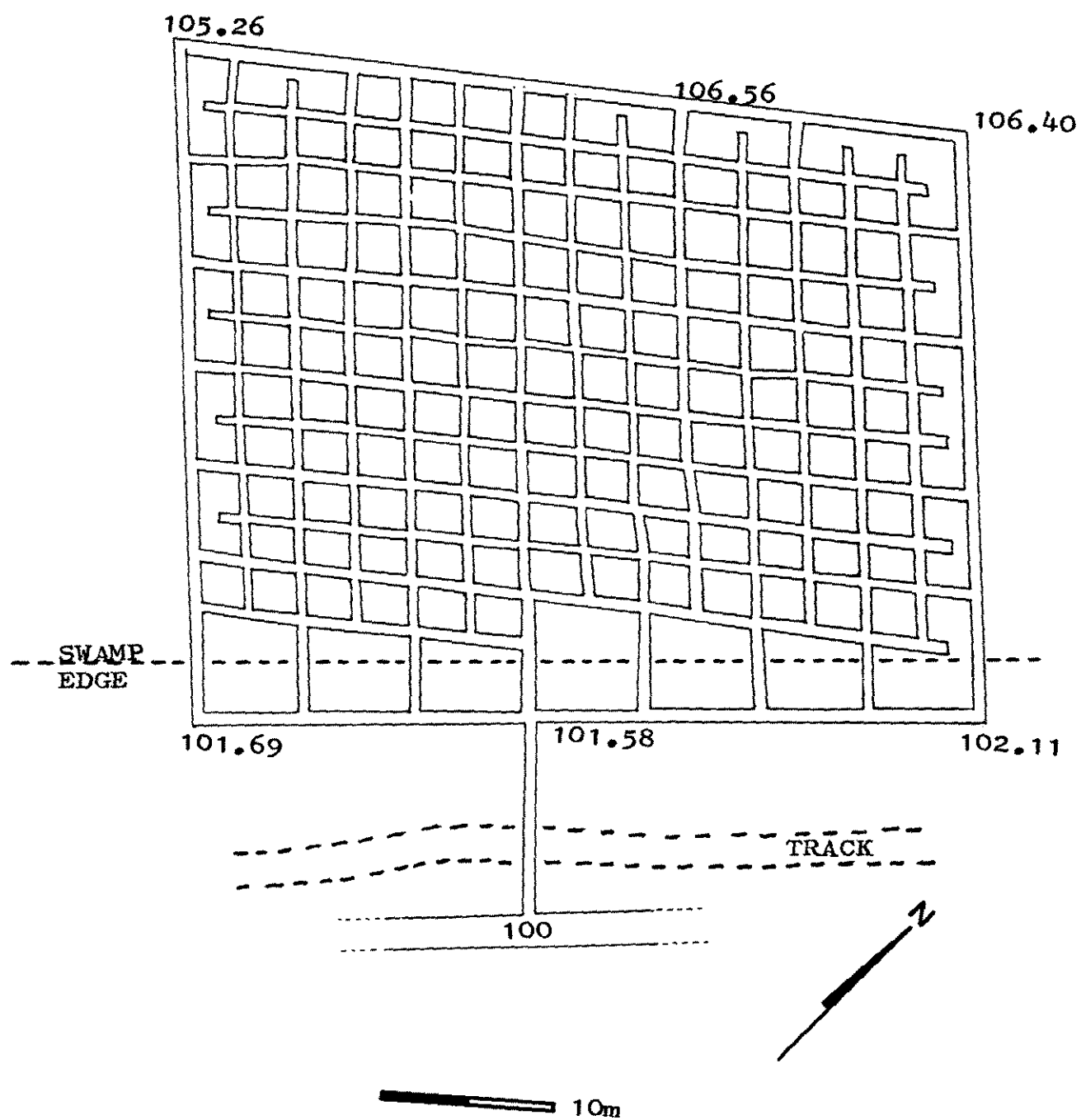
⊠ Sugar Cane

⊙ Banana

STAKE HOLE FOR	T O P					D E P T H		
	Number Recorded	Length Range	Length Average	Width Range	Width Average	Number Recorded	Range	Average
SUGAR CANE	50	4 - 20	10.9	4 - 18	10	15	29 - 55	39
BANANA	20	5 - 22	16.8	5 - 20	14.7	17	28 - 55	37
LADDER	10	14 - 32	22.6	12 - 19	15.2	8	37 - 52	45.1

Table 36. Dimensions of Stake Holes (in cm)

Fig.55. Korua Garden



Relative elevation of lowest point: 100m
 Relative elevation of highest point: 106.56m

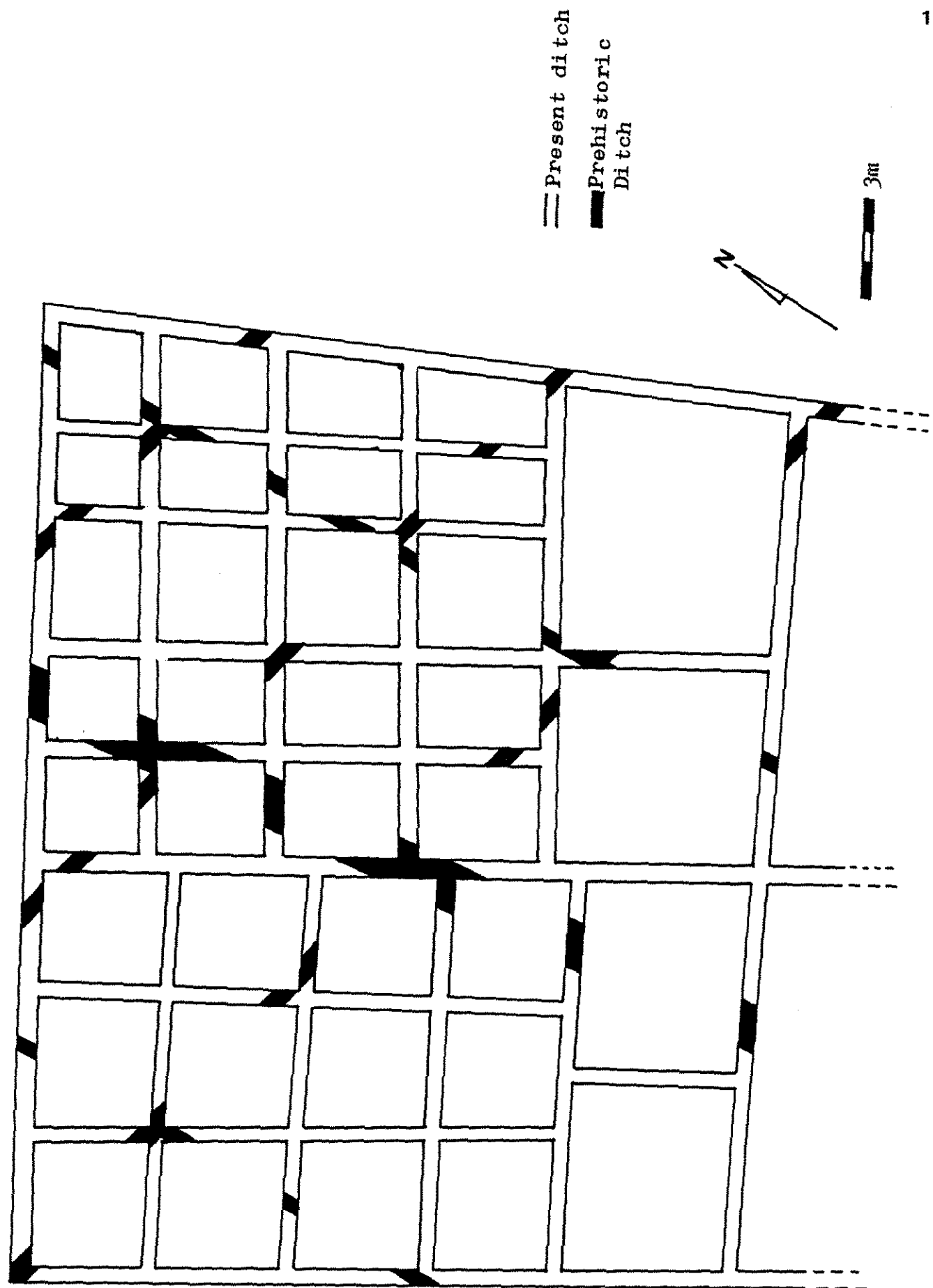
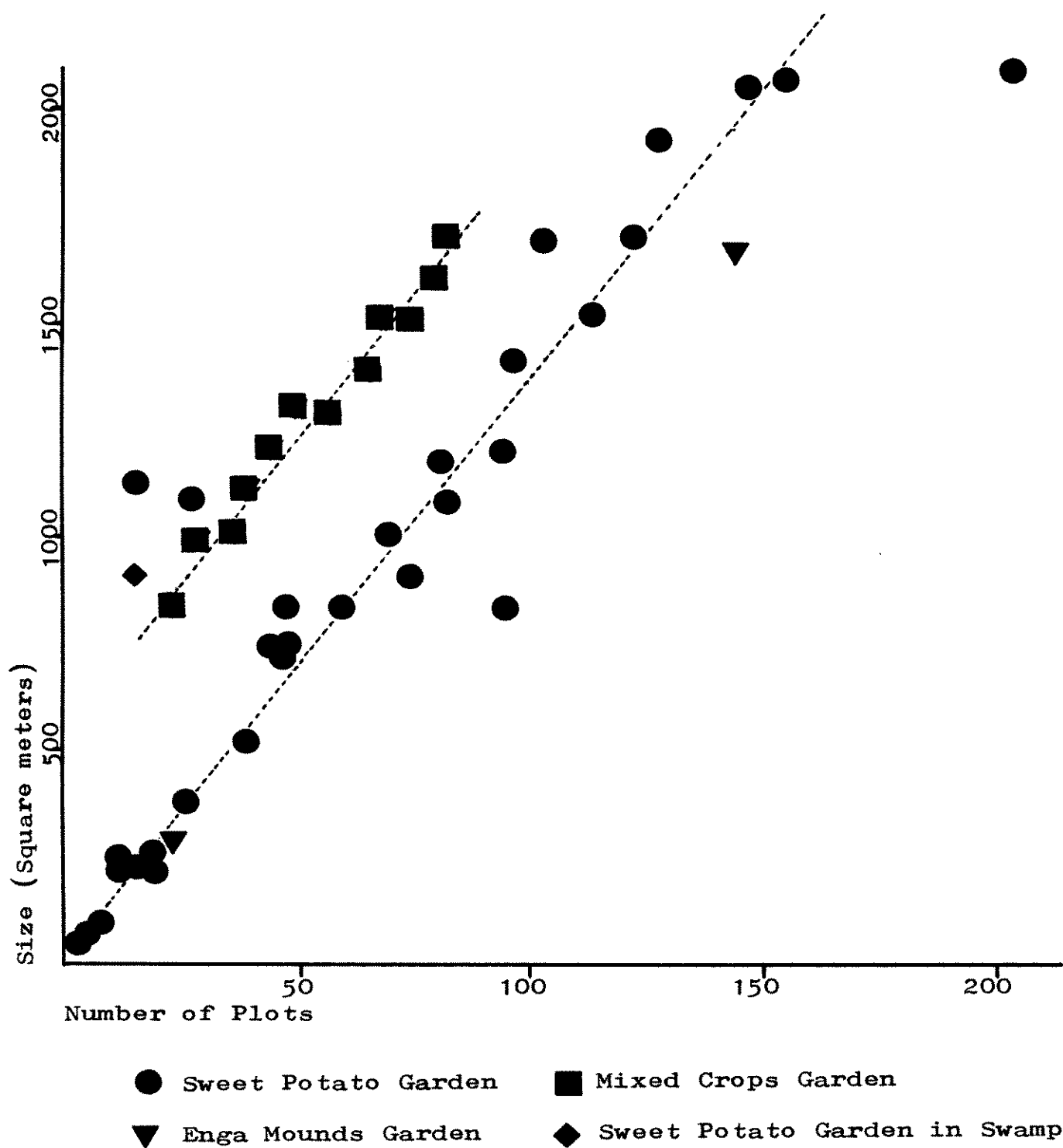


Fig.56. Nigints Garden

Table 37. Size and Number of Plots in Gardens



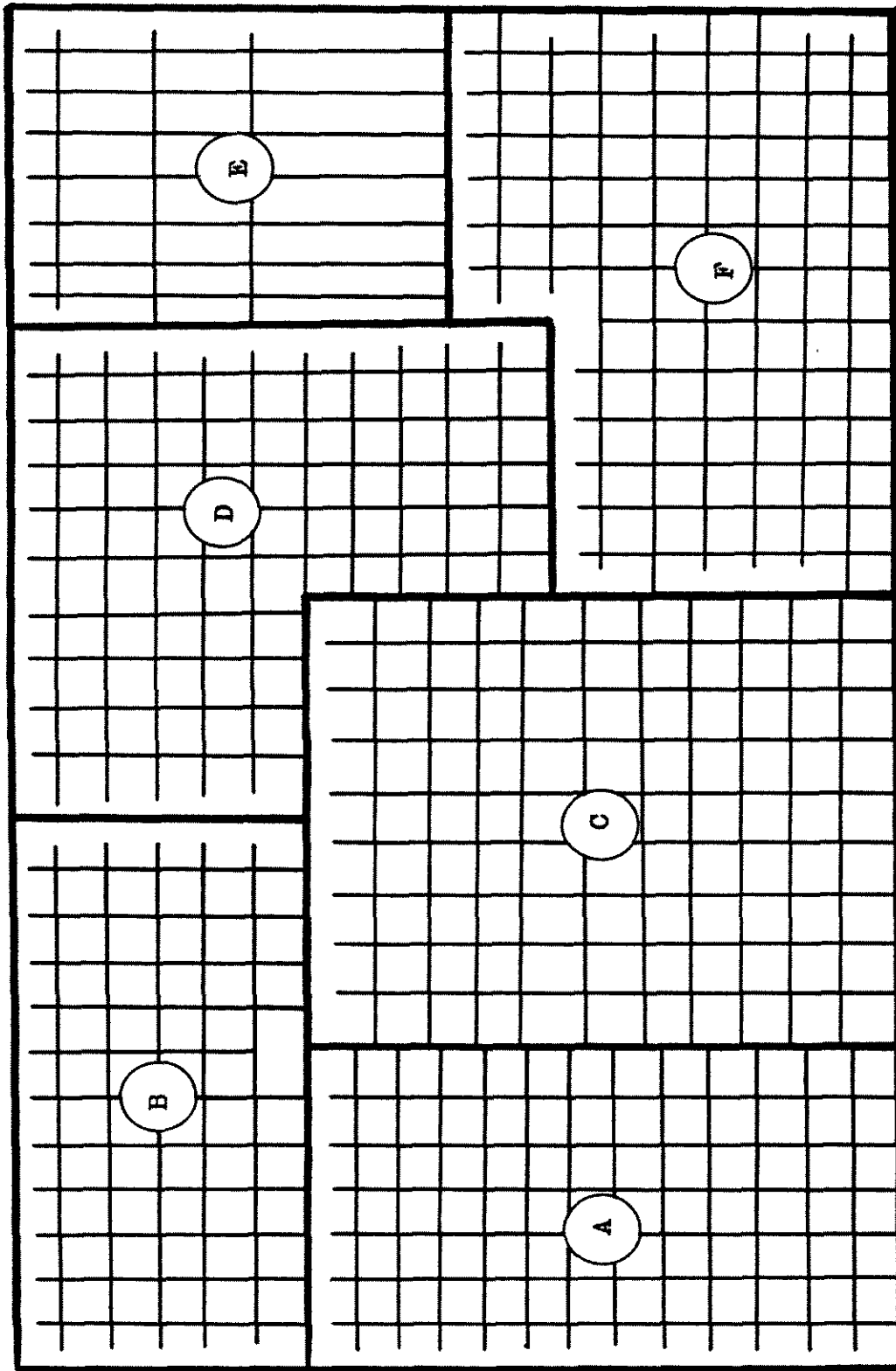
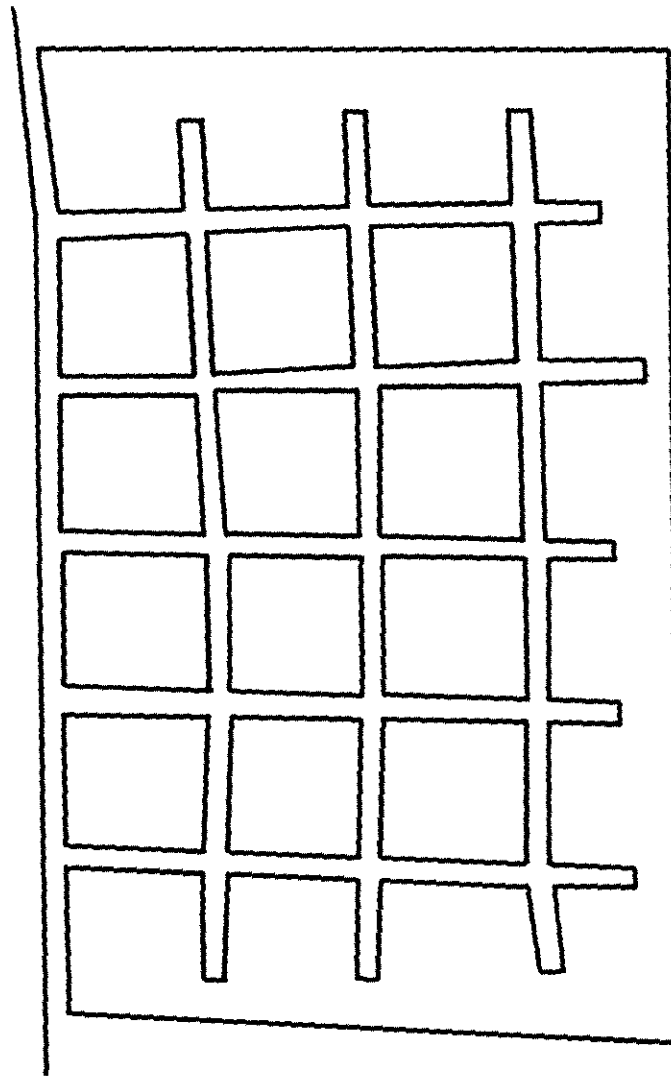


Fig.57. Communal Garden - Theoretical Model

Fig. 58. Muk Garden

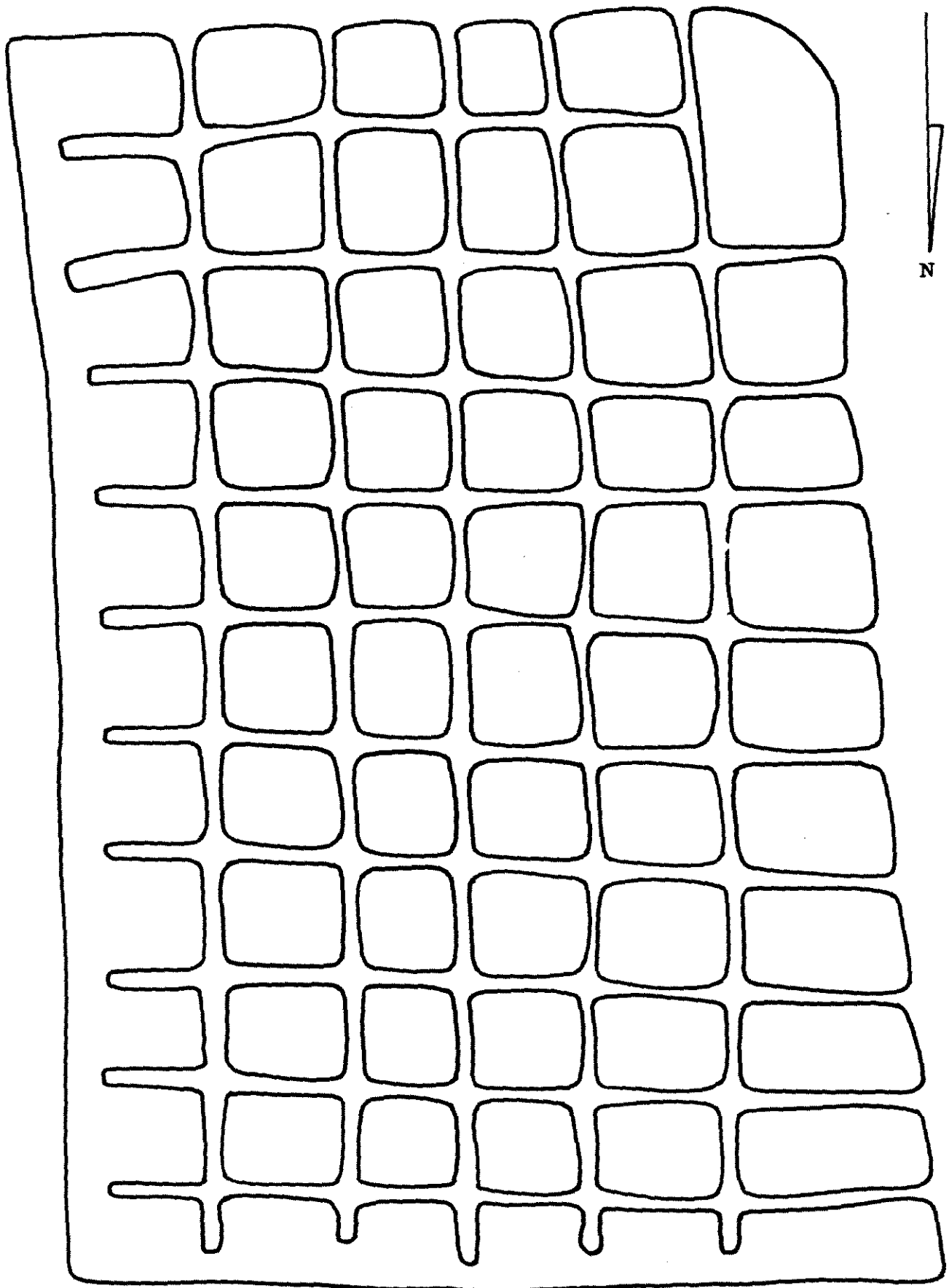


5m



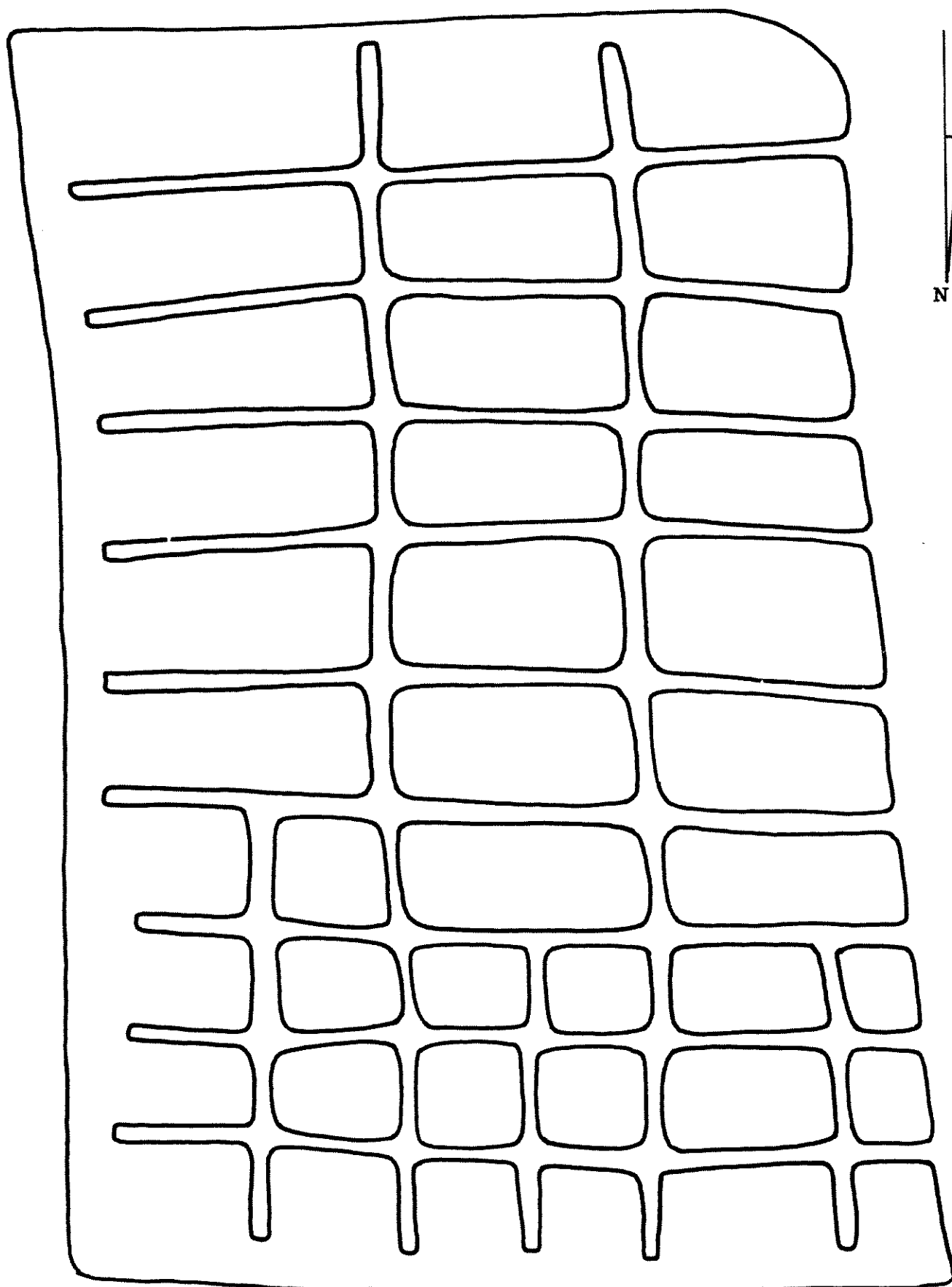
 5m

Fig. 59. Rea-Nung Garden.
Phase 1



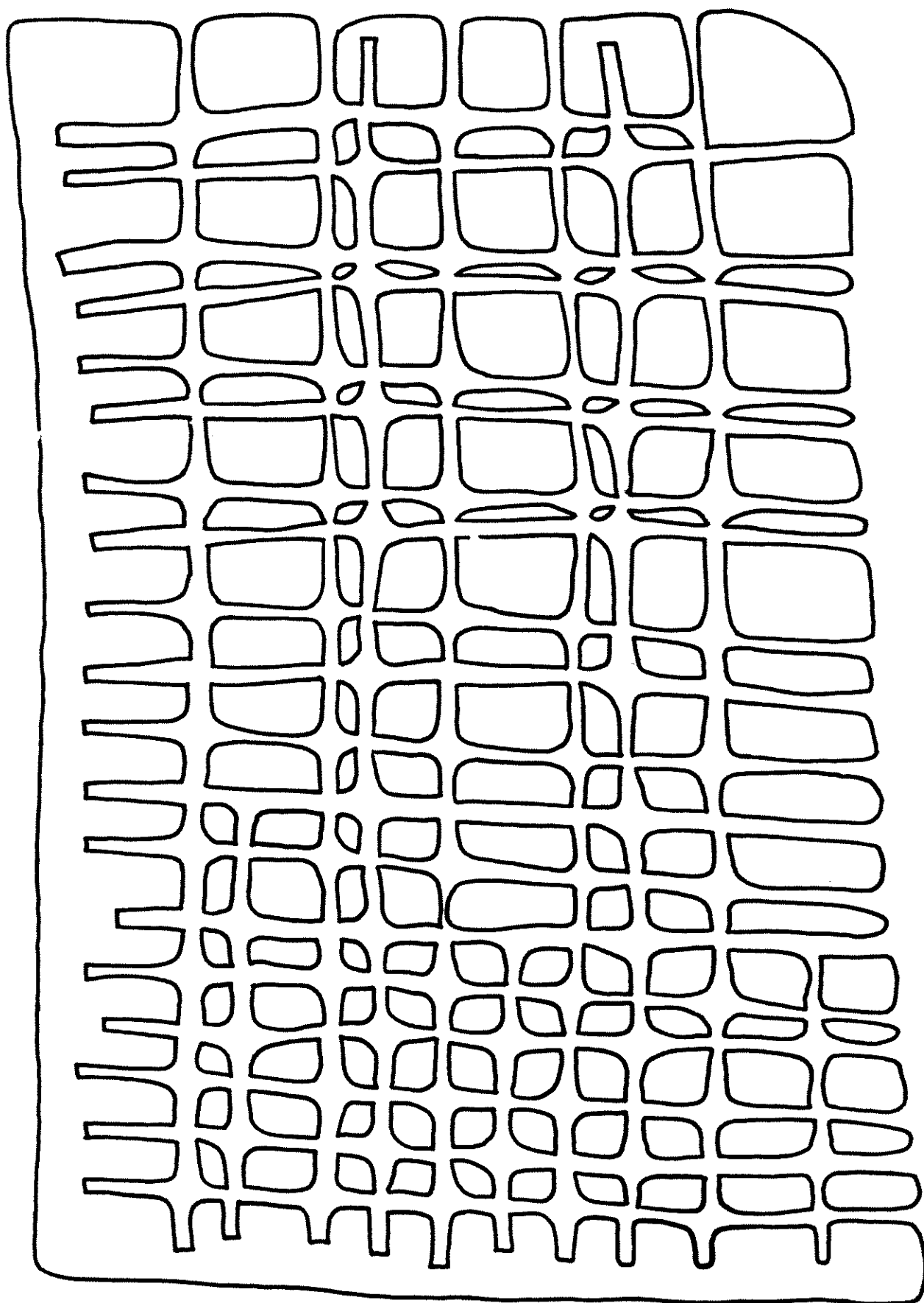
5m

Fig.60. Rea-Nung Garden.
Phase 2



 5m

Fig. 61. Rea-Nung Garden.
Phases 1 and 2



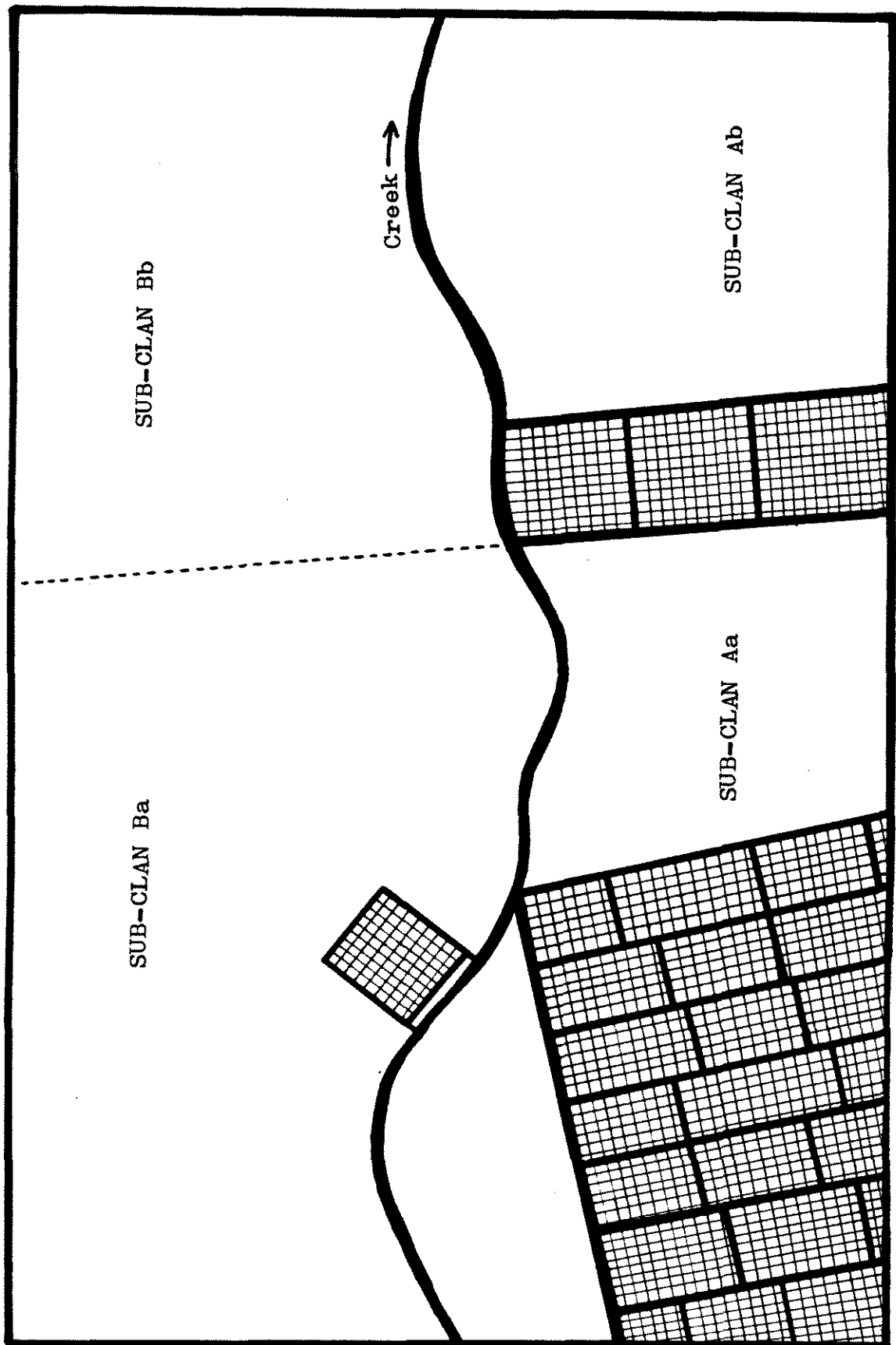


Fig. 62. Swamp Gardens - Phase 1

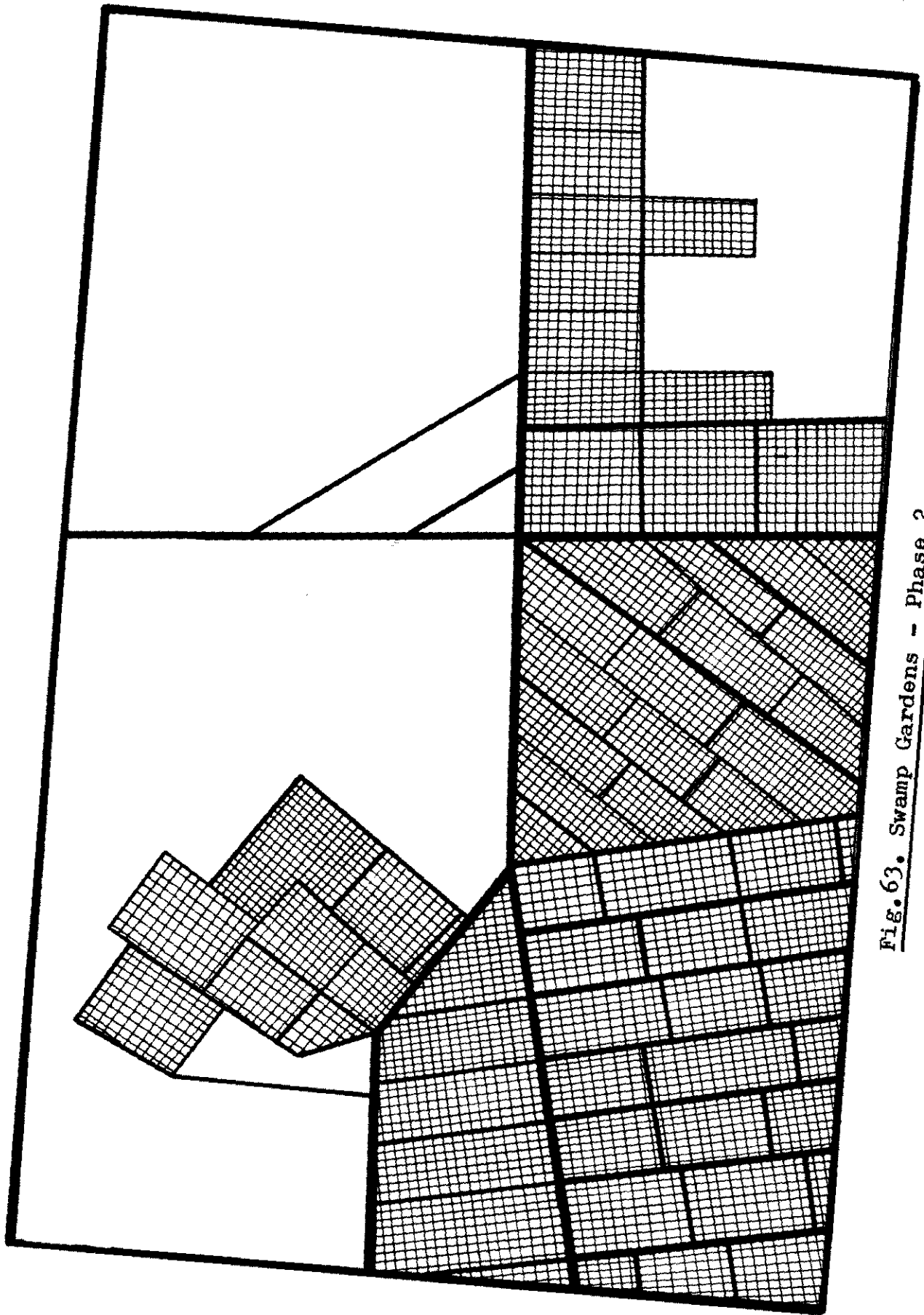


Fig. 63. Swamp Gardens - Phase 2

Table38. Formal Ditches

	MAJOR	TRIBUTARY	BOUNDARY	BLOCK	PLOT
DRYLAND			X	(X)	X
SWAMPLAND	X	X		X	X

Table39. Functional Ditches

	MAJOR	TRIBUTARY	BOUNDARY	BLOCK	PLOT
DRYLAND			X	X	
SWAMPLAND	X			(X)	X

Table40. Ditch Dimensions - Top Width(cm)

	MAJOR	TRIBUTARY	BOUNDARY	BLOCK	PLOT
DRYLAND			50-120	30-100	30-50
SWAMPLAND	100-200	80-150		30-100	30-50

Table41. Ditch Dimensions - Depth (cm)

	MAJOR	TRIBUTARY	BOUNDARY	BLOCK	PLOT
DRYLAND			80-100	20-40	20-40
SWAMPLAND	120-220	100-150		30-100	30-70

Map 26. Location of Garden Sites

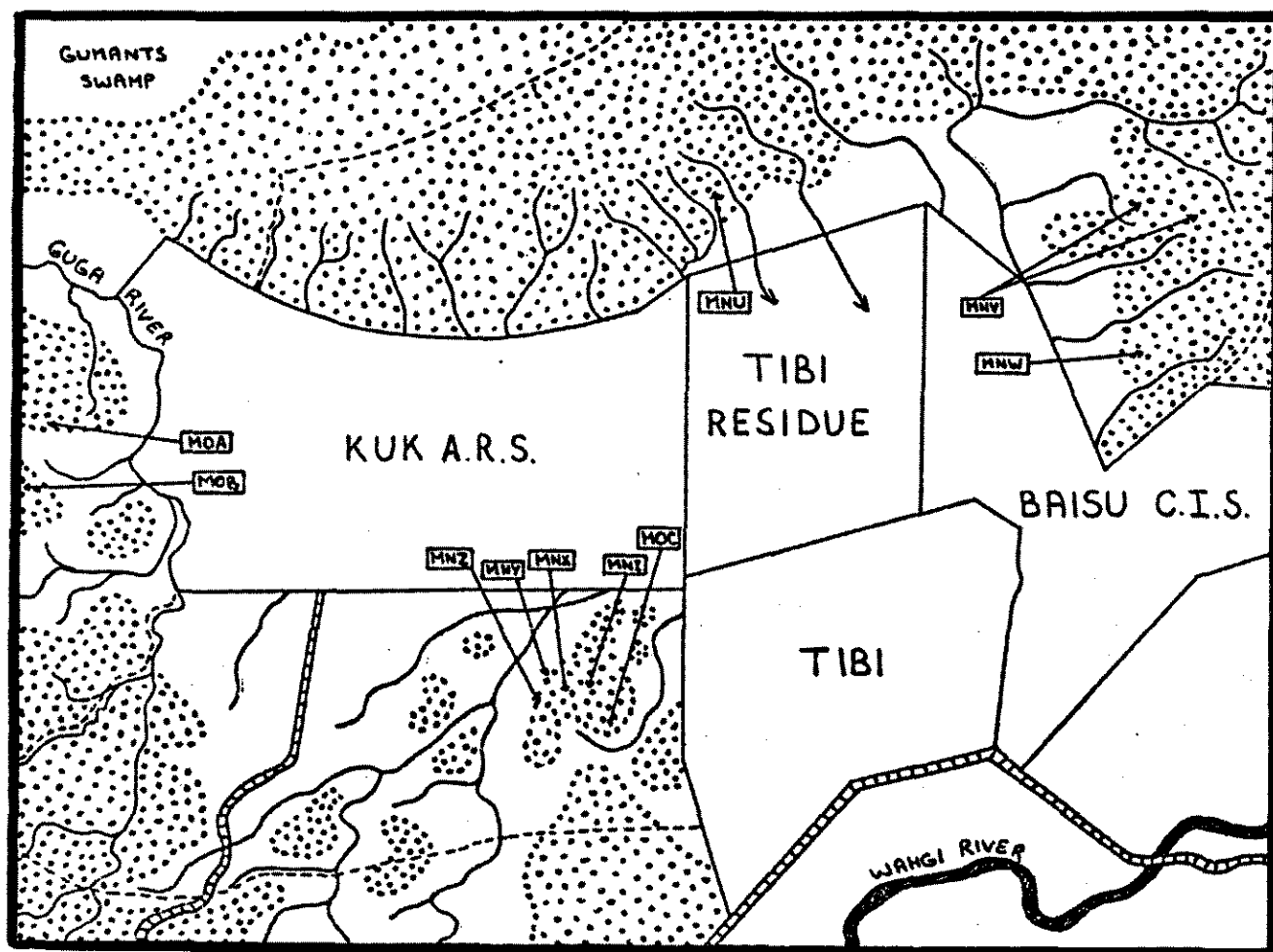




Plate 27. Site MNV/1, high up on the slopes of Ep Ridge.



Plate 28. Site MOA. Excavation in progress. Trenches NS2 left, EW1 centre, and lower part of NS1 right.

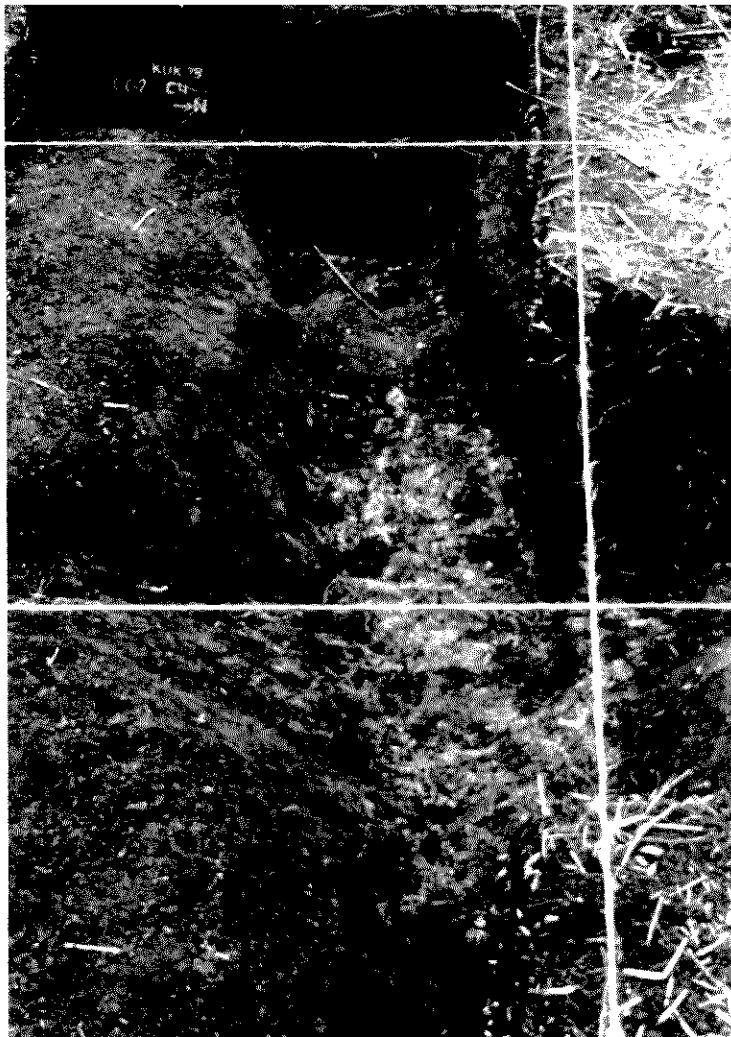


Plate 29. Site MNX.
Excavation in progress.
Note ditch dug well
into basal clay with
digging stick marks
at its base.



Plate 30. Site MNX.
Digging stick marks
at base of ditch.



Plate 31. Site MNZ. Ditch deliberately dug into basal clay.

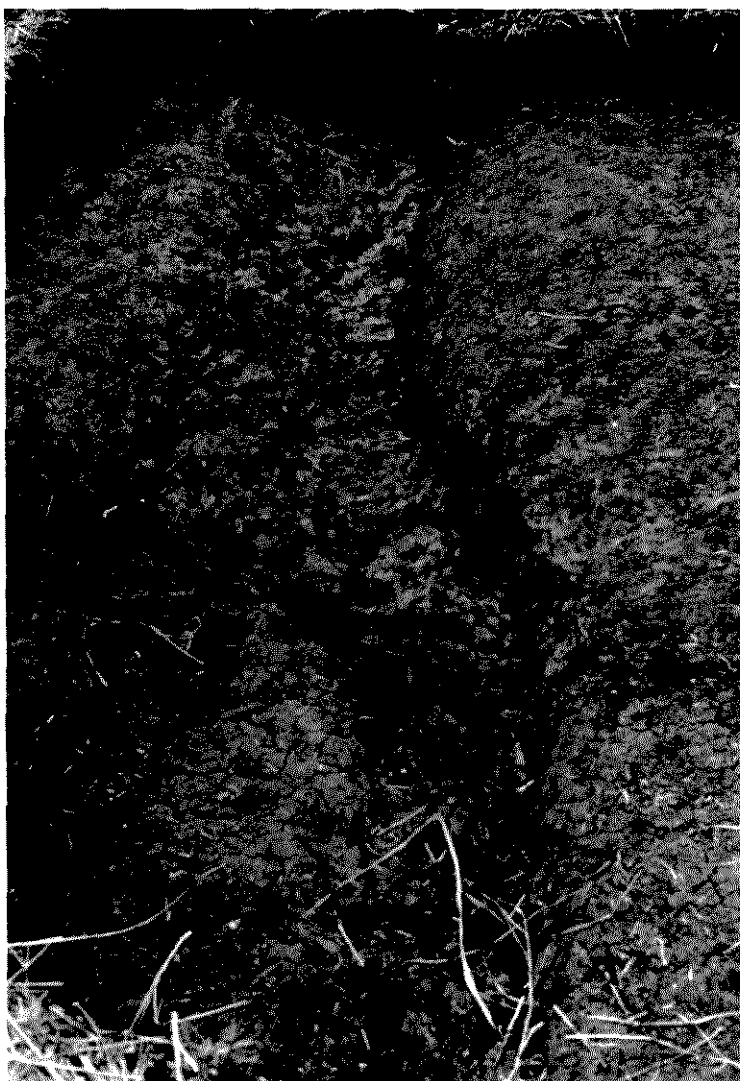
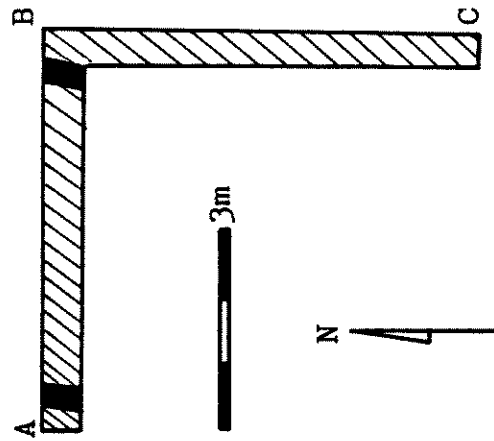
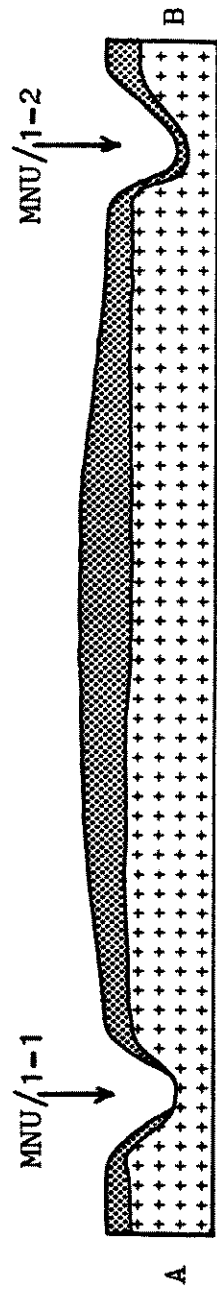


Plate 32. Site MOC. Bend in feature attributed to Phase 1.



1m

Top Soil
Clay

Fig.64. Site MNU/1

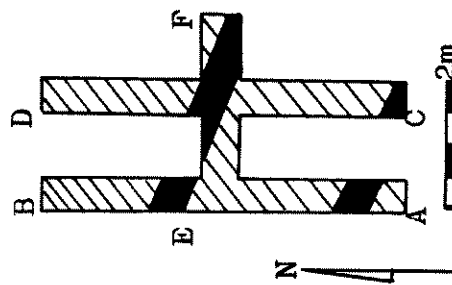
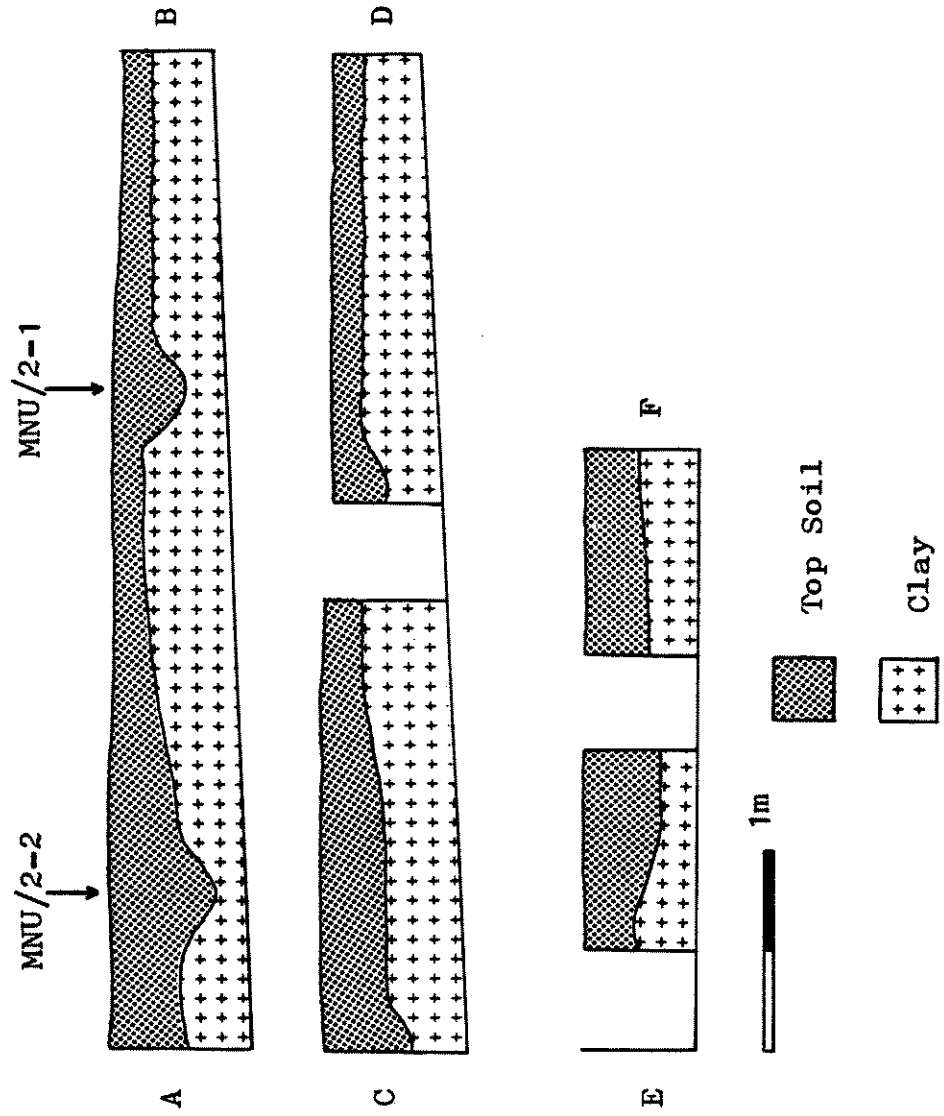


Fig.65. Site MNU/2

Fig.66. Trenches from MNU/3

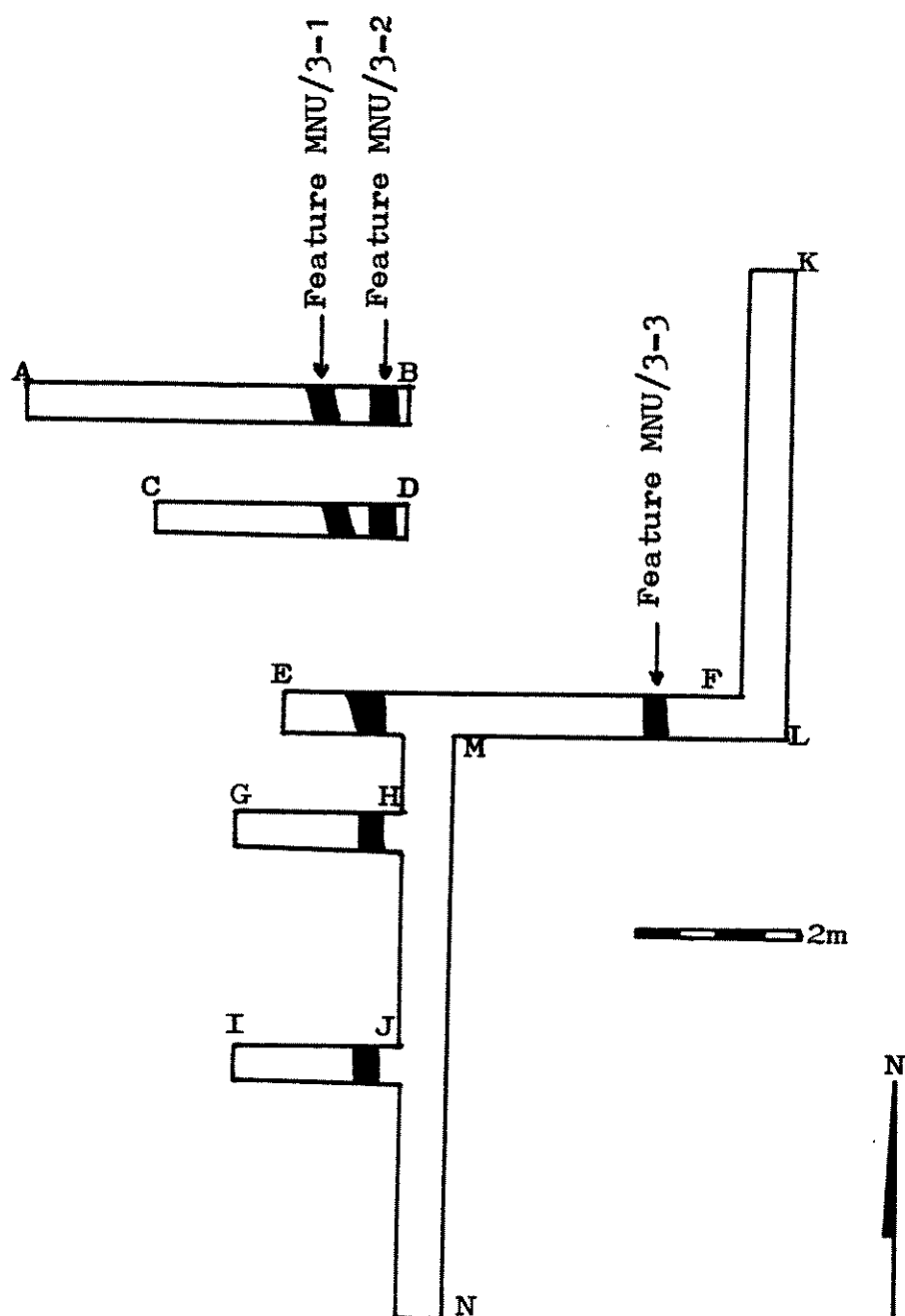
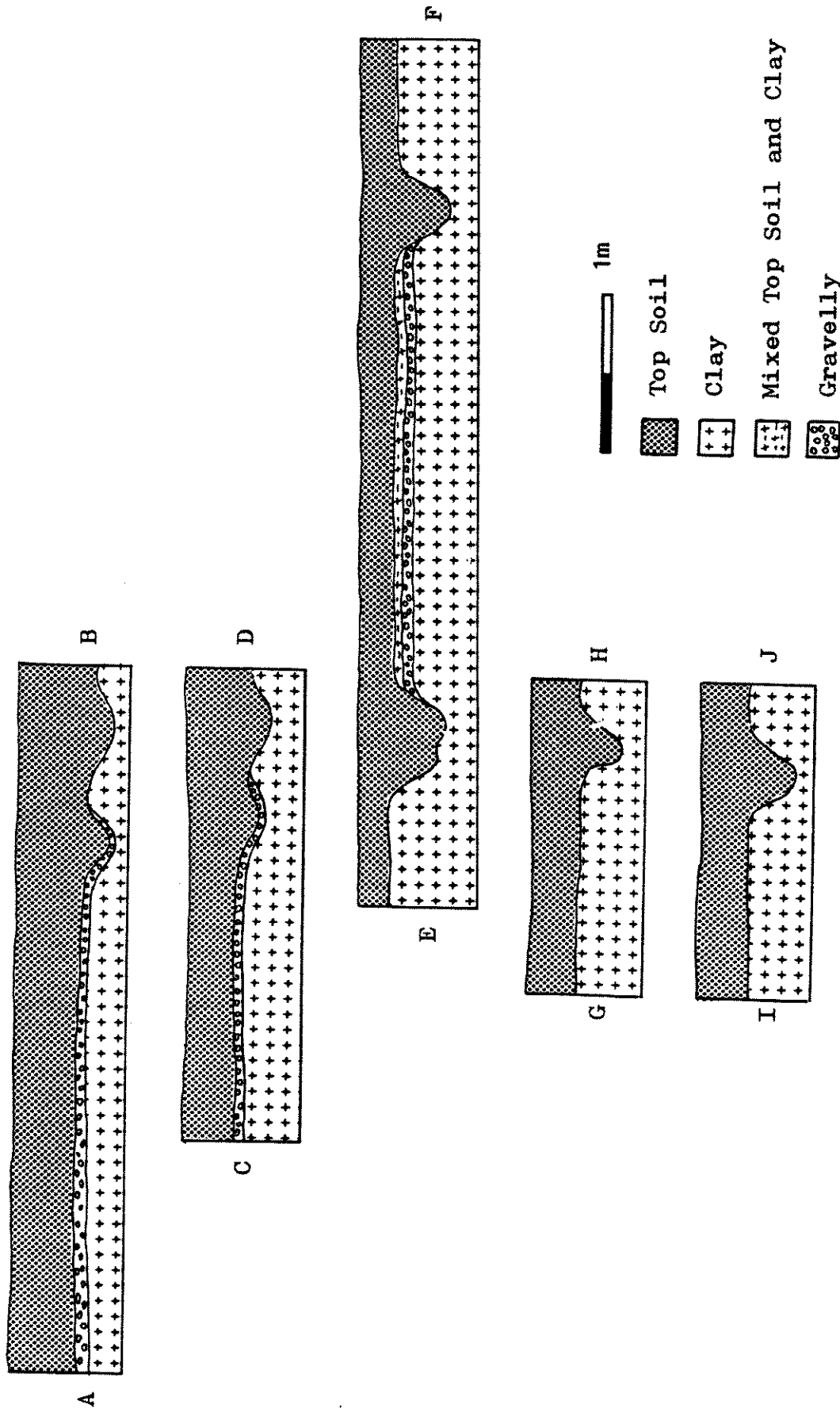


Fig.67. Site MNU/3



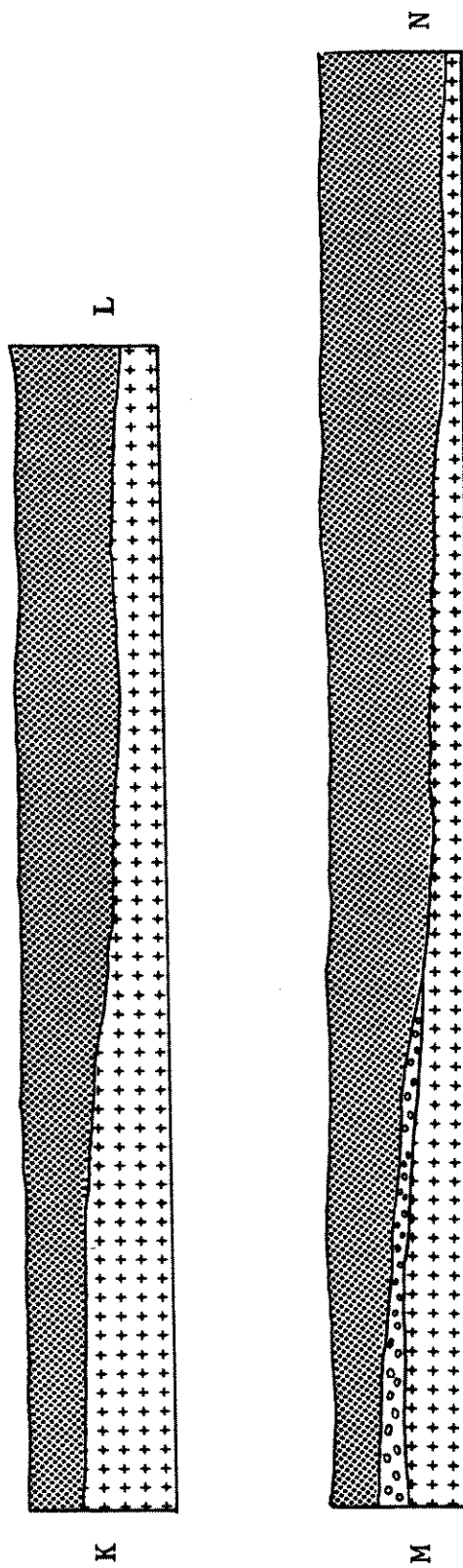
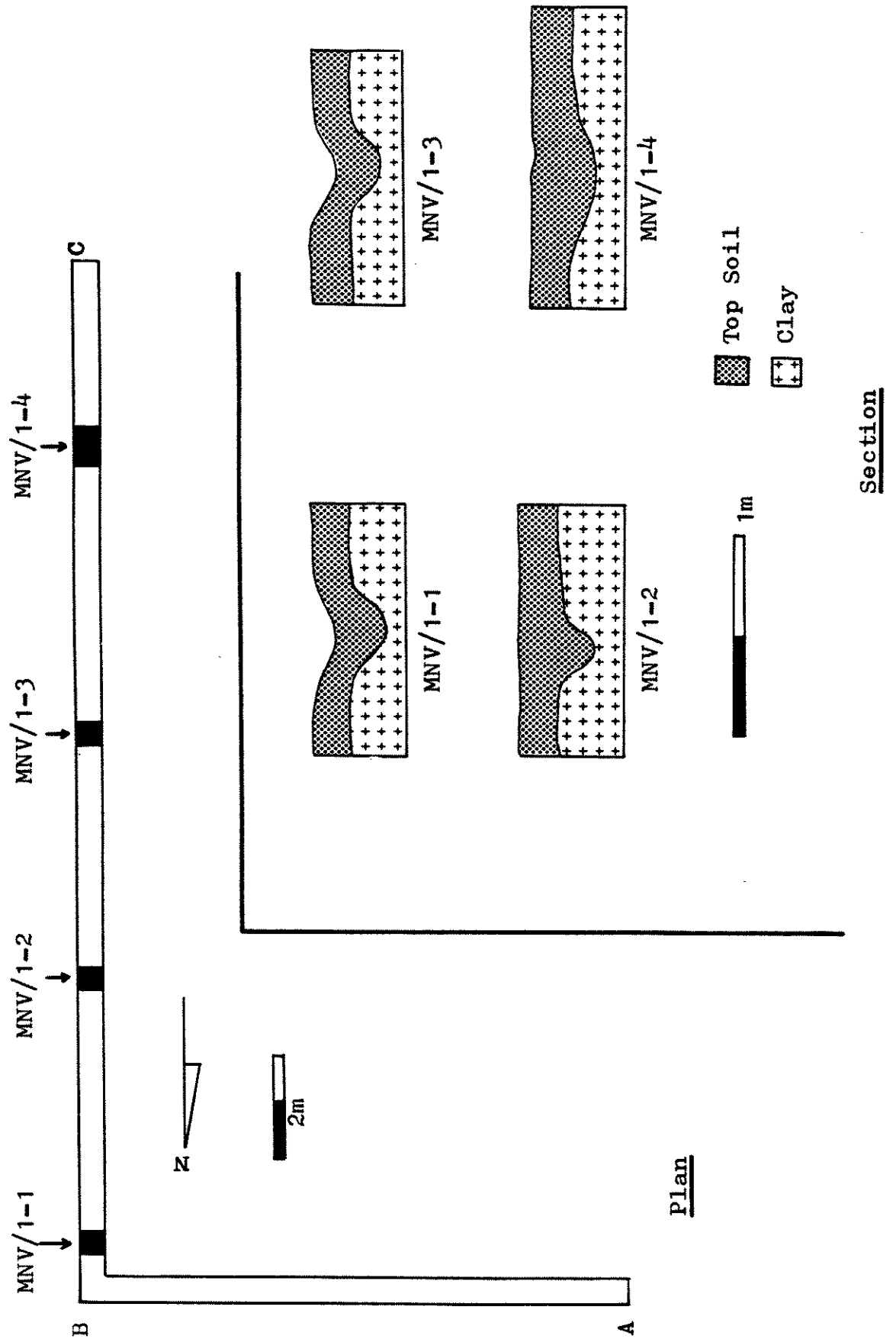


Fig.68. Site MNU/3

Fig.69. Site MNV/1



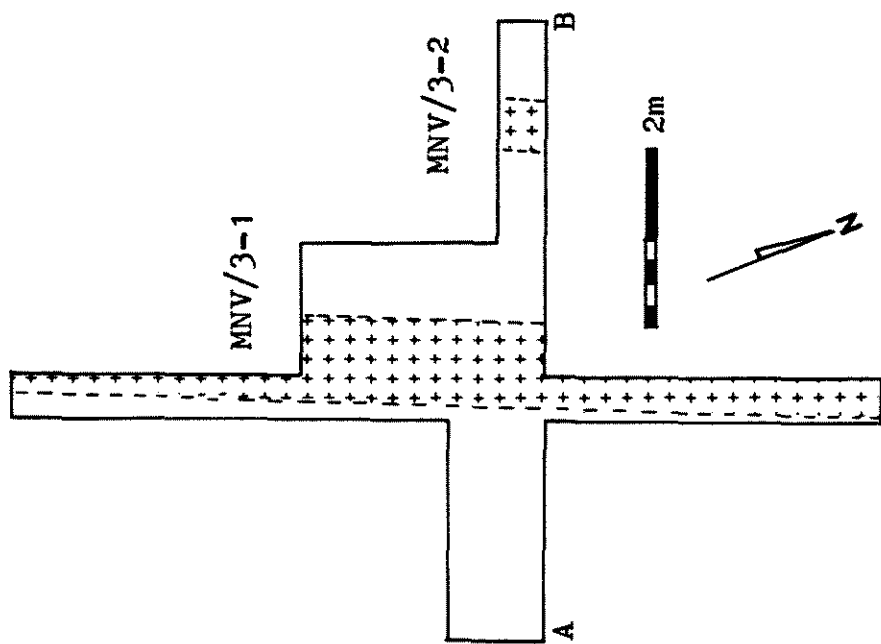


Fig.70. Site MNV/3

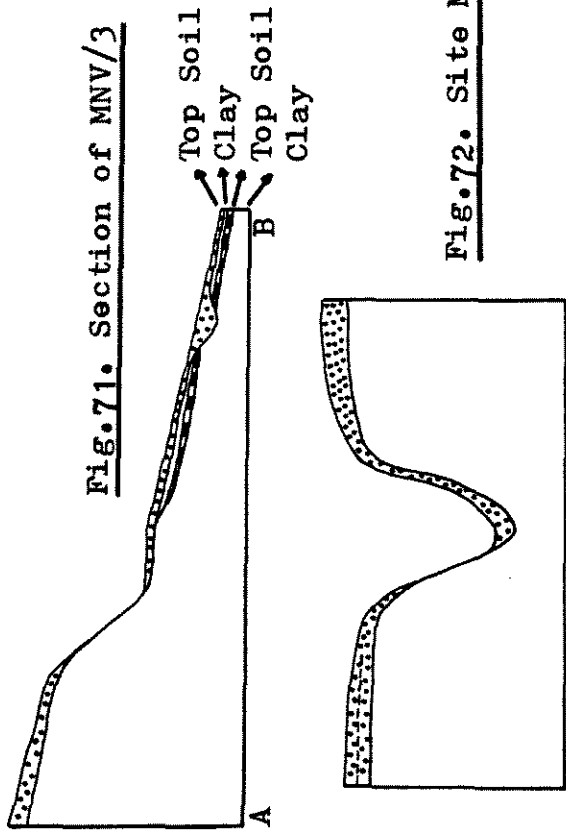


Fig.73. Ditch/Fence Enclosures

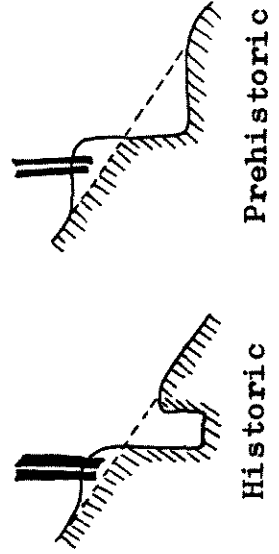


Fig.74 . Site MOA

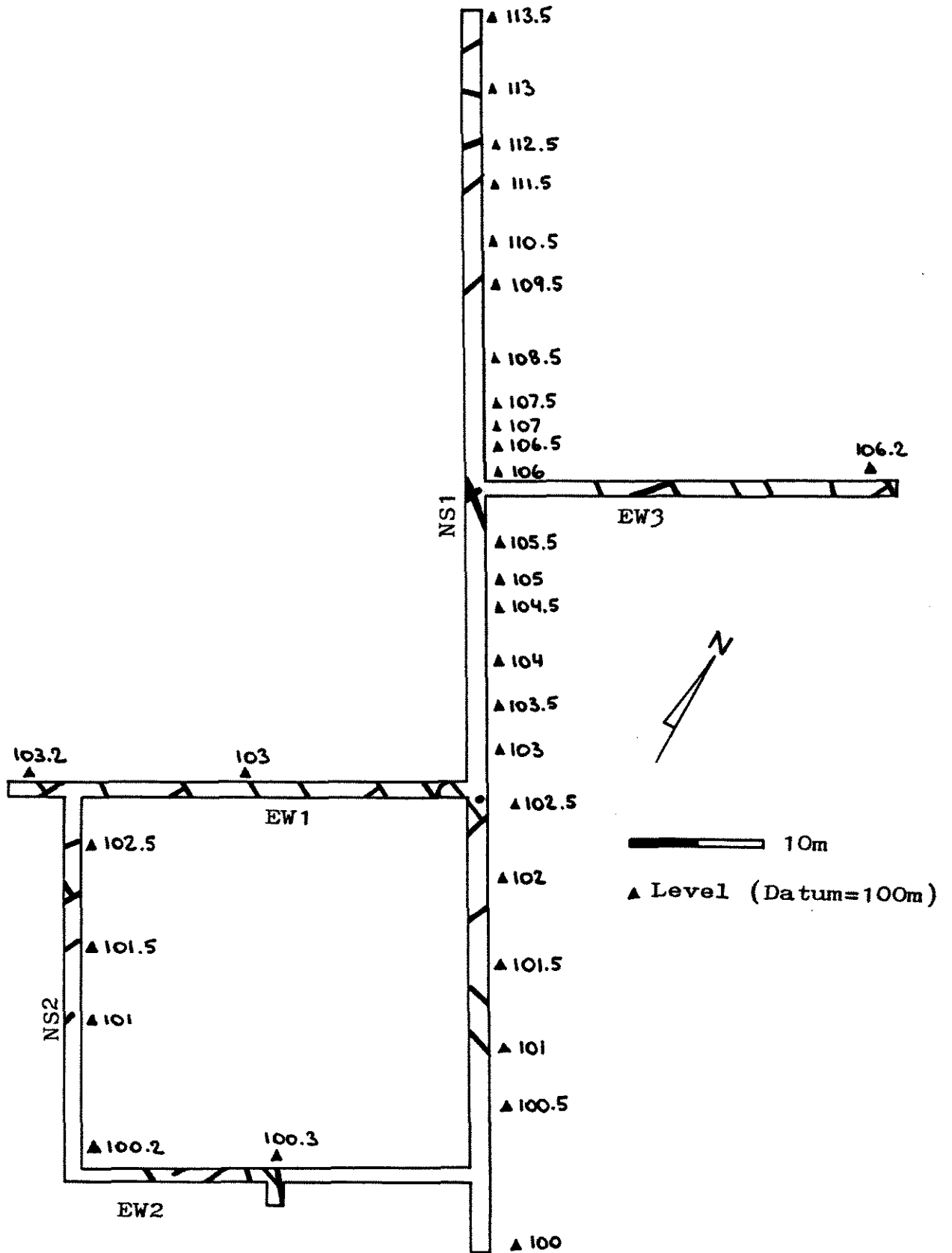
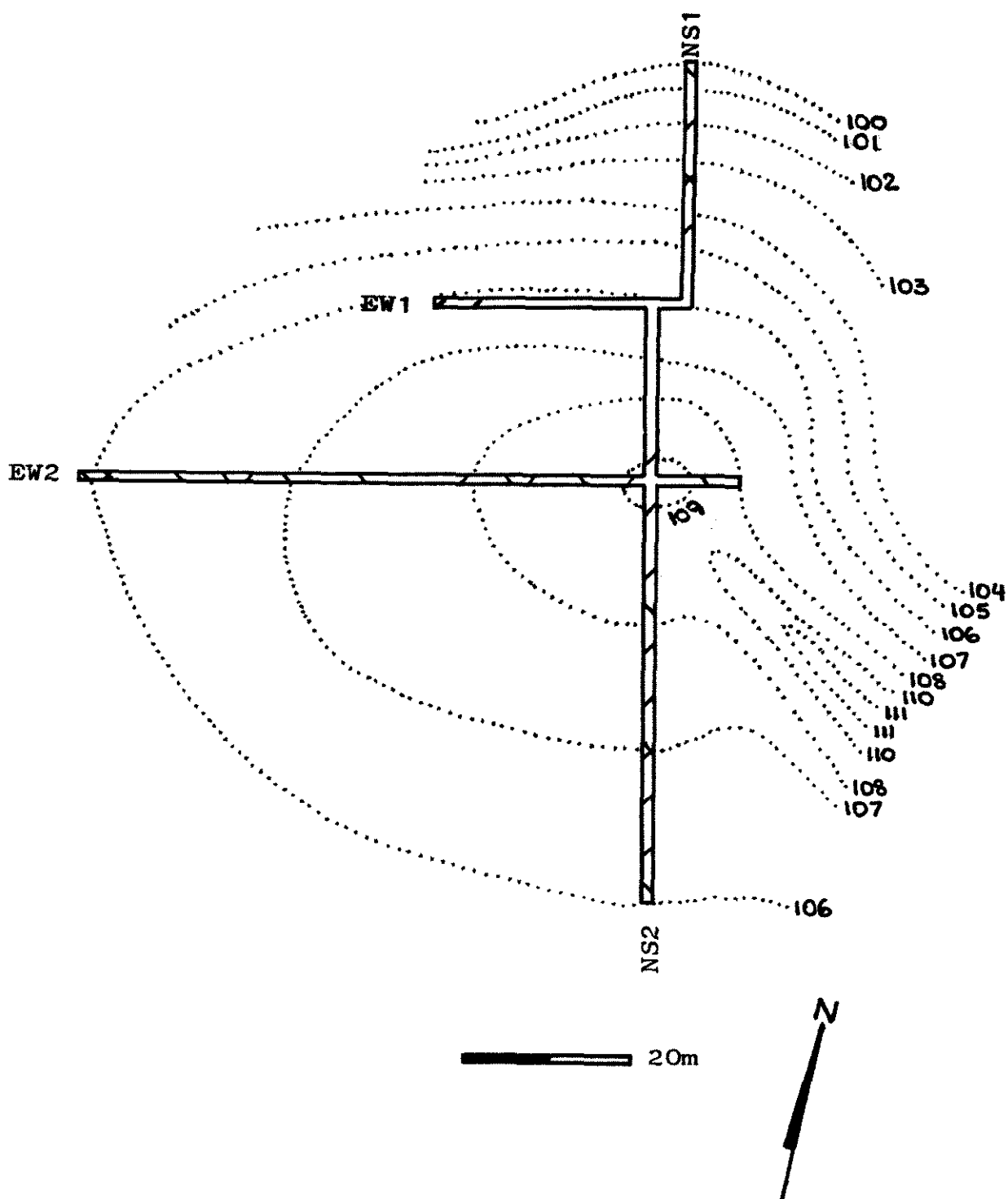


Fig.75. Site MOB



..... Contour Line - Relative Elevations.
(Lowest = 100m)

Fig.76. Site MOC

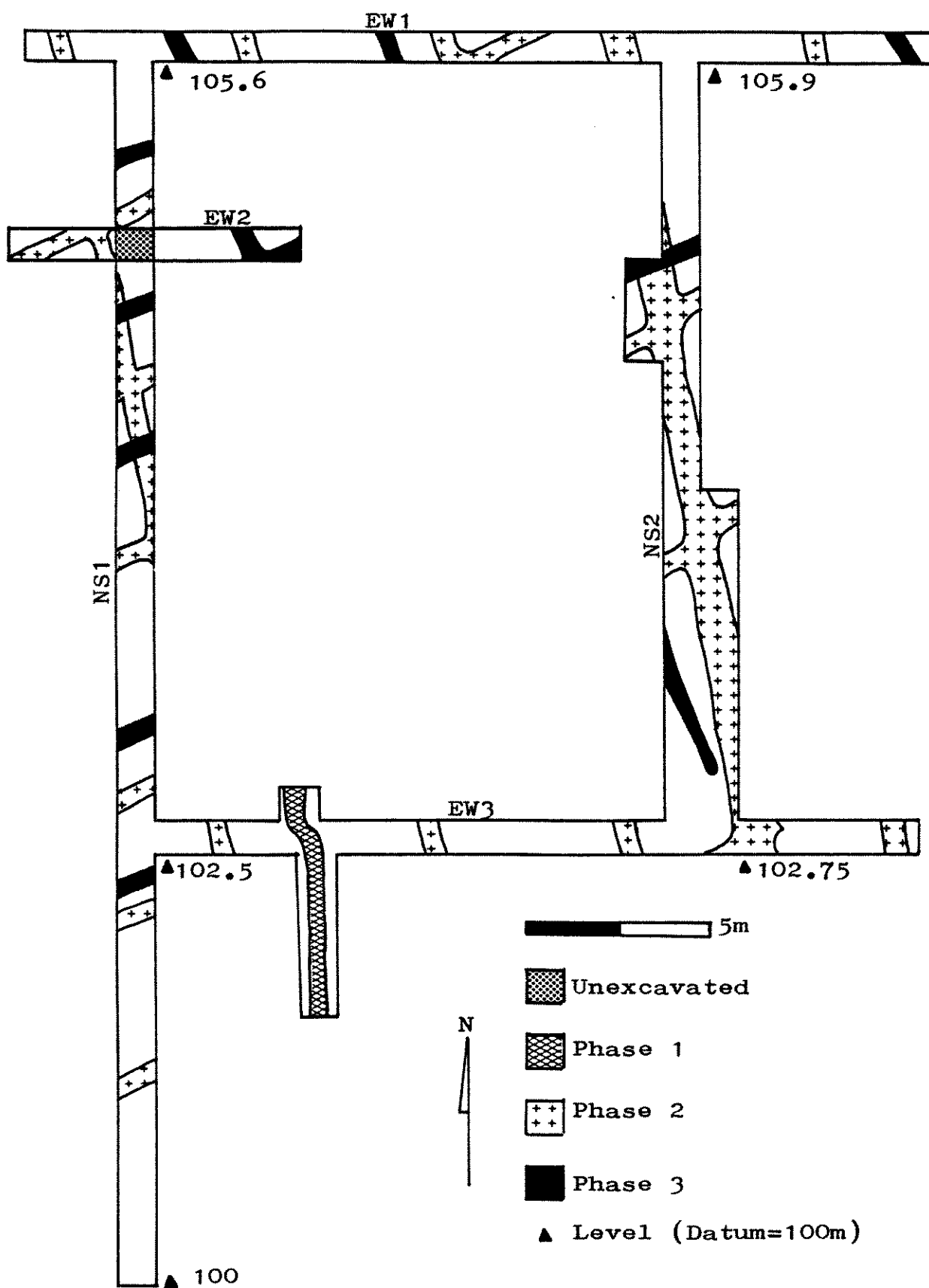
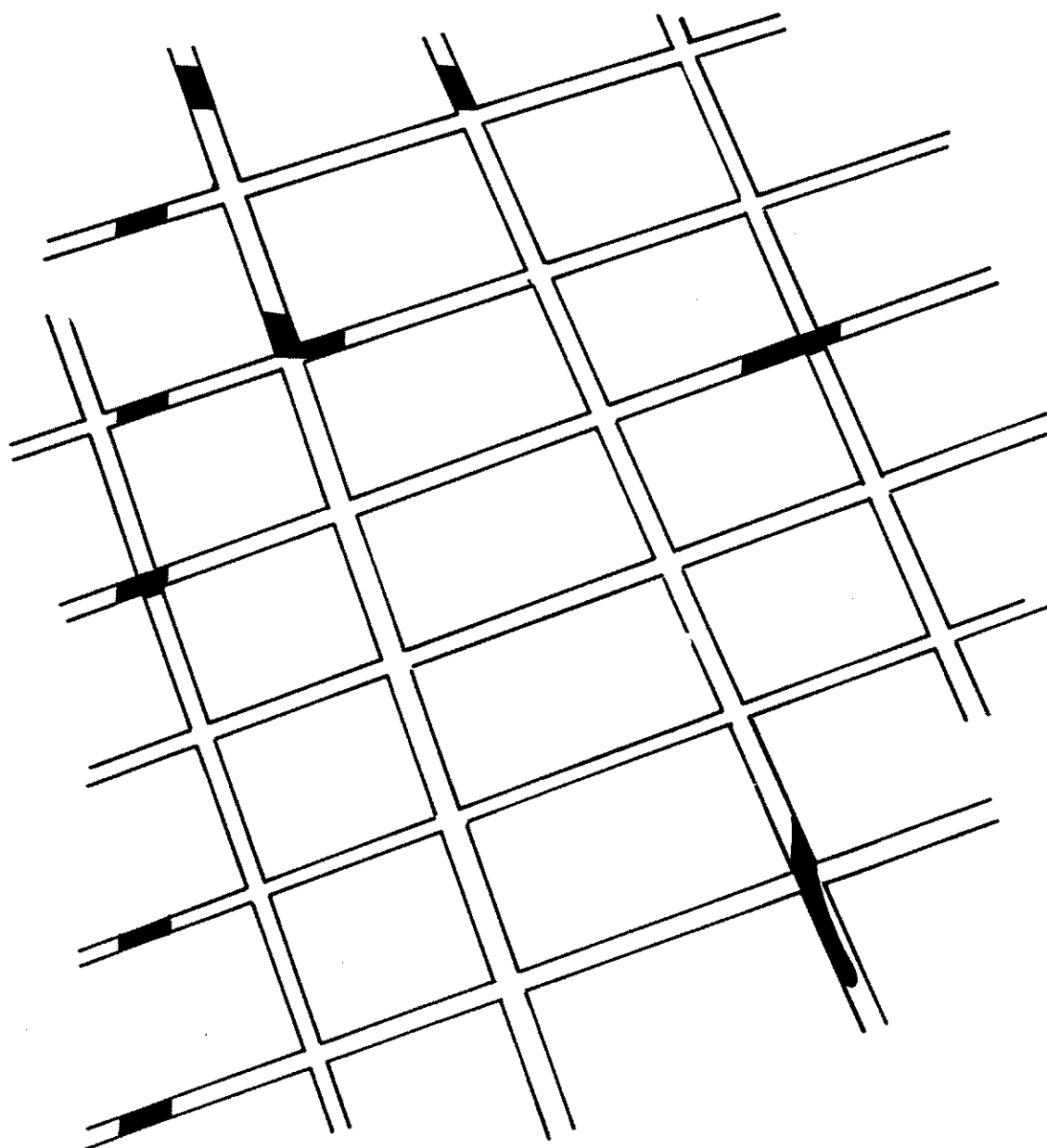
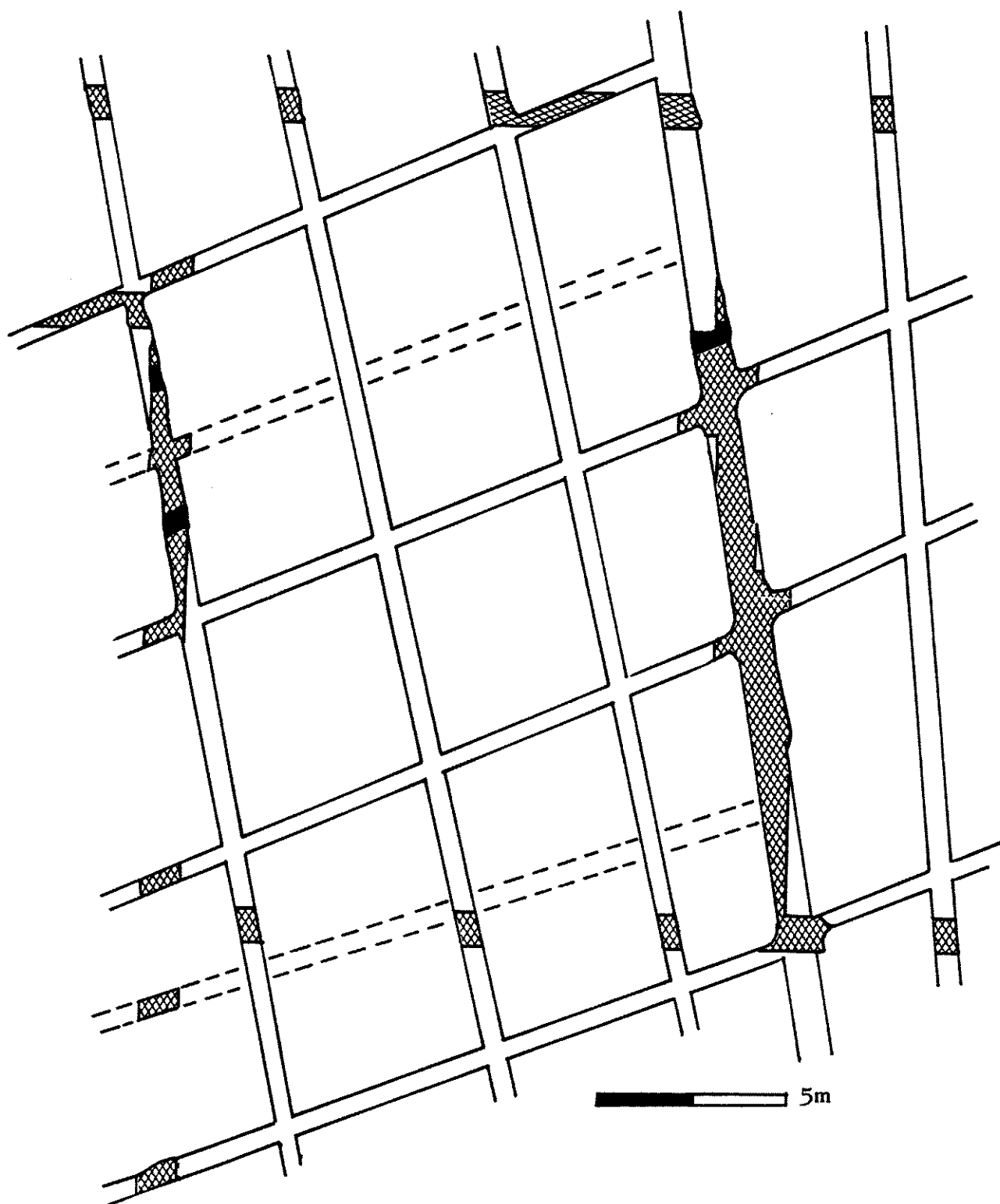


Fig. 77. Site MOC - Phase 3



5m

Fig.78. Site MOC - Phase 2



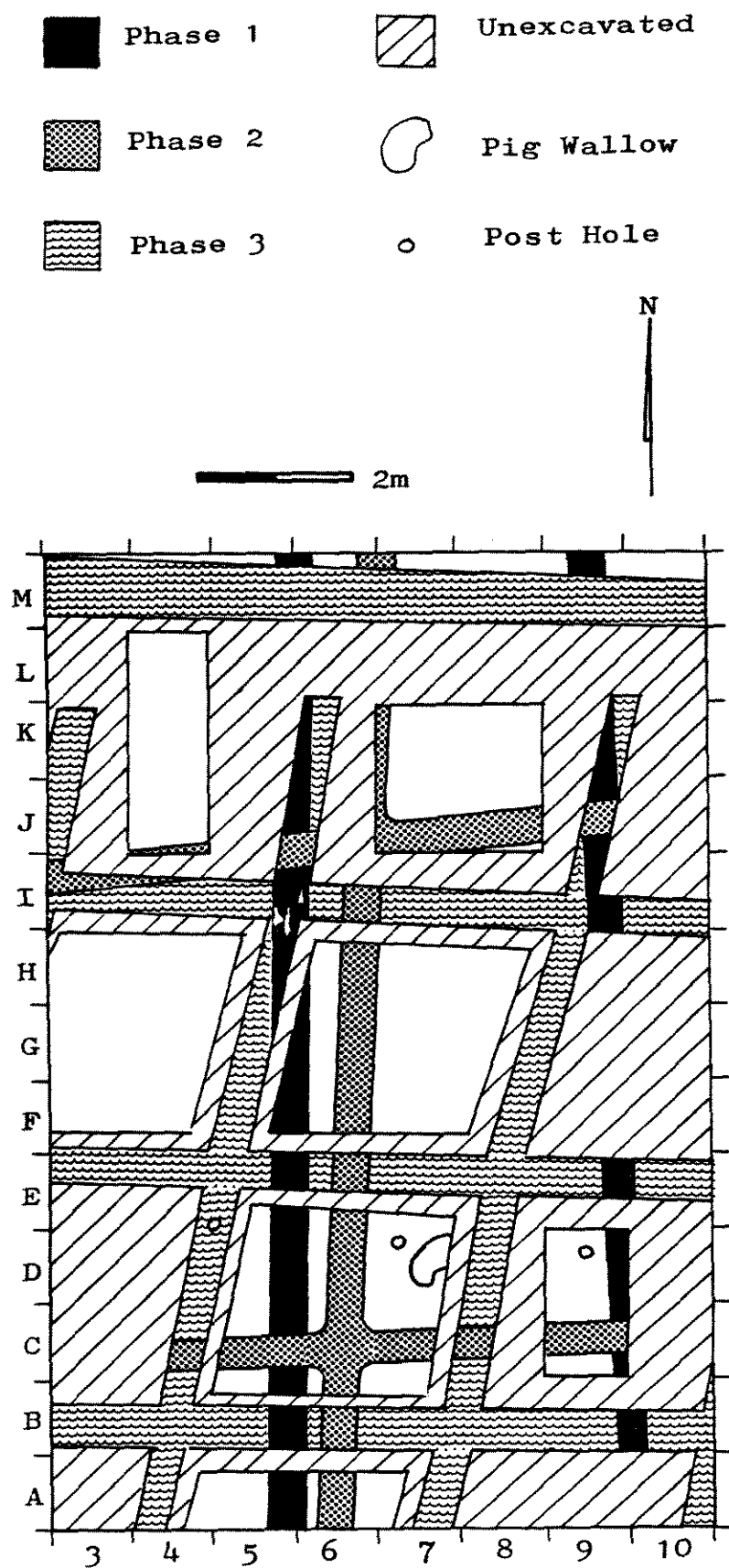
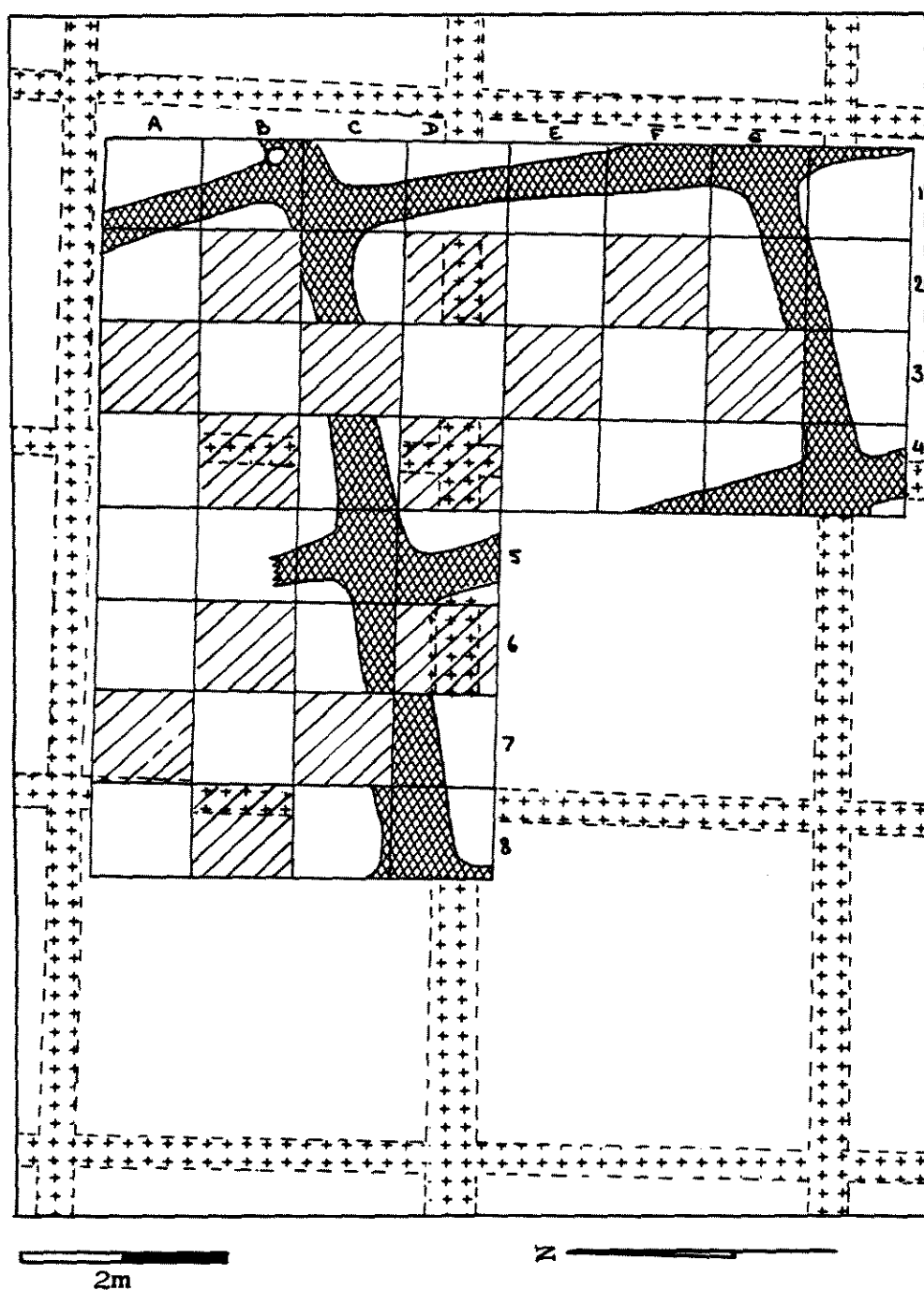


Fig.79. Site MNI

Fig.80. Site MNX



○ Post Hole



Phase 2

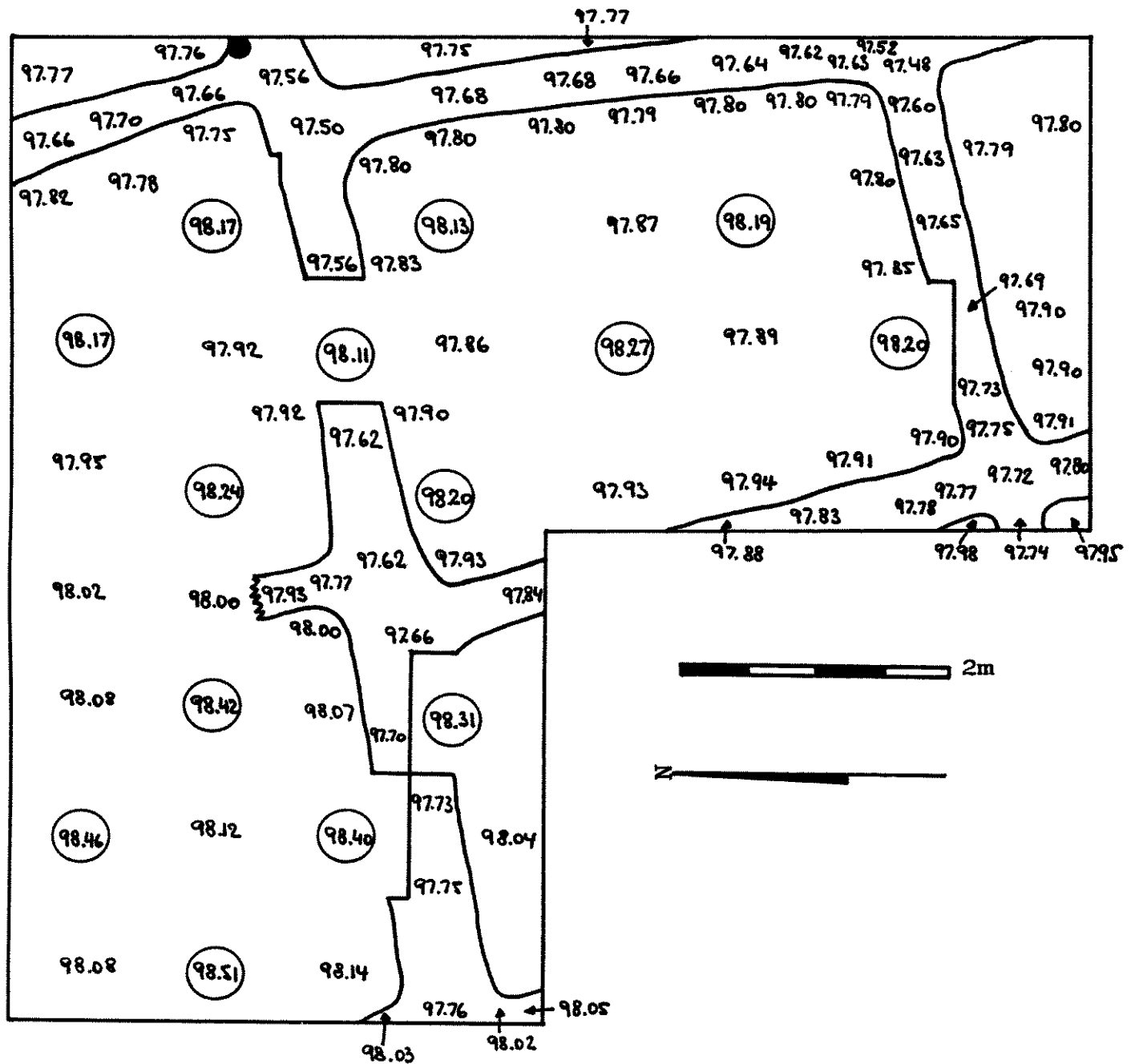


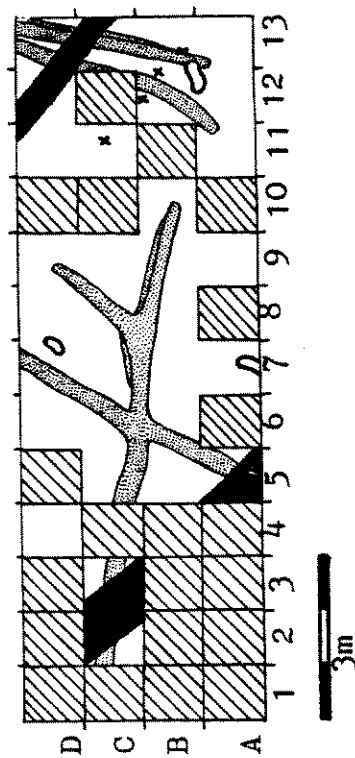
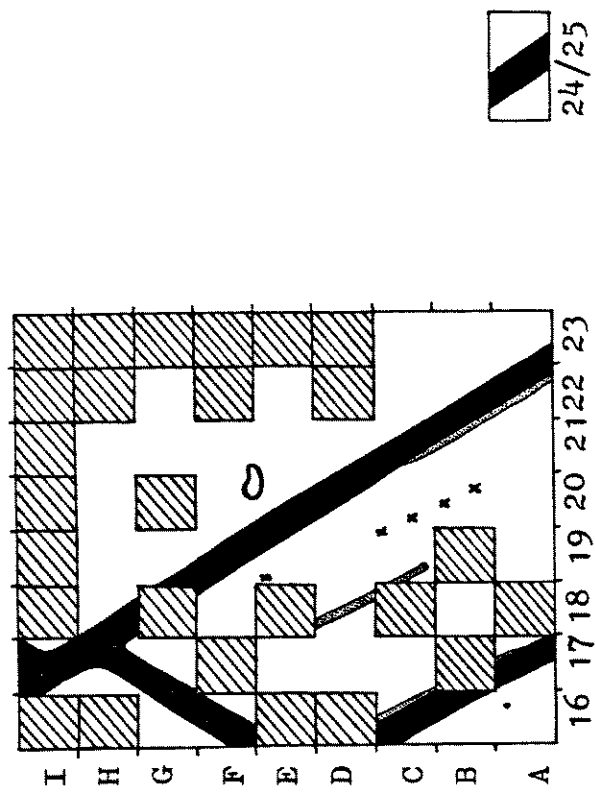
Unexcavated



Phase 1

Fig. 81. Site MNX - Levels





x Post (root) Hole

o Pig Wallow

Unexcavated

Phase 1

Phase 2

Fig.82. Site MNY

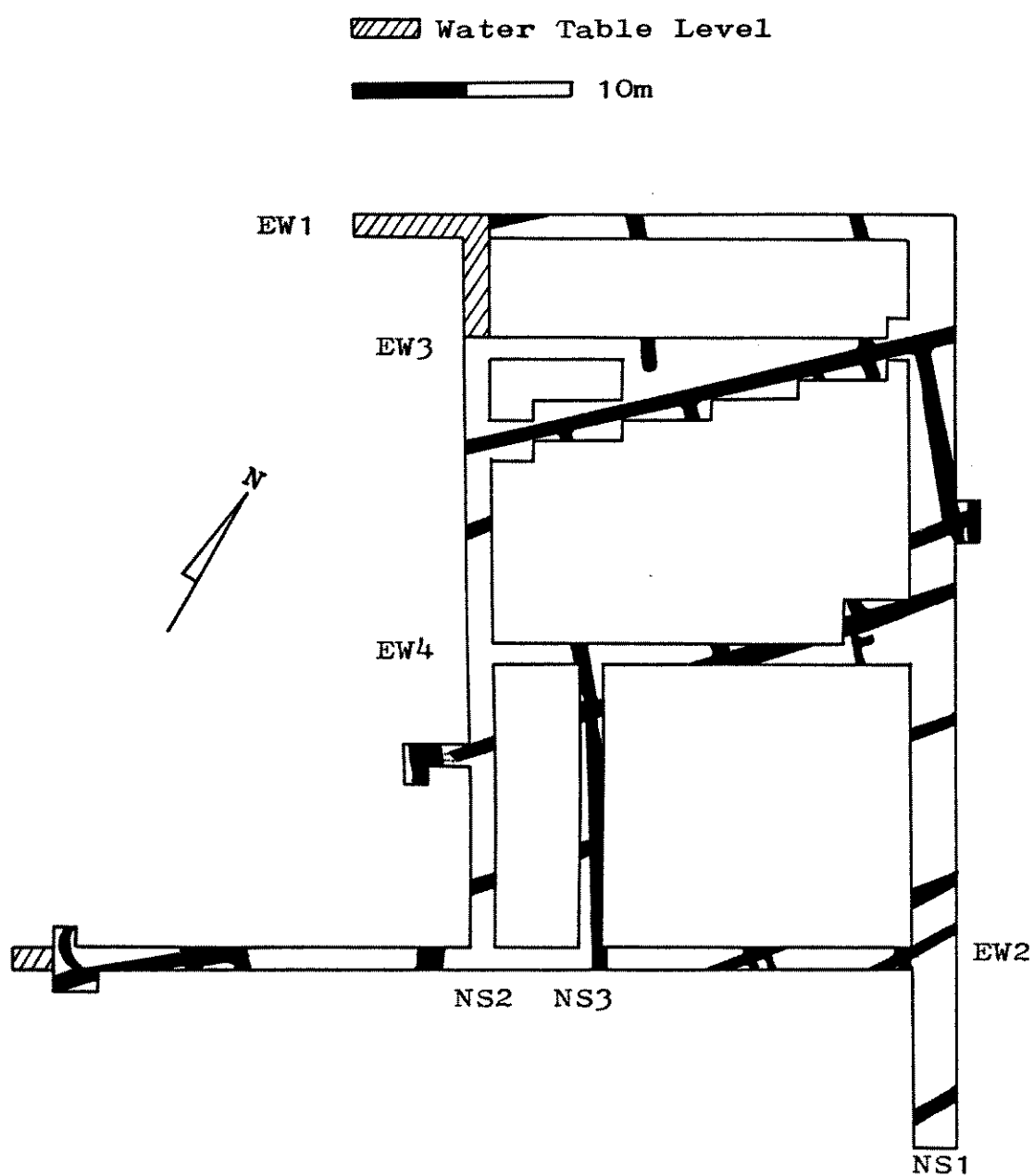
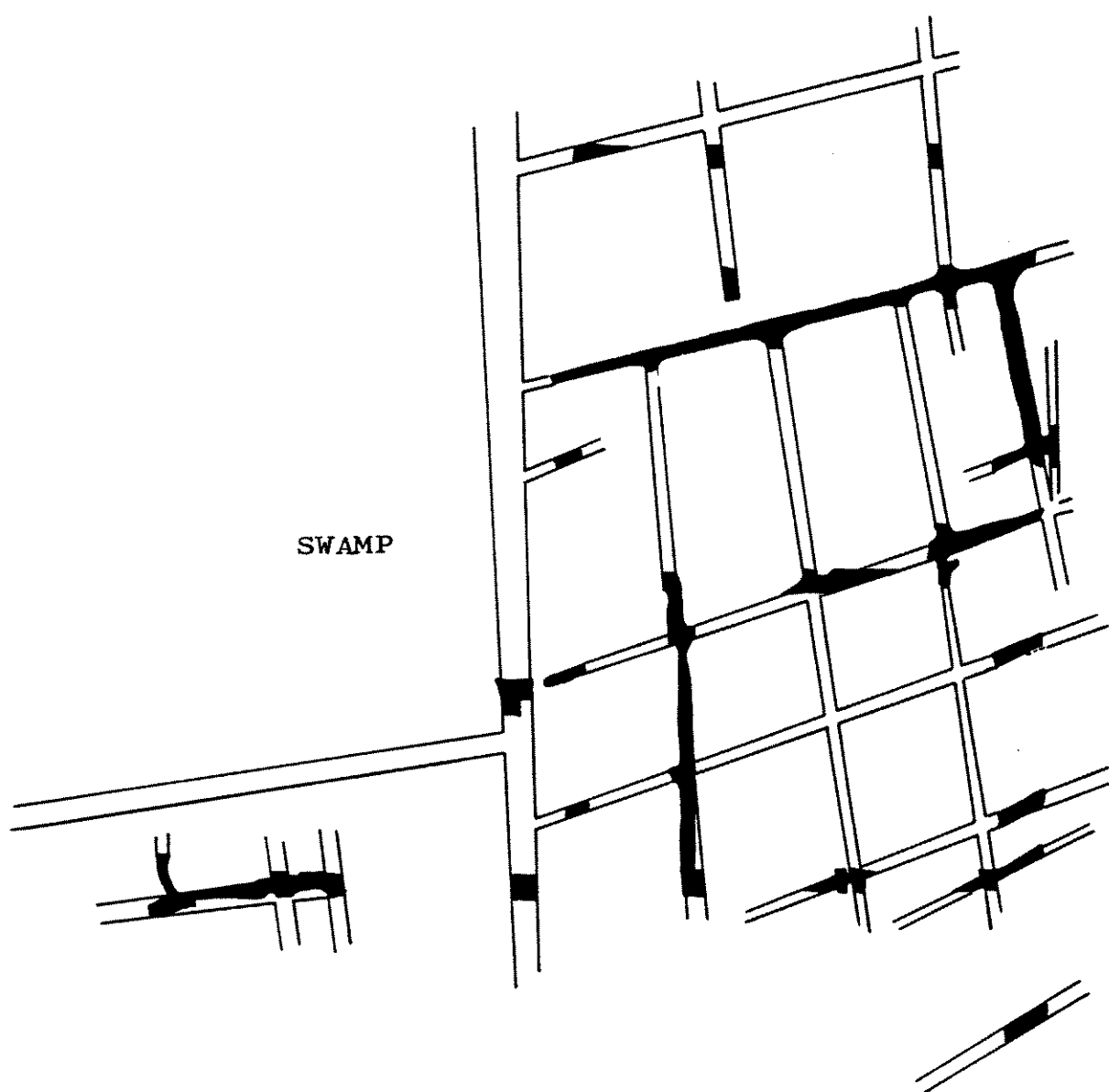


Fig.83. Site MNZ

Fig.84. Site MNZ - Interpretation



UNIVERSITY OF SYDNEY LIBRARY



0000000602024670

