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The Editor would like to thank John Chadwick for his continual advice and encouragement in both the organisation of the Colloquium and the editing of this volume. The breadth of scholarship and wide range of approaches represented at the Colloquium are also characteristic hallmarks of his own remarkable contribution to Mycenaean Studies. We should also wish to acknowledge the invaluable assistance of Mrs. F. Stewart and Prof. H. Kuwahara in preparing transcriptions of discussion sessions.

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1977.

ERRATUM: Figs. 6 - 8 of Ian Hodder's paper which should lie between pp. 33 - 34, have been inadvertently bound after Figs. 1 and 2 of Joan Oates' paper between pp. 106 - 107.

GLOSSARY

ALGORITHM: A name given to the procedure by which a set of equations may be solved, or any computation performed.

CENTRAL PLACE THEORY: This claims to show what the arrangement of cities, towns and villages would be on an infinite isotropic plain(i.e.where no one location is more advantageous than all others possible). The size of the central place is defined by the functions(banking, newsagent etc.)it has, which in turn depend on the critical threshold population needed to support them. Places of the same size form the vertices of a hexagon centred on the next largest central place, which in turn is part of a hexagon around the next level up the hierarchy. The theory has no mechanism for growth, and so is totally static. Due to the assumption of a uniform population distribution the central places have no extra population above that found elsewhere.

CLUSTER ANALYSIS: A general term for a collection of techniques for producing groups, e.g.places that have more in common with each other than externally, perhaps on the basis of their artefacts.

ENTROPY: Derived from physics, in the social sciences it is used as a measure of organisation, or information. Maximum entropy implies total randomness, that is all possible outcomes are equally likely.

GRAVITY MODEL: Originally based on analogy with Newton's Law, but since reformulated as the most probable state, maximum entropy, subject to constraints. Put in terms of migration the interpretation would be that the flows between two places would be a function of their populations and their distance apart. As their physical separation increased, ceteris paribus, the flow of migrants would decrease. The effect of distance is often called distance decay, or the friction of distance.

MODEL: A representation of reality in a simplified form, e.g. a set of equations, or a piece of hardware like a wave tank.

NEAREST NEIGHBOUR ANALYSIS: Developed in Ecology this compares the actual average distance to all n^{th} neighbours against a theoretical value. The result is an index value that ranges from 0 a completely clustered pattern, through 1(random/ Poisson), to 2.1491(dispersed). The results are often hard to interpret.

POTENTIAL MODEL: The summation, at a point, of the gravity model flows to all other places. The population potential of a place would give a comparative measure of its accessibility to the area population.

QUADRAT ANALYSIS: This operates by covering a settlement pattern with a lattice of quadrats, and counting the number of cells with 0,1,....n settlements. The resultant frequency distribution can be compared with various theoretical ones, e.g. Poisson - a random distribution produced by each cell having the same probabilities of receiving 0,1,...n settlements. The probabilities are independent so the number of places in one cell has no effect on any adjacent quadrat.

or, e.g. Negative Binomial - for settlement studies this can be generated in two main ways, both of which relax the independence assumption. Either the pattern could be produced by the growth of offspring outwards from a set of randomly located centres, or by a Poisson model in which the probabilities vary from cell to cell.

RANDOM SPLITTING MODEL: The basic Whitworth model gives the ranked proportions that would be expected if a homogeneous stick of unit length were broken into n pieces. The Cohen version allows for a minimum size for the smallest piece . It is related to the Rank Size Rule, produced by Zipf for city sizes on the basis of his principle of least effort.

REGRESSION ANALYSIS: A statistical method for finding the relationship between two or more variables, or put another way, for fitting a curve to a set of points. The least squares method of doing this requires that the relationships be linear, so variables are often transformed, e.g.by taking logarithms. Trend surface analysis is a form of regression where a variable is related to successively higher powers of the co-ordinates of the points at which it has been measured in space. Except in very simple cases interpretation of the results is very difficult. Due to this, and the possibility of having broken some of the assumptions of the least squares method, like ordinary regression it is best used for descriptive rather than explanatory purposes. The more terms in the equation the more'bumps'the fitted surface has.

REMOTE SENSING: Any technique that detects the existence of something from afar rather than directly e.g.aerial photography.

SIGNIFICANCE LEVELS: When carrying out a statistical test it is never possible to say, e.g.that two frequency distributions are not the same with any absolute certainty. It is always possible that one is dealing with a very unusual outcome of the same generating process, not a different process altogether. Significance levels are arbitrary cut off points beyond which a distribution is said to be significantly different, with a known probability of being wrong. A 99% significance level means that one would be right one time in a hundred, i.e.we are dealing with a very remote possibility of a relationship between two distributions.

THIESSEN POLYGONS: In analysing a point pattern we find the mid-points of lines joining each point to its immediate neighbours. Perpendiculars are then drawn at these mid-points and these are linked up to produce polygons around each point. The result is a series of'cells'or regions such that any location within them is closer to its cell point than any other point. First order neighbours are those points with contiguous polygons.

A. C.

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PREFACE

I have long been interested in the problem of identifying on the ground the numerous place names mentioned on Mycenaean documents, and I have always believed that such studies should be based upon the physical geography as well as the archaeology of the regions concerned. Hence I was glad to learn from John Bintliff of the work being done by himself and others on the Greek landscape in the Bronze Age; and we were intrigued by the possibility that computer studies might assist our researches. From these discussions arose the idea of holding a small meeting at which all with interests in this or relevant fields could meet and exchange information. The result was a very successful Colloquium held in Cambridge at Leckhampton and Corpus Christi College on September 27th and 28th, 1976.

We were extremely fortunate in the collaboration we received from a wide range of scholars: archaeologists, geographers and statisticians, as well as those primarily concerned with the study of Mycenaean texts. It is by bringing together different disciplines that we are most likely to make progress, for our ultimate aim is to understand the whole of Mycenaean society. Moreover, the techniques employed in this field may be useful elsewhere, and the methods used in other areas may have something to teach us. At least the enthusiasm shown at our meeting augurs well for future collaboration along these lines.

One of the people who had given us most encouragement to set up this meeting unfortunately did not live to see it take place. Dr. David Clarke, lecturer in Archaeology in Cambridge and Fellow of Peterhouse, died unexpectedly and tragically during the previous summer. We record our gratitude for his encouragement, and remember that some of the ideas we discussed arose out of his work and example.

In this volume we have printed the text of the papers read to the Colloquium, as subsequently revised by their authors. To these we have added such account of the actual discussions as we were able to make by means of recordings. These are inevitably less carefully worded than the papers, and although where possible the speakers have been consulted, it is possible that in editing we have not done justice to all the interventions; we offer apologies to any who feel they have been misrepresented, and warn against too strict an interpretation of what was said. But we thought it useful to keep the lively style of debate rather than to seek to formalise the discussion.

The Colloquium was held and these Proceedings are published under the aegis of the British Association for Mycenaean Studies. This organisation is a constituent body of the International Permanent Committee of Mycenaean Studies, which is itself a member of the Fédération Internationale pour Les Etudes Classiques. Its activities have heretofore been very limited, since the function of holding meetings has been adequately discharged by the Mycenaean Seminar held at the London Institute of Classical Studies, and the conferences on Aegean Prehistory organised at Sheffield. We hope that by this publication we may contribute something to the development of the subject, and if successful it may enable us to organise future meetings.

All the arrangements for the Colloquium were made by John Bintliff, who acted throughout as secretary and organiser, and has now edited the present volume. We all owe him a great debt of gratitude for the original inspiration and the consequent hard work which have produced these results.

John Chadwick
Cambridge, December 1976

SECTION ONE: ARCHAEOLOGY AND GEOGRAPHY

(Chairman: Dr. P. Warren)

THE HISTORY OF ARCHAEO-GEOGRAPHIC STUDIES OF PREHISTORIC GREECE, AND RECENT FIELDWORK

John Bintliff, Dept. of Archaeology, Cambridge University

Before the end of the 19th. century, fieldwork in Greece was almost entirely of a topographic character, relating visible remains to historical place-names. The French Morea Expedition (Expedition 1836) and Schliemann (1880) were quite exceptional and indeed revolutionary in organizing their field work around a team of scientists, but this example was largely ignored for many decades. Their efforts produced specific identifications of natural and artificial features, but achieved little in correlating past communities with their environment or technology. Another precocious development tied to a particular team, was the exploration and survey work of a group of British archaeologists on Melos and at Sparta, around the turn of the century (see especially BSA, 1896-99, 1906-10); admirably holistic in their interest in every period of occupation in the landscape, and significantly attempting to relate modern peasant life to ancient and prehistoric patterns, they nonetheless relegate geography to traditional comments on the presence or absence of farming land, defensive heights and springs. However on Melos they opened up a new field with practical investigations of landscape change. With Waldstein's collaboration with the geologist Washington on the Heraion excavation, published in 1902 (Waldstein, 1902), we find all too little interdisciplinary understanding and achievement. More promising was the work of Von Gaertringen on Theran archaeology and geography, appearing from 1899 onwards (1899-1909). These early writers we might group as a first major period. In the many important excavations of Carl Blegen (spanning the 20's to the 60's in their publication), we find no advance on this position: a static site environment is presented, described in little more than brief comments on plains, defensive positions, routes and springs, and without expert geographical assistance.

However, not long after Schliemann's 'Ilios', the great geographer Philippson had commenced his lifelong study of physical and human geography in Greece, which after innumerable papers and volumes culminated in the classic synthesis of 1959, 'Die Griechischen Landschaften'(Ed. E. Kirsten, 1959). His example led other geographers - mainly German, to wide-ranging syntheses of archaeology, history and geography in Greek lands. Of this school major publications by Bölte, Meyer, Lehmann and Kirsten span the 1920's to the 1950's (Cf. Bölte 1929; Lehmann 1932, 1937, 1939; Kirsten 1956 and refs.). This development of archaeo-geographers, increasingly co-operating with excavation and survey, might be seen as a second period: it provides accurate information on the physical world surrounding sites at the present day, a suitably common-sense approach to early agriculture, herding and fishing, with much stress on modern analogues, and an independence of thought that looks critically at the accepted opinions of historians and archaeologists. A typical statement is this by Lehmann (1939): "If we attack the problem of how settlements react in their location and layout to the contemporary cultural and geopolitical situation, then we must first of all investigate their natural subsistence base. In the predominantly agrarian structure of the economy concerned (Minoan-Mycenaean) this is the given land area suitable for farming and herding within those sectors of the landscape favoured by nature". However we find a crucial neglect of landscape change, and an uncritical application of traditional tenets and sequences from N.W. European historical geography, - such as a ready recognition of cultural cycles of climax and nadir, and underplaying of important regularities in economic and settlement behaviour, and the continuing importance attached

to historical factors like defence and personal prestige. Such local detail, now available to the informed archaeologist, has had rather a stifling effect on further joint theoretical advances by archaeologists and geographers in Greece. Thus Richard Hope-Simpson, and the UMME team (University of Minnesota Messenia Expedition), the archaeologist and expedition that have probably done most to advance our detailed understanding of Mycenaean geography, in the 60's and 70's, have produced an amazing body of novel information on prehistoric settlement patterns (cf. Hope-Simpson 1965, McDonald and Rapp 1972), yet generally interpret this with models and concepts taken directly from the school of Philippson and stemming with little change from Philippson's youth and the late 19th. century traditions of human and historical geography. These traditions are indeed invaluable sources of theory and practical knowledge, but have become something of a Gordion knot that must be severed to allow us to jump not only into Modern Geography, but into the New Geography. Tentative steps in this direction are taken in parts of the 1972 UMME volume, but the prevalence there of traditional approaches creates a good deal of confusion and inconsistency, both as regards economic and settlement behaviour and the processes of landscape evolution.

On Crete, a team of French geographers co-operating with compatriot archaeologists at Mallia, produced in 1963 a model analysis of the palace region (Et. Crét. 1963), inaugurating possibly a third period. This study is full of new and refreshing ideas and approaches, looking with a critical eye at traditional assumptions of archaeo-geography in Greece, both physical and human aspects. Important is the well-founded attempt to quantify resources, yields, population and territory, with direct comparisons to the modern and archaeological settlement data. More recently, Belgian archaeologists at Thorikos in Attica have employed geographers, but the interdisciplinary direction seems to be lacking, and hitherto we find much detail on the physical geography of the region, recognisably important to the history of settlements, yet not evaluated in any Man/Environment synthesis (cf. Paepe, 1969). From the 1960's to the present day, Colin Renfrew has developed the interdisciplinary approach to excavation and survey in Greece, from the all too common situation where the archaeological report is primarily that with a brief reference to human geography à la Philippson, followed by a number of specialist appendices rather tenuously linked to prehistoric enquiry, to a new field approach - where archaeologist and specialists radically alter each other's interpretations of the central archaeological data, with a thorough examination of the newest and oldest models conceivably relevant to the situation (Evans and Renfrew, 1968; Davidson 1971; Renfrew 1972; Davidson, Renfrew and Tasker, 1976). On Crete Peter Warren's 'Myrtos' volume (1972) and more recent work at Debla (Warren and Tzedhakis, 1974), are in the same adventurous new path, if less ready to jump into the deep end of the New Geography than Professor Renfrew. The same could be said of the ongoing project by Professor Rodden and a team of environmental specialists in the West Macedonia Plain (Rodden 1965; Shackleton 1970; Bottema 1974; see also Bintliff 1976b).

Let us now summarise the state of research in physical geography - the natural landscape of the Mycenaean age (Bintliff 1977b; in full detail 1977a).
GEOLOGY: Is Greece bobbing up and down? Although W. Crete shows quite localised historical uplift, and similar smallscale ups and downs are found on the Mainland and Aegean islands, very generally linked to phenomena such as the Thera eruption and repeated earthquakes at places such as Corinth, a careful study of all the evidence relating to dated coastline formations around the Greek coasts, back to the last Interglacial around 120,000 years ago, reveals that the Aegean coasts over this geologically short period are basically stable. The same analysis for Greece and the Mediterranean rejects that hypothesis, most recently presented at length for the Mediterranean Basin by N. Flemming (1972, 1973), that the sea rose in postglacial recovery till around 2000 BC, subsequent to which all apparent changes in sea-level observed archaeologically

are attributed to tectonic effects. The coastline evidence, taken with dated cores of sea-level in the Macedonia and Pamisos Plains, strongly favours the Eustatic Hypothesis, presented for Greece and the Mediterranean by several authorities, notably Lehmann-Hartleben (1923) and Hafemann (1960). In this scheme, as modified by the present author, sea-level has risen on average by 1 metre per millennium over the last 6000 years or so, Greco-Roman coastal sites are c.2 metres, Mycenaean $3\frac{1}{2}$ metres lower relative to the sea than originally. Physical science further demonstrates that vertical tectonic pressures as postulated by Flemming for most of the Aegean coasts are not at all widespread. GEOMORPHOLOGY: However, if we look at plains and river-courses, we find very dramatic changes within the last 2000 years, as far as sediment history is concerned; yet before Roman times, very little of note occurred there until we reach the last glacial period. My own work, applying in detail the general geomorphological study of C. Vita-Finzi on the Mediterranean valleys, confirms in numerous regions of Greece the validity of his dual alluviation scheme (Vita-Finzi, 1969, 1976; Bintliff, 1975, 1976 a, b)[1]. After major alluviation during the last Ice Age, producing the bulk of the sediments now flooring plains and valleys of Greece, only limited and agriculturally low-value sediments were deposited above the sea in subsequent millennia until Roman and Medieval times, when renewed alluviation produced fresh, extensive and immensely fertile alluvial zones on the lower plains and alongside watercourses. Analysis of possible causative factors argues climatic fluctuation, ruling out influence from sea-level change and erosion of the land by human activities such as deforestation and ploughing. Indeed, a re-examination of the erosional evidence for the Greek landscape suggests that this factor has been vastly overemphasized. Erosion is slight on good farming and building land, more substantial on less valuable steep and rocky slopes; the long-term effect of erosion on the Greek landscape can be considered to have been beneficial, since the land of least value has in the main created in the two alluviations, most of the land of most value today.

VEGETATIONAL HISTORY: We are hampered by a preponderance of pollen sequences from North Greece (Bottema 1974, Turner and Greig 1974), with only two detailed examples from South Greece (Wright 1972, Turner and Greig 1974), but these are consistent with a whole series of sequences from the general Mediterranean/Near Eastern zone. The overall picture for Greece and the wider area, shows substantial woodland clearance occurring only late in the prehistoric period, and even then almost always confined to clearance of important lowland arable land for cultivation purposes. Evidence for broader deforestation is generally lacking until a very recent period, in Greece usually within the last 150 years, and it seems likely that even the early arable clearances produced no significant degeneration of the land concerned, for in many cases this undergoes cycles of reafforestation and clearance until the Modern Period, and in most instances the land concerned seems to be fully fertile today. However, although this sequence accounts for considerable areas permanently free of woodland in Greece today, it is too infrequently pointed out that key areas such as the southeast Mainland and Aegean islands are climatically and pedologically unsuited to extensive tree growth and over much of their surface have always possessed their present barren or scrub-covered character. The recent Copais core, though claiming to demonstrate widespread and indiscriminate deforestation from Early Bronze Age times, with drastic effects on landscape fertility (Turner and Greig, 1974), seems rather on careful examination to document the clearance of the (still highly fertile) basin soils by early farmers, while the poorer-soiled hills around have always possessed but scanty cover.

SOILS: The dual alluviation scheme requires a dramatic rethink of any regional Greek landscape, for much of it is likely not to have been there in Mycenaean times, and that bit - the recent alluvium, is today widely determinant for settlement and agriculture. Remove it and plains and river valleys consist largely of the first (Ice Age) alluvium, not an ideal soil for prehistoric

farmers. More rewarding if less extensive soils lay on the hill and piedmont zones that characteristically in the Mediterranean intervene between bottomland and mountains - soils chiefly developed from Tertiary marls, sands and Flysch. These considerations, and not those generally popular previously, - defence, water supplies, soft rock for building and burial, account for the siting of almost every prehistoric and early historic site in Greece on or beside such hillands rather than on the full plain or totally amid rocky zones. Exceptions are very rare and can usually be found to occupy harbour sites and/or dominate key fishing grounds. The Roman-Medieval alluviation brought a remarkable shift of settlement and economy in every part of Greece. This conclusion requires considerable revision of most of the archaeo-geographic work in Greece already cited, including UMME, although continuing field research by Profs. Kraft and Rapp is now persuading the Minnesota team of the validity of this sequence.

Finally we should note that vegetational and geomorphological evidence as discussed above is almost certainly too insensitive to record minor climatic fluctuations, such as the drought in Late Mycenaean times postulated by Carpenter. In fact from the normal Mediterranean climatic cycle several serious droughts might be predicted for Greece in the time period covered by the Mycenaean era.

Let us now turn to the problem of archaeological surveys. Greece has been well-served in surveys for prehistoric sites, beginning with pioneers such as the BSA Melos team, Wace and Thompson (1912), Arvanitopoulos (1916), Xanthoudides (1924), Pendlebury (1939), Schachermeyer (cf. Lehmann 1939), Heurtley (1939), followed by later scholars such as Kirsten (1956 and refs.), Wroncka (1959), and Vermeule (1960), the Peneus Barrage Survey. But a veritable watershed is met with in the fieldwork of Hope-Simpson. His indomitable energy and eye for certain types of site have single-handedly changed major features of the map of prehistoric Greece, most notably in Laconia, and with others in Messenia and the Dodecanese (Hope-Simpson & Waterhouse 1960, 1961; Hope-Simpson & Lazenby 1962, 1970 a,b, 1973; Hope-Simpson 1965; McDonald & Hope-Simpson 1961, 1964, 1969). He initiated a new era of systematic survey, which developed very much under his influence. Major achievements have been Hood and Warren in W. Crete (1964-7), Sackett et. al. in Evvoia (1967), French in Macedonia (1967) and Thessaly (in press), Howell in Arcadia (1970), Renfrew in Thrace (Davidson 1971) and the Cyclades (Renfrew 1972), Jameson and Jacobsen in the Argolid (in prep.; see Bintliff 1977a), Branigan and Blackman in S. Crete (1975 and in press; see Bintliff 1977a), Papadopoulos in Achaia, Watrous in C. Crete (1975). Yet we have already suggested that the Hope-Simpson interpretation of settlement patterns rests on a traditional model derived from Philippson. This model severely controls the nature of a Hope-Simpson survey, and in particular it stresses the larger centres, the strategic acropolis site, at the expense of the lower levels of the hierarchy, both lower in population, status and function as generally in topography. Thus in the Sparta Plain we recognise major Mycenaean centres spaced at regular 5 km. intervals, but only scattered traces of smaller and satellite communities predicted from more intensively-surveyed areas. In the UMME Survey, where exploration was explicitly aimed at identifying prominent acropolis hills as Mycenaean centres that might feature in the Pylos texts or in Homer, the same bias towards acropoleis is carried forward on a massive scale of survey. It can be demonstrated from UMME's own statistics that in comparison to previous local work and more intensive surveys elsewhere, they were very good at finding such sites but much less successful at finding smaller and lowlying settlements - although these appear to be characteristic for many periods (Bintliff 1977 a,b). In the Helos Plain, where acropolis hills are very rare and where examined were generally unimportant sites, Hope-Simpson was led to consider the lowland areas, and discovered a remarkable number of lesser sites of all periods. A similar bias, though less pronounced, can be detected in the Evvoia Survey, but in the surveys of Hood and Warren, French, Howell and Renfrew, much more attention is given to

identifying a wider range of communities and locations. In the recent S. Crete, Argolid and ongoing Melos surveys we find the ideal pattern of intensive search of a defined area, devoid of a priori considerations of where sites ought to be, and equally important - what a site should be.

In the S.W. Argolid, as generally throughout Greece, the preliminary dot pattern of settlement sites taken period by period shows few Neolithic, Middle Helladic and Dark Age sites, many Early and Late Helladic, late Geometric, Classical and Late Roman sites. Colin Renfrew (1972) has elaborated on this cyclical pattern, interpreting it as others before him as evidence for the rise and fall of population and the linked rise and fall of sophisticated cultures; exceptional areas to the general trend are Crete and Messenia, where cultural development and population growth are considered to be progressive. Yet in the Argolid our few N, MH and Dark Age sites are regularly spaced units located over the landscape in a very similar pattern to the traditional nucleated village network in the same area. But many EH, LH and C sites are often a dozen or more per modern village territory, and often clearly represent a single farm or small hamlet. We know all too little about the size of N and MH settlements, but in the Argolid and in individual well-explored examples in the rest of the Mainland there is evidence for communities larger than the average EH or C site, especially in MH (cf. Mycenae, Berbati, Nauplion, Asine, Argos, A. Stephanos). Furthermore, it is striking both in the Argolid and other areas of the Mainland, that the few locations of low density periods frequently become the important centres in high settlement density periods. This has led me to suggest (Bintliff, 1977 a,b) that a major factor being recorded in the well-known increase and decrease cycle was a cycle of dispersed then nucleated then dispersed settlement, in which the overall population and contemporary intake of land need not necessarily have fluctuated between extremes, or even very greatly. For many of the widely-spaced communities could have been nucleated villages as in the presentday landscape, farming intensively up to $2\frac{1}{2}$ km. radius of land, whereas the dense-packed settlements of so-called flourishing periods would have farmed the same land but from many small units right beside individual holdings. I would ask us to re-examine the case for gross population flux, then, in the standard comparison of EH and MH, LH and Dark Age. I would agree that having allowed for this settlement behaviour factor, there exists a residual that suggests a population standstill or decline for MH after EH, Dark Age after LH, - but the magnitude of this has been greatly exaggerated. Likewise, and more predictably, a rise in population from N to EH, and from MH to LH is still observed after due consideration of the size/density factor, particularly in the latter case; for in contrast to the contraction of numerous small EH sites into large MH sites, LH (at least LH3) possesses both nucleated communities and small dispersed sites. Furthermore, it seems clear that the range of functions served by at least the larger Mycenaean centres outstripped those of MH nucleations - the comparison is perhaps between a large rural village and a provincial market town.

The picture is only slightly confused by evidence in MH of a settlement pattern consisting of wide-spaced (and usually acropolis) centresimmediately surrounded by satellite sites (as O. Dickinson has shown e.g. in the Argos Plain and at Athens, 1970; on the Evvoia Survey we also see that almost all the non-acropolis MH sites cluster closely below acropolis sites). This 'nucleation-suburb' model is quite different to the continuous scatter throughout all the fertile land and beside every bay characteristic for EH and to a lesser extent LH settlement. It may also help to account for the results of A. Chadwick's Nearest Neighbour analysis in Messenia (see this volume), where the average of all First Order Neighbours in MH is agreeably close to the average distance between all LH sites estimated at 100 or more occupants (range 5-6 km.), but where the individual Nearest Neighbour is at average distance of about 3 km. The latter figure is possibly a combination of the overall 6 km. spacing and acropolis satellites frequently much less than 3 km. from their node.

In the Cyclades the contrast between high numbers of Early Cycladic sites and low numbers of Middle Cycladic was already accounted for by Scholes in 1956, as reflecting the change on the islands from scattered small settlements, via synoecism, into one or two island 'townships' that are clearly more populous and sophisticated than the settlement norm in EC, and very much on the lines of the traditional island settlement model. I have been able to confirm this in my studies on Melos and Mykonos (Bintliff, 1977 a). In Late Cycladic, again paralleling the Mainland, there is some evidence on the islands that a combination of nucleated and dispersed sites demonstrates overall population growth.

How do we account for the apparent uninterrupted growth patterns of Messenian and Cretan settlement? In Messenia, we can demonstrate that there is a survey bias towards acropolis sites, and these are recognised throughout Greece as primarily favoured for occupation in MH and LH. Lowlying and generally less extensive sites are also known to be typical for EH, N, and satellite settlements in LH, - poor recovery of these sites gives but a fraction of what we believe to be very numerous settlements of this kind. Good acropolis cover, poor lowland and small site cover gives primacy to LH, then MH, lastly EH then N. Neolithic is poorest represented, as we suggest, not only because such sites are frequently lowlying but are fewer, more widely-spaced than EH and of very variable size. One can point by way of contrast to the density of Neolithic sites found by Blegen in the Corinth region - he was looking for such. On the rather more balanced Evvoia Survey we find the following interesting statistics: EH sites, about 50, of which 27 are known to disappear in MH - 20 of the drop-outs are lowlying; of the 15 EH sites known to be retained in MH, 13 are described as acropolis sites, 1 a slope site, 1 lowlying; of the full MH pattern, including new sites, almost every example of a lowlying site is in close proximity to an acropolis site; LH as expected has a clear balance of settlement in both topographical zones. On Crete we cannot believe that the island population confined itself to the handful of sites registered before late and sub-Neolithic times - and we have about two and a half thousand years of this situation from the first settlement of Knossos. During this long era settlement must surely have spread and multiplied throughout the island to numerous locations as locally favourable as Knossos and Phaistos, yet hitherto unrecovered. The rise of known sites in Early Minoan times is to a large extent due to the ease of recognition of built-tombs of this period, although these are usually without recorded associated settlement finds. Study of a major group of these tombs, the Messara tholoi (Bintliff 1977 a,b), suggests that they represent a similar pattern of dispersed smallholdings and farmsteads to that which we have postulated for the Early Bronze Age Mainland and Cyclades. Where these types of tomb were not the customary burial form, such small settlements are rarely discovered, though there are also traces of nucleated village communities about which we are naturally much better informed. With the full development of Minoan palace civilisation in the Middle Minoan period, a much increased population can be affirmed, for as on the Late Bronze Age Mainland and Cyclades rural lower levels of settlement are well-evidenced as well as expanding nucleated centres. On Crete then we might consider the overall trend as comparable to the Mainland sequence, after the removal of MH, i.e. N, EH, LH.

For the particular interest of the Mycenaean period it is clearly of importance to know more of the relationship between the better-known settlement pattern of LH3, climax Mycenaean, and early LH, late MH in each region. We have suggested that MH could be more comparable in population to EH, and to a lesser extent to LH, than is generally recognised. It is highly desirable for future research to establish the true size of MH settlements, of which so little is known - hints only from a few sites such as Eutresis, Malthi, Lerna, suggest a favourable density in MH, in fact predominating over LH. Perhaps significantly in the latter two examples we might argue that the MH population has partly dispersed in LH into the adjacent arable territory. It is certainly unwarranted for UMME to compare MH sites with EH/LH sites, merely on grounds of overall site

numbers, until we know more about settlement size; their assumption that the average MH site is half as populated as that of LH, is unfounded and in my field experience improbable. I leave more knowledgeable comment on this important MH/LH transition to Oliver Dickinson, for it is now clear that we know all to little about MH settlements. Even more valuable will be his comments on early LH. Surveys seem to have difficulty finding this period (UMME record in Messenia c90 MH sites, 190 LH3B sites, but a mere 20-30 for LH1, LH2 and LH3A), but allowing for this the resultant picture suggests that early LH continued the broad lines of the MH settlement pattern until a sudden explosion of settlement in LH3B - firstly at centres, which now grow prodigiously in size (e.g. Sparta Plain, Argos Plain major sites, also Zygouries, Pylos), secondly in the countryside around which now fills up with satellite settlements. Is this sequence realistic, and if so, what caused this phenomenon? It might be absolutely central to the origin of Mycenaean civilisation. However a strong caveat - undeniable bias against the recognition of early LH material could well be concealing a more gradual development of the settlement hierarchy. It may certainly become significant that there is in general a negative correlation between local tholos construction and this great shift in settlement pattern and population.

If we consider a series of maps of Mycenaean regional settlement, incorporating soil and coastal change data (cf. Bintliff 1977 a,b), we notice first the priority of hill soils and coastal locations. Both zones are admittedly close to local watertables, but this element seems in no way as crucial to explain distributions as are soil accessibility and shore facilities. Prehistoric, ancient and modern Greek communities not infrequently lie at some distance from water sources and rely often to a substantial extent on cisterns. As for defence, of a large number of suitable strategic heights in any region, suspected prehistoric citadels occupy only those on or beside key preferred arable and marine zones. Lehmann had long ago recognised the characteristic Mycenaean hilland location, but misinterpreted it as comprising (a) a limestone height for defence (b) soft hilland rocks for burial and springs (c) plain below for agriculture (1932, 1937). In these terms it is not surprising that he portrays the plains of Greece as indicating the centres of prehistoric culture, without commenting on the anomaly that southern Greek plains are far smaller than those in the north, yet north and west of Boeotia and the Thessalian coastal strip we find no high cultures to compare with southern Greece in Early, Middle or Late Bronze Ages. The answer seems to lie in two major factors. Firstly, as Colin Renfrew has reminded us (1972), in the restrictions on olive growing in north and much of west Greece - apparently a crucial element in Greek civilisation (and here the minor role played by prehistoric Arcadia is of interest, given its limited olive potential). Secondly we generally lack broad expanses of fertile hilland soils in the large plains of north Greece, as are common in the south (and hence the locations of Pylos and Knossos, inaccessible to plains but dominating high quality hilland soils). Now these considerations adumbrate once more the now indisputable overriding concern of Mycenaeans and Minoans with agricultural production (and in this I include herding, wool and flax , and fishing), an emphasis that is unavoidable even merely concentrating on the Linear B (and A?) archives. We are consciously playing down the more traditional line that these civilisations arose and were maintained by trade, internal or external, and mercenary service. But I find that I am neither alone, nor in the least original, in these reflections. Already in 1937, Lehmann was saying "Raubzüge und Handelsfahrten allein werden schwerlich den Bedarf an Lebensmitteln gedeckt haben" (p.112). UMME conclude (1972, 177): "Agriculture is the cornerstone of the economy of Messenia, and there is no doubt that it was the basis of the wealth of the kingdom of Pylos". And Gerald Cadogan, who has made detailed study of Mycenaean commerce, reports: "Trade hardly affected their civilisation" (unpub. paper). Of course there existed a well-traced trade both between provinces of both civilisations, and to distant lands, not least surely for metal, and in pottery (with whatever was contained in it).

But it is my belief that both high cultures arose and functioned as redistribution systems of locally-produced arable and pastoral products, and we do not require the intervention of foreign trade, or foreign service, to account for their flourishing.[2]

Earlier in my research I was still inclined to consider internal communications as an important factor in the location of the larger Mycenaean communities. If we take our distribution maps of regional settlement for Mycenaean times, in favoured districts with many important sites, it is clear that we can recognise several different categories of settlement; some are clearly minor, others medium-sized, the remainder large and important, or of uncertain status. In varying degrees of completeness we can therefore reconstruct what is called the Settlement Hierarchy. We can commonly pick out three levels, as just indicated, frequently distinct on evidence such as the extent of settlement debris, the number and type of burials associated (earth, cist, chamber tomb, tholos burial), the existence or otherwise of substantial buildings, or massive fortifications. These levels also tend to occupy regular intervals over the landscape: on average major centres, with frequently associated 'palace' buildings and tholos tombs, lie at about 5km. distance from each other (see also A. Chadwick, this volume); intervening or at the corresponding distance at settlement frontiers we find medium-sized communities at about 2½km. from major centres and each other, associated with a varying number of chamber tombs; finally the smallest settlements, that we might interpret as farms or hamlets, lie at even closer intervals in the interstices of the larger units, and associated with one or two chamber tombs, or no substantial burials (see Bintliff, 1977a). In some cases, medium centres appear at 5km. intervals. This is accounted for below. It seems reasonable to reconstruct an early Mycenaean situation, which probably developed in MH, when regions consisted of numerous contiguous settlement territories, each consisting of an area of land with (at least in some directions) a radius of 2½km., and a central settlement, the seat of a 'prince' or 'baron' whose family were interred in one or more tholos tombs. Perhaps now, but more probably by the late Mycenaean period, each of these territories contains dependent rural settlements of medium/minor status, their size and number being plausibly related to the resource potential of the overall territory, only exceptionally as at Mycenae and Pylos evidencing greater density of settlement and burial remains such as might be attributable to their role as regional service centres. This brings us to a much rarer fourth level of the hierarchy, the supercentres that we identify as the locations of provincial administration in the mature Mycenaean period, such as Mycenae, Pylos, the Menelaion. Within their 'kingdoms' by this stage are enclosed the numerous petty baronries of earlier Mycenaean times, and the widespread cessation of tholos construction might be linked to this centralisation of power and prestige. Some have argued that the early Mycenaean period saw no regional authority, each area being made up of independent states in coalition or at war with one another. However the unusual significance of Mycenae and Pylos in later MH times could suggest an early assumption of regional leadership, if more primus inter pares than the later tight control of the Linear B archives.

This reconstruction is based heavily on our best-known region, the Argos Plain, and if we turn to other regions such as the Sparta and Helos Plains, Messenia, the Argolid Peninsula, the fragmentary nature of the Mycenaean settlement picture allows us to make only partial reconstructions and comparisons. Let me add at once, though, that these fragments suggest in every case a very similar network and hierarchy and at approximately the same landscape intervals.

Let us return to the Argos Plain and the developed LH3 hierarchy. In Figure I, I have made use of Thiessen Polygons as an heuristic device to suggest possible territories for all major and some medium-sized centres. The map illustrated is in fact only one of many possible variants, and includes a weighting factor for coastal sites (to allow them equal hinterland to inland

centres), major status given to some sites of unclear importance, and finally the inclusion of hypothetical settlements on the regional frontiers (based on land resources and distance factors) where survey has been all but absent but substantial settlement exists today. The territories vary in size and in the enclosed amounts of different soils. Furthermore, although in numerous cases our 5 km. intersite radius is prominent, in some cases greater intervals can be observed. What is the significance of both variation and regularity? To summarize a complex analysis, there is good evidence that where major and some medium centres are at regular 5 km. intervals, this is because each settlement concerned possesses excellent to good arable land to a radius of $2\frac{1}{2}$ km. in the direction of its neighbours in the chain. All centres seem to be located so as to control enough farming land to fill a $2\frac{1}{2}$ km. circle, but this radius can be seen to be exceeded in intersite distances where the land to the nearest neighbouring major sites is either mediocre or actually includes poor soils and rock. If we take our map, and centre by centre, fit a circle of $2\frac{1}{2}$ km. radius over the main expanses of arable land in each associated polygon (Figure 2), not only can we often produce a correlation of 5 km. intersite intervals with contiguous circles (where the soil is high quality), but we find that some polygons have insufficient good land for the circle, others have noticeably more good land than required by the circle area. At this point I must introduce an element which was at first a stumbling-block to my analysis, and which may have become apparent to you also, the reader. For I have mentioned above, that on occasion it is a medium-sized centre that appears at 5 km. intervals, in place of the major centre.

Rather excitingly, if we grade the individual cells of the Thiessen polygons, according to (a) whether they have less than, as much as, more than, a circle of $2\frac{1}{2}$ km. radius of arable land (b) whether the result of (a) includes poor arable, mediocre arable, excellent arable land, - we find that cell productivity coincides agreeably well with the independently estimated archaeological importance of the cell centre. Much excellent land per cell supports a major centre. If the land beyond $2\frac{1}{2}$ km. from a major centre is mediocre or poor in quality, then we find at around $2\frac{1}{2}$ km. further to this point a medium-sized Mycenaean community (e.g. 3, 8, 9). However if the intersite distance between cell-centres is somewhat more than 5 km., we find that a larger than average territory of mediocre land compensates for low resources of excellent land - and still allowing a major centre in the cell (e.g. Argos and Tiryns). I have made some preliminary calculations of potential carrying capacities, based upon the prehistoric landscape, and these suggest a potential average of 1500 or more occupants in the richer cells - which is reasonably comparable to recent village populations in the same territories.

The same analysis on Mycenaean centres in other regions provides equally interesting results, showing that they too are regularly spaced because of the desire to enclose an area equivalent to a $2\frac{1}{2}$ km. radius circle of good farming land. Once more the quality of the arable within the cell coincides with the suggested archaeological status of the site in question. In the Sparta Plain the territories of the three major centres are as productive as the best of the Argos Plain centres, and continuous zones of excellent land gives a very regular spacing. But if we turn to the comparative backwaters of Helos and the Argolid peninsula, the spacing between Mycenaean centres remains the same or extends slightly, yet the far lower productive capacity of each cell in terms of the $2\frac{1}{2}$ km. radius circle is directly reflected by the low status of these centres archaeologically, in comparison to those of the Argos and Sparta Plains. In the Soulima Valley of Messenia, where good arable land is continuous we once more find regular spacing at intervals not far from 5 km., but only the Malthi cell has high arable productivity and a status fit for interegional comparison.

In summary, we now have before us the tangible foundation, not only for the differential archaeological importance of individual Mycenaean centres, but for

the differential importance of whole Mycenaean provinces - obtained by summing the total territorial resources. The agreement is precise. How do the super-centres relate to their regions? I have suggested elsewhere (Bintliff 1977 a, b) that palace centres at Pylos, the Menelaion, are centrally located to the densest and richest arable cells of their kingdoms; the Mycenae location cannot be seen as so placed for the Argos region, despite its great fertility, (Argos or the Heraion are better placed), but IS ideally placed dominating two regions, the Argos Plain and the immensely fertile Corinth Plateau. With the exception of this highest level in the settlement hierarchy, returning to my original question on internal communications as a locational priority, I would now contend that Mycenaean sites can be satisfactorily graded in almost all cases solely by reference to their immediate resource potential, defined by Thiessen polygons. There remain a number of sites that compensate for a location lacking in extensive excellent soils, and achieve thereby major status. How is this? These we could partly explain as being early foundations, enlarging territory as their populations rose; also sites in favourable harbour locations, where fishing and coastal trade, later on bases for regional fleets, may be elements encouraging an enlarged cell population, whose arable land must be correspondingly extensive.

We might compare the results of this interregional study with the analysis of the territories of 80 or so ceremonial centres of the Maya civilisation, by Norman Hammond (1974). Constructing Thiessen Polygons around each centre Hammond identified two types of pattern: a first group consisted of zones of densely-packed centres each including key sites for the development of Maya civilisation; a second group consisted of widely-spaced centres of a more provincial and less historically significant character. In Mycenaean civilisation, however, territorial spacings remain largely the same, with but a slight tendency for larger territories in less fertile and more provincial districts. But we find a notable decrease in status for cell centres, as we move from the most significant to least important regions of Mycenaean settlement - this is far more noticeable apparently than the status cline with Maya centres. The answer may lie in the larger lands controlled by Maya provincial centres, which imply that their territorial resources and human catchment were not vastly different from the smaller and arguably more concentratedly fertile territories of the key Maya districts. Marcus (1973) has interpreted the same dichotomy of Maya territorial patterns as indicating regional capitals (the dense zones) and their dependent provinces (surrounding zones of wide-spaced ceremonial centres). A similar pattern might be argued for Mycenaean Greece.

I hope that one major result of this analysis will be to focus attention on one of the most fruitful of traditional geographic concepts, the study of the determinism exerted by natural landscape units on the rise of small polities and larger state forms. Whittlesey laid stress on the 'core-region' that repeatedly offered the potential for sophisticated complex societal structures, and nearer at home both Lehmann and Kirsten have demonstrated how naturally Greece breaks into resource parcels at different community levels. Lehmann points out (1939) that study of settlement patterns from period to period frequently shows a shift in point patterns, but always the overall picture on a larger scale shows the priority of continued access to the same few localised areas of high quality resources: "The core-areas remain the same, but their significance changes with the historical situation". Kirsten, in a highly stimulating discussion of Greek historical geography, sets out the following programme (1956): "We are really interested in the long-term definitions of natural landscape pockets that function as counters in cultural history, land with a natural value. As for the communities based on these pockets - it is not so important for us to look at actual population totals and size, but their function as centres for these pockets. We do not start <u>a priori</u> to define the role of each community and the overall hierarchy, we start from these geographical areas and their central settlements and work outwards. Typical for this plan is the Greek Polis, arising

in a special Kleinlandschaft suitable for agriculture. It is usually not a Central Place as there are rarely other settlements - it IS the home of the farmers themselves." In Figure 3 I have redrawn one of Kirsten's examples of a small polis territory, where we actually possess details of the frontiers. There is clearly tremendous scope for future cross-cultural research in the Mediterranean in looking for territorial regularities, and well-documented historic examples such as this will be of great value for prehistoric territory studies.[3]

Colin Renfrew seems to be working along somewhat similar lines, in his quest for regularities in the territories involved in the formation of early states and 'proto-states' (1975, and see this volume). He comes up with the rather trendy-sounding 'ESM' or Early State Module, and he has constructed one for Mycenaean Greece on the basis of Thiessen Polygons around regional centres. In Figure 4, I have slightly altered his pattern, to take account of regions where major centres may be predicted. I have then compared this pattern to the regional capitals and major provinces in the pre-War period, and to the distribution of the most significant areas of fertile land. The fit seems a good one.

In conclusion it now appears feasible to take a prehistoric settlement pattern apart, into its constituent units, relate these to the differential potential of natural 'settlement chambers', individually and in combination, and finally put them together again in a dynamic model of weighted landscape interactions. But let us not categorize this approach as rigid environmental determinism. The landscape merely offers the potential, not the motor.

NOTES:

(1) Some rather curious and inaccurate attempts to disprove Vita-Finzi and myself on the date and causation of the Younger Fill may be found in Davidson's study of the Drama Plain (1971), and his restudy (after Bintliff, 1975, 1976 a) of the Younger Fill in the Phylakopi Plain, Melos (Davidson et. al., 1976). I have demonstrated elsewhere (Bintliff, 1977 a), that Davidson's Drama study provides, in fact, important confirmation of our chronology, and his claimed contradiction of Vita-Finzi rests merely on a misunderstanding of the latter's alluviation theory. In his recent Melos study we find no mention of the numerous well-dated alluvial exposures which I have described in every district of East Melos (and which he has been able to consult), or of the Klima valley excavation (see Bintliff 1977 a). These provide a consistent post-Classical dating for the Younger Fill, and include incontestable termini-post quem for the onset of the Fill. Davidson's Phylakopi catchment data seems once more merely to confirm our conclusions. It is admitted that the Fill contains Bronze Age, Classical and Late Roman finds, at different locations and different levels of the Fill. My much larger sample of exposures throughout Melos demonstrates that Late Roman material is almost always present along with older material in the lowest levels of the Fill. Secondly Davidson publishes an upstream section of river terrace, with an associated C14 date of 2816 bp. This section looks quite typical for Melos and on analogy with my own extensive fieldwork there probably represents a Pleistocene alluvial/colluvial deposit, followed by an organic level with the C14 date, above this the Younger Fill, finally capped by recent colluvium. The C14 date provides another useful terminus post quem for the Fill. Davidson also misinterprets Vita-Finzi by claiming that the factors responsible for the Younger Fill are still active on Melos today, since erosion is observable. Erosion indeed continues, but on such a lesser scale than in Fill times that everywhere streams are downcutting through the Fill (as Davidson's own terrace sections testify).

(2) This emphasis on investigating higher cultures as the result of local developmental processes, rather than reflecting the irradiation of European Barbarism

by the enlightened Near Eastern Civilisations, via trade or colonisation, owes much to Prof. Renfrew's incisive publications on this topic (cf. Renfrew, 1973). All the more surprising to find him resurrecting a modified 'Ex Oriente Lux' for Mycenaeans and Minoans (see this volume)!

(3) One may surely hope that our 4 levels of the Mycenaean settlement hierarchy will ultimately be related to the consistent local administrative hierarchy to be found in the Linear B tablets. John Chadwick has recently pointed to the development of the Attic demes as a possible parallel to the formation of the 17 provinces of the Pylos kingdom (unpubl. paper). This should remind us that Classical tradition divided Dark Age Attica into 12 units, and this could be an even older memory. Attica is approximately two thirds of the size of the Pylos kingdom, which might suggest territorial divisions of similar dimensions. Homer's Phaeacian kingdom, incidentally, was divided into 12 sub-kingdoms. Research into recurring territories seems a promising field in Mediterranean studies.

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DISCUSSION:

(Taking part: A. Snodgrass, F. Hammond, P. Halstead, P. Warren).

SNODGRASS: Could not the dearth of settlements in alluvial valley-bottoms be influenced by the fact that they are so hard to find if buried under several metres of the new fill?

BINTLIFF: The main part of the alluvial areas concerned would not have been settled at that time, because going back to our model Mediterranean valley, presented earlier, you have got these deltas being built out into coastal gulfs; these, as far as we can see, were not generally suitable for human occupation, people would not have been living there, but they might well have been putting their horses and cattle on them. This explains the Homeric references e.g. Argos with its fine grasses for horses and yet so dry - why? Because we have the wet delta zone and the dry upper plain zone. You have got this problem of burial further inland, in river valleys proper, but again, since the soil in the prehistoric period, now underneath the recent fill, was nowhere near as fertile (with very rare exceptions) as the recent fill, we probably haven't missed much and most of the settlement evidence both permanent and temporary in such areas we can confidently predict to be elsewhere on better soils. I say this because of course in places like the Argos Plain you have got an enormous expanse of the older soil (Older Fill) and people have walked over this, looked

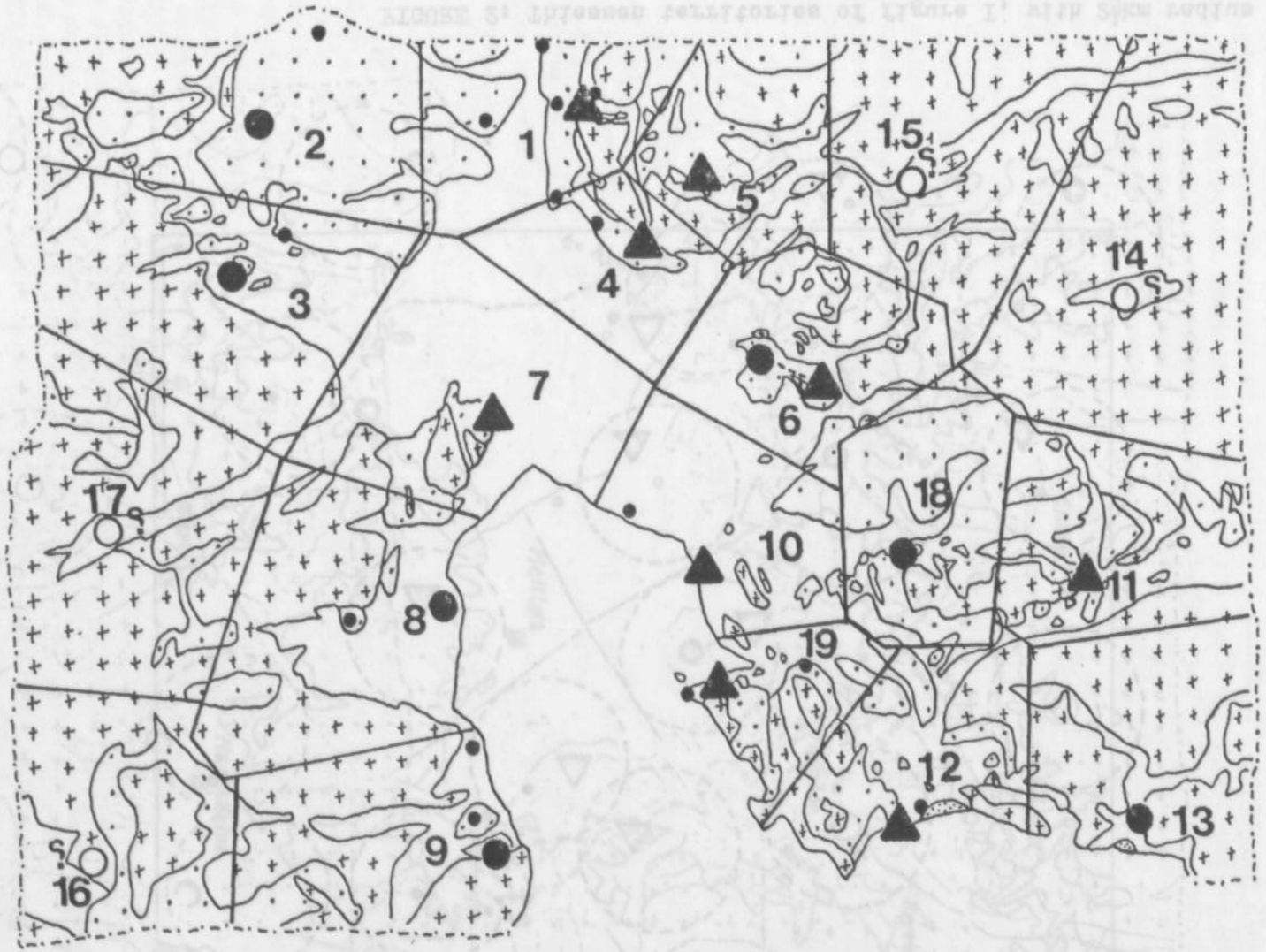


FIGURE I: A weighted Thiessen-polygon diagram for the Argos Plain. Coastline adjusted to prehistoric state, key soils dotted or blank. Late Mycenaean sites: major, triangles; medium and minor, large and small circles. Alternative polygons and site statuses have been considered elsewhere (Bintliff, in press).

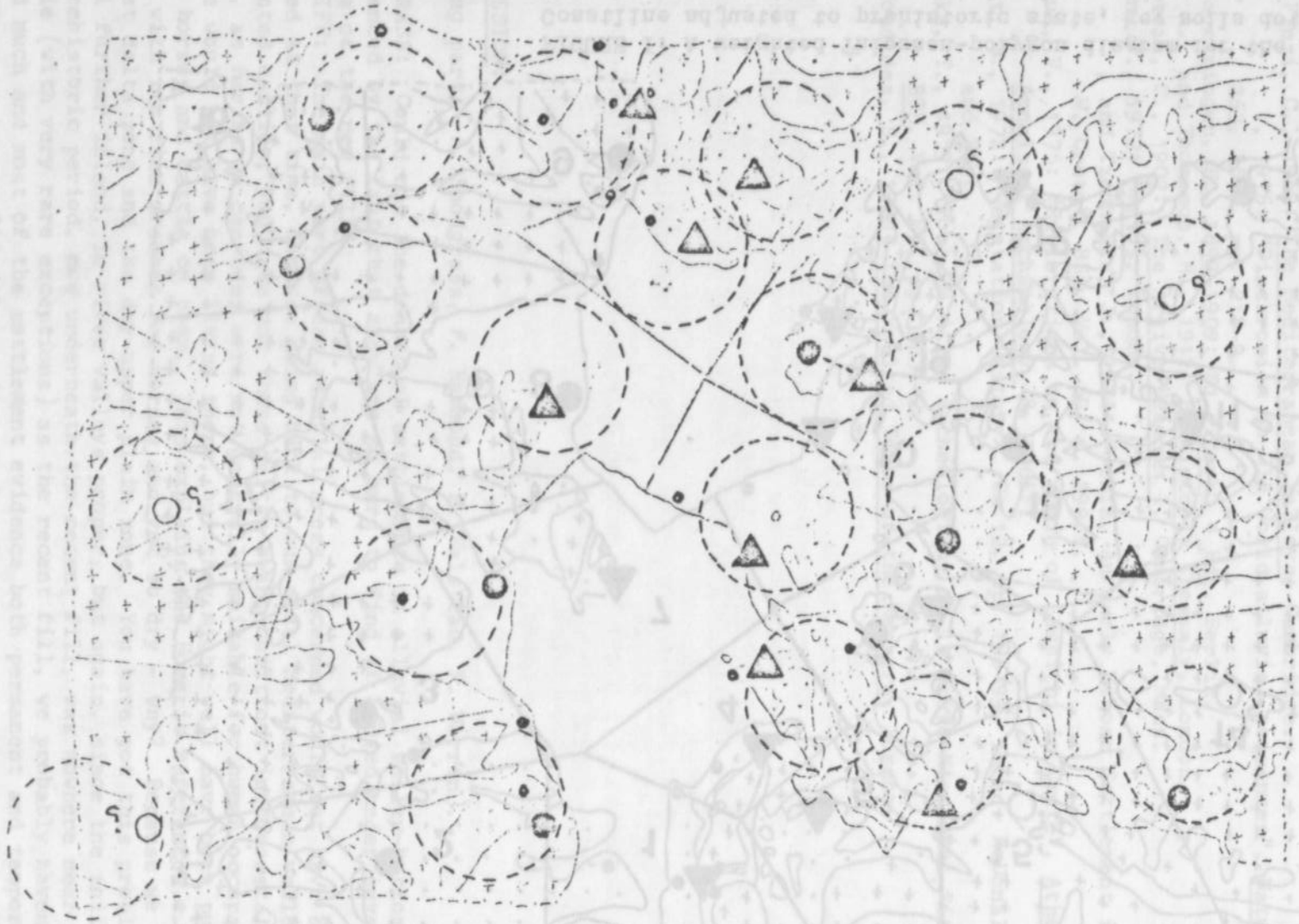


FIGURE 2: Thiessen territories of figure I, with 2½km radius circles placed over the densest area of fertile soils in each 'cell'.

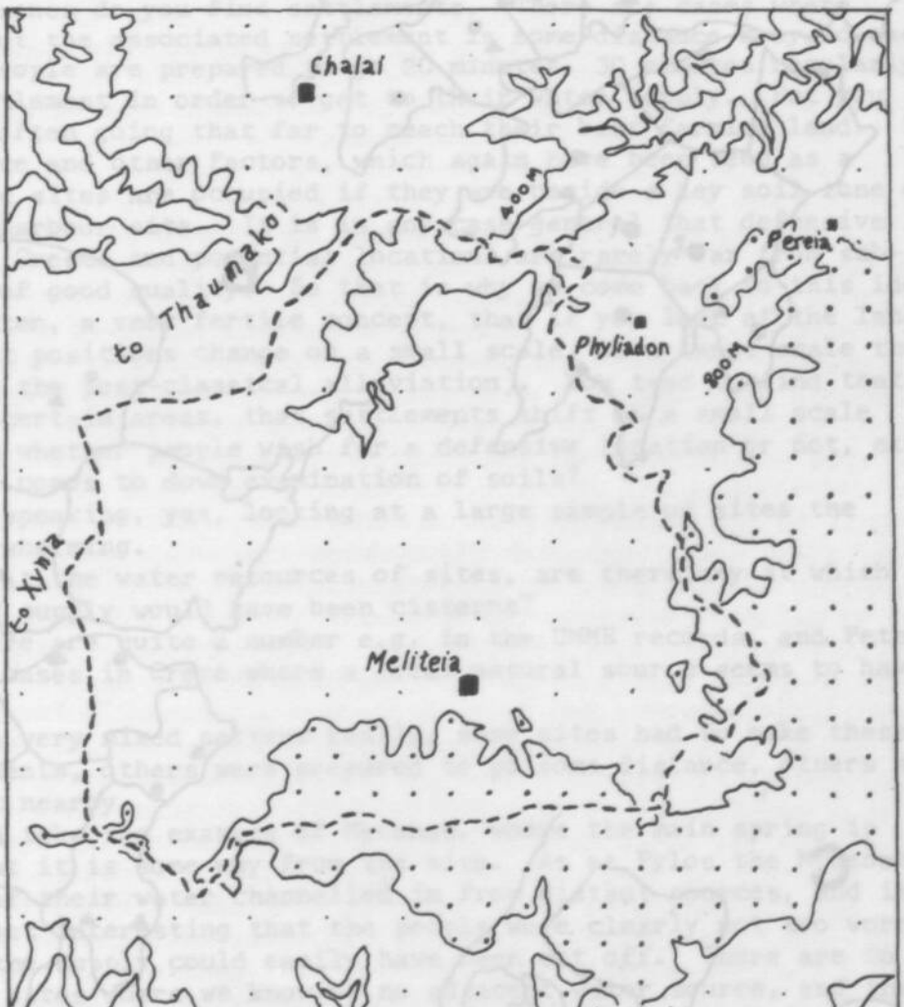


FIGURE 3: The known territorial limits of a small Hellenistic city-state, Meliteia in SE Thessaly. Land above 800m shaded. After E. Kirsten (1956, Fig.9).

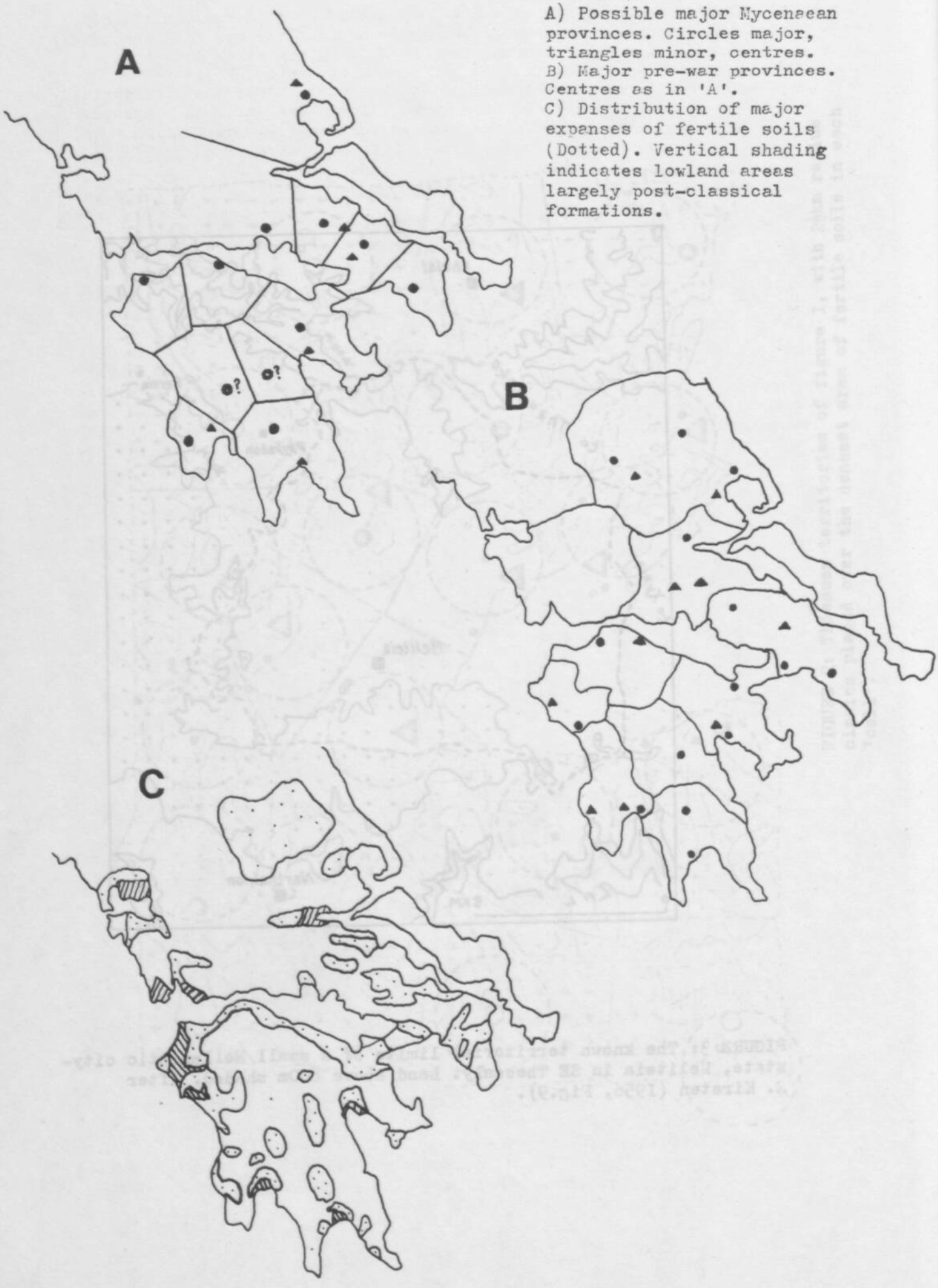


FIGURE 4:
A) Possible major Mycenaean provinces. Circles major, triangles minor, centres.
B) Major pre-war provinces. Centres as in 'A'.
C) Distribution of major expanses of fertile soils (Dotted). Vertical shading indicates lowland areas largely post-classical formations.

for sites, there are hardly any at all of any period up till Medieval times, when irrigation comes in on a significant scale both on the new fill and suitable areas of the Older Fill. So wherever we do find the older underlying soil preserved it is highly unusual to find settlement evidence, except in the large plains of North Greece, where we often just haven't got the alternative fine hilland soils and farmers had to use the plain older soils as a priority. Certainly except in these latter situations we would not expect to find major sites under the recent fill (allowing also for harbour sites such as Tiryns).
HAMMOND: What role does proximity to freshwater play?
BINTLIFF: A somewhat difficult point. It is generally stated that water is very, very important; but you tend to find that only where water sources are close to good soil zones do you find settlements. There are cases where springs are found but the associated settlement is some distance away, closer to good soil. So people are prepared to go 20 minutes, 30 minutes regularly from their main settlement in order to get to their water supply. But you don't find them so often going that far to reach their best farming land. The same goes for defence and other factors, which again have been seen as a priority: strategic sites are occupied if they are beside a key soil zone or by a major fishing/harbour site. It is in any case general that defensive positions abound in Greece and potential locations are rarely far from subsistence resources of good quality. So that is why we come back to this idea of Lehmann and Kirsten, a very fertile concept, that if you look at the landscape the settlement positions change on a small scale, on a large scale they don't (allowing for the post-classical alluviation). You tend to find that settlement follows certain areas, that settlements shift on a small scale within these, as to whether people wish for a defensive location or not, etc.
HAMMOND: So it all comes to down examination of soils?
BINTLIFF: Broadly speaking, yes, looking at a large sample of sites the correlation is overwhelming.
HALSTEAD: Looking at the water resources of sites, are there any at which the most likely sort of supply would have been cisterns?
BINTLIFF: Yes, there are quite a number e.g. in the UMME records, and Peter Warren may know of cases in Crete where a local natural source seems to have been lacking.
WARREN: Yes, its a very mixed pattern really, some sites had to make these artificial arrangements, others were prepared to go some distance, others had water sources right nearby.
BINTLIFF: We could take the example of Mycenae, where the main spring is indeed available,but it is some way from the site. As at Pylos the Mycenaeans actually had most of their water channelled in from distant sources, and in the Pylos case it is most interesting that the people were clearly not too worried about defence for the supply could easily have been cut off. There are in fact many cases of sites where we know of no adjacent water source, and that is surprising because they could have usually found a better location in the region if water had been a priority, rather than soils.
WARREN: One could add, putting the point more strongly than you have done - Braudel went so far as to say that soil is virtually unimportant in determining the formation of territories and settlement patterns, but laid stress not on the water supply for drinking but for watering fields and crops. This irrigation factor taken with soil must be considered as very important.
BINTLIFF: Well, I would really like to see Mycenaean irrigation played down very considerably. Some have given great scope, cf. UMME, but I don't at all endorse this viewpoint. At Malthi, e.g. I made a restudy of the area, where Loy claimed very significant prehistoric irrigation - I don't believe there is any evidence or even great feasibility of a Bronze Age irrigation system there. To some extent my view here rests on landscape change, but also on technology - a great deal of irrigation in Greece rests on technology certainly not available to Bronze Age peoples. Copais is again a problem case, there is some evidence here for water control, but once more if we allow for highly plausible land-

scape change I would suggest that irrigation was not at the scale even here that is commonly imagined. So I consider that it is dry-farmed agriculture in almost every case.

MYCENAEAN GEOGRAPHY: AN ARCHAEOLOGICAL VIEWPOINT

O. Dickinson, Department of Classics, University of Durham

For any study of the Mycenaean settlement-pattern and the deductions that might be drawn from it, the archaeological evidence from excavations and surveys is obviously of paramount importance. In the course of preparing, in collaboration with Richard Hope Simpson, a new Gazetteer to cover all Bronze Age sites on the mainland of Greece, excluding Thrace, Macedonia, and Epirus, and many of the islands, I have had to reconsider this evidence very carefully, and many of the following comments derive from this work. I would like to begin with some comment on the range and limitations of the evidence.

This is the centenary year of Schliemann's excavations at Mycenae. In the century since his spectacular discoveries there has been much excavation on the mainland of Greece, and, though a depressing amount remains unpublished, at least 50 major excavations can be easily listed. Systematic searches for prehistoric remains were already being undertaken before the First World War, but the greatest additions to our knowledge by site-surveys were made as recently as the 1950's and 1960's. At present some 770 Bronze Age sites are known in the area of the so-called Helladic cultures, and more are expected to be published. At least 100 have produced no material dating to the climax of Mycenaean development in the 14th and 13th centuries B.C., most of them small settlement-sites not occupied at all in Mycenaean times, and several more, caves, hoards, and some cemeteries, are unlikely to represent separate settlements, but the indicated total of known settlements remains well over 600. This may seem impressive, but nearly a quarter belong to a single province, Messenia, and these constitute close to half the total of known Peloponnesian sites. Similarly, north of the Isthmus of Corinth, almost as many Bronze Age sites have been identified on the island of Euboea, between 50 and 60, as in either of the important provinces Attica and Boeotia.

A major cause of these imbalances is that Messenia and Euboea have been systematically surveyed, while most other provinces have not. Sporadic bursts of activity have contributed sites in many areas, but in a haphazard way, and there remain some areas that have hardly been touched. The survey of Messenia now published by W. A. MacDonald and his collaborators is much the most thorough, though it still may not approach the coverage achieved by concentration on much smaller areas, e.g. by the Bristol University expedition to the Agiopharango Gorge in south Crete. Such variations in coverage remain an important obstacle to the formulation of general hypotheses, and constitute a warning against transferring deductions from regions where information is abundant to where it is scarce.

The interpretation of the results of surveys is subject to numerous pitfalls, as has been demonstrated most recently in Man, Settlement and Urbanism

(Ucko et. al. 1972, pp. 806-7): in four cases excavation proved Nelson Glueck's conclusions about Transjordanian sites wrong in some important particular. The effects of later occupation, cultivation, and erosion, and the provisional nature of dating based on often badly preserved pottery, have been stressed. It may be noted that the periods of greatest uniformity in pottery throughout the mainland are the middle of the Early Bronze Age and the developed Mycenaean period; before, after, and between these there are considerably local divergences, many still poorly known and lacking in diagnostic features. It is no more than a reasonable hypothesis that the multitude of sites which have produced material of the developed Mycenaean period were occupied simultaneously and until the disaster at the end of the 13th century B.C.; probably the great majority were, but much depends on the nature of the site. For, although it is generally assumed that the "small sites" which make up the majority in some areas (for instance Messenia, where only 20 or so of an estimated 130 within the likely territory of the state of Pylos seem to be over 2 hectares in extent) were true villages, with populations ranging from a few dozen to a few hundred, the possibility that at least some might have been of a specialist nature, perhaps only periodically occupied, for craftsmen, herdsmen, or priests, or may have been only large farmsteads, must be considered. No typical example has been excavated and published to give an indication, and this is another great gap in our knowledge.

Excavations, for a great many reasons, have tended to concentrate at large and long-occupied sites, where time and resources have rarely allowed the uncovering of really extensive areas, and thus we are, for example, still uncertain about what a typical Mycenaean settlement (if there is such a thing) would have looked like. But at least there is enough information available for general conclusions of the sort that I shall now present. An aspect that is strongly marked is that of continuity. It must be emphasised that the Mycenaeans are not a new people, to be distinguished from the earlier Bronze Age population; they are the direct heirs and descendants of the earlier population. There is no good evidence for theories of alien aristocracies settling in this or that province to precipitate Mycenaean civilisation; the Mycenaean material culture can be seen to develop from a blending of earlier traditions with new crafts introduced from the Aegean, and the spread of its more typical features relates to the spread of craftsmen and techniques and the growth of prosperity, not to movements of population-groups or aristocratic war-bands, at least as far as the mainland is concerned. The sudden appearance in quantity of such features elsewhere, as in the islands, may plausibly represent the arrival of people from the mainland.

The earlier prehistory of the mainland is, then, of the utmost relevance to the Mycenaean period. By the middle of the Early Bronze Age, when a society of notable material homogeneity and prosperity had emerged over most of the mainland, all the domesticated plants and animals that form the basis of Aegean life had been established, and the pattern of land-use that these required does not seem to have altered considerably from that time until the end of the Bronze Age, or, indeed, much later. Although this Early Bronze Age society collapsed in a still not clearly explained disaster, and although there is a strong case to be made for the arrival of the population-group whose language contributed most to the formation of Greek in the wake of this disaster, few settlements of any size were permanently abandoned; most survived, or were shortly reoccupied, to form the nucleus of the settlement-pattern in the Middle and Late Bronze Age, and it is only in inland Messenia, sparsely populated before, that many important new settlements are known to have been founded in the Middle Bronze Age. Thus the Middle Bronze Age population inherited a pattern of land-use stretching back many centuries in most areas.

I feel it necessary to stress at this point that this was where the main-

land parted company from the Aegean. The agricultural basis of their life may have been the same, but in the Middle Bronze Age the mainland developed no counterparts to the ordered towns of the Aegean, let alone the great Cretan palaces. The available evidence suggests a poor and limited society, short of resources and probably of manpower, on the mainland, based on largely self-sufficient and independent villages. Isolation meant the development of divergent traditions in the more remote provinces, most clearly seen in pottery but also evident in some cases in burial-customs; it would be unwise to assume that the underlying material uniformity indicates a similar uniformity in social structure or in even less tangible features such as religion and dialect, which could have differed even more widely.

Conditions did improve, and a growth in prosperity was certainly one factor in the emergence of Mycenaean civilisation, but the most prominent feature of this process is the emergence of a ruling class that acquired much of the available wealth, and whose tastes in art, tomb-types, and personal possessions, useful or ornamental, constitute most of what is called "Mycenaean" at this stage. The society over which these new rulers presided must undoubtedly have changed in many respects as a result of their rise to power and the growth in Aegean influences that accompanied it, but it retained many of its old traditions, and did not become a close imitation of the already established Aegean civilisations. The new wealth was not expended on rebuilding the new capitals or even on palaces of any size, but on works of art and tombs, and the interest in war, hunting, and the amassing of treasure displayed by the new rulers suggests that they might have been close in spirit to the rulers of later wealthy barbarian societies like the Celtic and Scythian. Sophistication grew as the period advanced, and the close relationship with Knossos evident in the period of the Last Palace, apparently dominated by a ruling class of mainland origin, may well have led to closer emulation of Cretan institutions, especially in the establishment of the great palaces and of literate bureaucracies to run them. But this was a change at the top. There are, especially at Mycenae, other fine buildings apart from palaces belonging to the 14th and 13th centuries B.C., well-built and fresco-decorated but still simple compared with most of the Cretan "villas"; the average late Mycenaean house may have been larger and more elaborate than its predecessors, so many of which belong to the simple hall and porch type. But in general there seems to have been a minimum of change in the settlements, which remained for the most part unorganised, undefended, and fairly small; drainage was introduced at some major sites, but there is still little sign of town-planning. Material culture was extremely homogeneous both on the mainland and throughout the Aegean, though there remain divergences in some important features; but this homogeneity can hardly be taken to prove political or linguistic unity, and there are hints of local divergences even in settlement-pattern.

There is no clear evidence for a great increase in the number of settlements in the early Mycenaean period, but this does seem to be a feature of the 14th and 13th centuries B.C., even allowing for the fact that settlements of this time are more easily identifiable. Its incidence is uneven. In eastern Arcadia, a probably backward area, the number of sites remains more or less constant throughout the Bronze Age, but their position shifts. In Laconia, the number of known settlements virtually doubles; perhaps half the new ones are reoccupations of sites abandoned since the Early Bronze Age, and all are small, $1\frac{1}{2}$ hectares or less in extent. In Messenia, nearly a third of the total seem to be new, of which only two are over 2 hectares in extent. In the north-east Peloponnese and north of the Isthmus new sites are not common, the clearest examples being outside the major plains in the north-east Peloponnese, and these territories seem dominated by large and long-established sites. There are at least 7 large sites in and close to the Argive plain, Mycenae, Tiryns, Prosymna, Dendra, Argos, Asine, and the perhaps relatively new Nauplia, whose

earlier prehistory is unknown; some may have grown in this period, and several seem to have had in their neighbourhood small dependent hamlets or larger settlements, a pattern also suggested by evidence from Athens and perhaps widespread. Here, then, the likely increase in population did not necessarily result in an expansion of the number of settlements; in the provinces where it did, there was definitely an expansion of the old pattern rather than a radical alteration. By this time some of the early centres whose importance is indicated by tholos tombs seem to have declined; the number of truly major sites, the capitals of states of first class importance, is very low, perhaps no more than 5 or 6. These were the sites of the great palaces, which inherited and to some extent maintained the traditions of Crete; outside their territories, life may well have continued in the simpler style of earlier times.

The degree of civilisation maintained by the palaces can be overestimated: for instance, the description of the Mycenaean people, in a recent letter to Nestor, as literate, with a sophisticated technology and extensive "international" trade relations, is something of an overstatement. On literacy alone, it may be commented that writing seems to have had an exclusively economic use; annals, law-codes, correspondence, and literature sacred and profane, all common features in the Near East, are wholly lacking, and it might well be that only the narrow class of scribes and officials knew how to read and write. Even dedicatory inscriptions, such as seem to be found earlier in Crete, are lacking, and the number of sites where any form of writing has been found is very small. Nor was the period necessarily one of unbroken peace. The state of Pylos was to a great extent protected from powerful neighbours by a great belt of rough country; in more central areas, there is evidence of a growing preoccupation with defence and plausibly of interstate conflict. But nevertheless the world of the great palaces was stable, and commanded great resources even in its final phase. The careful description of the "heroic age" by Thucydides, based on the poetic tradition, with its rootlessness and insecurity, may be applied much more plausibly to the period after the fall of the palaces, when there is clear evidence for major shifts of population and continual trouble, and when the total population seems to have gone into abrupt decline. Though many of the old centres survived through this, to emerge frequently as still important in Classical Greece, and though there is no reason to suppose any significant change in land-use, continuity was essentially broken at this time, to the extent that the later Greeks had no conception of the antiquity of their settled past, placing the foundation of their greatest cities a few generations before the Trojan War, and imagining their "heroic age" very much in terms of their own day or their recent past.

This account is obviously provisional; it has had to be brief, and in parts it is likely to be controversial. But I hope to have shown reason to treat Mycenaean civilisation as a distinct phenomenon, not necessarily comparable to its Aegean predecessors, and to have shown that the archaeological evidence, closely studied, allows the drawing of distinctions that should not be overlooked in broader surveys.

DISCUSSION:

(Taking part: C. Renfrew, J. Bintliff).

RENFREW: What is the earliest evidence we have from the Mainland for a palace organisation?

DICKINSON: I think the earliest evidence is in fact the foundation of the great palaces in Late Helladic 3A2, 14th century. I know of no really good evidence to suggest it before then, though one can assume, since it can hardly have sprung up over night, that there was something of a development between let's say the Shaft Grave period. The 15th century may perhaps have been the seminal phase, particularly the later part when there is this extraordinarily close relationship with Crete. But there is some evidence for buildings beneath the later palaces at Mycenae and Tiryns decorated with simple frescoes, there is some evidence at Pylos for a building of rather unclear date but certainly preceding the later palace. Otherwise I would say, if we were going purely on this evidence, then the big palaces indicate I would have thought the existence of a fairly complicated form of government, and this is not before the 14th century. I don't want to say that there weren't big states before then - I believe that Mycenae controlled more or less the same territory throughout its history from the Shaft Graves on, through the Late Bronze Age, that is the Argive Plain and quite possibly the Corinthian Plain as well, but that's another matter, I think they could have controlled it before they had conquered it.

BINTLIFF: It does seem, looking at reports of individual sites, and general surveys, that there is no comparison on the records, between LH1 and 2, and LH3. Just looking e.g. at the Plains of Argos and Sparta, sites suddenly shot to a much greater size in a very short time, and also site numbers shot up. Now either this is an artefact of our sampling problems, or else something did happen on a much faster scale and we haven't got a lot of development between Middle Helladic and LH3.

DICKINSON: Part of the problem is that the only form of Mycenaean pottery which most people are qualified to identify is the decorated. You are lucky if you find sherds of decorated Mycanaean pottery on the surface. I have been able, by poring closely through things like the UMME Survey original reports, to identify other sites where there might have been Early Mycenaean occupation, but I would say that you can take it as a general rule, at least I have, that where you have an indication of a site occupied in MH, and in LH3, unless something extraordinary has happened, there is no reason to suppose a break in occupation. Often enough material that has been attributed to MH might equally well be attributed to Early Mycenaean. It is one of these nasty problems, it's like the Dark Ages, everyone simply expects to find pieces with decorated circles and semicircles on them, and that's why you can't find many sites because nobody knows what to look for. It is an awkward problem. I've only really got used to what ordinary pottery is like in Messenia in the Mycenaean period as a result of having so much of it at Nichoria. And I'm sure had we dug Nichoria first then done the Survey, we would have had a lot clearer idea of the date of most of the sites. We might have found many more for Mycenaean. But I'd agree with you to this extent, that there wasn't much expansion on a number of settlements in Early Mycenaean times, it's Late Mycenaean that one can show it, and all I feel at the moment is that people do tend to assume that at all sites where you have got LH3, or just Late Helladic, these were suddenly occupied at the beginning of LH3 and occupied all the way through. I think if one excavated some of them you might find, (a) they were much smaller than people seem to imagine, and (b) they might only have been occupied in a single phase. There is one site that has an extraordinary history, it's only a very minor site down from the Argive Plain, Synoro, it was only occupied in Early Helladic and LH1 to 2A, never before and never after and never in between. I don't know the explanation, but excavation may show for certain.

On the whole, excavation tends to show that sites were occupied at least from MH right on through to LH3B. The Early Mycenaean finds are not very well distributed but we have usually such small areas opened up that you can't really tell their overall significance over the site.
BINTLIFF: To take e.g. the Argos Plain, it has been claimed by several excavators for their sites that the areas of occupation were much smaller in LH1 and 2 than in LH3.
DICKINSON: I don't think frankly that the evidence is susceptible of that sort of fine division.
BINTLIFF: So it is really sampling bias, the problem of recognition?
DICKINSON: Partly that, and partly people place too much on where they find their material. I've got evidence that around Mycenae already by the end of the MH there is a whole pattern of tiny settlements, just based on the discovery of MH sherds in the dromoi of chamber tombs, and I think already we have the pattern from which later Mycenae developed.

PREHISTORIC THESSALY : THE SUBMERGENCE OF CIVILISATION

Paul Halstead, British School at Athens

Introduction

By Greek standards, the plains of Thessaly are of unusual size and fertility, and in the Neolithic they supported a large population of cereal farmers. There was little scope, however, for further population growth on a broader agricultural base, or for the development of a Mycenaean type of intensive and highly organised economy. Mountains separate the large inland plains from the sea (fig. 1), and leave only the small plains of Volos and Almyros in direct contact with the maritime networks so important in the development of Mycenaean civilisation. Moreover, harsh winters inland restrict the olive to the coast, while no alternative crop in ancient Thessaly had the olive's potential for both subsistence and industrial exploitation.

Significantly, only near the coast does pottery of a truly S. Greek appearance occur in quantity and throughout the Mycenaean period. Inland, the pottery is provincial in character, is common only in LHIII and appears along with local wares. There is no evidence for the mobilisation of large labour forces such as those which built Mycenae, nor for a prestigious social elite, as reflected by the palatial art and numerous tholos tombs of S. Greece. As for the Mycenaean bureaucracy, the only suggested palace is at Volos on the coast. Evidence for craft specialisation and external trade is poor. Thessaly supported a substantial LBA population, but not the type of society and economy characteristic of Mycenaean civilisation.

The aim of this paper is to examine the cultural divergence between Thessaly and S. Greece in the light of settlement data. The main source for Thessaly is a survey carried out by Drs. David and Elizabeth French (2); as data are only sufficient for the eastern plain, I shall restrict discussion of settlement to that area. I shall briefly discuss LBA settlement in E. Thessaly, and then describe its development during the 6000 years from the EN to the LBA in relation to:

a) economic strategy : the location of sites relative to the environment.
b) social and economic organisation : the size of settlements and the distance between them.
c) population density : the number and size of settlements.

LBA Settlement in E. Thessaly

The largest communities in an ideal settlement system embrace the widest range of occupations and so act as service centres for smaller communities around them. Where economic organisation is advanced, a multi-level hierarchy exists in which centres at a higher level are fewer, larger and further apart than those at a lower level (fig. 2). In reality the size of settlements and the spacing between them reflects the distribution of resources across the landscape. The degree to which the ideal pattern superimposes itself depends on the level of organisation and variability of resources and terrain in a given area.

Documentary evidence shows that Mycenaean settlements in Messenia were ranked in an organisational hierarchy, which was administered from Pylos and a subordinate provincial capital through sixteen local centres (3). Since numbers of industrial workers and bureaucrats were concentrated at the two capitals (4), some equation between size and importance might be expected. In fig. 3a the largest site is Pylos, and the great preponderance of small sites over large ones may well reflect this hierarchy.

In E. Thessaly geographical variability is less likely to distort an ideal hierarchy, nor is the paucity of small sites the result of an inadequate sample. Here, however, economic organisation is hypothesised to be at a low level, and the evidence of site size does suggest the existence of several local centres of comparable importance (figs. 3b and 9).

The development of settlement in E. Thessaly from the EN to the LBA

In the seventh millennium farmers colonised most of the plain, choosing soils which offered both fertility and ease of cultivation (fig. 4). They ignored only the heavy soils near Lake Karla, which were probably subject to unpredictable floods, and the driest part of the plain watered only by intermittent streams off the central hills. In a wooded environment, sheep were preferred to cattle and pigs, probably because they could be grazed on small areas of stubble, fallow and sprouting cereals.

In Macedonia the gradual spread of clearance is well illustrated by a series of pollen diagrams; in Thessaly it is reflected in the growing importance through the Neolithic of cattle and pigs, which could exploit cleared ground in its various stages of regeneration. During the MN and LN (figs. 5 and 6) a steady increase in the number of sites seems to have so reduced available land that colonisation of the 'dry area' became worthwhile - as is evinced by a rash of small sites. By the FN (fig. 6) most of the latter were abandoned, while over the rest of the plain a rationalisation of the LN pattern occurred, with the loss of a few sites.

So far the pattern of settlement had been very stable, with continuity from EN to MN and from MN to LN at 75% of the sites; moreover, only one FN site was not previously occupied in the LN. This steady development was interrupted in the EBA (fig. 7), when there was a continuity of less than 50% with either the preceding or the following phase; indeed, many sites of this lengthy period may not have been occupied throughout its duration. In the northern half of the plain the pattern of site location suggests a change of economic emphasis: sites are on the edge of the plain, with access also to

wet ground or mountain slopes, or both. The slopes and wet ground offered complementary seasonal grazing, while use of the slopes during the BA for vines or nut trees might be inferred from the Macedonian pollen diagrams. On the margins of the wet ground intensive gardening was possible and the marshes and the Pinios river were rich in fish and wild fowl. To the South there was a further loss of sites in the 'dry area'. Here the decline in soil moisture after forest clearance would be critical for the viability of hoe agriculture. Without a significant rise in work-input expectations, the area might best be exploited for sheep and goats as a winter complement to the rich summer grazing near the lake. Such a development may have contributed to the renewed importance of sheep and goats at this time; a further innovation was the exploitation of sheep specifically for their wool.

Although continuity of settlement into the MBA (fig. 8) was limited, sites on the edge of the plain were still typical in the northern area. Here continuity of economic strategy is stressed by the fact that sites with continuity of occupation are generally those which attain the greatest size in the LBA. The southern part of the plain was settled with a line of sites, each presumably exploiting a broad transect across the plain. Growing pressure of population on land may have made extensive plough agriculture with animal traction an economic proposition. Sheep, which continued to grow in importance, may have been reintegrated into the agricultural system as the plough made large areas of short term fallow available for grazing. Overall, the regular spacing of sites implies a high level of mutual repulsion and thus the exploitation of most of the plain. The distance between sites indicates large territories, which in turn suggests another innovation dependent on animal traction - the ox-drawn cart. The latter greatly expands the area exploitable from one site.

This realignment of settlement to a more broadly based economy was successful, since 75% of MBA sites were also occupied in the LBA (fig. 9). In the latter period this pattern was expanded by the founding of a few new sites and the considerable growth of some old ones. By now most of the lowland will have been under the plough or in pasture, with growing areas of upland in use for tree crops or seasonal grazing. As well as scattered upland finds, specialised sites included a fishing village on the eastern shore of the lake and two sites on its western edge, with access to the extensive summer grazing. While population was most dense in areas of environmental and economic diversity, two large sites between the lake and the hills were exceptional in exploiting a uniform type of territory. Their proximity to the lakeside would have allowed the maintenance of enough oxen to plough this area, and large-scale grain production may explain their presence in this hitherto ill-favoured area. If the rapid integration of Thessaly during LHIII into the cultural networks of S. Greece reflects economic relations, then the southern part of the plain would have best combined a capacity to produce a grain surplus with proximity to the harbours at Volos. This is speculation, however, and the rather dispersed form of authority suggested for LBA Thessaly would not have been the ideal vehicle for the regular export of grain to S. Greece.

Changes in social organisation

Ethnographic analogy suggests that EN society in Thessaly was egalitarian (5), with influence dependent on personal qualities and perhaps on prestige gained in such exchange systems as are implied by fine pottery networks(6). Tendencies towards group fission would keep communities small prior to the development of some form of social stratification backed with authority (7). Once a few larger and more organised communities emerged, their greater capacity for raiding would rapidly select for a larger normative size. The range of settlement sizes (fig. 10) remained stable in E. Thessaly from the EN to the end

of the LN - a period of nearly 3000 years. At a few LN sites, however, a part of the settlement had been cut off from the rest by ditches, perhaps reflecting the emergence of a formalised elite. FN settlements are certainly somewhat larger; if the bell-shaped storage pits of this period were partly intended to save space above ground, they may also have been more densely occupied than previously.

Raiding may have been intensified in the EBA by the availability of the horse, and also by the possible occupation of the southern part of the plain by pastoralists only loosely integrated with the agricultural settlements. Consequently, violence as an explanation of the breakdown of settlement and the tradition of making fine pottery need not imply incursions from outside Thessaly. Violent disruption on a large scale, however, need not be assumed: changes in settlement location are explicable in economic terms, while the social role of pottery may have been superceded by more destructible goods in wool or metal. Without doubt, a social order existed throughout the BA with sufficient authority to keep together large and growing communities. Only in the LBA do we find strikingly rich graves. While these suggest authority dispersed on a fairly local basis, there may have been some higher level of organisation, able to resolve peaceably conflicts between communities. Although the close packing of sites must have left LBA communities treading on each other's economic toes, there were a few tiny sites, apparently integral to the regular settlement network, which would not have been viable had raiding been a regular occurrence.

Population growth

The estimation of changes in population size from the total area of all settlements suggests two cycles of growth, interrupted by a decline in the FN (fig. 11a). This pattern is distorted, because not all sites of a given period were occupied throughout that period. Consequently, allowance must be made for the following factors:

The fertility of virgin forest soils may have prompted the periodic movement of earlier settlements; certainly stratigraphic evidence from excavations suggests intermittent occupation at some neolithic sites. Later, the lack of fresh land will have restricted such moves, and increasing permanency of occupation is suggested by the vertical growth of sites: although BA houses will have left less debris than their shorter lived predecessors, the rate of accumulation appears to have been fairly steady throughout.

Also, the regular spacing between sites of the FN, MBA and LBA suggests more permanent settlement. Conversely, in the LN many sites in the southern part of the plain were short-lived, and in the EBA settlement was apparently unstable. Both of these factors will have been exaggerated by the exceptional length of these two periods. By contrast, the LBA is very short, and size estimates are lacking for many of its larger sites.

A diagram (fig. 11b), in which compensation has been made for these various problems, may present a more representative population curve. It shows rapid growth in the earlier Neolithic, which levels out in the later Neolithic and falls in the EBA.A second phase of growth in the MBA and LBA completes a demographic pattern which parallels that of cultural and economic change in E. Thessaly - two cycles of development interrupted by a period of realignment in the EBA. The picture for E. Thessaly, of rapid growth only in the LBA, contrasts with the faster and more sustained growth suggested for S. Greece during the BA (8), and matches the picture presented by material culture, of a much lower level of social and economic complexity in Thessaly than in S. Greece.

Conclusion : Culture change in prehistoric Thessaly

Early farming communities were connected by a lattice of social relations reflected in the distribution of fine pottery styles. Initially, individual styles of decoration characterised each locality, and the overall homogeneity in Thessaly need only imply a series of local, overlapping exchanges of ideas, produced perhaps by exogamy.

As population grew, a wider variety of crops was cultivated, while the exploitation of seasonal grazing outside their immediate territories probably brought communities into greater mutual contact. Such seasonal activities, coupled with a growing diversity of economy and shortage of land, would have increased the interdependence of communities. This may be reflected in the increasing uniformity of pottery styles, in E. Thessaly at least, during the later Neolithic. Intensified social and economic relations, both within and between communities, were matched by the emergence of a social elite occupying a separate part of the settlement and probably acting as a focus for much of the movement of prestige goods (9). The nucleation of settlement during the FN offered increased scope for craft specialisation. At this stage of development in S. Greece, the growing social need for prestige goods expanded earlier exchange networks into intensive, long-distance trading, thus radically altering demography, economy and society. In Thessaly this fundamental change did not occur until the LBA, when the contemporaneity of a sharp rise in population with a rapid integration into the Mycenaean cultural world suggests external stimulus, rather than organic evolution, as a spur to cultural development.

The role of luxury goods in the development of a stratified society and trading economy was minimised in Thessaly by the area's isolation from the Aegean. For S. Greece, conversely, sea fishing presented a good opportunity for trade, while the islands offered a variety of minerals. In the South the olive, too, offered a luxury product as a by-product of a subsistence activity, while making possible a significant increase in calorie output. Such an increase in BA Thessaly would have involved more intensive cereal agriculture - and more work at times of the year when labour was already committed.

Demography is of interest in the study of cultural change in Thessaly. A rapid initial growth of population levels out in the later Neolithic - the greater work-input needed to support this growing population on finite resources may have acted as the sort of "density-dependent restraint" on growth which is supplied in animal populations by territoriality or stress. However, local disequilibria, such as the over-exploitation of the dry area of the plain, will have ensured that pressure of population on resources was a recurrent stimulus to change. The cultural retardation of BA Thessaly shows that this stimulus was effectively balanced by other factors.

Of more particular interest to Thessaly is the large population supportable at a neolithic level of culture and economy. The plain is not broken up by any major impediments to human contact - unlike S. Greece where smaller areas would form relatively closed behavioural systems. Arguably, in the larger Thessalian system more stress could be absorbed without structural change. A contrast to this "Cultural Inertia" of Thessaly may be offered in S. Greece by the emergence of Mycenaean "kingdoms" controlling whole regions. As an organic development this would tend to happen piece-meal, with the gradual emergence of fewer centres controlling more territory - rather as Dr. Chadwick has suggested for Pylos in Messenia (10). In E. Thessaly, with so many sizeable sites in direct contact, there were too many players in the game for an early resolution of such competition.

These are some of the factors which interacted in the divergent cultural

development of Thessaly and the Peloponnese in the BA. Essentially, Mycenaean civilisation supported a large population at the price of a high cost in labour. Bronze Age Thessalians were more attracted by the "principle of least effort" than by the "idea of progress"!

Acknowledgements

My thanks for help in the preparation of this paper to Peter Callaghan, Hector Catling, Richard Jones, Bill Phelps, Andrew Sherratt and Jenny Webb - and to Tony Spawforth for his heroic defence of the English language.

Notes

(1) Throughout, these abbreviations are used: 'EN' for Early Neolithic ('Pre- and Proto-Sesklo' etc.); 'MN' for Middle Neolithic ('Classic Sesklo'); 'LN' for Late Neolithic ('Dimini'); 'FN' for Final Neolithic ('Larisa/Rachmani'); 'BA' and 'EBA', 'MBA' and 'LBA', for Bronze Age and Early, Middle and Late Bronze Age respectively.

Chronology (fig. 11) is based on C14 dates (cf. D. R. Theokhares, Neolithic Greece (Athens 1973) 119), recalibrated according to R. M. Clarke in Antiquity 49 (1975) 251ff.

'S. Greece' refers to the Peloponnese, Attika, Boiotia and Euboia - the Mycenaean 'heartland'.

(2) I am indebted to David French for giving me his survey data in advance of publication; this data was the source of most information concerning the location, dating and size of settlements, as well as for the distribution of pottery styles.

As this paper presents the outlines of research which will be treated in more detail in a doctoral dissertation, references have been kept to a minimum.

Major recent summaries of Thessalian prehistory: V. Milojcic, Hauptergebnisse der Deutschen Ausgrabungen in Thessalien 1953-58 (Bonn 1960); Theokhares, op. cit.

Comparative settlement data for Messenia: W. A. McDonald and G. R. Rapp (eds.), The Minnesota Messenia Expedition (Minneapolis 1972) 264ff.

Macedonian pollen diagrams: S. Bottema, The Late Quaternary Vegetation History of N.W. Greece (Groningen 1974).

Faunal material: J. Boessneck in V. Milojcic et al., Argissa Magula I (Bonn 1962); S. Bokonyi in Theokhares, op. cit. 165ff., and pers. comm.; C. Schwartz, Ergon (1973) 18, and pers. comm.; J. Watson, pers. comm.

Plant remains: J. M. Renfrew in Theokhares, op. cit. 147ff.

The mysteries of Mycenaean pottery: A. G. Hunter, The Bronze Age in Thessaly (unpublished B. Litt. thesis, Oxford 1953); also conversations with Liza French, Dr. K. Kilian, Penny Mountjoy, Sue Sherratt and Mrs. M. Theokhares.

(3) J. Chadwick in McDonald and Rapp (eds.), op. cit. 105ff.

(4) Ibid. 111.

(5) E.g. A. Forge in P.J. Ucko, R. Tringham and G. W. Dimbleby (eds.), Man, Settlement and Urbanism (London 1972) 363ff.

(6) Cf. infra.

(7) Forge, art. cit. (supra n. 5).

(8) C. Renfrew, The Emergence of Civilisation (London 1972) 251-4.

(9) E.g. at MN Sesklo, painted pottery was concentrated on the 'akropolis' part of the site (D. R. Theokhares, pers. comm.).

(10) This volume.

Captions to plates

Fig. 1: Map of Thessaly.

Fig. 2: Example of an ideal settlement hierarchy, after D. L. Clarke, Models in Archaeology (London 1972) 862.

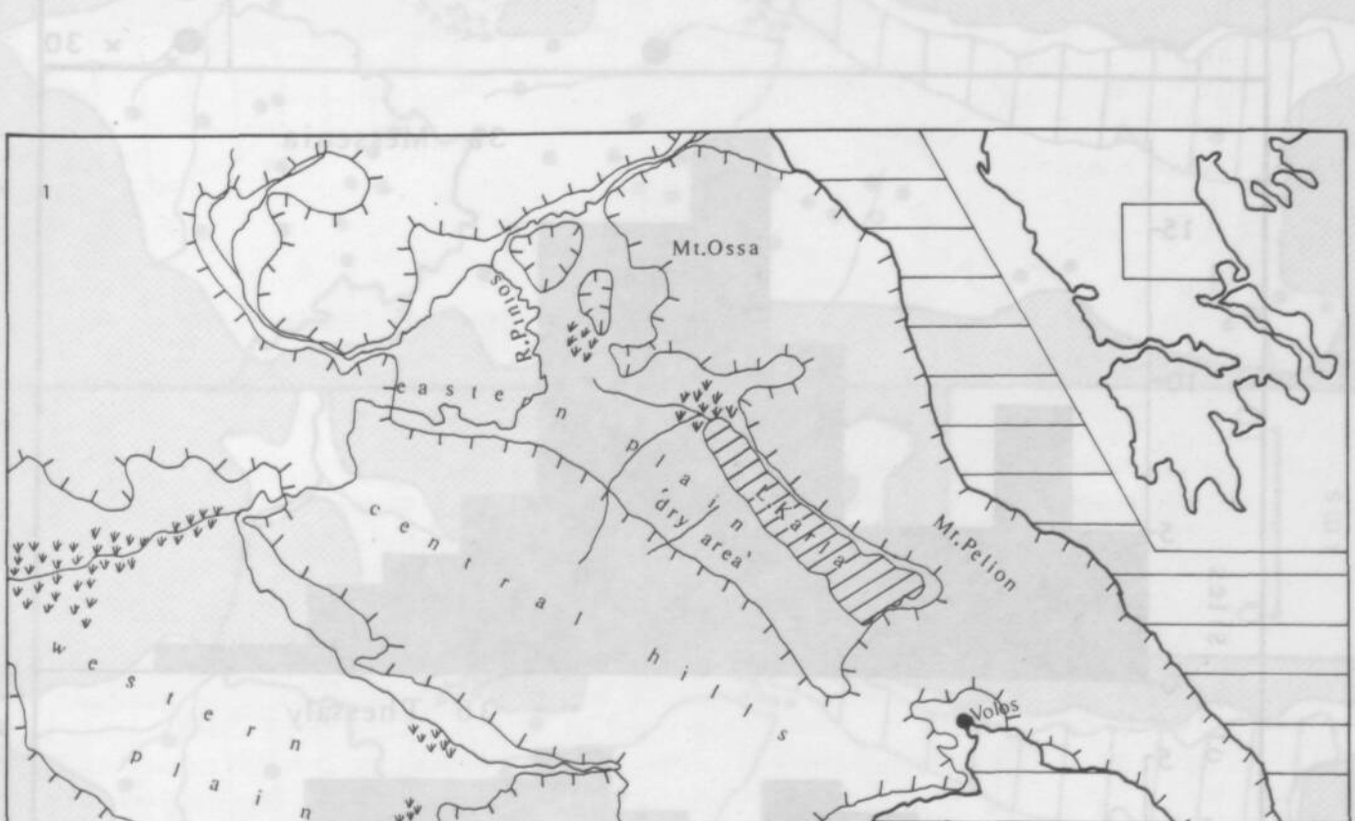
Fig. 3: Comparison of range of settlement sizes in LBA of Messenia and of E. Thessaly (Horizontal scale is logarithmic).

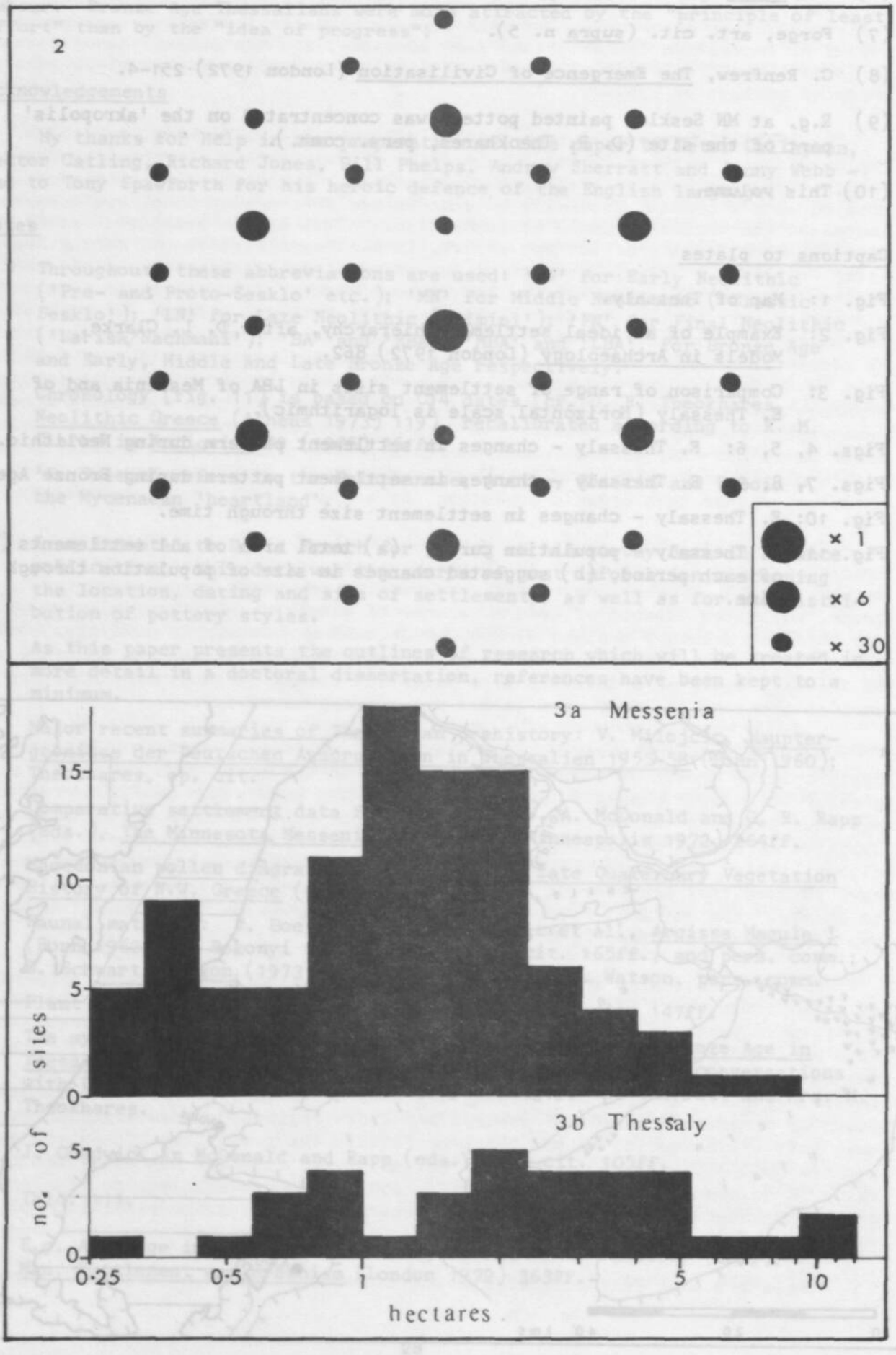
Figs. 4, 5, 6: E. Thessaly - changes in settlement pattern during Neolithic.

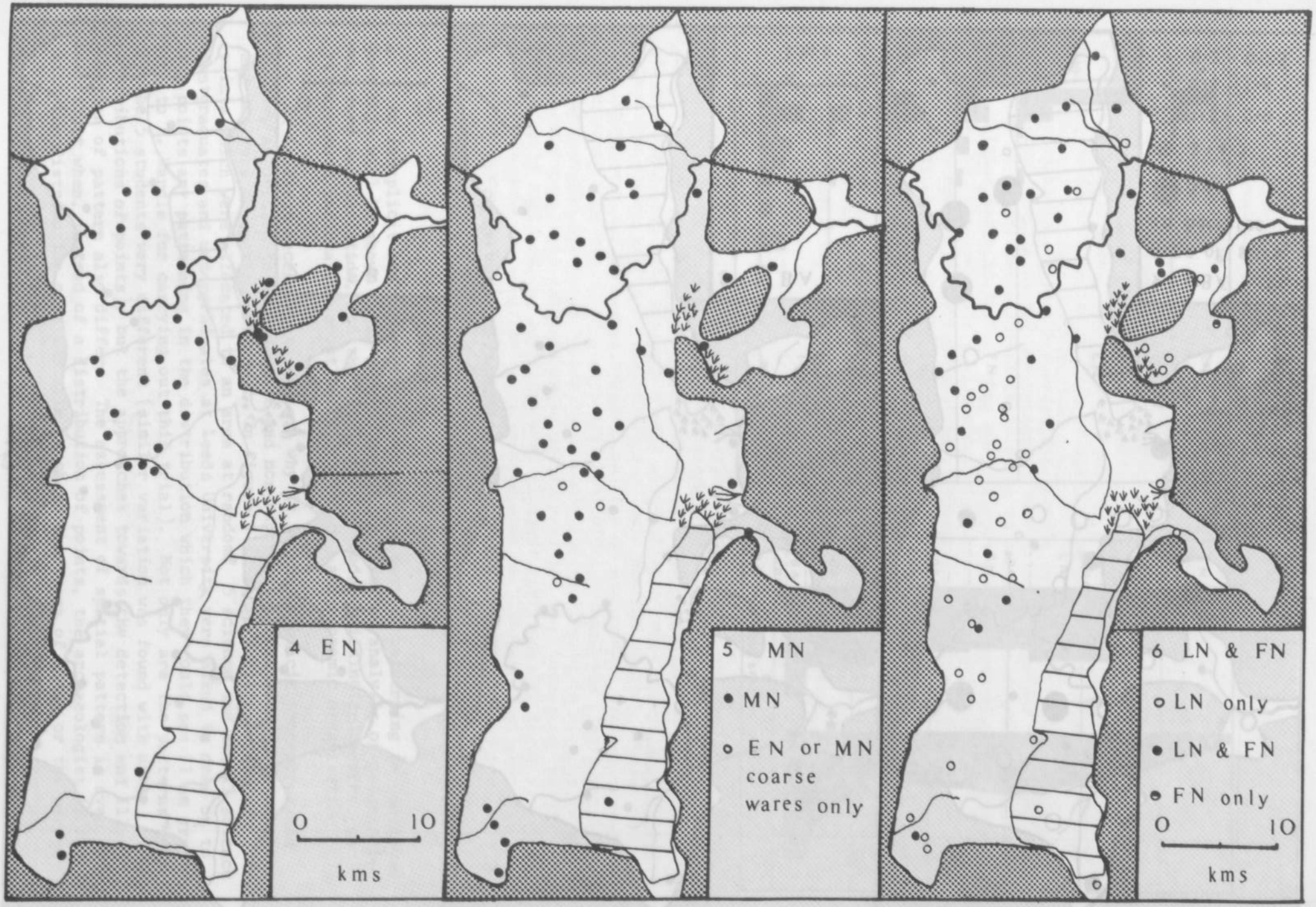
Figs. 7, 8, 9: E. Thessaly - changes in settlement pattern during Bronze Age.

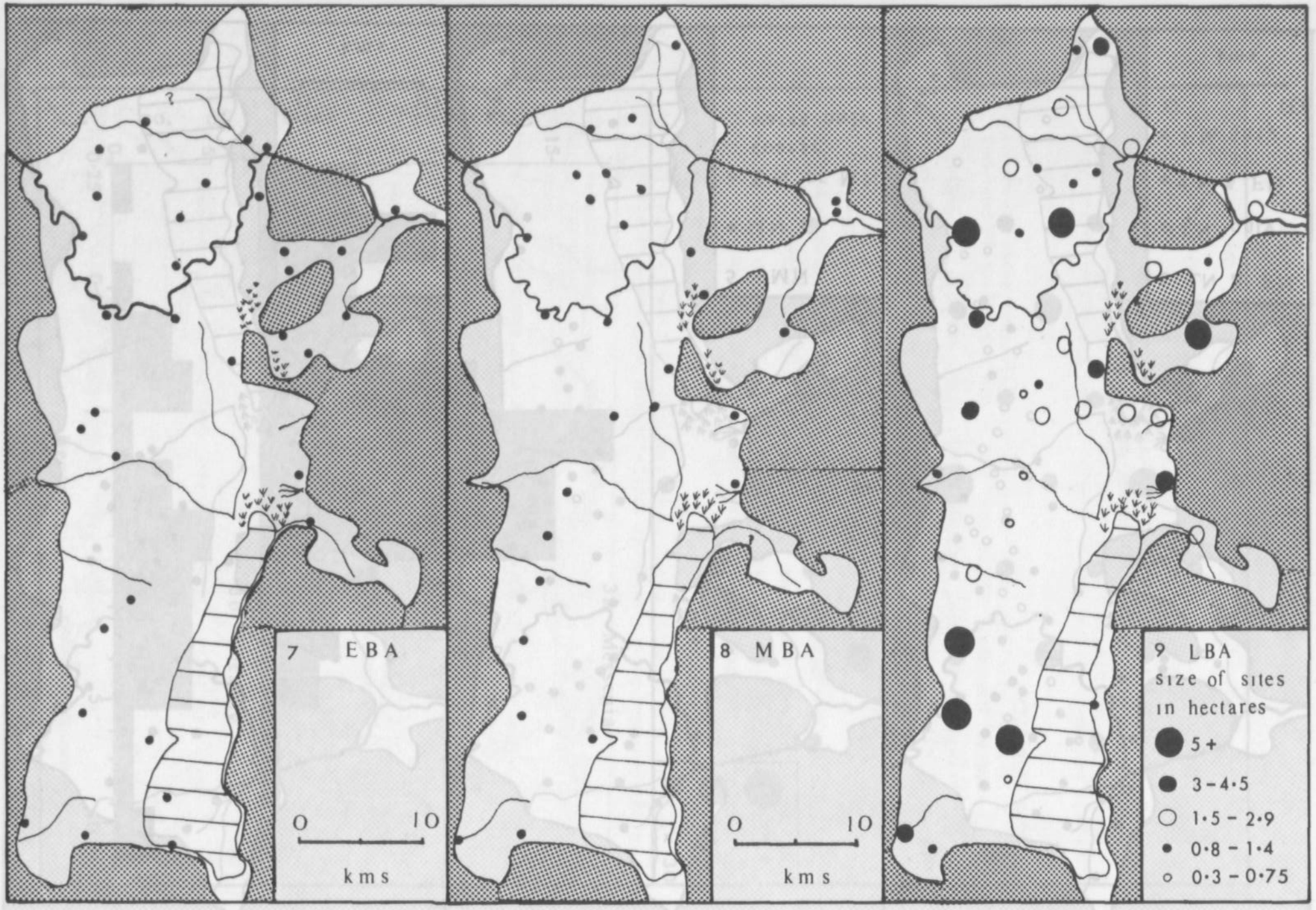
Fig. 10: E. Thessaly - changes in settlement size through time.

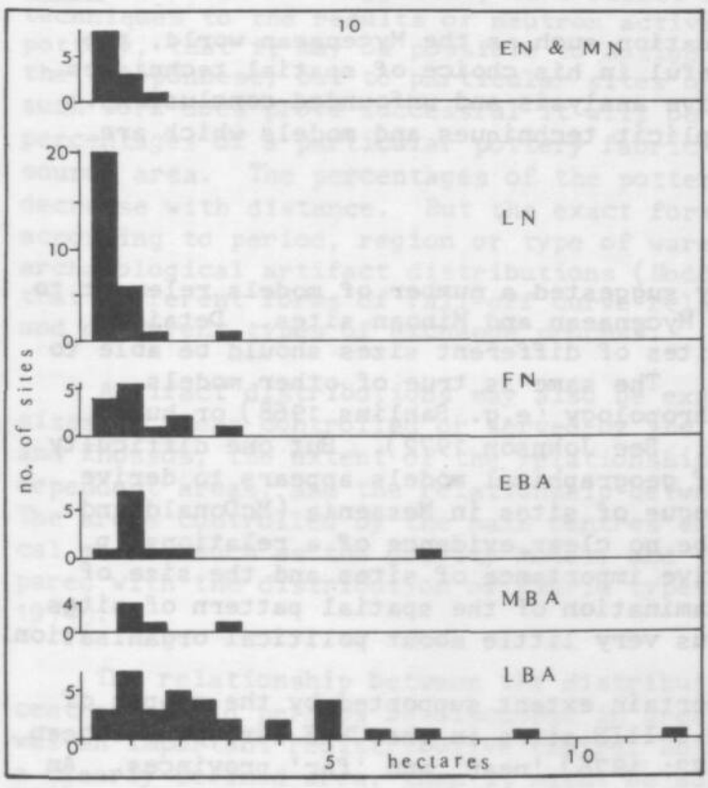
Fig. 11: E. Thessaly - population curve. (a) total area of all settlements of each period, (b) suggested changes in size of population through time.

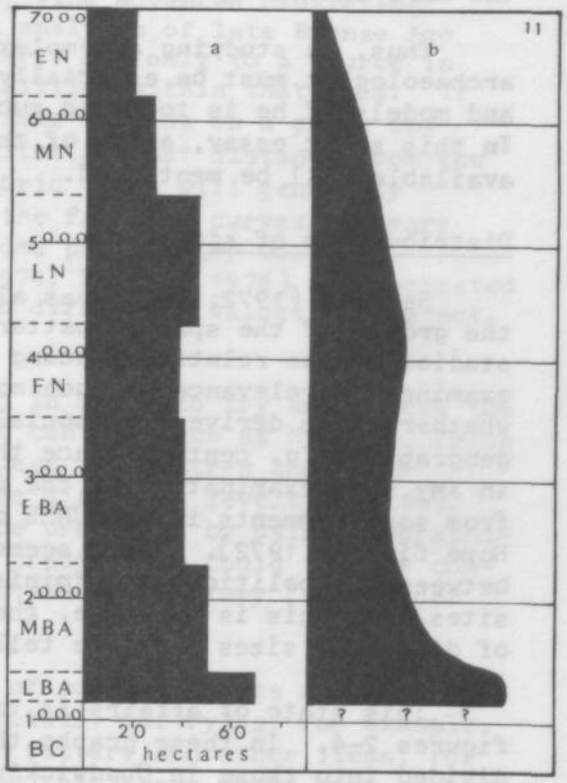



2
× 1
× 6
× 30
3a Messenia
15
10
5
0
no. of sites
3b Thessaly
5
0
0·25
0·5
1
5
10
hectares


4 EN
0
10
kms
5 MN
• MN
o EN or MN coarse wares only
6 LN & FN
o LN only
• LN & FN
• FN only
0
10
kms


7 EBA
0 10
kms
8 MBA
0 10
kms
9 LBA
size of sites
in hectares
5+
3-4·5
1·5 - 2·9
0·8 - 1·4
0·3 - 0·75





GEOGRAPHICAL TECHNIQUES AND MYCENAEAN ARCHAEOLOGY

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An inexplicit and non-rigorous approach to spatial patterning is dangerous and this will be shown by an example. But the detailed analysis of geographical form and variation, and the application of model-building procedures, can release much information about social, political and economic organisation.

It is first necessary to explain why it is that explicit procedures and models, as opposed to a subjective and non-rigorous approach, are necessary in any analysis of spatial patterns. In figure 1 is shown a distribution of points which were allocated to an area at random. 5 science and social science postgraduates and undergraduates at Leeds University were asked to draw on to the points any patterning in the distribution which they could see (I am grateful to P. Wardle for carrying out this trial). Not only are the patterns seen by the 5 students very different (similar variation was found with other distributions of points), but the approaches towards the detection and illustration of pattern also differ. The assessment of spatial pattern is even more difficult when, instead of a distribution of points, the archaeologist is faced with the distribution of a variable (such as the sizes of sites or the percen-

tages of a pottery type). In general, the more information there is on maps, the more susceptible are those maps to different and contradictory assessments.

Thus, in studying a complex situation such as the Mycenaean world, the archaeologist must be especially careful in his choice of spatial techniques and models if he is to avoid subjective analysis and unfounded conclusions. In this short essay, a few of the explicit techniques and models which are available will be mentioned.

Distributions of sites

Renfrew (1972; 1975) has already suggested a number of models relevant to the growth of the spatial pattern of Mycenaean and Minoan sites. Detailed studies of the relative spacing of sites of different sizes should be able to examine the relevance of such models. The same is true of other models, whether these derive from social anthropology (e.g. Sahlins 1968) or human geography (e.g. central place theory. See Johnson 1972). But one difficulty in any such examination of the fit of geographical models appears to derive from some comments in the UMME catalogue of sites in Messenia (McDonald and Hope Simpson 1972). There seems to be no clear evidence of a relationship between the political or administrative importance of sites and the size of sites. If this is the case, then examination of the spatial pattern of sites of different sizes might be telling us very little about political organisation.

This state of affairs is to a certain extent supported by the graphs of figures 2-4. In these graphs the LH I-IIIB sites in the UMME survey have been divided into those in Chadwick's (1972; 1976) 'near' and 'far' provinces. An alternative division of the sites has been made in order to separate those from the Kyparissia valley. In each of these groups, the sites have been ranked from largest to smallest. The rank of each site has been plotted against its size in the graphs (see Haggett 1965). Apart from the fact that there are few sites below a certain minimum size, the graphs show a fairly smooth and gradual decrease in size. There are no distinct stepped levels of the type predicted by Berry (1961) and Crumley (1976) for less-developed or markedly hierarchical societies. In addition, the political heartland of the Pylos region (that is the 'near' province) does not have larger sites than the 'far' province. However, it is of interest that there is less variation in the sizes of settlements in the Kyparissia valley. In this northerly region there are none of the larger range of settlements found in the south. This difference may be due to variation in agricultural potential and productivity. But it also seems possible that the difference relates to the greater complexity of political and administrative life to the south. Indeed, there are more large sites as one approaches the core area around Pylos.

Crumley (1976) has suggested that variation may occur in the rank-size graphs over time during the development of state systems. If further work allows those sites inhabited before the Late Helladic to be identified with some confidence, then it will be possible to examine hierarchical development and contribute much towards the understanding of the growth of complex societies.

Distributions of artifacts

Information about the spatial organisation of society can be gleaned not only from distributions of sites, but also from the detailed distributions of artifacts. The flow of goods, artifacts and ideas through the spatial system of sites may be able to indicate the area of influence of each centre and the relationship between centres.

Artifact distributions can be examined and modelled in a variety of dif-

ferent ways. Consider a situation in which the distribution of pottery or stone from an identifiable source or source area is known. For example, Bieber et al. (1976) have suggested, as a result of applying advanced clustering techniques to the results of neutron activation analysis of late Bronze Age pottery, that it may be possible to assign pottery, not only to a source in the Peloponnese, but to particular sites or localities within that area. If such work does prove successful it will be possible to plot on a graph the percentages of a particular pottery fabric at sites against distance from the source area. The percentages of the pottery fabric types will generally decrease with distance. But the exact form of the fall-off curves may vary according to period, region or type of ware. Some preliminary work on archaeological artifact distributions (Hodder 1974; Renfrew 1976) has suggested that different forms of fall-off curve relate to different 'values' of object, and different types of exchange process.

Artifact distributions may also be examined in order to try and assess the sizes of areas controlled or served by the major centres such as Mycenae, Pylos and Knossos, the extent of the relationship between the centres and their dependent areas, and the relationship between the different levels of sites. The areas controlled by the main centres could be predicted by using geographical models such as the gravity model, and these predictions could then be compared with the distribution of fabric types or styles of pottery (cf. Hodder 1974).

The relationship between the distributions of pottery styles and major centres should perhaps be discussed in greater detail. If Pylos, for example, was an important redistributive centre, both for pottery and other items, for a clearly defined area, then it might be supposed that there would be a high level of similarity in pottery assemblages between sites in that area (Fry and Cox 1974, 210-11). This may not only be true of pottery fabric groups but also of stylistic traits. A high degree of stylistic homogeneity might be expected in the areas served or controlled by the major centres. This is especially the case if pottery production was controlled to the same extent as the local bronze production seems to have been controlled by Pylos (Chadwick 1976). In any such situation, the centralised pattern of contact (whether this contact involved the movement of pottery or not) may have led to an inward-looking retention of minor stylistic traits. An alternative situation is one in which the movements, contacts and relationships between people are not highly centralised; the inhabitants of minor settlements may have travelled to obtain pottery from, or may frequently have visited, a variety of different centres. Specialist potters may not have been based solely in the major centres, and may have worked in more than one 'realm'. In this more decentralised situation there would be less clear areas of distinct pottery similarity around the major sites.

There are several other ways (such as trend surface analysis and the analysis of spatial autocorrelation: Hodder and Orton 1976) in which it would be possible to examine how the Cretan and Mycenaean centres functioned in relation to their surrounding areas. But this general type of approach - that is looking at the detailed distributions of fabrics and styles of pottery and perhaps other artifacts around the major centres - seems especially appropriate in the Mycenaean context. This is because attempts have already been made to reconstruct, using documentary evidence, the areas controlled by, for example, Pylos and Knossos (Chadwick 1972; 1976). It ought to be possible to test these ideas by detailed artifact studies. It ought also to be possible to develop detailed models about the functioning and economic importance of different types of site. For example, exactly how independent of Knossos were the minor centres and palaces in Crete, and how big were the areas dependent on them?

But is this sort of approach possible in the Mycenaean and Minoan world?

For example, is there sufficient stylistic variation in pottery to allow detailed study? The period of greatest uniformity in pottery styles is usually accepted as being LH IIIA-B (Desborough 1964, 3), but Desborough (ibid.) also notes that the relative popularity of shapes and decorative motifs does vary regionally (Sherratt, pers. comm.). Sackett et al. (1966, 105) have suggested that LH IIIB painted pottery in Euboea is of local manufacture and that there is no reason to suggest an exterior source for the Euboean LH IIIA pottery.

In LH IIIC, "the diversity of local styles is very marked" (Desborough 1964, 9), especially in the relative popularity of particular pottery shapes (ibid., 10). Regional variation in IIIC pottery assemblages has also been noted by Popham and Milburn (1971, 347).

Thus, both the analysis of pottery fabrics (e.g. Bieber et al. 1976), and stylistic considerations, suggest some degree of regional variation in pottery throughout LH III. But if further information about spatial organisation is to be gleaned from the data, then the precise structure and form of the regional variation must be examined. To what extent do pottery assemblages from sites vary? What is the structure of the fall-off in similarity between assemblages with distance? Do any marked boundaries or breaks in the fall-off occur? What is the relationship between the spatial pattern of similarities and the distribution of major sites? Two examples suffice to show that there is a potential for a long programme of detailed work to answer such questions and to release considerable information for the archaeologist. However, it is important to stress that the aim of this paper is only to illustrate a methodology and its application to Mycenaean pottery. No more is intended.

One specific and easily identifiable type of pottery which occurs in LH IIIC in the Aegean (beginning in LM IIIB in Crete) is the stirrup jar with octopus decoration. Desborough (1964, 271) notes that such jars are found commonly in Crete, but that they were "also made in the Dodecanese, on Naxos and at Perati". But, "not one of these stirrup jars has been found in the extensive cemeteries of Achaea and Kephallenia. Their rarity in the Argolid is also remarkable" (ibid., 272). Stubbings (1947, 23) also noted that the octopus stirrup jars "exhibit various differences in style, and may therefore reasonably be supposed to have been manufactured in different places. .... The clay and paint of the Dipylon example show clearly that it is a local product". Stubbings (ibid.) also suggests that the Porto Raphti road example is an import from Rhodes. At Perati, it is stated that the octopus stirrup jars are sometimes so alike in the detail of decoration that they must be the work of the same vase painter, while some of the jars show a style "probably originating in Naxos, but conceived and designed locally" (Iakovides 1969, 435-6).

Although a number of researchers have noted the regional variation in the style of the octopus jars, there has been no attempt to quantify this variation and examine its structure. The author was able to obtain quantified data from a small number of published jars of this type. These came from Greece and the Aegean and included 10 from Perati, and the information collected comprised the body width/total height ratio, the body width/width of spout ratio, and the body width/distance between handles ratio. Using average figures from Perati, the similarity between the jars from the 6 sites could be assessed. In figure 5 each point indicates the similarity between a pair of jars related to the distance between those jars. In view of the comments made at the beginning of this paper, it must be said that no very marked trend emerges from the graph, although there is a suggestion that nearby jars are less dissimilar than widely spaced jars. Distance may therefore have some relationship to the similarities between the jars, and the fact that widely spaced jars can be extremely similar may be the result of trade (cf. comments by Stubbings noted

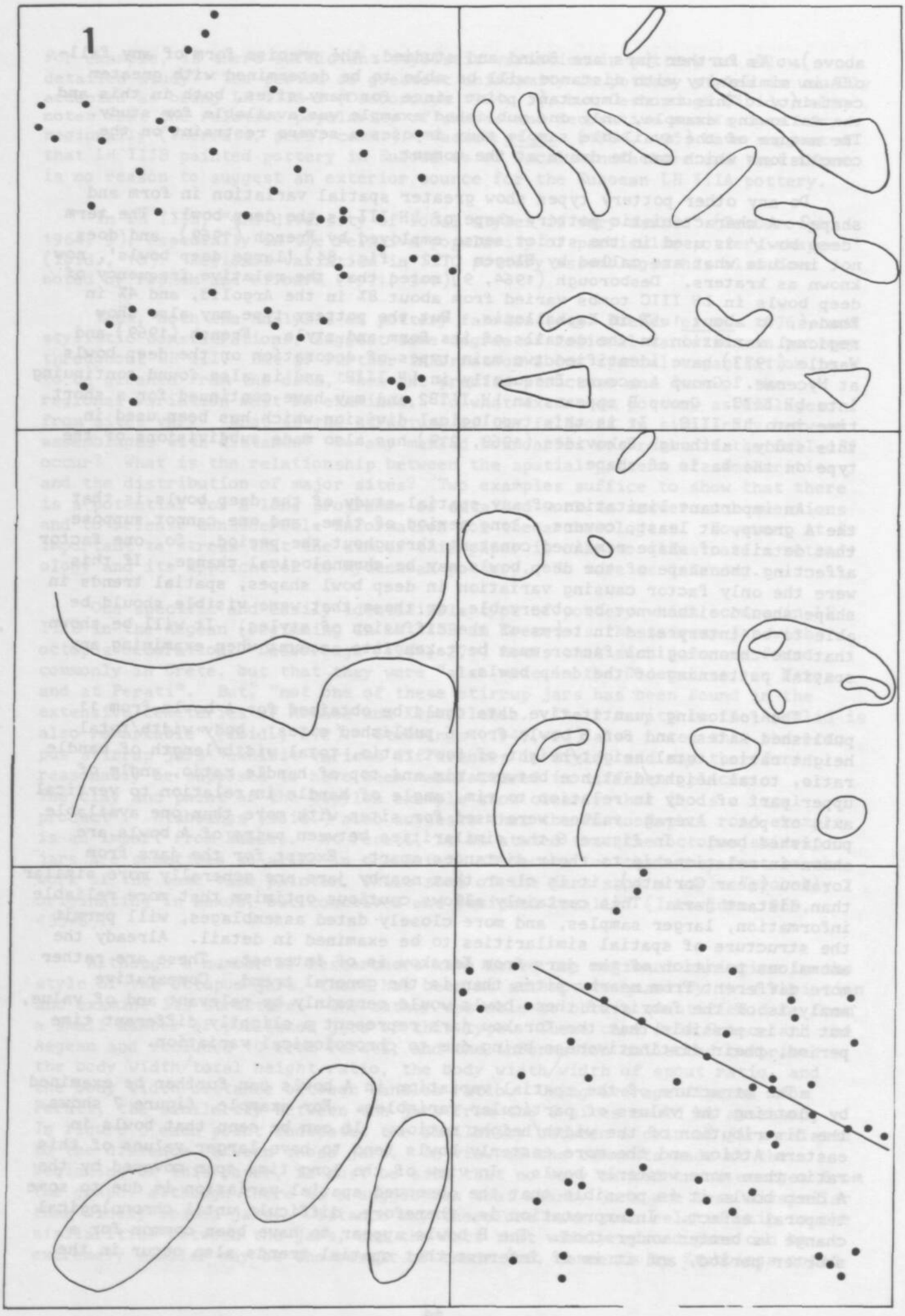
above). As further jars are found and studied, the precise form of any fall-off in similarity with distance will be able to be determined with greater certainty. This is an important point since for many sites, both in this and the following example, only one published example was available for study. The nature of the available sample thus imposes a severe restraint on the conclusions which can be drawn at the moment.

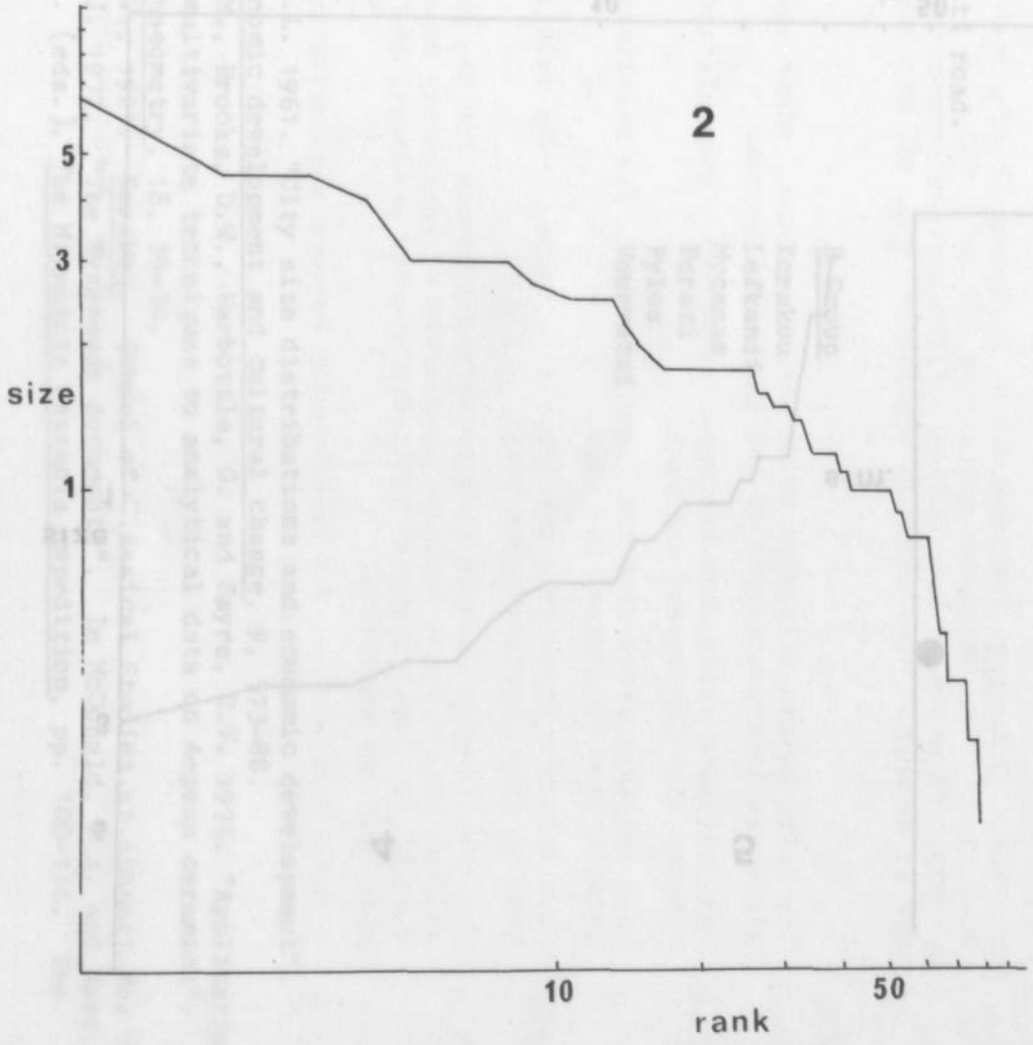
Do any other pottery types show greater spatial variation in form and shape? A characteristic pottery shape of LH III is the deep bowl. The term 'deep bowl' is used in the strict sense employed by French (1969), and does not include what are called by Blegen (1921, fig. 84) 'large deep bowls', now known as kraters. Desborough (1964, 9) noted that the relative frequency of deep bowls in LH IIIC tombs varied from about 8% in the Argolid, and 4% in Rhodes, to about 1.5% in Kaphallenia. But the pottery type may also show regional variation in the details of its form and style. French (1969) and Wardle (1973) have identified two main types of decoration on the deep bowls at Mycenae. Group A occurs frequently in LH IIIB1 and is also found continuing into LH IIIC. Group B appears in LH IIIB2 and may have continued for a short time into LH IIIC. It is this typological division which has been used in this study, although Iakovides (1969, 219) has also made subdivisions of the type on the basis of shape.

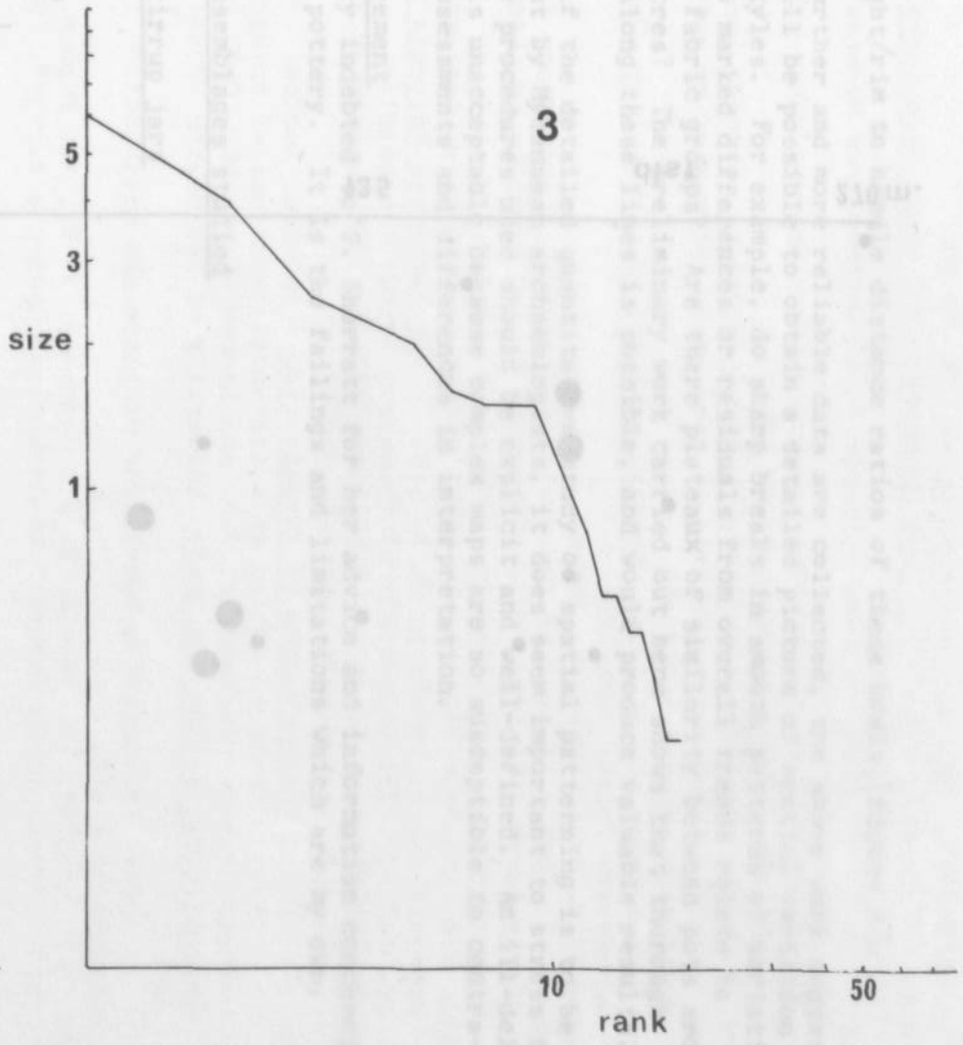
An important limitation of any spatial study of the deep bowls is that the A group, at least, covers a long period of time, and one cannot suppose that details of shape remained constant throughout the period. So, one factor affecting the shape of the deep bowls may be chronological change. If this were the only factor causing variation in deep bowl shapes, spatial trends in shape should either not be observable, or those that were visible should be able to be interpreted in terms of the diffusion of styles. It will be shown that the chronological factor must be taken into account when examining any spatial patterning of the deep bowls.

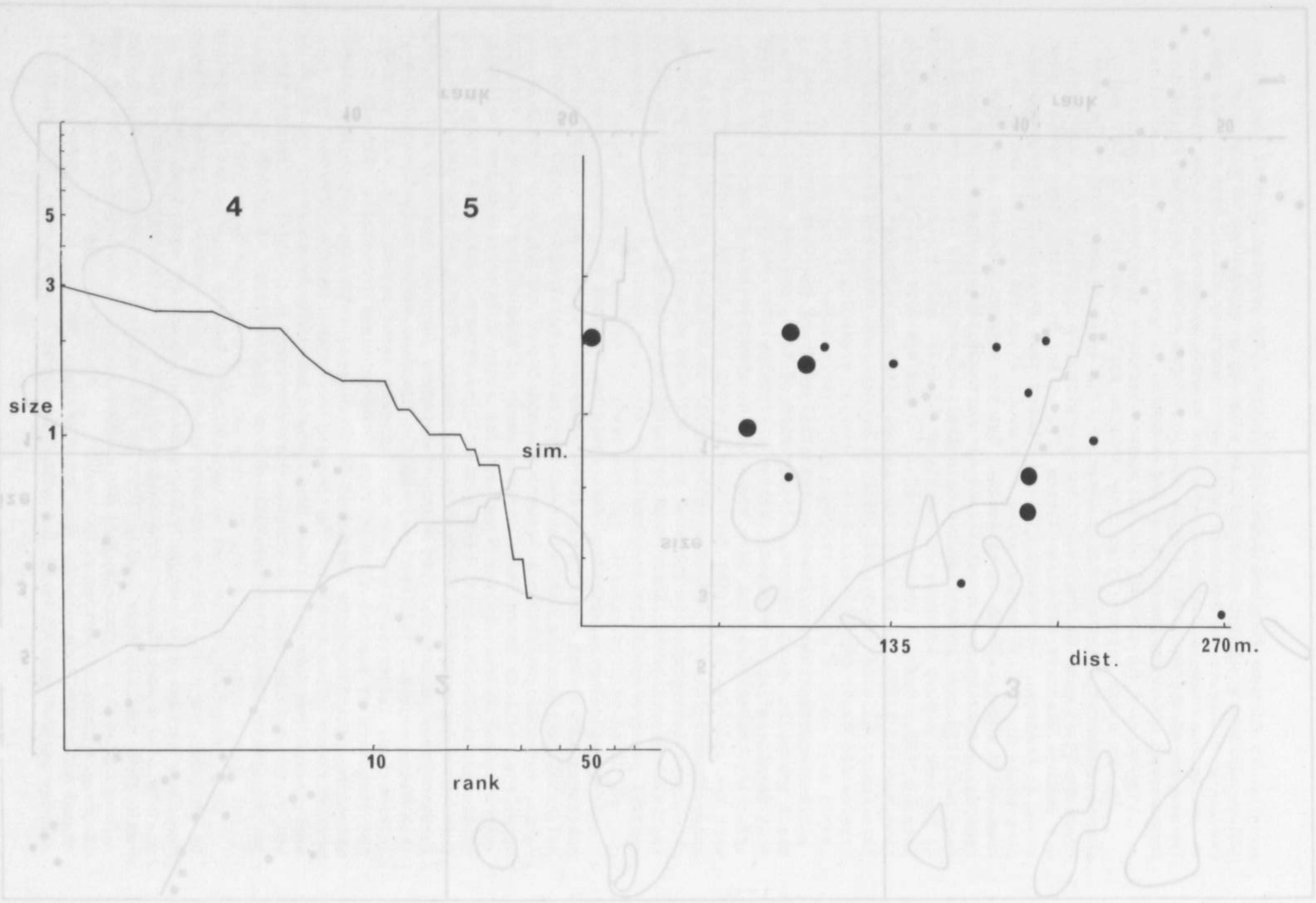
The following quantitative data could be obtained for A bowls from 11 published sites, and for B bowls from 6 published sites: body width/total height ratio, total height/height of foot ratio, total width/length of handle ratio, total height/distance between rim and top of handle ratio, angle of upper part of body in relation to rim, angle of handle in relation to vertical axis of pot. Average values were used for sites with more than one available published bowl. In figure 6 the similarities between pairs of A bowls are shown in relationship to their distances apart. Except for the jars from Korakou (near Corinth), it is clear that nearby jars are generally more similar than distant jars. This certainly allows cautious optimism that more reliable information, larger samples, and more closely dated assemblages, will permit the structure of spatial similarities to be examined in detail. Already the anomalous position of the jars from Korakou is of interest. These are rather more different from nearby sites than is the general trend. Comparative analysis of the fabric of these bowls would certainly be relevant and of value, but it is possible that the Korakou jars represent a slightly different time period, their distinctiveness being due to chronological variation.

The structure of the spatial variation in A bowls can further be examined by plotting the values of particular variables. For example, figure 7 shows the distribution of the width/height ratios. It can be seen that bowls in eastern Attica and the more easterly bowls tend to have larger values of this ratio than more westerly bowls. In view of the long time span covered by the A deep bowls it is possible that the observed spatial variation is due to some temporal effect. Interpretation is, therefore, difficult until chronological change is better understood. The B bowls appear to have been common for a shorter period, and it is of interest that spatial trends also occur in the


1


2
5
3
1
size
10
50
rank


3
5
3
1
size
10
50
rank


4
5
size
5
3
1
10
50
rank
sim.
135
270 m.
dist.

total height/rim to handle distance ratios of these bowls (figure 8).

If further and more reliable data are collected, the above work suggests that it will be possible to obtain a detailed picture of spatial variation in pottery styles. For example, do sharp breaks in smooth patterns of variation occur? Do marked differences or residuals from overall trends relate to different fabric groups? Are there plateaux of similarity between pots around major centres? The preliminary work carried out here shows that thorough research along these lines is possible, and would produce valuable results.

But if the detailed quantitative study of spatial patterning is to be carried out by Mycenaean archaeologists, it does seem important to stress that any future procedures used should be explicit and well-defined. An ill-defined approach is unacceptable because complex maps are so susceptible to contradictory assessments and differences in interpretation.

Acknowledgement
I am deeply indebted to S. Sherratt for her advice and information concerning Mycenaean pottery. It is the failings and limitations which are my own.

Pottery assemblages studied

Octopus stirrup jars

Kos
Monemvasia
Mouliana
Naxos
Perati
Porto Raphti road.

Deep bowls

A Group	B Group
Athens	Korakou
Ialysos	Lefkandi
Kalymnos	Mycenae
Korakou	Perati
Kopreza	Pylos
Lefkandi	Vourvatsi
Mycenae	
Pikermi	
Prosymna	
Pylos	
Vourvatsi	

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Captions

Figure 1. Top left: points allocated to an area at random. Other squares: different interpretations of the same distribution.

Figure 2. Late Helladic sites from the 'near' province. The relationship between size (hectares) and rank.

Figure 3. Late Helladic sites from the 'far' province. The relationship between size (hectares) and rank.

Figure 4. Late Helladic sites from the Kyparissia valley. The relationship between size (hectares) and rank.

Figure 5. The relationship between similarity and distance for the octopus stirrup jars. Large symbols: assessment of similarity based on three variables. Small symbols: published sources prevented the use of more than two variables.

Figure 6. A group deep bowls. The relationship between similarity and distance. Open circles: bowls from Korakou.

Figure 7. The distribution of the width/height ratio for the A group deep bowls. The size of the circles indicates the magnitude of the ratio.

Figure 8. The distribution of the height/rim to handle distance ratio for the B group deep bowls. The size of the circles indicates the magnitude of the ratio.

SECTION TWO: THE TEXTUAL EVIDENCE

(Chairman: Prof. A. Snodgrass)

THE INTERPRETATION OF MYCENAEAN DOCUMENTS AND PYLIAN GEOGRAPHY

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When we began to study the clay tablets found in Mycenaean palaces, it was immediately clear that we had nothing but lists of persons, animals, materials and manufactured objects. But these cryptic lists also contain a number of words which we could identify as names of places or adjectives denoting origin. A few of these are instantly recognisable as known place names of the area; others could be identified partly by their having derived adjectives, partly by their occurrence with a suffix meaning 'towards'. Then, once a few names had been identified as geographical terms, it was possible to assign many other words to the same class by observing that they occupied the same position in a formula as a known place name. Thus although there are many doubtful cases, it is possible to establish lists of the place names in use at the two main sites, Knossos in Crete and Pylos in Messenia; the material from the other sites is still too scanty to be useful for this purpose.

Naturally the first thing we looked for was place names which had retained the same form into classical times. This was a little easier in Crete than in Messenia; but in both cases there were a great many names that did not appear in accounts of the classical geography. Hence we had to find other methods to establish possible locations for these names, and we should be interested in any techniques which can confirm or extend our inferences from these documents.

If we assemble all the references for each place name, we can in some cases deduce a certain amount about the places. If a name is mentioned mainly as a location of sheep, but appears not to have other agricultural products, we might guess that it lay in a tract of hill country with good pasturage but little arable land. If it produces timber, it must have extensive woods. If it is the location of seamen, it will be a port, and so forth. Production returns, or as I prefer to call them, tax returns may also give an indication of the size of a settlement, though there is obviously the danger that a town which produces a great deal of one product may simply specialise in that product, and is not necessarily bigger than one which produces less. But some grading of settlements is possible by this means. A further problem here is that the production of a district may be recorded under the name of its principal settlement, and we cannot always tell whether a name refers only to the settlement and its adjacent land, or whether it stands for the whole district. There are also cases where two

names appear to be interchangeable, probably because one refers to the district, the other to its capital.

We have sometimes been attacked for attributing apparently excessive production to particular places; but it is not we who are to blame, for all we do is to report the documentary evidence. But it is easier to accept some of these high figures, if we always remember that the name of a town may stand for a whole district. There is no room on the hill of Phaistos for all the sheep located by the tablets at this site, but we must understand Phaistos here as standing for a large area, probably the whole western half of the Messara.

Now I shall leave Crete to Dr. Killen and talk about the place names of the Pylos tablets. We have about 1200 tablets, some of which are fairly large, and in a few cases we have a series of tablets making up a long document. Much more often the tablets are small and contain little information; but these too are like single cards from a card-index, and we can often reconstruct the contents of a file, though of course it is rarely complete and we cannot determine the order of the items. The date to which the tablets refer is the year immediately preceding the destruction of the Palace, probably about 1200 B.C.

Naturally the first task is to compare the list of place names with those known from classical geography. But immediately the question arises: within what limits can we seek parallels? There are names on the list which resemble Korinthos, Olympia and Iasos. Corinth is an impossible area for control from Pylos, and Iasos in eastern Arcadia is hardly more plausible. But Olympia looked at first more attractive, since the Iliad places the northern frontier of Nestor's Pylos on the Alpheios, and Olympia is on the north bank of the river. Against this one may argue that the Mycenaean form is too different to be identified with Olympia. Other examples may be cited to show that this method leads to utter confusion. This is because place names in the course of history move from one site to another, either by relocation within the same district, or because people abandon their homes and move to a different area. Consequently names are frequently duplicated, and all may in an earlier age have been located elsewhere. The evidence of Homer (8th century B.C.) is self-contradictory and in my view worthless; few of the towns other than Pylos which he mentions in this area are to be found on the Mycenaean documents.

The tablets confirm that Pylos is the name of the Palace and the surrounding town. It is clear that it does not figure in taxation documents, and stands outside the hierarchy of minor towns. I suggest therefore that computer studies should ignore it, since its inclusion can easily distort the picture.

For administrative purposes the country was divided into two provinces, which we call Hither and Further as a rough translation of the Mycenaean names. Each province was subdivided into districts. The Hither Province contained nine districts, the Further Province seven, though some documents give a total of eight, one district being divided into two. The total number of place names is over 150, and is probably around 200; but it is often difficult to judge whether a place lies within the kingdom or not.

The nine districts of the Hither Province are enumerated in at least four lists in a constant order. Since on the evidence of the tax-returns the order is not based upon magnitude, nor is it a quasi-alphabetical one, it must be geographical; but how a writer who has never seen a map will order the surrounding towns is something we cannot guess. The seven districts of the Further Province are once listed in what may have been a standard order; but study of their tax-returns has demonstrated that they seem to form four groups of roughly equal size. We have another important document which specifies the establishment of an observer corps to keep watch over the coast-line; hence all the places

mentioned as the location of look-outs must be on the coast. Of course many of these places are not mentioned elsewhere.

To establish the frontiers I make three assumptions. (1) There were similar kingdoms elsewhere in the Peloponnese, certainly in Lakonia and the Argolid; another is probable in the north-west; hence the size of the kingdom of Pylos is unlikely to be more than a quarter of the Peloponnese. (2) The capital at Pylos will have been located so as to maintain easy communication with all frontiers, and is unlikely to be very eccentric. (3) The frontiers will be delimited by geographical features and will preferably be easy to defend.

From these assumptions it follows that the sea formed the western and southern boundaries; the only frontiers to be ascertained are those to the east and north. The eastern frontier can hardly lie beyond Tayghetos, the high mountain which divides the south of the Peloponnese into two halves. The northern is best placed on the river Neda and the mountainous area immediately to the south of it, for it is not only easily defensible, but is remarkably bare of habitation sites, both ancient and modern. The lower range of mountains running north-south nearer the Palace will have split the kingdom into two halves, which we can identify roughly with the two provinces.

The Hither (West Coast) Province is mostly a narrow coastal strip; hence the fixed order of districts is probably a north-south sequence. Since the first and second districts have connexions with the Further Province, they should be located in the valley system which connects the west coast at Kyparissia with the Upper Pamisos valley. The last district has a name which is reported by a classical source as an old name of the town now called Koroni in the south of the peninsula. We believe that Pylos was situated within or adjacent to the fourth district. Thus a north-south order, with slight adjustment to the east at either end, seems certain.

The seven (or eight) districts of the Further Province are less easy to place since the area is a rectangle not far short of square. There is reason to place its eastern frontier on the river of modern Kalamata, and the eastern shore of the gulf is probably excluded. However, the grouping of the districts into four regions corresponds to the natural division of the territory. The river Pamisos and its tributaries form a north-south division, and a low range of hills an east-west one. Two of the districts can be plausibly assigned to the SW and NW regions, hence the location of the others can be guessed approximately.

The name Kuparissos (Greek for cypress tree) is located by the tablets towards the north of the Hither Province. A town of this name or very similar is mentioned by Homer and existed in classical times on the site now again called Kyparissia; the exact location of the Mycenaean site is unknown, but is probably in this area.

In the Further Province there seems to have been a royal establishment, probably like Pylos an administrative centre. Its name is Leuktron, but the classical site of this name (Leuctra in the Mani) is too far away. A site (Nikhoria) in the SW region has recently been dug by an American expedition, but it seems more likely that this is only a district capital.

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DISCUSSION: (Taking part: Colin Renfrew, Oliver Dickinson, John Bintliff).

RENFREW: I should be interested to hear your reaction, if I were to say as a kind of Devil's advocate, that accepting most of your principles, but taking up this very interesting point, that the Hither Province and the Further Province may have been amalgamated; following the logic of that it would not necessarily follow that a new capital would be set up. So if the Hither Province came to dominate the Further Province, and the Hither Province had its capital originally at Pylos, then on your analogy for instance of England and Scotland becoming unified, London becomes the capital of Great Britain, - an equally eccentric centre. And I'm interested in this question of the scale, and I don't think it would really violate the spirit of your principles if one were to suggest that Laconia might have been assimilated to an original Hither Province, as the Further Province - including the two regions you have already suggested, so that the final situation would be Laconia plus Messenia. The capital of the Further Province might then be easier to locate, perhaps at the Menelaion, rather than in the Pamisos Valley. Obviously you've got lots of points of correlation which check up rather nicely, but I'm not sure this contradicts your principles.

(J) CHADWICK: No, in theory I don't think I could argue against that. It's perfectly true, and I think this is confirmation of this idea, that there has been an amalgamation of two separate administrative units - one hesitates to call them 'kingdoms' if you get smaller and smaller units. But the location of the palace at Englianos is very good for the Hither Province, it is not so good though perfectly tolerable if you then throw in the Further Province. If you were then to throw in Laconia it would I think become gravely eccentric, and there would be a temptation to found a new palace, though I must say I don't know where. One of the points which I am trying to make here, and having discussed this at great length with Dick Hope-Simpson and Bill McDonald, who know this area much better than I do - we all feel that the country divides itself naturally into these units, this is not something we are superimposing, this is something which flows from the natural look of the land and you can see it when you go and look at it. It's awfully dangerous to do what we are doing here, look at maps and say 'Oh, yes, well why don't we bring this in.' But if you actually go and stand there - you stand in Kalamata and say 'Could I administer Laconia from here,'- and I think you will say at once 'No'. It's quite impossible. Equally it must have been very difficult for the Spartans to administer Messenia from Sparta. There is a grave problem here, for we do know historically that this is true - but it is not a natural thing and it must be the result of a fairly artificial situation. I am supposing here that Mycenaean kingdoms tended to follow for the most part natural lines of division. And when we were still debating the question of the northern frontier, I went out and spent some time with Hope-Simpson and McDonald while they were still doing their surface survey and they said 'Well there's no problem about it, this is quite obvious, it must be the Nedha. Come on and we'll show you'. And they did.

DICKINSON: Perhaps I could suggest a third model, which would be in the area, the same area, but it's always seemed to me, that on the basis of where the sites really are, the two greatest concentrations of settlement are the Pylos district through to Nichoria and the Messenian Gulf, and the Kyparissia Valley through to the Upper Pamisos Valley. Moreover the Kyparissia Valley in early Mycenaean times has one of the most important sites in Messenia - Peristeria. Whereas Nichoria, which is one of the most important known to me at the moment in the eastern provinces, is fairly minor by comparison. Now would it be absolutely

impossible to have your Hither and Further Provinces on either side of the northern and much more barrier-like Aegaleos Range, because as you say there is a saddle further south, and if one travels across from Nichoria to Pylos, as I've had occasion to do many times, one isn't conscious of crossing a very difficult area, in fact. And I would conceive, on this model, of the Hither Province being as it were the southern half - the division in fact being between the Kyparissia Valley and perhaps the Upper Pamisos at that end, and the Pylos district going across to Nichoria and including the rest of the peninsula to the south, at the other end. I know you've quoted things that would not fit that.
CHADWICK: The major objection to this, is simply that if we are right in saying this is a north to south order, and there is very good reason for that, namely as I have said, the first two areas fit very closely with the Further Province, - that suggests they must be near the Kyparissia Valley if not actually in it. The first one incidentally is one of the major sheep areas, and if you know that area , I'll think you'll agree that it is a prime region for sheep grazing. Now if you put those areas then south of Kyparissia, where I agree there is a perfectly good geographical division, you then have to get four districts between there and Englianos. And that seems to me to be crushing the scale to such an extent that you cannot possibly get in any more to the south, particularly as one of those areas, the Petono area, is the biggest on the tax returns, and that fits in on my theory in the region of Filiatra.
BINTLIFF: If I could add a point. I think there is more evidence for the plausibility of what Oliver is saying. Not only just looking at the LH distribution maps, as he says, is it clear that we have an almost solid block along the Kyparissia Valley into the Upper Pamisos Valley, then another major distributional block from the Pylos region right across to Nichoria. As for drawing divisions, it is almost impossible to further subdivide these groups of sites, just two solid masses, then the rest of the country is covered with separate and smaller groups. But also geographically these two major zones are almost continuous areas of very good soils. So you've got two natural pockets with dense continuous occupation, and if one was looking at them from a mildly deterministic approach, as leading naturally to some kind of small kingdoms, then one would be happiest in seeing these two units as separate, and later brought together as the two natural provinces. Now if Dr. Chadwick is right, there is then an interesting problem as to why these natural units have been unnaturally broken up - how it occurs, why it occurs, for I think it seems to be against the geographical situation, to divide Messenia vertically instead of horizontally.

THE KNOSSOS TEXTS AND THE GEOGRAPHY OF MYCENAEAN CRETE

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What I'd like to try and do in this short paper is to look at the work that's been done since the decipherment of Linear B on locating the place-names on the Knossos tablets on the map of Crete. I shall be drawing heavily on the work of others, notably Dr. Chadwick [1], Miss G. R. Hart [2], Professor L. R. Palmer [3] and Dr. J. L. Melena [4]: though I shall have one or two contributions of my own to add, particularly towards the end of the paper.

I'd like to begin by saying a word about method. The technique which all the writers on the subject so far have adopted, in trying to locate the 70+ places on the tablets, is first of all to try to establish relationships between places by examining which places tend to occur on the record in contact with

which others, and, second, having done this, to attempt to establish the location of places whose identity is not transparent from their relation to the (relatively few) places whose spelling on the tablets, together with the evidence we have for their relative location, enables us to identify them, virtually certainly, with known sites. Given their spelling and the evidence on the tablets for their relative location, we can site five places with virtual certainty: ko-no-so, Knossos, a-mi-ni-so, Amnisos, pa-i-to, Phaistos, tu-ri-so, Tylissos and ku-do-ni-ja, Kydonia, Chania; and we can also accept as highly probable the identification of a-pa-ta-wa with Aptera: though the Mycenaean and Classical spellings don't in this case match exactly, the fact that Aptera is close to Chania, and that a-pa-ta-wa and ku-do-ni-ja are regularly associated on the tablets, makes the identification more or less watertight. Its also attractive to identify wi-na-to as Inatos, which we know to have been spelt originally with an initial digamma as Winatos; but since there's not a great deal of evidence in the tablets for the relative position of wi-na-to, this must necessarily remain a somewhat longer shot. And, as I say, the method is to try and establish relations between these more or less fixed points and the unidentified places.

All the writers I've mentioned use what I suppose is best called an intuitive approach to the problem: obviously, mathematics are important in the sense that the number of correlations between places on the tablets is one of the factors which one takes into account in assessing likely sitings; but none of the published studies employ formal statistical methods. I myself also use the intuitive approach in a discussion of geography which will appear in a book I'm writing at the moment on the Cretan textile industry; and in what I've got to say here I will be concentrating on the possibilities and limitations of this method. I must just add, though, that Dr. Wilson, who is with us today, has, in a so far unpublished study, applied statistical methods to the material. I hope he'll tell us something of his methods and conclusions; but I might just anticipate him to the extent of saying that by and large our conclusions do coincide.

So the principle is straightforward, and the method, it would seem, is legitimate: if one does a dry run on ko-no-so and a-mi-ni-so, two places which we can identify and know to have been close to one another, one does find a substantial number of linkages between them. But in practice there are a number of problems.

In the case of some places there is a good deal of evidence for their relationships, and this happily points, not only in the same direction, but also in the direction of one of the identified sites. For instance, there is a good deal of evidence that there is a close link between da-wo and pa-i-to, Phaistos. This includes the fact that they are paired on one of the Dn records of area totals of sheep, which seem likely from all our evidence to list places in adjacent areas. Nor, at the same time, is there evidence which links da-wo with different areas of the island. And I might just add that the conclusion that this evidence suggests that da-wo is close to Phaistos, is supported by evidence of another kind. One of the tablets in the F series ((2) F 852) records what looks like the production of a minimum of 775 tons of wheat at da-wo; and as Dr. Chadwick has pointed out, one of the few places in Crete which is capable of producing wheat in this kind of quantity is the Messara plain (in the south) in which Phaistos stands.

Again, we have evidence from a number of sources that the places mentioned in the Co series of records of mixed livestock in hand 107 form a coherent group. Not only do these appear on the Co records, and not in the other main series of livestock records, the D tablets, which deals with places in the centre of the island: some of the names also appear together in other contexts, notably the Lc(2) CLOTH records, again to the exclusion of the places which appear on the D

tablets. As we shall be seeing in more detail later on, all the evidence suggests that the Co places are located in the west of Crete (they include ku-do-ni-ja and a-pa-ta-wa); and it's again perhaps worth noting that this conclusion is consistent with the evidence of the contents of the tablets which list these places. The Co tablets list oxen, for instance, and the lush coastal plain in the west is the place par excellence in modern Crete where one sees oxen; and among the other series in which some of the names occur is the Np SAFFRON records, reminding one of Evans' observation in The Palace of Minos [5] that a number of place-names in Crete, and particularly in the west, are derived from the Italian term for saffron, indicating that in the Venetian period they were centres for the production of the plant. Writers in the 18th and 19th centuries like Savary mention the area around Rethymnon as being particularly famous for its saffron production [6].

Another series of place-names which appear to form a coherent group, and are probably to be located somewhere near Tylissos, are those which appear together, again to the virtual exclusion of other places, on E 749, Og 833 and the Pp 'ADZE' tablets. And here again, there is possible support for the location of these places in the Tylissos area in the content of one of the records which lists ga-ra, one of the places in the group. Gv 863 almost certainly lists ga-ra in connection with 420 vine plants; and it may not be an accident that the north central area of the island is nowadays the great centre for vine-growing in Crete.

In the case of other places, however, it is much more difficult to establish locations. The tablets may provide evidence which suggests a close link with another place, but only with a place whose location is itself not certainly established. Or there may be very little evidence for linkages of any kind; or what evidence one has may all or mostly come from sources of only doubtful reliability. And this is a point which I think does need emphasis: that not all the evidence which appears to suggest a relationship between places is equally reliable: that experience suggests that evidence of particular kinds needs to be treated with especial caution.

Some of our experience in this matter of reliability is provided by those cases where the tablets pair together places which we know in fact to be widely separated from one another. And from examining the reasons for the pairings in these cases, one can sometimes gain insight into the kind of records where evidence for linkage may not be significant from the point of view of geographical positioning.

E 777, for instance, links Knossos, Amnisos and Phaistos; and while Knossos and Amnisos are near each other, Phaistos is some distance away. We don't know the precise purpose of this tablet, and we can't therefore say why these three places are listed here; but one factor which could be at work is the status of Knossos and Phaistos. Knossos is of course the main administrative centre of the island; and there is also some evidence to suggest that Phaistos, which was of course a palace site in the earlier LM IB period, is a place of major importance. Now it would clearly not be surprising if there were a tendency in records of certain types for major centres to be linked together, rather than places in the immediate vicinity of one another. If one looks at the page of the modern telephone directory which tells one where to ring up for a weather forecast, what one finds grouped together in this case are not places in the same area, but places which are major administrative centres: the kind of county capital which tends to have a meteorological office. And there is in fact evidence to suggest a similar tendency on the tablets. On the Sd CHARIOT records, for instance, all four of the places mentioned appear to be major centres: Knossos (not actually written, but clearly implied on a number of the tablets), Phaistos, Kydonia and the unidentified se-to-i-ja. The likeliest

explanation of the purpose of these place-names is that they indicate where the chariots on the tablets are stored; and it is not difficult to think of reasons why this would have been done exclusively at major centres.

Another tablet which lists places which are widely separated from one another is Ce 59: on 1.3b here, OXEN at ku-do-ni-ja are listed after OXEN at Tylissos. We again don't know the precise purpose of the record; but one possibility is that although the scribe here is following a geographical order in his listing of places, the area between Tylissos and Chania didn't have oxen of the particular kind that he's recording. And this brings me to a group of tablets which are regularly referred to in discussions of geographical location, but whose evidence it seems to me ought to be treated with considerable reserve. These are the Ap tablets in hand 103, which seem to provide summaries of information about various of the groups of women textile workers whom the same scribe is concerned with recording in the Ak tablets. Ap 618, for instance, deals with women described as 'missing' (a-pe-a-sa). Almost certainly, this is 'missing' from Ak groups. Now on 1.1, 618 mentions first do-ti-ja and then *56-ko-we. But it would be extremely dangerous in my view to conclude from this evidence that the two places were close to one another. All that the linkage may mean is that these were among the few areas where the groups happened to have women missing from them. There's precious little evidence elsewhere for a connexion between do-ti-ja and *56-ko-we, and it's perhaps significant that Ap 618 is the only record of its type in the archive (though this could be an accident): suggesting that 'missing' women could in fact have been a rare commodity.

And the danger of using records like Ap 618 as evidence for relationships are highlighted by another record in the same group: Ak(1) (=Ap) 5876. This lists children in the Ak groups qualified as di (which probably means 'under instruction') and it includes references to the two places qa-mo and ri-jo-no. Here we know for certain from the existing Ak tablets that di children are a rare commodity; and the chances must clearly be high therefore that these two places are linked on 5876 simply as two of the few places which happen to have children of this type.

Another kind of evidence which is regularly used for establishing links between places, but which seems to me should again be treated with caution, is that of the scribal hand of tablets. Now there's certainly some evidence that groups of tablets ('sets') written by the same scribe can deal with a particular area of the island, to the exclusion of others. I've already mentioned the Co records of livestock in hand 107 and the (2) Lc CLOTH records in hands 113/115: these, as we have seen, appear to deal exclusively with places in the far west of the island. But there are two dangers in using scribal hand as a guide to location. First, the geographical positioning of the places concerned is fairly clearly not the only factor which can lead to a set of tablets being written by a particular scribe. We have already seen the case of the Sd CHARIOT tablets, where the places listed are all evidently major centres, rather than places in the same area. In other instances, too, there would seem reason to adopt at least some degree of caution. It has recently been argued, for instance, that all the places on the Dq(3) sheep records in hand 217 may be in the same area, because of the identity of hand. But it could be significant that another point in common between all the surviving tablets in the group is that they all list sheep 'owned' by 'collectors', a group of individuals to whom we shall be returning in a moment. Though we can't say for certain, it could be that this is the fact which has led these records to be written by the same scribe, and not any geographical considerations.

Another danger with the evidence of hand is that the fragmentary state of the archive may give one a misleading impression of the area covered by a

particular 'set'. It's sometimes argued that if, say, three of four place-names preserved on tablets in a 'set' have a similar location, the fourth is likely to be in the same area. But if we had the 'set' in its original state, it's quite possible that the missing records would have given us quite a different picture of the area it covers.

Finally, a brief word on another kind of evidence which has been recently put to considerable use in discussions of Cretan geography : that of the distribution of flocks belonging to the so-called 'owners' or 'collectors'. These 'owners' are somewhat obscure figures; but on about 30% of the records in the great archive of sheep records we find the name of what looks like an 'owner', as opposed to the shepherd; and it's plausible to think that these flocks are in some sense less directly owned by the palace than those on tablets on which we find no such name. Now there is some evidence that the distribution of flocks of some of these 'owners' follows some kind of geographical principle: some 'owners' 'have' all or most of their flocks in a particular area, for instance. But in the case of others it's very difficult to see any coherent pattern in the distribution, and some of the recent arguments which assume that the flocks of a particular 'owner' have a single focal point, and grow less frequent as one moves away from that centre, seem to me very hazardous indeed.

So the point, then, does need to be made that one cannot use this material 'raw' as it were: the context for a linkage may be important. And there are two circumstances in which the likely reliability of the evidence would seem of especial importance.

First, obviously, if one hasn't got much evidence, and it' s all from suspect sources, one is going to have to treat any conclusions based upon it with particular caution. Second, if one's evidence points in different directions, the likely reliability of the sources can be of help in assessing where the truth is likely to lie. First, there may be cases where although most of one's evidence points in a particular direction, there are also one or more pieces pointing in a different direction; and it can at least provide encouragement for the belief that one's main evidence points in the correct direction if the aberrant evidence comes from a normally or possibly unreliable source. A case in point here may be the evidence for the location of the so-called Tylissos group. Nearly all the evidence which we have points in my view very strongly towards the north, probably the area around Tylissos itself, as the location of this group; and so coherent is the group on the records that it's attractive to think that all the places in it were reasonably close to one another. There is, however, one piece of evidence which suggests a slightly different picture. As(1) 604 lists MEN in various localities; and it contains references to two of the tu-ri-so - group places, pu-so and u-ta-no, in close proximity to two references to the place wi-na-to. Now it's very attractive, as we have seen earlier, to identify wi-na-to as classical Inatos, which is almost certainly to be located at the modern Ini, just inland from the mouth of the Tsoutouros river on the S. coast of the island. And on the evidence of this record, it might be tempting to pull pu-so and u-ta-no somewhat towards the south of the island. But as I say, all the other evidence points very strongly towards the north as the location of these places; and it could well be significant that although we don't know the precise purpose of (1) As 604, it is a record of a type which we have found elsewhere not to be particularly reliable in matters of geography (like Ce 59), or whose reliability is at least open to some doubt (like the Ap tablets).

I suppose that in this case of the tu-ri-so - group places a statistical approach would produce the same result as I am suggesting for more intuitive reasons: such is the concentration of evidence suggesting a northerly location for the group, that this apparently contrary evidence could be disregarded as

statistically insignificant. But there are perhaps cases where some purely statistical methods might give one, not perhaps a totally misleading picture, but a somewhat distorted one. In his unpublished paper, for instance, Dr. Wilson has pointed (quite rightly) to the strong links between the places da-*22-to and ku-ta-to, and also to the strong links between ku-ta-to and da-wo; and this leads him to place da-*22-to in a group of places which includes pa-i-to and da-wo. But there is also evidence, not perhaps substantial in quantity, but high in quality, which suggests a link between da-*22-to and the west and north of the central region. This doesn't feature in Dr. Wilson's tables, since he disregards cases for linkage between places based on less than 5 correlations; but given its quality, my intuitive reaction would be to say that it ought to have some account taken of it.

So much for method, then: what kind of picture do we get of Cretan geography from the Knossos tablets? In brief, the areas in which the palace appears to be involved are, first, the centre of the island and, second, the west. There is remarkably little evidence to suggest any involvement in the east of the island: a number of identifications of place-names on the tablets with sites in the east have been suggested from time to time: se-to-i-ja as Setaia, u-ta-no as Itanos, ra-to as Lato, ru-ki-to as Lyttos; but all these look prety unlikely, given the associations of the names concerned on the tablets. se-to-i-ja, for instance, seems to have close associations with Knossos; u-ta-no and ru-ki-to are both members of the tu-ri-so group; and such evidence as we have for the siting of ra-to, though there is not a great deal of it, points again towards the centre of the island, and perhaps in particular the Tylissos area. The only place on the tablets which seems certain to have an easterly location is di-ka-ta(-de), which is pretty certainly Dikte; and although there is some dispute as to where Dikte in fact was, the two most probably suggestions, Lasithi and Palaikastro, are both in or towards the east. But though Dikte is mentioned in the tablets, the point is worth making that it only occurs in the context of religious offerings: it's not a place with which the palace is involved in the sense that it extracts tribute from it, or engages in manufacturing industry in it.

There remains, however, the question of the Thebes stirrup jars. A number of jars found at Thebes on the mainland in the 1920's have painted Linear B inscriptions on them; and in a number of cases these inscriptions include one or other of two place-names also found on the Knossos tablets: wa-to and o-du-ru-wo. Cretan place-names (as these certainly are) are indeed a regular feature of Linear B inscriptions on mainland stirrup jars: among those found at sites other than Thebes are da-*22-to (on a jar from Eleusis) and *56-ko-we and (probably) si-ra-ro on jars from Tiryns.

In the 1960's, the clay of the Thebes jars was analysed at Oxford by H. W. Catling and A. Millett [7]; and they reached two conclusions: first, that the jars were indeed, as the names would suggest, Cretan; and, second, given the similarities between their clay and that of pottery from Palaikastro and Zakro, that the far east was their likely provenance. And the suggestion naturally followed that wa-to and o-du-ru-wo might be the Mycenaean names for Palaikastro and Zakro.

Now if this is right, it will be evidence that runs counter to what I've just been saying about Knossos not having any interests in the far east of Crete: wa-to and o-du-ru-wo are, as we have seen, names which actually occur on the tablets. But it now looks as if Catling and Millett's suggestion that the jars come from the east of Crete may have to be abandoned.

First, as L. R. Palmer has all along insisted, the associations of wa-to and o-du-ru-wo on the tablets are a good deal easier to explain if they are in

the far west of the island, rather than the far east. As we have seen, they occur with ku-do-ni-ja and a-pa-ta-wa on the Co tablets; and there's other evidence of a similar nature. Some of us wondered at some stage, given the Catling-Millett suggestion, whether the explanation of the link with Kydonia and Aptera might be that the island was divided into two administrative areas, one dealing with the centre and the other with both the far east and the far west; but there is no doubt that matters would be a great deal more straightforward if wa-to and o-du-ru-wo were in fact, like Kydonia and Aptera, in the far west.

Second: if wa-to and o-du-ru-wo are in the far west, this would be in keeping with the evidence of the other place-names on the Cretan jars. All the terms on jars which are certainly identified as place-names are either (a) also members of the Co group (like si-ra-ro) or (b) more crucially, places which seem from the internal evidence of the tablets to have been towards the west of the central area (like *56-ko-we and da-*22-to).

Third: recent re-examinations of the statistical basis for Catling and Millett's conclusions have succeeded, at least to this inexpert eye, in casting considerable doubt upon it.[8]

And I'd like here to add a fourth piece of evidence. Among fairly recent finds of Linear A and B inscriptions at Chania (Kydonia) are some sherds of pots with Linear B inscriptions. Amongst these is a fragment, number KH Z5, which shows the reading]de-so[. As its discoverer, E. Hallager, has pointed out[9], this at once reminds us of the terms ta-de-so and ta-*22-de-so which occur, again painted, on the Thebes stirrup jars. (Both terms are probably variant spellings of the same personal name.) But not only is it attractive to think that the same name may have occurred on stirrup jars at Chania and Thebes; there is to my mind very little doubt that both the Chania fragment and the Thebes examples of ta-de-so / ta-*22-de-so are written by the same scribe. The form of the de in both cases is not only virtually identical, but also highly idiosyncratic (there's nothing like it elsewhere, either on tablets or on other jars); and what survives of the so at Chania is also very similar to the so's at Thebes.[10]

Now if I'm right about the hand, two important consequences will follow. First, it will suggest that the date of the Thebes material, which has been put at various points, is LH IIIB, the date of the Chania finds; and second, it will provide added encouragement for the belief that the source of the Cretan stirrup jars on the mainland is in fact the west of Crete. It is possible, of course, that the Chania fragment is part of an import to Chania from another part of the island; but given the other evidence which suggests a westerly location for the place-names on the mainland jars, it would seem much more plausible to think of it as a native W. Cretan product. In other words, therefore, the evidence that Knossos had any interest in the east of Crete is very slender indeed.

NOTES:

1. See especially J. Chadwick, "Relations between Knossos and the Rest of Crete at the time of the Linear B Tablets" in Proceedings of the Third International Crètological Congress I (Athens 1973) pp. 39-45.
2. G. R. Hart, "The Grouping of Place-Names in the Knossos Tablets" in Mnemosyne 18 (1965) pp. 1-28.
3. See especially (i) L. R. Palmer, "Mycenaean Inscribed Vases II. The Mainland Finds" In Kadmos II (1972) pp. 27-46; (ii) L. R. Palmer,

"Context and Geography", paper read at the Sixth International Colloquium on Mycenaean Studies, Chaumont, Switzerland, 7-13 September 1975 (in press).

4. See especially (i) J. L. Melena, "KU-PA-RO en las tabillas de Cnoso" in Emerita 42 (1974) pp. 307-336 esp. pp. 329-331; (ii) J. L. Melena, Studies on some Mycenaean Inscriptions from Knossos dealing with Textiles. Supplement no. 5 to Minos, Salamanca 1975.

5. IV. 721.

6. Savary, Lettres sur la Grèce (Paris 1788) pp. 245-6.

7. H. W. Catling and A. Millett, "A Study of the inscribed Stirrup-Jars from Thebes". in Archaeometry 8 (1965) pp. 3-85.

8. See J. McArthur and J. McArthur, "The Theban Stirrup-Jars and East Crete: Further Considerations" in Minos 15 (1975) pp. 1-13; A. L. Wilson, "The Provenance of the Inscribed Stirrup-Jars found at Thebes" in Archaeometry 18 (1976) pp. 51-58.

9. E. Hallager, "Linear A and Linear B Inscriptions from the Excavations at Kastelli, Khania 1964-1972" in Opuscula Atheniensia II (1975) pp. 53-86.

10. Hallager op. cit. 67f. considers the possibility that the Chania and Thebes inscriptions are in the same hand, but rejects this in favour of an alternative explanation that both are in the same 'scribal tradition'. For arguments against this view, see my review of A. Sacconi, Corpus delle Iscrizione Vascolari in Lineare B (Rome 1974) in Classical Review (forthcoming).

(Editorial note: The Pylos tablets are partially ordered by their archivists, but the difficulty of relating this structure to geography is demonstrated by discussions at this conference - see supra and infra. But the Knossos tablets lack this internal ordering, yet we possess a number of reasonable geographical fixes. It is felt that the application of statistical and computer techniques would be more appropriate and possibly more successful on the Knossos series. As a result of this conference, Dr. Killen and Professor Kendall are working on a joint analysis of the Knossos archives along the lines of the Pylos study reported by Prof. Kendall in this volume.)

DISCUSSION of John Killen's paper, which led into a general discussion of the Textual Evidence for Mycenaean Geography:

(Taking part: Colin Renfrew, Anthony Snodgrass, John Chadwick, Anthony Wilson, Paul Halstead, Peter Warren, John Bintliff, Oliver Dickinson).

RENFREW: Is there any evidence to confirm the identification of Kydonia with Khanea, on the Khanea Tablets?
KILLEN: No.
SNODGRASS: There is indeed now an interesting contrast which has become obvious between the two different palace series of tablets.
KILLEN: This is a function of the difference in material. One of the things we don't have at Knossos is those nice lists of towns in the two provinces recurring time and time again.
SNODGRASS: But one of the things you do have is a whole fistful of geographical fixes.
KILLEN: Yes.
CHADWICK: Could I bring up a point that comes out very much more strongly from Dr. Killen's paper than my own, that is, the validity of any sort of statistical approach, even allowing the validity also of the facts, which I hope Dr. Killen has satisfactorily demonstrated. I'm not by any means against statistical approaches, I'm only too anxious to use them, if we can see the means of

doing so. But I wonder if anyone would be prepared to argue that when we come to statistics we should forget all that we can infer about the order, the ranking of names, or the basis in a given documents for their association - and one should merely count how many times they are associated.

SNODGRASS: I wonder if we could hear from Dr. Wilson, who as Dr. Killen has mentioned, has worked on a statistical approach to the Knossos Archives?

WILSON: Dr. Killen referred to an unpublished study of mine on the contextual links between place-names in the Knossos tablets. This preliminary investigation is to be published elsewhere, but the approach was essentially as follows. A link was defined as the occurrence of two toponyms within the same context, and the number of such links for each pair of common toponyms was counted, equal weight being given to each occurrence. Approximate estimates were then made statistically of the number of links required to justify a conclusion that the observed number did not arise by chance; only pairs of toponyms with statistically significant numbers of links were then further considered. Dr. Killen mentioned that some links are of greater value than others in indicating a real association between toponyms. There seems to me some difficulty, and also some danger of subjectivity, in examining each link individually and trying to assess its value. The approach followed represents the other extreme, and was prompted by a wish to ensure that all evidence was taken into account though this is clearly not necessarily correct. The interpretation put on the term 'the same context' governs the number of links present in the tablets. When I interpreted the term as the same or adjacent lines of one tablet, my approach gave a broadly similar pattern of significant links to that described by Dr. Killen. It is interesting that my significant links also indicated the presence of 'closed' groups of toponyms, i.e. no member of one group was significantly associated with any member of any other group. Dr. Killen also mentioned groups of toponyms, and the groups emerging from my analysis appear to be similar to his and to those proposed by Professor Palmer. For example, Dr. Killen spoke of a pairing between the places pa-i-to and da-wo, and these also appear (with others, e.g. da-*22-to), in one of my 'closed' groups; similarly his tu-ri-so group appears to be essentially the same as my 'closed' group that includes tu-ri-so. In some cases, however, my groupings suggest locations for places different from those generally accepted. For example, my pa-i-to (Phaistos) group includes the place ku-ta-to, often placed in the north of the island. Such differences arise as a result of the different interpretations of different scholars of the term 'the same context' and the different weights attached to the various links; at present there seems to me no sure way of deciding which of the various approaches is more correct. I attempted, therefore, to seek alternative evidence for grouping of toponyms by analysis of the SHEEP TABLETS. Most Sheep Tablets show a man's name (many different names appear and they are commonly thought of as shepherds), a place-name and the number of one or more types of sheep. Some tablets, however, also contain a second man's name, and only few names appear in this position. The function of these latter men is not generally agreed, but they are thought to have had either ownership of, or special responsibility for, the flocks with which they appear; they are known as 'collectors' or 'owners'. When the number of flocks with a 'collector' is counted for each toponym, it turns out that the six places with most 'collectors' flocks are just the six places in my pa-i-to group. Professor Palmer and Dr. Hart have both previously used this evidence, but I also attempted a more quantitative test by calculating, for each toponym, the ratio of the number of sheep associated with 'collectors' to the total number of sheep. This showed a mean ratio for the pa-i-to group which was statistically, significantly greater than for other groups which also differed from each other. Another test was based on Dr. Olivier's demonstration that the number of sheep recorded against a toponym in the Dn series of tablets represents the total number of sheep in the Da-Dg series not associated with 'collectors'. I calculated for each place-name the ratio of the total number of such sheep in the Da-Dg series of tablets to the number in the Dn series. The results indicated

a significantly smaller ratio for the pa-i-to group than for other groups. These calculations appear to support the idea that the groups of toponyms suggested by the contextual associations reflect some real form of association between their member places. To interpret such association as geographical is tempting, but further study of this aspect is necessary. However, the facts concerning the flocks with 'collectors' may indicate a geographic significance. As Dr. Hart has previously suggested, one interpretation of this concentration of 'collectors' in the pa-i-to group is that its relative remoteness from Knossos might well have led to greater numbers of flocks being under the control of men other than the ruler of Knossos.

KILLEN: Well, one of the interesting things was that we did reach very similar results on most of the main issues. And as far as that group that you've just mentioned, i.e. your da-wo, pa-i-to, da-*22-to, e-ko-so etc., group, I think my query would be how large it is in terms of area. The point which I was trying to make was that da-*22-to does seem to have other strong links - for instance, it's grouped with another place *56-ko-we on one of the Dn area totals, which normally seem to link adjacent places, and that would suggest the west and the north. You may well be right about this grouping, but I'm just wondering whether it doesn't cover a wider area than you're suggesting.

WILSON: When, in counting contextual links, I interpreted 'the same context' more broadly, (i.e. as the occurrence of a pair of toponyms on the same tablet or in the same set of tablets), the place e-ko-so became a member of the pa-i-to group. Now e-ko-so is paired with su-ki-ri-ta (=Sybrita?) on a Dn tablet, and as you say, this particular type of link is often regarded as a specially strong indication of geographic proximity. If that view is correct, then it may imply that the pa-i-to group was not restricted to the area of the Messara, but also extended into the central mountains.

HALSTEAD: To develop what you were saying about picking up the links between the different name-groups, that you were detecting not only geographical proximity but also some kind of hierarchical system - Knossos and Phaistos being on a similar plane. When you merely use all the data together you get inconsistencies, because of the inclusion of different categories of site. Could one envisage a technique in which the analyst did several runs, missed out this or that category, so as to isolate which combination of data produced the most consistent picture, so e.g. to define with greater clarity which categories of information are producing geographical information, and also those that have been missed out as producing greatest inconsistency - these could perhaps be residuals providing information on hierarchical arrangements.

WILSON: This could be done with computing techniques. But in the Knossos case I see no definite geographic ambiguities. If you put all the data together, in fact the group of names supposedly in the north of the island are associated with names supposed to be in the south. This is to me not a discrepancy, although it calls into question established views on how far apart these places are meant to be, and their true geographic location.

CHADWICK: If we had enough data then this statistical approach would undoubtedly sort out these things, and a lot of the problems are arising precisely because we haven't got enough documents for the statistical approach to be wholly valid. I know you can calculate levels of probability and so forth, but to establish this really clearly we want a lot more information than we have actually got available. This is surely our major problem, that we are still at the very limits at which you can use a technique of this sort. If it produces a confirmation, we will welcome it with open arms - but if it cannot confirm our work we do not feel it destroys our conclusions.

WARREN: I would like to ask a question particularly concerning Crete, there might also be some information from the Pylos Tablets - whether there has been any attempt to group the places mentioned in terms of settlement function. Dr. Chadwick pointed out that certain places could be seen e.g. as sheep-grazing places, or for timber. Has there been any sort of systematic attempt to group places on the basis of internal evidence following economic function?

If such clusters were established we could then perhaps turn to the landscape itself and its picture of geology, geomorphology and so on, for a match.
CHADWICK: For the Pylos Tablets there are quite a number of particular groups which associate places together on the basis of the production of a particular commodity. There are a lot of difficulties here, and we have tended therefore to concentrate on the main districts where we have a series of returns - in the Ma case for six different commodities, we don't know what all these commodities are, but we possess the figures for which these commodities are being assessed and these remain in fixed proportions. If they have 20 of one thing they ask for 20 of something else - now that proportion remains the same all over the kingdom, therefore we must be looking at staple products which every district can produce. That's one type. Now we have the other type, as for instance, the flax production records: we have flax production for nearly 100 different places scattered over the kingdom. Here they are not grouped together as districts as the others were - clearly there will be areas that produce no flax, others will be good flax producers, and therefore it is a quite different picture. The real difficulty I think is that so many of the names do not occur anywhere else. We have another list of bronzesmiths, about 20 places with groups of bronzesmiths, but only about 4 or 5 are those that we know very much about. In many cases there are very likely to be places where there are good supplies of fuel - it is easier to take your supplies of raw metal to fuel supplies than vice versa, so you may get metal work done in rather remote districts. This is really where the statistical approach falls down for lack of sufficient information to say what these are. We can produce for you lists of towns concerned with flax, with bronze, with rowers, but the overlaps between them are hard to show.
WARREN: There's a danger of confusing two things here: one is the precise geographical determinants of place, which I wasn't actually thinking of; the other is identification of function - which might then be thought of in terms of the resources of an environment in a particular area. For the sake of argument take flax, and say that on the grounds of presentday evidence one can narrow its chief production down to a particular area - one needn't say on the ground "the toponyms must be here", spot by spot, but one could say they are likely to be in this kind of environment. I was really thinking of the function of a settlement in terms of its particular resources rather than trying to pin it down as north, south, east or west - obviously there may be places that crop up in the context of timber, and only timber, likely to be placed amid good forest resources. How far can this be taken?
CHADWICK: The principle is excellent, but how far can it be applied to our material? I don't think we can get much further than is being done. When this detailed survey, which was done by Sainer a few years ago, is published, - this is an extremely detailed work and does examine how much in all can be gleaned from the tablets, - one can extract categories of this nature from that work.
BINTLIFF: If one compares the Knossos and Pylos Tablets, is it not possible to detect differences in the overall organisation by the respective palaces of their archived place-names, in terms of the functions of the different places listed - where things are happening?
CHADWICK: I think Dr. Killen would agree with me that there is a very different 'feel' about the administrative systems. I put it in that rather crude way, because it's very difficult to be precise about what we mean here. But where at Pylos I feel fairly confident that for most purposes the king of Pylos knew how to organize production - when he wants to set up an emergency collection of scrap bronze, he knows precisely who are the people to put in charge of this operation and how much to allocate to each as the ration they have got to produce. That is all highly organized and is very clearly shown by the way the documents are drawn up. Now in the case of Knossos, it is very much less clear how Knossos organized itself, and the way in which the detailed operations of textile production - as Dr. Killen has shown us - go on in the Phaistos area but are in fact recorded at Knossos about 40 miles away, suggests that there is

very much less devolution between those centres than we should expect. The impression that one gets from these tablets is that Knossos is controlling all of the Central Area very much in detail from the central administrative centre - not using local administration for most purposes. The West I think is a different case. The places which lie, as Dr. Killen has shown, in the far west, do not appear to be in the same relationship to Knossos as the rest, but the Central Group, (and how far east it goes I don't know - I would be tempted to place it a little further east than Dr. Killen), but anyway the Central Group are very closely controlled by Knossos.

SNODGRASS: Well I think we should always bear in mind too the chronological factor, that at Knossos we may be seeing a pioneering stage of a system that has been more or less perfected by the time of the Pylos Tablets, which on any account must be dated 100 to 200 years later. Would any of the statistical fraternity like to take up the slightly veiled challenge that has been thrown at them, that their methods are very precarious in the particular context of the Linear B documents?

RENFREW: Although not of that fraternity I would like to make one comment if I may. As we have heard, on certain tablets association of names reasonably indicates propinquity. On certain other tablets, it can reasonably indicate the reverse. Thus we have been hearing that we have administrative centres listed, and in discussing the locational principles of administrative centres we find that they are remote from one another, thus giving the reverse of propinquity. And surely it would not be very difficult to devise a technique - after all statistics is as good or bad as you make it in this kind of situation - whereby you label each linkage in terms of the commodity or other nature contained in it. You would then have a higher dimensionality. As a good first step obviously we simply say, right, any pair of names is an indication of propinquity - so we will try and get a map on that basis; you can then get a much more complicated map with a much greater number of dimensions, whereby you would have a separate dimension, or shall we say better separate plane, for each commodity that you were talking in terms of. You could then perhaps ask somebody to give a weighting, you could try all different kinds of weighting. You might even get a negative weighting when you are dealing with administrative centres, a very positive weighting as Dr. Chadwick was suggesting, when you are dealing with commodities that are ubiquitous. And therefore you might well assume that on a tablet dealing with cereal production, the listing of names that are adjacent may well suggest geographical adjacency, but if you are dealing with a rarer commodity, they may be at the opposite ends of the kingdom. And I don't really think it's a reflection on statistical methods, it's merely that the methods we have been hearing about have quite reasonably taken the simplest approach - saying that if you find the names mentioned together this indicates propinquity. So I'm sure a more sophisticated approach could be used, although I take Dr. Chadwick's point that there is a limited amount of information available, and we may not be able to do a great deal more until more tablets become available.

WILSON: But in dealing with a small sample, and attempting to extract the maximum information from such a partial picture, surely this is precisely where statistics is most essential for an impartial evaluation of the degree of linkage between different categories of information? At the simplest level in studying contextual links, statistical techniques are helpful in indicating the number of links that could have arisen by chance and, therefore, do not represent valid information.

SNODGRASS: Earlier we began a discussion on the suggestion that the data from the Pylos Tablets could be re-interpreted in terms of a horizontal arrangement of provinces in that kingdom, across the map, rather than a vertical one which Dr. Chadwick favours. Can we develop this further?

DICKINSON: Well, there's only one other very minor point I wanted to add, and that is that going again only on the excavated sites, there is archaeologically an enormous concentration of what appear to be Early Mycenaean sites in the

Pylos district. They are so close together that it is difficult to believe that they could have been at any time independent of each other. And I would be tempted to believe that perhaps the districts at the heart of the Provinces were based on these original places. And possibly we should not assume as has Dr. Chadwick, that all your town centres of Tablet districts/provinces, are equal, even relatively equal large places separated in this sort of polygonal fashion. There might even then have been a concentration of districts, just as population seems to have concentrated in these belts. Peristeria, in north Messenia, with Pylos is one of the two chief Early Mycenaean centres. Around Pylos, you have sites represented as being pretty important, and often continuing as such well into the Palace period - at Tragana and Koukounara, whilst Routsi is definitely an early one, and there are quite a number of other big sites where one has got rather poorer evidence (though a big site is not necessarily an important site, it might just be a site where a lot of people happen to live.) But these sites have got rich early tholos tombs, and therefore they are, at an early stage at any rate, important. One is not assuming necessarily that each represents a petty independent principality. As may be seen on the Survey maps this group is so close together (and there is another possible group to the south with the earliest of the Messenian tholoi), that independence is hard to envisage - they might be different branches of a dynasty or something like that. On the other hand Koukounara could have been separate, the country is very broken up and would offer sufficient barriers. So perhaps you might have had a concentration of small districts - small in terms of geographical area, but possibly not in population, in this region, and it might therefore prove possible to start the Further Province not far away at the most striking division of the central mountain range and its east-west running slopes. The Further Province seems reasonably based on the dense Kyparissia Valley settlement, on the far side of the mountain barrier. The Hither Province is naturally to be based on the dense Pylos belt of settlement - but there seems no real reason why it shouldn't include the dense settlement running from Nichoria across to Pylos, and we are still keeping in mind the Pion - Asine link that is surely the absolutely basic fixed point. In this connection I'm tempted to say that there are two rivers, a Nedon and Nedha, to SE and NW, and could the Ne-do-wo of the Tablets have changed its name slightly?

CHADWICK: Perhaps I could laborate on a point that I sketched in rather briefly earlier - the coastguard organisation. Once more there have been different interpretations concerning the meaning of this document, but I think I'm right in saying that it is generally agreed as being a list of people who are exercising some sort of function on the coastal areas. The introductory formula says precisely that, and no-one has disputed that that is what it means. Now the way in which this document appears to work is that it is spread over 5 tablets and each tablet has 2 paragraphs; now each paragraph begins with a coherent formula, and this suggests that each paragraph concerns what I call a 'sector', which must be therefore a sector of the coastline. Now it is extremely difficult to be sure that we have got these tablets in the right order - this is one of our major problems - but I have set out my arguments in print, why I think a particular order is to be preferred. We have no doubt which is the first and last of these, and this matters much with this particular argument. The first of these must be somewhere up at the north end of the west coast; we cannot prove where on the west coast this is, because here again it is one of those places that is hardly mentioned at any other point of the Documents, but is the centre of the first sector. Well, if as I say this is based on the river Nedha, that will pack all in this rather sparsely inhabited area in the far north. The sectors then proceed round the coast, round the southern cape and up into the Messenian Gulf, until we come to the last tablet which has 2 paragraphs - the first paragraph is dealing with a sector based on the Cape Akritas area, and we are then left with precisely one sector which seems to represent the coastline of the Further Province - because the

one place which we can identify as belonging to the Further Province lies as the headquarters of the coastguard in sector number 10. Now if you are going to put sector number 10 in the far northwest, this will involve a curious scribal route north to south then back again further north. But the reference to sector 10 gives the Timitoakee placename (which we know is one of the main district names of the Further Province), and says that a small body of men are being sent to Nedon. Now it seems to me very difficult to believe that that is not to cover this short section of coastline of the Further Province, and if one tries to measure coastline lengths we have about 160km, roughly 100 miles - and 1/10th of that gives you exactly 10 miles, exactly that sector. I agree you could perfectly well put that sector in the northwest - but I would hesitate to try and make all the other bits fit once you have done this. And if you agree with me that we have a fixed point or perhaps two fair fixes, added to which we have the association between the 4th district and the palace itself, all this does seem to me to give a coherent picture, and I can't really see how you can juggle with that and still produce a coherent picture. What you're saying about the history of this I don't dispute for a moment, I'm merely saying that as the administration is set up, it isn't always purely logical - before the recent boundary changes who could possibly have predicted that England was administered by a curious system of counties which in some respects reflect the distribution of kingdoms in England before the Norman Conquest. This might also be so for some of the things we have been looking at, they're not purely logical.
BINTLIFF: If I could add a point here. I suggested earlier that Messenia had two natural north and south provinces by the natural distribution of high quality soils (the balance has changed due to the historical alluvium), and Mycenaean settlement can be shown to follow this division very strikingly. Just assuming for a moment that these provinces are, as Oliver Dickinson heretically suggests, the two provinces of the Pylos kingdom, then it will be seen that the northern, Further Province has but a tiny sector of coastline quite out of proportion to that owned by the Hither Province. Hence the sector of the Further Province on this scheme might well be out of order in the coastguard tablets, even placed last though geographically first.
CHADWICK: We do happen to have a basis for comparing the two provinces, because we have the total figures for flax production in each province. Unfortunately the vital bit is broken and we don't quite know what the difference in scale is. But we do know that the Hither Province produces a good deal more flax than the Further Province. I don't know how that ties up with flax production in modern times, but this is the kind of information which we do have.
SNODGRASS: The difficulty with the archaeological evidence for early Mycenaean administrative units is that the tholoi sites are two or three centuries earlier than the Tablets.
DICKINSON: If the administrative system of the Tablets reflects as it were the absorption of earlier political units, I think there's certainly some reason to suppose that key early sites are of great significance. But I can see that one could argue that the Peristeria site was absorbed before the Further Province - indeed the site does drop out rather abruptly. But we do have this basic rectangle of Mycenaean settlement distribution in Messenia, and the weakest link is undoubtedly down the C. Messenian valley, along the Pamisos. Here the lowest number of settlements is found, though some of these are quite large.
CHADWICK: Well I have a suspicion that they just haven't been found. I know that's always a dangerous argument to use, but the nature of the ground is such that any settlement on good agricultural land will have been ploughed in, and on the hills it will stand a good chance of being eroded. The UMME team have certainly looked. We have this problem of the marshes - considerable parts of the Messenian Plain are marked as a marshy area - I don't know if anyone can tell me what the actual agricultural potential of these areas was - I don't think it was as high as it is today, because it seems to me unlikely that they had drained those marshes, and of course the coastline went further inland too. The road linking the Kyparissia Valley and the Upper Pamisos Valley, to make a

further geographic observation, narrows quite perceptibly to an appreciable pass - so there is in fact a basic geographical line somewhere along that valley representing of course the watershed.

BINTLIFF: As regards the state of the Messenian Plain in the Late Bronze Age: it would certainly be as the other coastal plains I have been studying, and indeed I have accomplished a small study in its southeast at Akovitika. The lowest plain would probably have been chiefly sea, or thin deltaic fill not amenable to agriculture. This pattern is confirmed here as elsewhere by the distribution of settlements in both prehistoric and ancient times, and the accompanying resource picture demands that farming interest must shift back inland towards the heartland of classical Messene city. Professor Kraft's Messenia Plain study provides detailed evidence to support my reconstructions here - with the lower plain, now the richest land, then sea or seasonal grazing. From this one can indeed predict the prehistoric settlement pattern, a concentration on the contemporary best soils - the marine silt hilland e.g. where Nichoria lies (as Pylos), but also clearly the Upper Pamisos Valley is even more fertile, and I would predict important sites of the Mycenaean period to be located there based upon the same extensive flysch soils as ancient Messene city, before we get into the Kyparissia Valley area.

DICKINSON: Yes, the northern plain, Stenyklarian Plain, is an extremely rich natural area, and the one the Spartans were particularly interested in.

BINTLIFF: Exactly, and I think it is here that we might look for a very major centre, quite possibly the capital of the Further Province. Would this still fit into Dr. Chadwick's view on the division of the Provinces?

CHADWICK: Yes, we do have a very promising environment in this upper and inner plain zone, with notably a great amount of water available - certainly this was significant for flax production. However we are not clear about the relative merits of the Kalamata region further south.

DICKINSON: Well, the Thouria (Ellinikon) site is in fact the largest in the Pamisos Valley, and one might even speculate that this key Mycenaean settlement was perhaps the Further Province capital.

CHADWICK: If only we knew for sure where that capital was - its name on the tablets is Leuktron, and we do have in fact the site of classical Leuktron, but unfortunately this is almost certainly outside the area which we are talking about. It's one of these names which I'm more and more convinced had radiated from Messenia in the troubles after the destruction of the palaces, when indeed archaeology shows the population to have dropped enormously. A population element could have migrated from west of the Messenian Gulf to Leuktron and taken the name with them.

SECTION THREE: Discussion of papers presented in Sections 1 and 2, and a communication from R. Hope-Simpson. Discussion of the PRE-PALATIAL BACKGROUND To Mycenaean civilisation, with particular reference to geographical factors and settlement studies.

(Chairman - John Chadwick)

MYCENAEAN GREECE : A NOTE ON THE CURRENT STATE OF FIELD EXPLORATION

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I am very grateful for the indulgence of the organisers of this colloquium, who have allowed me to present this short communication, and to Oliver Dickinson for consenting to deliver it to you. The notes here are a kind of progress report on the emerging pattern of Mycenaean settlement in Mainland Greece and the Aegean, together with some suggestions for future exploration.

Oliver and I have almost completed our Gazetteer of Bronze Age sites on the mainland and in the islands, for Paul Aström's Studies in Mediterranean Archaeology series; and I am also currently engaged on an illustrated book of the topography of Mycenaean Greece. We hope that these works will together provide a brief conspectus of Mycenaean exploration already carried out. I wish here to discuss mainly the work which remains to be done, and in particular the gaps in the field work.

We are all well aware how expensive excavations have become, and the cost of major excavations is now almost prohibitive. In the late 1960's we were led to believe that a programme of large scale excavations would be possible in the 1970's, and that we would be able to build up steadily a pattern of excavated type-sites, especially to provide samples for regions not previously explored by excavation. Thus, for instance, Nichoria was chosen in eastern Messenia, in order to supply some measure of counterweight to the many excavated sites in western Messenia. By 1973, of course, it was already clear that such large scale ventures would probably not be possible elsewhere for some time to come, and UMME's plans for the conservation in situ of some of the Nichoria structures have had to be cancelled.

Some have indeed wondered whether such purely research excavations as Nichoria should be mounted at all at this stage, or whether we ought not rather to stick to salvage or "rescue" excavations only, for the present. And it is true that urban sprawl, together with extensive deep ploughing and levelling for agricultural purposes, have greatly increased the threat to ancient sites in Greece. The Greek Archaeological Service is barely able to cope with emergency excavation of tombs, and has neither the personnel nor the funds to undertake systematic excavation of settlements. Mycenaean tombs are thus more often rescued than Mycenaean settlements. Yet it is the settlements which are the more important, especially for palaeoecology. The type of excavation that is best suited to the limited funds is of course the correspondingly limited, and mainly stratigraphic, type. Nothing could be more appropriate than the work being done by the British School at the Menelaion in Laconia and at Phylakopi on Melos. In both cases the significance is proportionately greater than the financial outlay, since these excavations supplement, explain, and

correct, the previous pre-World War I investigations.

But the greatest need at present, and the least expensive proposition of all, is not so much for excavation as for surface survey, to record the sites, especially the settlement sites, both in the previously unexplored areas and in the threatened areas. How are we to establish the priorities? Armchair predictions, based on extrapolation from better known areas, are worth very little, unless at least a pilot survey has already been made in the new area. Otherwise there is no guarantee that an area which is now agriculturally productive was in fact fully settled, or even settled at all, in the Mycenaean period.

It is true that we (the UMME Survey team) were somewhat surprised by the number of upland sites that we found in Messenia, but are we going to get similar results in Triphylia or western Arcadia, or in parts of Laconia? I drew a blank in some areas beyond Mount Parnon in eastern Laconia, and I have a private belief (based on limited autopsy) that large tracts of western Laconia, especially the upper part of the Vardhounia Valley (inland from the Gythion coastal strip) may have been still virgin forest in Mycenaean times. Here I take as a clue the fact that most, if not all, of the terracing in this area appears to be modern, or comparatively so. Old terracing was almost uniformly good, with high walls, and flat terraces. Modern terracing, on the other hand, is usually irregular, with little use of walling, and very little modification of the hill slope. This distinction between old and new terracing was of paramount importance to us in Messenia in 1965 and 1968, when we were using mainly the air photos as a guide to finding the settlements. Incidentally the Messenia survey is not by any means complete: the Nichoria excavations from 1969 onwards channelled our activities away from the survey work, and I have plenty of "possibles" marked down already from the air photos in Messenia and Triphylia. I should here remind you that the success rate from the air photos in 1967 and 1968 was 60 to 70 percent.

I will get back to the point. What sort of surface survey should we be doing now? The Messenia survey was of an overall type, designed to amplify the existing pattern and to explore at least part of every main region of Messenia. The main questions we were trying to answer were simple. Were the Mycenaeans here? How big were the sites? How provincial? Were some regions more densely populated than others? This kind of survey I would like to define as extensive. In most cases sites were visited (and usually at the same time discovered) only on the one occasion. We seldom had time to re-visit most of our former discoveries.

In 1967 and 1968 we were scoring Mycenaean or Bronze Age sites at a rate of over one per day; and about 3 hours of each summer day was consumed in travel. The late afternoons and early evenings were spent in writing up the day's work and preparing for the next with the maps and air photos. And we spent only about 4 weeks actually in the field in each of these two campaigns.

Intensive work, by contrast, involves field by field survey of the area around a site as well as the site itself, and repeated visits. Such work has rarely been done in the past, and has in any case usually been confined to the immediate surroundings of an excavated site. And only recently has full attention been paid to environmental studies. We are of course doing such a study in connection with the Nichoria excavations, and Bill Loy, one of our geographers, has done (for his doctoral thesis) intensive environmental studies in the vicinity of Malthi-Dorion, and of two un-excavated sites, Parapoungion, a hill site in the Upper Pamisos Valley, and Aristodhemion, based on two adjacent low mound sites in the Lower Pamisos Valley. Some of our work in Messenia, especially in the Pylos District and in the Soulima Valley, could be described as midway between extensive and intensive. And the same is true of my former

survey of the Skala plain in Laconia - a survey that has now been enriched by John Bintliff's intensive environmental work.

Unfortunately I cannot claim that my work in the Sparta plain was similarly intensive. I regret to say that here I paid too little attention to the villages at the edge of the plain, especially near the springs at the foot of Mount Taygetus.

This was partly due to a fruitless chase after Menelaus' palace - as John Chadwick has recently suggested, such over-addiction to Homer can at times constitute an impediment.

What then should be the prescription? Quite obviously a mixture of extensive and intensive survey work. And all intensive work should be both all-period and fully environmental.

Where are the main gaps for Mycenaean? I mentioned a few in my old Gazetteer. But things have changed.

In Laconia some more extensive work still remains to be done - and I have my own eye on this. Good subjects for intensive work are of course the Sparta plain and the Vatika plain. I have never been able to open my eyes underwater, so I failed to find the key site in the Vatika plain, Pavlopetri. And I never looked at the hill villages on the east side of the Vatika plain; although for some curious reason I went over Elaphonisi island field by field. Messenia still needs some more extensive coverage, and we must fill in that gap in exploration between the Neda and the Alpheios, i.e. Triphylia. Presumably the Minnesota team ought to be re-formed for this task. Roger Howell did eastern Arcadia, but western Arcadia is crying out for attention. Was it packed with early Mycenaean IIIC refugees? The intensive Peneios Barrage rescue survey shows how much is ripe for the plucking in Elis. And we still need those settlement sites to go with the Achaea tombs.

You may perhaps be thinking that this wraps up the Peloponnese. But I would remind you that the Argolid in particular still needs extensive work as well as intensive. This was shown dramatically by the Indiana and Pennsylvania Universities joint south-western Argolid exploration project (which John Bintliff can tell you about in detail). This partly extensive and partly intensive all-period survey (which is not yet published) revealed an incredible number of ancient sites in a comparatively small and rather remote area. Even in the main Argolid plain there are almost certainly more Mycenaean sites to be found. And I would recommend the Asine plain in particular as a good target for an intensive survey.

I will be briefer with the rest of Greece. Attica is now mainly a subject for salvage operations. Boeotia is also being dealt with adequately, again mainly by the Greek archaeological service. (Editor: here O. Dickinson registered disagreement: too little is known about Boeotia still.) Euboea has been excellently surveyed by Hugh Sackett and his team of collaborators, with a mixture of extensive and intensive work. And Lefkandi provides an excellent excavated type-site for the region. In Thessaly and western Macedonia David French has done a magnificent job of tidying up loose topography and collecting sherds (mainly from known sites); and the basic prehistoric patterns are fairly clear. Intensive work is much needed here, and Paul Halstead is making up for this deficiency with his environmental work in Thessaly. Ken Wardle has put in order the whole of Northwest Greece and is now tackling the whole problem of the Mycenaean northern boundaries. There are still, however, some large gaps in the fieldwork coverage of Central Greece. East and West Locris, Aetolia, and Acarnania need extensive work, and there are several

smaller gaps.

Turning to the islands, we are in good overall shape in the Cyclades, due largely to the recent work by Colin Renfrew, Robin Barber, and John Bintliff, among others. And the rather rapid and sporadic coverage of the Dodecanese by John Lazenby and myself is being improved on by Christopher Mee.

I will not here venture into Asia Minor or further afield. If I have left out anything important, no doubt the members of the colloquium will be able to fill in, or rather to point out, the gaps.

I would very much have liked to be with you, but as usual the expense, time, and distance, and my own commitments have precluded this.

May I wish you all the very best of luck.

(After Hope-Simpson's communication was read, the following general discussion ensued, in which there took part: Colin Renfrew, John Cherry, Oliver Dickinson, John Bintliff, John Chadwick, Fred Hammond, Anthony Snodgrass, Caroline Murray, Paul Halstead, Peter Warren, Joan Oates, Russell Meiggs).

RENFREW: One point on which both I and John Cherry would like to comment, in relation to what Hope-Simpson was saying about a distinction between extensive and intensive survey. What is the meaning of the word intensive - I think a rather higher standard, a rather higher level is desirable today; what intensive ought to mean is fieldwalking the entire surface of the area in question, or at least a sampling strategy, which is what John Cherry has worked out for his survey on Melos. On the latter we covered a proportion of the surface of the island, and felt that the strategy was such that we can make generalisations about it. But it did involve a very exhausting as well as exhaustive walking across the fields so that for certain areas, e.g. one kilometre squares, it is possible that the whole area had been surveyed. I'm not sure that Hope-Simpson was using the word intensive in quite that light, and if one wants to get the kind of evidence that's going to allow quantitative statements such as people like John Bintliff and Ian Hodder were alluding to earlier, probably this is the way to do it.

CHERRY: Yes, in the extensive survey one commonly finds a very large area which is being investigated, often an archaeological blank, perhaps without a large personnel, and often without any systematic strategy of survey; but in an intensive survey a much smaller area is being looked at in a detailed way by a large team, who moreover have some planned strategy of how they are looking - but this is generally just surveying around a known site, a strategy merely providing a hinterland to a known site, and this is not really the way I would interpret or conduct an intensive survey.

DICKINSON: I don't think that's really what Hope-Simpson said though, is it? He said that's what people had done but he didn't say people should do that. He does indeed believe in field by field, much closer and slower work, in certain small areas like the major plains. I think he's only too well aware of the deficiencies of his earlier work, and one of the most alarming features is the way that this has been used, on the assumption that this is all that can reasonably be found - that it can be used as a reliable basis. In fact I would like to point out that Messenia is the only province of the Greek Mainland that has had even a fairly good extensive coverage, and there the surveyors still had theories about the sort of sites people lived in, and if they had looked everywhere they might have found a lot more. So although there are only 130 sites found in the area that you suggest is the state of Pylos, you have 180-200 placenames, and these are clearly only the more significant.

BINTLIFF: I think it is unfair to take Hope-Simpson too much to task, as we

seem to be tending towards, because primarily he was doing a pioneer job, he worked extremely hard and he found such a mass of new sites, even if only particular kinds of site. This is very necessarily extensive work. There are many of us who are interested in smaller sites who see that here Hope-Simpson's surveying fell down completely; he was looking rather at the upper hierarchy, he wanted the larger sites, he was interested in the more important centres that were likely to appear on the Tablets, or which Homer mentions. And I think to a certain extent, to be quite honest, these sites are likely to be more interesting, except that we don't know enough about the lower settlement levels e.g. how they supported the upper settlement hierarchy. So he has done tremendously important work for us to get as far as this, and we could not proceed as we are now proposing had he not put in the main guiding points. And as far as I can see, where he has identified centres I think he very often has identified THE centres, and here there aren't any more to find. What he missed is the satellite network in between, the smaller centres that e.g. Ian Hodder was mapping in his theoretical distributions and which we can find recorded by accident in the field, occupying the resources between major sites and working their fields also for them, after my analysis. It is interesting that he mentions air-photos as important, because this is closely related to the type of site that he looks for - picking out from air photos a prominent hill; there are lots of references in the UMME volume such as "we climbed every suitable hill". Clearly there is a certain type of site which he has found par excellence and in practice these do tend to coincide with the more important Mycenaean sites; we now want the lower settlement infill. But what he did is terribly important as pioneer work, and we can only build upon it.

CHADWICK: When I first knew Hope-Simpson he was climbing every hill in view, and he gradually realised that he was spending 80-90% of his time in a fruitless errand, because very few of these hills turned out to have any pottery on. And it was the result of years of experience and devising his technique, and then the idea of using the air photographs, even getting the Greek Air Force to fly sorties especially for them. But as a result of this he and UMME learnt that certain hills were likely to be occupied, some were unlikely. I don't know how many hilltops there are in the area he covered, but if you were going to walk up every single one of these - you'd need to spend 20 years working 50 weeks a year to cover the area. And there's another point which he and Bill McDonald made to me - that the rate at which scrub is growing on hilltops is such that they feel that in ten years it will not be possible to do what they were doing in the Messenia Survey.

HAMMOND: Didn't they develop a sampling plan once they realised that they simply weren't finding enough sites?

CHADWICK: They developed a technique of picking out possible sites from their own experience, and using the air photos for this purpose; also looking for suitable agricultural land, and water sources. There was some mention of wells earlier, but Hope-Simpson and McDonald have said to me that they didn't believe there were any Mycenaean wells. There are cisterns.

SNODGRASS: The Agora has one in Athens.

DICKINSON: Yes, there are several examples, e.g. Mycenae - they're not very big but they are wells.

CHADWICK: Well the UMME team were looking for sites with running water, particularly springs, and as they went round they talked to local people, one of the questions immediately being asked was "where is the nearest spring?". It doesn't have to be actually on the site but it has to be within a reasonable distance.

MURRAY: The problem of reconciling the number of names on the Pylos Tablets and the number of sites found is not as real as it may seem. For surely there are going to be geographical areas where sheep were pastured - where we might have merely shepherds' huts in use for six months of the year, or something like this, which nevertheless did have for the records a recognised geographical name.

CHADWICK: Yes, not in the sense of a settlement with stone buildings or anything so substantial.
DICKINSON: You might still expect to find a pottery scatter on a lot of these such sites - there are a lot of small sites with pottery scatters which you could see including interim encampments.
CHADWICK: I doubt whether there are more than 100 settlements on the Tablets, but there is very little evidence on which to differentiate between, say, a headland occupied by a lookout, a summer pasturage camp, on the one hand, and a small town on the other. We've got settlement information but we can't be at all sure what kind of sites these names represent.
RENFREW: I think a useful distinction which can be drawn is between a survey which is geared to finding the maximum number of sites most efficiently in the smallest time - which has been Hope-Simpson's method - which I'm not criticising, after all he started off looking at the Homeric Catalogue of Ships seeking to identify the major Mycenaean centres, quite a difference research design really - that is one approach. Another approach, which doesn't find so many sites, but perhaps does tell you a great deal more about those you do find, is to survey an area intensively, including the mountain slopes that are not going to have sites just to make sure that they haven't - that's a different strategy but it does give you a rather surer basis for extrapolation.
CHADWICK: Well where you've got an island like Melos you can do this sort of thing, but you couldn't do this all over the Plain of Argos.
RENFREW: Using a suitable sampling strategy, agreed you couldn't cover huge areas but you could perhaps obtain insight for extrapolation.
CHADWICK: I wonder if Mr. Cherry could tell us something about the method and theory of sampling strategy in archaeological survey?
CHERRY: If one is trying to get some estimate of population size, but lacks sufficient time and personnel for exhaustive survey, one tries to select units of terrain - since if one doesn't know where the sites are to start with one can't sample a population, one has to sample it in some kind of population and terrain unit. Now this can be done artificially by lines, transects, or a grid across the landscape, or these are natural units - some kind of natural topographic divisions of your area e.g. catchments of river drainages etc. But an underlying concept is that one can investigate a small proportion of the total region for thorough search, one can then extrapolate to the total population in terms of sample theory - which allows you to do this in certain very rigorous mathematical ways.
HALSTEAD: One of the great values of an intensive survey of a small area is that one can take, in extrapolation, another area, such as Hope-Simpson's survey areas, as the bones on which one can flesh, moving from intensive to extensive to build models for future testing.
HAMMOND: Has John Cherry's technique got a specific title?
CHERRY: The technique we are currently using on Melos is Systematic Random Transect Survey.
WARREN: I see the greatest possible danger in extrapolating from any intensive survey, however accurate within a very small area, to larger regions - especially in the highly nebulous field of population estimates. To do this without considering the variables that might affect other areas, could be quite unjustified until such variables are understood. I wasn't even aware that the prime purpose of the manoeuvre was to get at population, rather than settlement type, land use, and so on.
RENFREW: I think John Cherry was using the term 'population' in a slightly technical sense, as a population of sites from which you are sampling, although one would wish to use this information to establish human population. I would also disagree on another point: that if one has only a limited time to invest on site survey, or money, it is surely necessary to have a systematic approach to site survey, where you are setting down clearly what you want to do and sticking to it. This is going to be a better base for general inference and discussion than rushing around following your nose, though here I am not

thinking of the invaluable groundwork laid by Hope-Simpson, and one does need particularly after that groundwork has been laid a very thorough and systematic basis.

WARREN: I'm not so sure - John Cherry's final remarks did seem to be very near to saying it was all about actually estimating the human population. What I remain to be convinced of in some of these very intensive surveys, and here I would not include the Agiofarango Survey in S. Crete in this sense, but this field by field walking - I remain to be convinced as to what real, what positive new evidence this technique is going to throw up. I have yet to see any real evidence that it has developed our knowledge over and above the Hope-Simpson type of initial survey, or even the work that was done in S. Crete in which quite a large valley was studied through time quite intensively. But I shall await with great interest the results of this work on Melos.

BINTLIFF: Well actually the two surveys I have been involved with, that in the Agiofarango with Keith Branigan, and in the Argolid with Michael Jameson, have indeed been intensive field-by-field surveys, indeed this was I consider a far better technique rather than Cherry's sampling strategies, being a total coverage. But returning to what Professor Renfrew was saying about population, one does need to look at site numbers on the ground - and consider what does their number mean in scale, how many examples are there of each type of site. My own viewpoint as to what the cyclical site population pattern in prehistoric Greece means, in contrast to Renfrew's human ups and downs, i.e. closely linked to dispersal and nucleation of settlement - this is the result of studying intensive surveys such as those of the Agiofarango and the Argolid. One takes the intensive area, looks at its geographical scale, looks at the precise number known of each type of site, one is then able to realize e.g. that the standard interpretation is not the correct one - we have elsewhere missed a whole range of settlements, sites that are common but of a different type to what you have been led to expect - and at least for those intensive areas you have some approximation to the total site and human population. One can indeed then take a fragment of another area, for instance take the Sparta Plain - here Hope-Simpson published a survey, picking up the acropolis sites running down in a line along the axis of the plain. Now other people had been there and found the odd smaller non-acropolis site, either by accident or because they had been looking for such, and I feel confident in predicting that here we have the first traces of an extensive lower level series of sites in the Mycenaean settlement hierarchy. Hope-Simpson wasn't very interested in finding them. Now this doesn't prove that we do have perhaps the tip of the iceberg in surveys there, and my prediction and overall interpretation rests largely upon intensive surveys elsewhere, but is significantly locally supported by our clearly small and biased sample in Sparta. Moreover I would maintain that one CAN do something about population: one can take a region, start off with a range of carrying capacity, there are figures from antiquity that suggest that one can generalise from modern yields - altogether there is little variation observable here. We know a good deal about the crops concerned. We can now look at the number of settlements, at the size of settlements, and I think one can get very reasonable figures from this set of data.

DICKINSON: I'm sure it would be highly valuable to undertake an intensive survey of one district of the Messenia Survey. On the other hand I would reiterate the view that one should not make deductions on the basis of an intensive survey in one area to another AREA. I think for example that the settlement pattern in Messenia and the Argolid may have been rather different. There are these very large and very ancient sites in the Argolid, and I think they dominated the scene to a much greater extent than the few large sites, relatively few, in Messenia. In fact the population is dispersed amongst a much greater number of settlement units. That's why I think the results of an intensive survey can only really initially be expected to apply to the province in which you have done your intensive survey. One should also make a point about the limitations of surface survey. The material is dirty and shapes are extremely worn, one is

very lucky to find easily recognisable material on the surface of a site; one is fortunate to find many pieces of pottery with a clear shape and decoration, which are most easily and closely datable. If you then excavate even just a trial trench down through the site, you should get a quick cross-section with the different wares that you will have found lying around on the surface stratified, and you will be able to say "this kind of rough-looking coarse jar base is only found in x period", but you're not going to know this just finding it on the surface. One could say "OK, it's found with Bronze Age stuff, it ought to be Bronze Age", but part of the problem with a lot of the Messenia Survey pottery - if you read the original reports, they weren't at all sure which phase of the Bronze Age this surface material should belong to, and this is what they called their 'provincial material' i.e. the domestic pot which makes up 90% of everybody's pantry. It's the decorative pottery which is the most easily dated, (or better the fine pottery, because it's not always actually decorated,) and that is the stuff that is found least of on the surface except under good conditions. You do really need an excavation to tell you, as the Nichoria excavation has told us, what the local pottery sequence is - where a lot of these anomalous jar handles and coarse bases are going to fit into the sequence. And so I've always been very doubtful about pinpointing dates too closely on the basis of surface surveys. All too often one can pick up the extremely diagnostic kylix stem - one knows that's indisputably 14th or 13th centuries, but you try to find a clearly Early Mycenaean site, you need to prove in detail that you've got it. Or a Middle Bronze Age site outside the area where Grey Minyan pottery is being made, or an Early Helladic site where they are not using sauceboats. What can we make of the Dark Ages in Messenia, where something like 9 out of 10 sites vanish from sight?
BINTLIFF: But on the Dark Ages, how much of this apparent site loss is an artefact of our ignorance?
DICKINSON: This is certainly a difficult question, although we have had a Nichoria Dark Age specialist going back through the Survey finds again without picking up very much at all. But I admit the problem - Dark Age pottery is just coated black or blackish-red paint and such pieces could belong merely on this characterisation to a great number of other periods.

BINTLIFF: To take up a point from an earlier paper, I would like to ask Paul Halstead why he stated that the Mycenaeans did not produce any notable monuments in Thessaly. What about the Petra site?
HALSTEAD: I have visited this site. Its an awful lot messier on the ground than reports suggest. Milojcic has made the suggestion of a Mycenaean date for the walls. In theory it's huge, larger than Gla.
BINTLIFF: Yes, its one of, if not the, largest walled area of Mycenaean date.
HALSTEAD: A 4-5 km. walled perimeter. But there is a certain amount of evidence that the walls are in fact Medieval or later.
BINTLIFF: Why couldn't they be Mycenaean?
HALSTEAD: I agree, one should look at the site for convincing proof - its very hard to find anything on the site except rock and tortoises.
BINTLIFF: Milojcic did actually section the walls, rather than merely collect surface material, to obtain his Mycenaean date for them - this is published.
DICKINSON: If you build on a Mycenaean site, using earth from it in your walls, and if you also scatter Medieval pottery around the site, you won't necessarily get any evidence within the walls except Mycenaean finds that went into them. Material in a wall is just a terminus post quem. At Knossos I had some walls that were undoubtedly Roman but they only had Minoan sherds in them.
HALSTEAD: Both the Gla and Petra walls have been seen to have had very recent pottery coming out of them.

BINTLIFF: Professor Renfrew, in your book 'The Emergence', you wrote of the development of 'Mediterranean Polyculture'. Were you viewing this as allowing a denser population, or in terms of a broader economic base, which led then to

more complex social organisation.
RENFREW: Both these things, though I know that both have been criticised. Clearly it does allow a much larger population if it allows thereby new areas to be brought into cultivation - olive trees flourish where arable crops do not. The main point I was thinking about, is that it can mean in my view that one moves towards a redistributive economy, you do now have the possibility of a central organisation arising usefully to mediate exchange of these localised products, as in the case of the Minoan and Mycenaean palaces.
BINTLIFF: While agreeing with you on the potential higher density due to the spread of olive cultivation within a cereal economy, practical experience of Mediterranean farming does not support any idea of new areas being farmed because of the olive. It is a choosy tree and only grows WELL on a reasonable and lime-rich soil - in theory it competes with cereal crops for such soils; but in reality they are invaluable partners as complementary crops on the same soil. For the olive tree produces significantly only every other year - a really good crop comes but every 6 years or so; olive trees can also respond to slightly different climatic fluctuations from year to year from cereals. Cereals are grown on an alternate fallow system - hence your field may yield a crop every year if, as is usual, cereals are grown under the olive trees. On Melos one reason for the very slight number of olive trees is that they can only be grown in quantity on the cereal land - but such soil is so scarce it must be reserved for the chief bread crop. The arrival of olives throughout Greece as an important crop must be seen as allowing denser population in areas already suitable for cereals - but not for a move into non-cereal areas. (However, one could argue that a mediocre cereal district might be avoided for permanent settlement until olive intercultivation raised its production to a higher and more secure level. Ed. Note.)
HALSTEAD: One problem with this scheme is that, if one talks to rural Greeks today about how they operate their broad-based economy, most of the villages tend to have territories exploiting a wide range of the environment, and in that situation the broader the base - one could argue - the more cushioned they are against the sort of situations demanding greater social organisation. This would be the opposite of your viewpoint - only in situations where you get villages stuck exclusively on monocrop land might one expect a forced exchange of products between communities, ultimately encouraging more complex social organisation.
RENFREW: In most of the major olive-producting areas of Greece today, which of course does not necessarily include any of Thessaly, olives are produced as a cash crop - the oil is a cash product - and this cash for the olive product is of course a substitute for the redistribution of the product in a non-monetary economy.
BINTLIFF: Indeed in the recent past, and still in places today, in traditional Greece, olive oil has been used as a form of money, in exchanges for other products - in some places apparently the major medium of local exchange.
HALSTEAD: Talking to people in Cyprus it was very clear how the oil made a tremendous contribution to the calorie intake, and Allbaugh has shown this for Crete. If one considers presentday 'Neolithic' communities e.g. in S. America or the Pacific, they exploit a broad range of food sources that have no parallel in Europe, but no-one is suggesting that they are thus enabled to achieve high-level social organisation in the same way as with our much narrower European base.
RENFREW: I cannot really conceive of a highly stratified social organisation that was not originally built up on some kind of give and take of this redistributive kind.
DICKINSON: As soon as the Aegean communities start requiring metals they have got to be producing something to exchange for metal, because they haven't got much metal of their own. They would have had to pay for this, and olive oil might have been an obvious thing to use as a 'cash crop'.
ANON: But do olives make money for their primary producers. This summer I was

talking to olive farmers on Lesbos and they complained of the profits being eaten up by the middlemen.
WARREN: All farmers seem to say that, although it certainly does make money for those who organise its distribution - yet it does also equally for the farmers themselves.
DICKINSON: I think we should consider these exchanges in terms of nuclear villages. I don't envisage shipping magnates floating around in vast ships.
CHADWICK: There is evidence in the Tablets for perfumed oil, which would be a luxury product, and that I strongly suspect was being exported - the kind of high value export that would be very useful for such exchanges.
HALSTEAD: Again I don't think we should assume that a broader-based economy must lead inevitably to more complex societies. There's the possibility in many cases that what is being moved about are non-essentials, take the perfumed olives just mentioned. In the traditional Greek villages most people have diverse plots scattered so widely that they are in fact cushioned against situations that would promote considerable economic interdependence.
RENFREW: Yes, I wouldn't push the argument that the olive produces the palace. However it does seem to be the case that the Tablets confirm our belief that the palaces were acting as central clearing-houses for great quantities of commodities, and that one can at least say that where we have palaces of this kind, specialisation is an important part of their economic foundation.
HALSTEAD: Two of the economic items that we rate as fairly crucial to the prehistoric Greek economy - sheep and olives - have an ambiguous valency - you can eat them or sell them at high prices, as wool and oil - so the overall validity of the specialisation argument still seems quite open to me. Another point is that the distribution of olives around the Mediterranean ties up fairly well with communities that have very good or reasonable access to the sea. And as John Bintliff has pointed out, major trading centres in early Greece frequently coincide with key fishery locations - the two go hand in hand. Prof. Renfrew has correlated olives with Minoan-Mycenaean civilisation - yet to what extent is there the factor of coastal resources, fishing and harbours, accounting e.g. for the low density of Mycenaean finds in north-west Greece, where the olive nonetheless continues to flourish. Perhaps the good harbours and fishing were not present in comparison to the east coast.
BINTLIFF: Well they are present, as is the olive, but nobody has studied this area in sufficient detail for this to be well known.
RENFREW: Thrace and Macedonia also have good access to the sea, but low olive potential and little trace of Mycenaean culture.
DICKINSON: I think this concept of Mediterranean polyculture could well provide for the development of civilisation where there was continuity, as in Minoan Crete, where we have no break. But on the Mainland we do have a break, there is this awful reversion after the middle of the early bronze age - for the moment all the archaeological evidence known to me defends this position, that the Mainland took a step back and remained stagnant for several hundred years, as a result of the disasters at the end of EH2. Crete and the islands however in different ways continued to develop - so something more was required in order to produce the Mycenaean states.
RENFREW: Yes, you must be right, the polyculture simply opens up the possibility, but local history in different parts of Greece clearly controls the full development of that possibility.
BINTLIFF: I would argue that much of our evidence, particularly as regards size of settlement, supports the theory that people became more dependent on each other. After the more dispersed and perhaps independent small units of the EBA, in the Middle Bronze Age on the islands, and possibly on the Mainland, we find nucleation arguably for cooperative purposes. This nucleation might decrease the strength of cushioning that had previously been typical in EBA economic independence, although the olive could now compensate by providing secure returns from all land. Clearly something as complex as Minoan-Mycenaean civilisation rested on a very firm economic foundation - and Renfrew has

indicated what this was in his polyculture theory. As the UMME volume tells us, and I endorse this - 'Agriculture was the basis of the Kingdom of Pylos, as it is in this region today'. From the Tablets it is clear that Mycenaean palaces were primarily concerned with organising basic products such as wool and oil. One cannot avoid the underlying parameters imposed by the country - the prehistoric communities concerned had to play with basic elements of the landscape and with the continuing potential of a limited group of crops. Even if we toy with invaders, or internal groups doing things in an historic sense, they still had to manipulate this raw material. Without this inbuilt system no higher cultures could have got very far - this is why we can pick out the Argos Plain, the Pylos area as core landscapes where things are happening - this is demonstrably conditioned by environmental potential.

DICKINSON: The problem there is that one can see the conditions that allow a large surplus, but what does one do with it? You might just as well eat it. At the beginning of the Late Bronze Age, let us assume that the villages of the Argos Plain got together, we don't know how, and there was then a communal surplus, a considerable surplus, of products to get rid of. Who was going to buy them? The Cretans were producing enough for themselves, probably they were producing enough for the islands if some of these were poor in particular crops.

BINTLIFF: I couldn't agree more. I think we should think seriously about the value of imports of ordinary olives and wool, or flax, to those E. Mediterranean lands which are generally assumed to be the goal of Minoan and Mycenaean trade. In many respects large shipments of everyday products of this nature would be 'coals to Newcastle' - for one of the great centres of olive oil production has always been the Levant, wool was produced in vast quantities throughout the Fertile Crescent, and flax was a major product in Egypt. So trade in significant amounts must have been in something special - e.g. the perfumed oil or very finely-woven woollen products. The same I suspect goes even for internal trade within Mycenaean Greece - except perhaps for flax - everyone had wool and olives. So we are driven back to looking not at trade as such, for these communal surpluses, but at Prof. Renfrew's redistribution - these basic products that everyone had were being gathered up to feed people who were doing other things than primarily produce. When we look at lists of people in the Tablets doing a wide variety of tasks - it is unlikely that many of these tasks existed before the palace system, a new range of functionaries. These are the people it all centres on, and you are not selling the produce to them, you are providing rations for them so that they can do new things, and this is what I think these two civilisations are all about.

DICKINSON: But these civilisations still had to purchase raw materials?

HALSTEAD: Yes, but surely that is what these luxury export goods were for?

CHADWICK: Could they not have been buying their raw materials, then working e.g. metal up and re-exporting finished articles?

DICKINSON: There is no archaeological evidence for this; there is certainly no wide scatter of fine Mycenaean metalwork all over the Near East, or for that matter in the W. Mediterranean. I have argued that it was however an involvement in the metals trade that in the Early Mycenaean period gave Mycenae enough basic capital to put it on a steady basis for ever after, and possibly Messenia too. Some areas have got local products like Attic silver which were valuable, but the Argive Plain and Messenia have not. The problem with early Mycenaean Messenia is that it's not a single big state like Mycenae, it's a lot of petty states, and yet they are all burying their notables with vast quantities of gold and goodies from Crete, and this still needs explanation. I don't think the conventional models will explain it, one has to involve the Mycenaeans in something hypothetical like channeling metals from Europe - this would be an easy answer, there they can be middlemen and take a cut off the top.

BINTLIFF: I think a good research topic in this context would be to look at how much metal there was - I am personally rather doubtful that there was a great amount of material arriving. Perhaps only the elite were using metal,

possibly the warriors singled out by Homer were overwhelmingly important because the common folk were basically restricted in access to metal - hence the prominence of single combat. Our emphasis on metal richness is in large part influenced by wealthy tomb furniture.
CHADWICK: In the Tablets we have large numbers of bronzesmiths, even if most of them are underemployed.
BINTLIFF: Yes, but could not most of their raw material be recycled?
OATES: Speaking from another area altogether, I am very interested in this discussion, from the Mesopotamian point of view. We have almost no evidence at all for vast quantities of metal archaeologically, and yet on Sumerian tablets there are such mentioned of copper. We seem to be dealing with commodities of which almost no evidence has been preserved on excavated sites. Furthermore, although our lowland cities are thus importing such masses of raw materials, we cannot yet demonstrate what went back to the upland sources.
DICKINSON: Do you think in fact that the hill peoples might have made use of a surplus of food produced by the lowlanders of Mesopotamia in exchange?
OATES: It's very difficult to imagine this. It's one of the unsolved problems as to what the Sumerians gave for their imports, they did have to import such a great range of materials, yet their own products - fish and textiles, can hardly be envisaged as providing any satisfactory equivalents.
CHADWICK: Well this is exactly the same problem as Mycenaean Greece; we do have evidence from the very few unplundered tombs of vast quantities of metal, including gold. No-one has identified a gold source in Mycenaean Greece.
DICKINSON: It has been claimed, but not substantiated.
CHADWICK: Lacking proof of such mines, we are left with a payment problem for metal imports. One answer is the textile one, and Dr. Killen has made a detailed study of a possible textile trade, based on the weaving of various kinds of textiles which must have been highly specialised, they weren't just ordinary everyday kinds of products.
DICKINSON: But what we are doing again is talking about the beginnings of Mycenaean Greece but using evidence from the end of it. By the time of the Tablets you have got your system working, and it has probably been working for some considerable time. But it is the beginning that is our problem, when Mainland Greeks had probably never sailed beyond the Aegean, let along to Egypt. And there isn't any evidence that they did for another half century after the Shaft Graves.

WARREN: Returning to the question of the olives, I think John Bintliff made a very important point in stressing that the real value of the olive (along with the calorific value) is that it will feed large numbers of people and hence these can be practising other crafts or functions which are not productive at a primary level. The argument presented in earlier studies, e.g. Colin Renfrew's, about the labour/time saved by cultivation of olives as aginst purely cereal crops, perhaps has to be modified, because an examination of agricultural calendars such as those published by Aschenbrenner and Du Boulay, shows clearly that olive cultivation requires quite a considerable amount of time in the course of the agricultural year. So we may have to modify its labour-saving value, but it certainly made a great impression with its food value and allowing much denser populations; and as has just been pointed out, the olive acts as a safeguard in case of cereal harvest failure.
CHADWICK: But isn't it true that a lot of work on the olive is done at seasons of the year when people are otherwise uninvolved, fitting off-peak times rather well?
BINTLIFF: I don't think we can stress enough how powerful a combination the intercropped cereal and olive culture can be for higher culture.
DICKINSON: That's true now, but surely when you had to produce all your own bread, was not the Argive Plain once more like a cornfield than the orange grove that it is now?
BINTLIFF: No. Because the part of it that existed then is still a cornfield,

the lower part - your orange grove - didn't exist then. And the olives were largely on the hills, which is where the sites are and why they are there.
DICKINSON: So the olives were likely to be growing there amid corn, or at least sites grew both there.
BINTLIFF: Olives could be said to be fussier than corn, they need more concentrated calcium, so to some extent their soil requirements demand more than cereals - which can be grown on a wider range of soils.
CHADWICK: As John Killen showed us this afternoon, there are a series of Knossos tablets, which show that wheat quantities are combined with olives on the same tablet. Of course this doesn't prove that the grain was growing underneath the olives but it certainly suggests that they are being grown closely together.
WARREN: In Thessaly, the eastern part round Volos is teeming with olives, the whole coastal zone, and clearly this was the part most affected by Mycenaean culture.

DICKINSON: Turning to Ian Hodder's paper, what he had to say about pottery and the ways of analysing it won't work with Mycenaean pottery because it is far too homogeneous. You don't get local industries in the same way you do with local Roman pottery and its distribution. There isn't that amount of diversification.
BINTLIFF: Surely on Crete, e.g., we have some amount of concentrated production of particular styles that may be eventually tied down to localised sources?
DICKINSON: Like LM2?
WARREN: There are such, and this is in fact a field that is crying out for programmes of scientific analysis, e.g. neutron activation and so on. For really there is great scope here. One can argue on good stylistic grounds for certain localised sources, many styles e.g. centre on Knossos itself. Such applications could well revolutionise this subject. But I would emphasize this idea of a large programme, not just half a dozen analyses and then drawing conclusions on that.
DICKINSON: But you'd have to do so many, and any time you wanted to look at a new site you'd have to do a whole new lot of analyses - it's incredibly time-consuming.
RENFREW: That's the misfortune with Mycenaean pottery. It's less useful in some ways for this approach than materials which once you've identified purely by analytical methods can then be continued by eye - for instance with coarser wares. With most of the British Wares up to Roman times you can use petrological techniques, either thin section or heavy mineral analysis, and very soon you can recognise individual fabrics and you know where they come from, and that is that. But with Mycenaean pottery we have the trouble that it's so well made and so fine that ordinary petrological techniques don't give you results, but the chemical analyses e.g. neutron activation, are really much more difficult to interpret. In petrological techniques you see a particular pot, and in it bits of stone - you can say 'I know where that rock comes from', but with trace elements you have to have much more sophisticated interpretative programmes for adequate results. I think Ian Hodder's choice of examples was not ideal here, a much better example would have been obsidian. (General laughter.)

(There followed a more general discussion of the background to Mycenaean civilisation, introduced by a brief paper on this theme from Peter Warren:)

THE EMERGENCE OF MYCENAEAN PALACE CIVILISATION

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The following factors are seen as fundamental to the emergence of the mainland palace system (1). They are for the most part well known and may be categorized as A) chronological (that is, in social terms developmental), B) economic and C) socio-political, although in reality the factors were always interdependent.

(A)

1. The Early Bronze Age 1-2 proto-urban florescence.(2)

2. The destruction of that achievement in central and southern Greece, in eastern Thessaly perhaps (Pefkakia), and in at least some of the Cyclades in EB 3, c. 2200- 1900 B.C., and the contemporaneous arrival of new features from western Anatolia (3). The arrival of groups from western Anatolia may plausibly be postulated as an explanation of the destructions.

The absence of any major change in the cultural process in Crete at this time (EM III - MM I A) (4).

3. The evolution of a palace and urban system in Crete, with wide foreign connexions, c. 1900 B.C.

4. The slow, static life of the Middle Helladic farmers until a) the growth of vigorous chieftain families in late MH (the earliest Messenian tholoi, the Argos tumuli, the Mycenae grave circles, the elaborate late MH graves at Eleusis and the Marathon tumuli are some examples documenting this social growth) and b) the civilizing influence of the culture of the Second Palace period in Crete.

5. The 15th century as the key to Mycenaean development. There was a steady expansion of Mycenaean culture, on present evidence still non-palatial, in LH II, culminating in the occupation of Crete in the later 15th century.

6. The establishment of a Mycenaean mainland palace system about 1400 B.C. or early in the 14th century (5). It seems clear that the largest single explanatory factor in the establishment of the palace system on the mainland was the Mycenaean occupation of Knossos, where they adopted the Cretan palace system, based on written records. We do not yet know, however, since Minoan Linear A is undeciphered, whether the Mycenaean centralised, redistributive, quota-based economy was derived from the Minoans or created by the Mycenaeans themselves. This brings us to economic factors.

(B)

1. In the economic background to the emergence of Mycenaean palaces one fundamental point is that the agricultural base of all Aegean culture was fully established before the Late Bronze Age. The origins of stockraising and cereal cultivation lay firmly in the Neolithic period, of viticulture in the Final Neolithic (Sitagroi IV) or Early Bronze Age. But the growth of olives, vines and cereals as a tripartite agricultural system was an Early Bronze Age achievement, as C. Renfrew has emphasized (6). In this triad the olive was much the most important (7). It does not take much less time or work to cultivate than cereals (8). Its major importance lay in its high nutritional and calorific value (9) (as well as in the use of its leaves for animal fodder, its wood for building and its oil for lighting). Perhaps the predominance of olive farming at Early Minoan II Myrtos, Fournou Korifi, was typical for Crete (10), while Knossos and Lebena have shown that the crop was grown elsewhere in Crete in the EBA and there is the evidence of EBA lamps and oil from the Cyclades (11). The

ability to produce oil with such a high food value may in part explain population (and so settlement) growth in these areas.

Now this EBA agricultural development is clear in Crete and the Cyclades. By contrast it is a remarkable fact that no olive remains (other than a fragment of charcoal at Argissa) seem yet to have come to light from either the Early or the Middle Bronze Age on the mainland, not even at Lerna, where Dr. Maria Hopf has so thoroughly studied the plant remains (12). The tripartite system of olives, vines and other fruits, and cereals seems to have been established for the first time not long before the Mycenaean palace period. It may be that this was a significant factor in the emergence of the Mycenaean palace system (13).

2. Metallurgy was a second major aspect of the economic background. While the explanation of early Mycenaean (LH I) metallurgical richness is a complex problem, for which various hypotheses have been proposed (recently, for example, by G. Cadogan and O. Dickinson independently (14), that Mycenae may have been controlling metal supplies coming from north-west of Greece and en route for Crete), it seems clear that for the Mycenaean palace period (LH III) abundant metallurgy is part of the success of the Mycenaean palace system as a whole. Thus, unlike the obtrusive gold and bronze of LH I, it does not require any special, exclusive explanation. For the intervening period of the 15th century, LH II, we have less evidence, but it is clear (C 2, below) that supplies of metal were to have an important result.

But although a well-developed agricultural system and metallurgical supplies in the 15th century carried an important potential, in that without them it is difficult to see the Mycenaean palace system emerging, their existence as such does not seem a wholly sufficient causative factor in that emergence. We need to build into the economic framework a political and social dimension.

(C)

1. It may be suggested that the Mycenaean military change in the 15th century to produce a class of organized, armoured warriors with metal corslets, helmets and greaves (15) was of critical importance. They must have formed the vanguard of Mycenaean (perhaps Argolid) advance and are known to us through the Dendra panoply and the Knossos tablets. Whether they conquered Crete at the start of LM II, around 1450 B.C., or took advantage of the vacuum created by the devastation of the Thera explosion matters not in this context; they are there at Knossos and may be the men buried, without their corslets, but with their swords, spears and helmets in the warrior graves (16). They will have played a major role in enabling Mycenaean culture to adopt the palace system from the Minoans, as outlined in A 6 above. Such warriors remained basic to the maintenance of that system, since corslets appear again on the Pylos tablets.

2. The basic economic and political unit of the civilisation of the Mycenaean palace period was the village or small town. The Mycenaean tablets show clearly that the redistributive economy, although centred on the palaces, had the village unit as its base. This unit had long preceded Aegean palace civilisation and remained the basis of settlement after it, from the 14th century onwards in Crete and from the 12th on the mainland.

Discussion of the more immediate factors in the emergence of the Mycenaean palace system may therefore focus specifically on three aspects. One is the Mycenaean take-over of the Cretan palace system. How much was there of adoption or adaptation or new creation, such as the degree of economic centralisation and the question of military organisation and control? A second aspect is the nature and date of the earliest Mycenaean palaces (see note 5). The third is the study, through settlement excavation and research, of the mainland village or town pattern in the crucial 15th century, a time which was to lead up to the mainland palace system. This requires on the one hand locational and distributional

analyses, study of settlement size, type, function and hierarchy and, on the other, investigation of modern Aegean rural villages, for the possibility of many micro-economic continuities or insights, of course within a somewhat different macro-economic structure (17).

NOTES:

Abbreviations: Crossland and Birchall: R.A. Crossland and A. Birchall (eds), Bronze Age Migrations in the Aegean (1973).
McDonald and Rapp: W. A. McDonald and G. R. Rapp, Jr. (eds), The Minnesota Messenia Expedition. Reconstructing a Bronze Age Regional Environment (1972).
Renfrew: C. Renfrew, The Emergence of Civilisation. The Cyclades and the Aegean in the Third Millennium B.C. (1972).

(1) This introduction to discussion of the prepalatial background to Mycenaean civilisation was prepared at the request of the organisers of the Colloquium. It appears here much as given, with some expansion of points that seemed important, and with some references.

(2) For a full and outstanding exposition and documentation see Renfrew.

(3) E-M. Bossert, Archaologikon Deltion 22 (1967) 53-76. J. L. Caskey, Archaeology (1970) 342; in Crossland and Birchall 30; Hesperia 41 (1972) 370-5 (pottery). D. H. French in M. R. Popham and L. H. Sackett, Excavations at Lefkandi in Euboea, 1964-66 (1968) 8; in Crossland and Birchall 52. Archaeological Reports for 1972-3 21; AR for 1973-4 20 and fig. 38 (Milojcic's excavations at Pefkakia). J-P. Michaud, Bulletin de Correspondance Hellénique 97 (1973) 330 and fig. 164 (Pefkakia). This citation of evidence is not exhaustive. For the historical interpretation cf. M. S. F. Hood, Mélanges de prehistoire, d'archeocivilization et d'ethnologie offerts à André Varagnac (1971) 428-9; P. Warren, The Aegean Civilizations (1975) 58, 76.

(4) P. Warren in Crossland and Birchall 43.

(5) The date of the construction of the first Mycenaean palaces, highly germane to the Colloquium, is a problem requiring detailed study, especially in relation to the latest analyses of Mycenaean pottery. As such the question is well beyond the scope of the present work. The best discussion of the mainland palaces is by G. Mylonas, Mycenae and the Mycenaean Age (1966) chapter 3. In particular he brings out the several constructional phases at Mycenae, Pylos and Tiryns, placing the earliest in the 15th century in his chronological table (op. cit. 236). E. Vermeule (Greece in the Bronze Age) nowhere discusses the question as such, though she points out that at Pylos the first palace may have been no earlier than c. 1300 B.C. (op. cit. 162-3). But this date is inferred back from a palace life of about one hundred years and a destruction around 1200 B.C. If Pylos was in fact destroyed well before 1200 B.C. (end of LH IIIB), as seems probable (see St. Alexiou, Archaologiki Ephemeris (1970) 12 note7; C. Dickinson, Antiquity 48 (1974) 229), construction of its palace probably took place in the 14th century. (This date would still hold even if its construction was in LH III B, since evidence is accumulating that the transition from LH III A2 to B may have been around 1340 B.C. (see V. Hankey and P. Warren, Bulletin of the Institute of Classical Studies 21 (1974) 148, 150)).

At Thebes the LH III B new palace was preceded by an earlier one (the 'House of Kadmos') on a different alignment, datable to the 14th century (see S. Symeonoglou, Kadmeia I (Studies in Mediterranean Archaeology 35, 1973), especially 72-4; cf. A. Snodgrass, Gnomon 47 (1975) 313-6).

At Mycenae the pottery again suggests a 14th century date for the first megaron complex. Wace (Mycenae (1949) 75 and fig. 94) refers to pottery dating

the construction of the Great Court, Throne Room and Megaron of the palace as LH III A, and Dr. E. B. French (letter 17.11.76) kindly confirms this dating for me (op. cit. fig. 94 n being pre-LH III). Mylonas (Mycenae and the Mycenaean Age (1966) 59 and fig. 14) follows Wace in reconstructing an early palace, of LH I-II date, on the top of the citadel, though he admits that this depends on "conjecture and imagination". That the earliest Mycenaean palace should have been at Mycenae itself is no surprise and a date in the later 15th century would in no way conflict with the background developments argued in this paper.

The complex question of the date of Tiryns' first palace I have only glanced at. Construction is placed in the 14th century, but the dating evidence seems far from clear (K. Müller, Tiryns III (1930) 162, 205-8; W. Voigtländer, Tiryns VI (1973) 244, 260-1, 266. Cf. Mylonas, op. cit. (1966) 48).

(6) Renfrew 280-8, 304-7.

(7) Renfrew 304-5.

(8) See for example the agricultural calendar published by S. Aschenbrenner in McDonald and Rapp 50-2 and fig. 4-2 and table 4-2.

(9) The excellent discussions of the dietary importance of the olive in Renfrew 298-302 and in Aschenbrenner, op. cit. 58-9, in which around 2,650 calories is the acceptable daily food intake (Renfrew), while olive oil consumption of 30-125 kgs per year in a Messenian village (Aschenbrenner) yields an average daily intake of 80-320 grammes, may be correlated with further data on the nutritional and calorific value of the olive: it provides 900 calories per 100 grammes, has iron (vitamin A) and more calcium than any other fruit, while among all fats that in its oil is closest in chemical composition to the fat in the milk of the human female; at the same time it has a high content of unsaturated and monosaturated fat, giving the advantage of very little cholesterol (see Reader's Digest (May 1976) 125-30).

(10) P. Warren, Myrtos. An Early Bronze Age Settlement in Crete (1972) 145-6, 255-6, 263-5; O. Rackham in Warren, op. cit. 299-304.

(11) K. F. Vickery, Food in Early Greece (Illinois Studies in the Social Sciences 20, 1936) 51. Renfrew 285-7.

(12) M. Hopf, Nutzpflanzen vom Lernäischen Golf, Jahrbuch des Romisch-Germanischen Zentralmuseums, Mainz 9 (1962) 1-19. Remains of olive wood charcoal were found, op. cit. 16-8, but unfortunately the contexts of the finds are not stated.

(13) The evidence for olives is cited by Vickery, op. cit. 59. Late Helladic olive remains were found at Mycenae and Tiryns, while lamps in the tombs of Kakovatos and Vapheio suggest olive oil fuel in the early Mycenaean period. The absence so far of EH or MH remains may of course be due to the chances of discovery. But given the number of EH and MH sites this seems less likely than that olive cultivation did not begin on the Greek mainland until the Mycenaean period.

To this chronological evidence for physical remains of mainland olives we may add the evidence of pollen cores from Messenia. H. E. Wright's radiocarbon-dated olive pollen horizon after about 1200 B.C. is well known (Wright in McDonald and Rapp 193, 199) and, given the date, has naturally been thought problematical. In fact there is no problem. If the C-14 date is calibrated, adding about 300 years, it yields precisely the same result as the evidence we have cited above, namely that the olive became a major food crop for the first time in the early Mycenaean period.

(The reference to Early Helladic olives given by Van Wersch, in McDonald and Rapp 181, is incorrect; G. Mylonas refers only to grape seeds found at Aghios Kosmas, both in his preliminary report cited by Van Wersch and in the final report of 1959).

(14) G. Cadogan, Bulletin of the Institute of Classical Studies 18 (1971) 147. C. Dickinson, BICS 19 (1972) 147. But no evidence or references are here given for these hypotheses.

(15) See most recently P. Guida, Le armi difensivi dei Micenei nelle figurazioni (Incunabula Graeca LVI, 1973/4), especially 106.

(16) M. S. F. Hood and P. de Jong, Annual of the British School at Athens 47 (1952) 243-77; Hood, ABSA 51 (1956) 81-99.

(17) In my opinion progress in such matters is likely to come by focussing attention on the ground, on the components of terrain and physical relief (as J. Bintliff has recently done, in a fruitfully traditional way), on ecological, environmental and resource factors, rather than on theoretical constructs, which so far seem to have contributed no substantial, previously unsuspected insights into Aegean Bronze Age settlement distribution or settlement relationships.

DISCUSSION:

CHADWICK: I find your suggestion that the cause of the rise of Mainland palaces be sought on Crete hard to swallow.

WARREN: I think you've put my view perhaps a little too bluntly, but I'm certainly against such a system emerging on the Mainland unless the local society were receptive enough to avail itself of this system. They went to Crete, conquered Crete, took over this type of administrative system, but their society had to have reached a certain level itself to operate what it had learnt. It wasn't simply a 'Barbarian Conquest', and there is certainly evidence for increasing Mainland sophistication throughout the 15th century. But if we had to isolate one single contributory factor I would say that it was the pre-existence of the palace system in Crete.

RENFREW: Could I ask Dr. Dickinson and Professor Warren, if in Greece of the 16th and 15th centuries, there is likely to be nothing in the Mycenaean world comparable, shall we say, to the House of the Tiles of Lerna. I do agree that there obviously was some kind of setback after that Early Bronze Age period. But at the time of House of the Tiles Lerna, we do have a centre that could very well be imagined as some kind of redistribution centre, with all these seals - a sort of 'proto-palace'. Is it not just that we, or certainly I, am really appallingly ignorant about settlements, and particularly impressive settlements in the Shaft Grave period and so on until we get to the 14th century?

DICKINSON: What I had to consider for my thesis, as much as anything, was all the evidence from Middle Helladic and Early Mycenaean sites. I think one can say that at the moment there are no buildings in Middle Helladic Greece that are comparable at all with the House of the Tiles. There are traces under the Mycenaean palaces at Mycenae and Tiryns of buildings that must have had some sort of very simple frescoes on the walls, and I say that because it's the fresco fragments which have survived, they seem to belong to the early 15th century. In the case of Mycenae this is the East Lobby deposit, and the latest pottery in that is certainly LH2A, i.e. 1500 to 1450, conventionally. In the case of Tiryns one has a continuous sequence of buildings under the Little Megaron, and right at the bottom in a pit on the bedrock there are some more fragments of fresco which are described as being with pottery of Shaft Grave Style, by which I think they mean Shaft Grave I, i.e. LH I. After that you've got the Menelaion, the big LH2B building, that is after the conquest of Knossos, and you've got a similar building not only following it at the Menelaion in the early 14th century (LH3AI), but another one at Nichoria (also LH3AI). These are all proto-palaces, I would agree. But I don't think they were anything like even as complex as a Minoan villa. Had they been I suspect you would have had much more surviving of them. But I don't want to suggest that your Shaft Grave prince or his equivalent in Messenia did not really live in anything more than a megaron. He lived in

something slightly more elevated, but I think it was only slightly. At Peristeria there is a big early building which was cut into by the largest tholos tomb, tomb I; that building is dated to LHI partly by this fact and partly by material from the floor. It's an interesting building, containing a good number of rooms, but its style of architecture and the fact that they are still burying children below the floor do not suggest that the living standards in it were particularly high. Well I am arguing to some extent from negative evidence. All I can say is that there is so far no indication of anything comparable to Lerna, there were no sealings - I don't think there is a single sealing from an Early Mycenaean context, and there are only two seals known to me from MH contexts and both seem to be imports from Crete. There is certainly the well known quantity of seals in tombs of the Early Mycenaean period, but as far as one can tell these were just collected as gew-gaws, they weren't used. One is really arguing that there's nothing to make one suspect that the Early Mycenaean chieftains and their immediate predecessors, the chieftains at the end of the MH, were running a sort of system that one imagines for EH Lerna and on a much vaster scale for the Cretan palaces. It is something of a negative argument, but I think it's safer, given the generally low living standards of MH Greece, which is all these people had to draw on; and one has to remember how many centuries are involved. I don't think you can start Grave Circle B, which is one of the earliest pieces of evidence, much before 1600 B.C., and I don't think you can put the destruction of the House of the Tiles much later than 2200, so you've got six centuries during which nothing much happened. There are a few big megara, but that's all they are - big megara - they had a slightly more important man living there but he wasn't living on an altogether different scale - he was very much a village chieftain. As for the development of these chieftains, I think as Peter Warren said, the significant point is the appearance of a ruling class of considerable wealth and power, where there apparently had been none before. And the fact that it is a ruling class more than a system of government, which is what you've got in the palaces of Crete, I think made all the difference.

BINTLIFF: Oliver Dickinson has rightly stressed the general problem of identifying varieties of MH and most Early Mycenaean pottery, but the general impression we get still, at least on large sites, is the dramatic change in the area of settlement between Early and Late Mycenaean. Not only this but at the same time we get the appearance in the later phase of true satellite settlements, and this is something that you can't easily account for in any long term picture starting in the MH period.

DICKINSON: Which sites are you thinking of, can you name an example?

BINTLIFF: Well for example in the Argive Plain, we find apparently a much wider extent of finds in later Mycenaean times at Tiryns, the Heraion, Navplion, Asine, Argos, Midea-Dendra.

DICKINSON: Argos is a funny place, I wouldn't like to use it as a clear example. But Asine, from the latest excavations you already have MH suburbs well outside the Acropolis rock, beyond the Lower City they've got cemetery and occupation finds in more than one area. The same at Tiryns, there's evidence for an extensive Lower Town.

BINTLIFF: But it's smaller than in EH times.

DICKINSON: Yes, and there's also for your case the fact that the greatest extension of the Lower Town is Late Mycenaean, spreading over areas never settled before, especially in the north. There is EH and MH on E, W and S, but I think you're right there is here evidence for contraction. Prosymna, here we have a scatter of MH cemetery material on surrounding slopes - I'm not sure that doesn't indicate already some satellite settlement even then. But I wouldn't deny that there was an expansion in settlement area - Zygouries is another site with population moving down off the acropolis onto the plain; but frequently you can find evidence that there were people already living there (i.e. in the plain), but not so many, in MH.

BINTLIFF: Yes, but I think we have quite a different pattern here. As you have

shown in your PhD thesis, there are a few MH sites where we find several find-spots clustered together at slight intervals, seeming to belong in a satellite relationship to a major site. But these really are very close to it e.g. Mycenae, Argos, Athens. Also if we look at the Evvoia Survey we find the same thing: interestingly enough between EH and MH there you get a great change, nearly all small sites in low topography for EH being replaced by great acropolis sites in MH, and any EH site that wasn't an acropolis location doesn't carry on in MH. The only MH sites which are lowlying small sites, are demonstrably close satellite sites below these big hill sites, but very close - there isn't this infill of the landscape that is found over wide areas, and that characterised the growth of independent satellites in late LH. So I think your suburb type satellites are really a form of nucleation.

DICKINSON: Well I'd certainly say that it was the establishment of the Mycenaean state that caused this takeoff. I'm not trying to argue that there was a steady development, leading up to this takeoff, you are certainly right there; I think there was some development, an expansion of population that was partly responsible for late MH prosperity such as it is, and this was a basis on which Mycenaean achievements could be built up. But definitely I would agree for the Mycenaean period, that the graph in site numbers and sizes abruptly spirals.

BINTLIFF: Yes, and surely this is due to new functions; as you have been telling us, before Late Mycenaean times there were just not the palaces, and hence not the numerous palace functionaries; we have new tasks and a new range of concentrated service people being accommodated.

WARREN: However much one stresses the emergence of new and larger Mycenaean sites in later Mycenaean times from the 15th century, and going back takes cognizance of the extraordinary development of late MH civilisation - the Shaft Grave and tumuli series, - even when we have made the fullest possible allowance for that, it is very hard to see a palace system developing out of this late 15th century stage. We do require something, and a major factor, to get the thing off in the 14th and 13th centuries.

DICKINSON: Yes, it is necessary to consider the sequence in terms of time. And I agree with you, I believe they did pick it up because one bunch of them had taken over Knossos with the fall of Crete. They had a good enough basis to introduce the system, and perhaps they introduced it gradually - we've only got it at its most developed form, and we can only then compare it with its most developed form on Minoan Crete.

WARREN: I was wondering if Professor Renfrew's mind was running in another direction.

RENFREW: No, I don't disagree, I think there are some elements of continuity that should not be overlooked and which have been overlooked. Mr. Tripathi has recently completed a dissertation on Early Mycenaean metalwork, and has stressed the very large extent to which he thinks we can trace most of the technique and most of the forms back to the MH period, which he has also studied. So he would rather argue against massive borrowings from Crete in that particular period. However one may argue so specifically, that doesn't actually contradict the main point you are making, an organisational one, and I agree that it is difficult to see good antecedents such as one finds in Crete - these are not at all clear in the MH world. But I would be much more confident about that if we had some fairly extensive MH or Early Mycenaean settlement sites excavated, this is one of the great gaps in the excavation record. We haven't really excavated where the palaces ought to be in order to substantiate the sort of negative picture that Oliver Dickinson is quite reasonably drawing on the basis of our present knowledge.

DICKINSON: At Lerna there are indeed a lot of MH houses, and I mentioned a number of big buildings at various MH sites - but they are not the kind of structures you are looking for.

HALSTEAD: Would one not expect the Early Mycenaean proto-palaces to be directly under the Late Mycenaean palaces, which have been hidden under the latter - naturally preserved for tourists?

DICKINSON: Well archaeologists have looked underneath, there are any number of trial trenches beneath later palatial structures.
HALSTEAD: But you mentioned finding frescoes in this way.
DICKINSON: But that's all you do find, if there had been slightly more substantial buildings you would have more left of them, as you do at Knossos, Phaistos, Mallia.
RENFREW: But if it weren't for the House of the Tiles, what would we really know about EH Mainland Greece? There is also of course a somewhat similar large structure - the large circular building at Tiryns. But what else would we really have to go on?
DICKINSON: There are other examples, e.g. Akovitika.
WARREN: Yes, it's not just one large building on the Mainland, we have a growing number of sites with them.
CHADWICK: One of the major difficulties is that there is not the slightest indication that Linear B at Knossos is in a primitive form, it is already fully developed. And Mainland Linear B cannot be derived from the Knossos style. These two belong to two different traditions, without a common origin possible in the 15th century, that common origin must be put back to a much earlier period. Despite the time interval between the Thebes and Pylos Tablets, about a hundred years (1300-1200?), we cannot tell the difference between them. But even if the Knossos Tablets go back to nearer 1400, the differences are quite striking to the Mainland. If the Greeks had taken over Crete and then adopted the Minoan book-keeping system, we would expect a fairly close copy - but it is not. And can we imagine the Mycenaeans being able to take over in Crete unless they were already organised in powerful states? No-one keeps accounts unless they have to. That stage would arise when the Mainland chieftains had been able to group enough units together to produce a state large enough to require the services of proficient book-keepers. Perhaps they then sent to Crete for someone to devise a related system for them to the Linear A one. But for the above reasons, this would have to be some time before the Mycenaeans were on Crete, and this argument would tend to suggest a higher degree of organisation on the Mainland than say Oliver Dickinson would allow.
WARREN: However is it not true that Linear B on Crete and the Mainland is uniform in its fiscal procedures, down to detailed arrangements, as if they were written on the same day?
CHADWICK: It is so that book-keeping procedures are almost identical, and the same goes for much of the political side. Pylos is rather better developed and organised, but this is the result of a long period of development of the system.

SECTION FOUR : STATISTICAL AND COMPUTER STUDIES

(Chairman: Professor G. Brown)

(Introduction by Professor Brown)

I think we must beware of extrapolation. This is perhaps saying no more than that at all times we must use our common sense. My personal belief is that such commonsense should be at the forefront in all these analyses, and perhaps we need to be much more critical when we have a computer at our disposal, because there are so many different calculations which we can attempt, and if we are not

careful we tend to parametrize this and lose sight of the original data. This is plainly wrong. In the final analysis I think it is important that we can all appreciate how the results of an analysis correlate with the input data, and unless someone can demonstrate this correlation to me, in very simple terms, then I'm afraid I don't tend to accept it. We must understand the whole process at every stage of the calculations, and repeatedly put the question 'what are we precisely trying to do?'. One uses statistics to analyse data, facts, to test an hypothesis or model, and to try to define limits within which the latter are acceptable or unacceptable. We might not have all the data we require, and that data may not be as accurate as we would wish for. Nevertheless we can still state that the data has a value - but the number of pieces of the data, its detailed accuracy, will be important for defining the probability limits which one accepts or rejects. And we must finally strive to be definitive, to provide the probability of a relationship, and if we do not, we are merely stating something that can be said without a statistical analysis - that an hypothesis is plausible. So input data is crucial; it must be kept in sight all the time; our conclusions must reflect the quality of that data. We must consider of the following papers, as we did in yesterday's Tablet discussions, to what extent these principles have been followed.

INVESTIGATING THE POLITICAL GEOGRAPHY OF AN EARLY STATE BY MULTIDIMENSIONAL SCALING OF LINEAR B TABLET DATA

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This paper describes an attempt to put names to places in late bronze age Messenia. Since studies of the political geography of the southwest Peloponnese (and of Crete) in the Mycenaean period have already generated a body of literature out of all proportion to the quantity and quality of relevant data, it is worthwhile briefly indicating the wider context in which this work must be set. Chadwick (1972: 111) recently remarked that "...too much of the information the tablets contain is unusable owing to our inability to equate names with sites"; rephrased slightly, difficulties stem from our lack of an adequate grasp on the spatial context of the operations and interactions documented by the Pylos bureaucrats. This, however, is a problem common to archaeological research on most of the early civilisations, whose central places have usually received much more intensive study than the environmental and social landscapes in which they and their satellites existed. The very process of state formation is bound to be poorly understood if we cannot take into account changes in the network of interactions between population aggregates - changes which seem to have been both a cause and an effect of the emergence of "early state modules" (Renfrew 1975; Wright and Johnson 1975; Crumley 1976).

A prerequisite for investigating spatial phenomena of this type is some form of map, and while detailed archaeological surveys showing changing patterns of settlement in the sustaining area of early urban centres (e.g. Adams and Nissen 1972) are invaluable in this respect, they do not of themselves allow us to bring place-names known from archival sources into correspondence with nameless archaeological sites. Actual maps are known from northern Mesopotamia as early as 2400 B.C., and there are hints of the existence at a later date of sets of plans showing an organizational centre in relation to its hinterland (although the earliest extant map from Greek lands is on a 4th century B.C. Ionian coin reverse). In general, then, we are working in the dark when investigating internal interactions of early states, and any method which might help us create our own maps from intransigent scraps of information in fiscal records or temple

archives seems worth pursuing. Specifically, if we could make even slightly more certain statements about the size of the realm of Pylos, its subregional divisions, the location of several key settlements, and the variability in their productive potential, serious discussion of patterns of commodity distribution and mobilization within the realm would at last become possible.

Faltering steps towards this goal have been taken over the last 25 years; in particular, a new air of realism has entered the discussion with the publication of results from the University of Minnesota Messenia Expedition (McDonald and Rapp 1972) which, for the first time, provide some notion of the location, spacing and hierarchy of settlements within a Mycenaean state. Nevertheless, differences between hypothetical maps of the realm of Pylos, such as those by Pugliese Caratelli (1958), Deroy (1963), Was (1970), Hiller (1972) and Chadwick (1972; 1976), reveal our continued inability to relate names to specific locations, or in some cases even to broad regions. One problem common to several of these studies is over-reliance on shaky assumptions about the order and starting point of scribal "itineraries" used in listing place-names (Chadwick 1972: 105-111): leaving aside a number of detailed objections, the very ambiguity involved in using one dimensional lists to place names on a two-dimensional landscape rules out a solid basis for further work. Difficulties also exist concerning the extent of the polity itself (and, by implication, the scale of the operations documented on the tablets), more or less our only reliable pieces of information in this regard being (i) the attenuation in Mycenaean settlement density north of the Kyparissia River valley, (ii) reference to the River Nedhon in the coastguard tablets. Renfrew's (1975) interesting observations on the striking cross-cultural regularities in the territorial extent and modular arrangement of early state organisations could usefully now be brought to bear on this question. Nonetheless, map reconstructions from tablet data are suspect if, by asserting that the state's boundaries coincide with certain natural topographic boundaries, they try to fit place-names into suitable positions within a circumscribed terrain (e.g. Loy 1970: 144-7). We should allow the structure of the place-name positions relative to each other, and determined independently, to define the realm. In the work discussed below, problems with itineraries and scale are avoided by beginning from a mass of small bits of tablet information which are transformed, using a minimal set of assumptions, into a computer map representing the positions of towns relative to each other in a scale-free space: this can be fitted to reality more closely at a later stage.

Two-dimensional map making by computer

Non-metric multidimensional scaling (MDSCAL), the computer algorithm used in this study, is a method for expressing the relationships among a large number of units (e.g. people, assemblages, species of insect) in the convenient form of a map or structure of low dimensionality (Kruskal 1964, 1971; Shepard, Romney and Nerlove 1972). A trivial illustration of this general process is the accurate reconstruction of the relative locations of a set of cities, given an AA mileage table showing the driving distances between them. (Ed.: see Kendall, this volume). The computer initially generates a random configuration of points to represent the location of each city, then iteratively shifts them to create a progressively better match between the rank orders of the distances in the table and the distances on the computer map. The machine's success in accommodating a high-dimensional array of points in a space of far fewer dimensions is measured by a "stress" value for each configuration, and if numbers are large and distances accurate, MDSCAL can be very successful indeed. However, it is important to realize that the resultant map represents a rigid structure that can slide around in the plane, be reduced or expanded in size, be rotated about a point, and appear as a reflected image: thus, relating an MDSCAL map of city locations to the real world requires information on the actual positions of two

cities to provide scale, and of a third to orient the map. In short, we are working in a Euclidean space in which some of the most important rules of geographers' maps are relaxed.

Archaeological applications of MDSCAL using an output space of two or three dimensions have shown the algorithm's value for simplifying structural patterns in a mass of data and indicating their dimensionality - "seeing pictures in the fire" as Kendall (1975: 554) has called it. The space is an abstract one, since distances in the "mileage table" can be replaced by any suitable set of proximity of similarity measures between units. Of course, special interest attaches to studies in which units are compared in terms of their relative locations in both geographical maps (based on mileage) and MDSCAL maps (based on strengths of association) (e.g. Fry and Cox 1974; Tobler et al. 1970). These uses of MDSCAL for data reduction are distinct from specifically two-dimensional applications in a real (or assumed) geographical space, into which we seek to map the details of a structure of known general form. Computer mapping of this kind is trivial when the complete mileage table is input, but what if we supply some surrogate metrics of proximity whose information content is relatively slight, and which are also incomplete and corrupted by errors? The remarkable mapping ability of MDSCAL and related algorithms is demonstrated in two well-known studies by Kendall, in which he succeeded in fixing the locations of the 8 Otmoor parishes of Oxfordshire using only data on standardized intermarriage rates (1971a), and reconstructed the positions of the 88 departments of France by telling the computer simply "whether or not one of the 3828 pairs of departments shares a common boundary" (1971b). The task is even more difficult if each point is connected on average with few other points, but methods are now being developed to handle the problem - for example, FLATMAP, which Kendall (1975) has used to reconstruct the geographical layout of a 15th century Yorkshire manor from information on boundaries in a contemporary cartulary.

Linkages and distances in the Pylos archives

How can such techniques be applied to Linear B data? Given a set of Mycenaean place-name data and a method for transforming a "mileage table" into a two-dimensional map, the object is to create a picture of the relative locations of the settlements: obviously, the missing ingredient not expressed directly in the tablets is some measure of distance between settlements. Most scholars working in this field have obviated the difficulty by scrutinizing the company place-names kept on the tablets (i.e. linkages), and assuming that a recurrent tendency for two or more place-names to appear together reflects a degree of spatial proximity in their topographic locations (e.g. Chadwick 1972; Palmer 1963: 65-76; Hiller 1972). A similar approach is adopted here, but a few comments in justification may be helpful.

Time and cost factors serve as strong constraints on most types of social and economic interaction, which display a characteristic decay in intensity when plotted as functions of distance, yet the strength of interaction clearly relates also to the sizes of the interacting units (Olsson 1965; Taylor 1975). Relationships of this type can be approximated by some form of "gravity model" where $d_{ij}^{a} = kp_i^{b} p_j^{c} / I_{ij}$ (d is distance between i and j, any pair of units with sizes p_i and p_j, I_{ij} is their interaction, abc are exponents, and k a constant). Moreover, if size (i.e. attractive power) can be held constant, interaction simplifies to a monotonically decreasing function of effective proximity, a form of spatial decay which important theoretical and empirical research has recently shown to be applicable to a very wide range of archaeological situations (Renfrew, in press; Hodder 1974, 1976). While the 17 place-names of this study (the canonical list of 16, with e-sa-re-wi-ja replacing a-si-ja-ti-ja, and pu-ro brought in to make up the 17) are unlikely to represent units of exactly equiva-

lent size, at least 15 of them probably do refer to roughly comparable political entities involved in the upward and downward allocation of commodities within the hierarchical organisation; relevant data here include (i) the presence on Jn 829 and On300 of a ko-re-te and po-ro-ko-re-te at all the towns of the canonical list, (ii) the frequency with which a name occurs in the archive as a whole (fig. 1), perhaps proportional to the town's size, though this is clearly not always the case, (iii) the contribution ratios of the Ma tablets. Crude differences in the degree to which different parts of the realm interact are hinted at in the tablets, for example by the numerical dominance and higher average frequency of occurrence of names associated with the near province. We have a Pylos-centric view of the realm, probably reflecting a "measure of decentralization in the administration" (Chadwick 1976: 46): little is ever said, for instance, of the local central-place functions of re-u-ko-to-ro.

What distance measure is appropriate? Assuming we ignore as uninformative relationships implicit in the very mention on tablets written in Pylos of towns necessarily located at a distance from Pylos, we might proceed by examining each tablet in turn, linking two places whenever their names occur on the same tablet: this was, in fact, the metric adopted in Tobler and Wineburg's (1971) somewhat unsuccessful study of the location of place-names in the network of Assyrian merchant colonies in bronze age Anatolia. In the Pylos case, it would introduce distortions when dealing with those summarizing texts which clearly include places from several regions in both near and far province. The assumption made here is that a pair of names occurring adjacently as nearest neighbours within an ordered set of names has a higher probability of representing places in interaction than any pair separated by a number of intermediate names. This presupposes nothing about the nature and direction of scribal itineraries, and allows the randomizing effect of tablets (e.g. Jo438) which mix together places from both provinces to be minimized, since the few local errors resulting from such "false adjacencies" are over-ridden by the constraints of the global geometric structure which MDSCAL produces by accounting for all links simultaneously. After eliminating the many isolated names which represent dead ends, scrutiny of Bennett and Olivier's (1973) transcription yielded data on c. 50 names, which were stored in a symmetrical matrix with the ijth entry showing the sum of adjacent occurrences of town i with town j. Experimentation so far has been limited to a reduced 17-row version of this matrix with $\frac{1}{2}(n^2 - n) = 136$ possible links, 36 of which actually occur; less than 2% of the c. 1200 tablets is effectively involved. Both incidence (fig. 2) and abundance forms of this matrix have been submitted for scaling, and the close comparability of the results demonstrates the robustness of the method itself, and its capacity for operating on information of an extremely sparse character.

Preliminary results

Since experimentation with these and related tablet data is still in progress, results described here must be regarded as merely preliminary illustrations of the possibilities of the method outlined above. Computation was performed initially courtesy of Mr. G. Gammer of IBM UK Laboratories, Winchester, using his own version of the original Shepard-Kruskal algorithm, and subsequently by this author using MDSCAL 5M on the ICL 1906S at Sheffield University. As in most scaling applications, the data sets were run from a number of different random starts (i) to detect "bad" configurations representing local, rather than global, minima, (ii) to search for structures invariant over multiple repetitions of the programme, (iii) to produce a least stress solution which is regarded as the best picture achievable subject to the constraints of the input data. Each input combination has been repeated at least six times; secondary treatment of ties and global scaling into a space of two dimensions have been used throughout.

Figure 4 shows the lowest stress solution achieved in the first series of

runs using the matrix of figure 2 in abundance form; dots represent the final positions of the place-names in the configuration, while straight line segments join all pairs of towns mentioned adjacently on the same tablet. As noted above, the picture's orientation is arbitrary, and we cannot provide a scale. The stress associated with this configuration is fairly large, but does not invalidate it: for mapping applications of scaling algorithms, goodness of fit in a prescribed number of dimensions is less important than the production of interesting, interpretable and relatively invariant structures, since experience has shown that quite large stress values sometimes correspond to meaningful configurations. The structure of this set of points indicates the production of encouraging patterns. A line drawn from top to bottom of the picture between a-ke-re-wa and ti-mi-to a-ke-e neatly partitions the configuration into two sets of points lying well apart, near province towns on the left, far province towns on the right (all outputs have been rotated by eye to facilitate comparison with existing maps). Comparing these with Chadwick's predicted positions (fig. 3) we find that both maps locate sa-ma-ra, ti-mito a-ke-e, and e-ra-te-re-we nearer to the near province than any of the other far province towns. Similarly, there is a striking correspondence in the linear ordering of towns in the near province, with towns predicted to fall north and south of Pylos being on opposite sides of the plot. Actual distances in the plot indicate interaction, not mileage, and are probably affected by the friction of communication difficulties (e.g. across a range of hills).

Pu-ro's less than satisfactory position, to one side of the diagram midway between a near and far province town, must be partly explicable by supposing that interactions between Pylos and the rest of the realm are in some way different in kind from connextions amongst the satellite towns; the "implicit" interactions noted earlier may also play a part here. Figure 5 represents the best of 8 runs using the matrix with pu-ro experimentally withdrawn, all of which consistently resulted in less satisfactory maps associated with higher stress values. The situation is illustrated clearly with the result of a single-link cluster analysis of the original abundance matrix input to MDSCAL (fig. 6), in which the near province towns form a coherent cluster to the left of the dendrogram, joining with the remaining towns at the lowest level of similarity. The only rogue, again, is pu-ro. Although this result is slightly unfortunate, I strongly suspect that comparable statistical data from a modern state would produce a similar anomalous class of connexions for the capital city.

Use of the matrix simplified to incidence form results in little real information loss (compare fig. 7 with fig.4). Again, it is possible to circumscribe two distinct sets of points, although in fig. 7 e-ra-te-re-we falls just within the smallest convex set including all the near province towns, a not unexpected result since this town can be shown to have connexions with the near province (Chadwick 1976: 48). It is gratifying that the computer is repeatedly able to reproduce the one basic partition of the data about which we can be confident. An interesting point of detail is the position of e-re-e, which in virtually every output appears on the same edge of the plot as ri-jo and not far from it. Given the moderately probable location of ri-jo (fig. 3), this perhaps suggests a coastal, or nearly coastal, location for e-re-e, possibly near the marshy mouth of the Pamisos River, in accordance with the name ("Helos" = marsh), rather than in the northeast quarter of the far province (Chadwick 1972: 111). On the other hand, ti-mi-to a-ke-e regularly assumes a rather central position in the plot, which does not fit well with its hypothesized position close to the coast as the seat of the headquarters controlling the far province coastal sector in the coastguard tablets.

Comments and conclusion

Since the bulk of this work was undertaken, Professor Kendall (at my sugges-

tion) has run these same data using several innovative options to MDSCAL specifically designed by him for use in mapping applications: his elegant results appear elsewhere in this volume and do not require comment here. However, a number of improvements and options for use in further work on the problem, some of which have already been introduced, are summarized below:

(i) Expanding the analysis to include other tablet towns, especially places such as re-u-ko-to-ro and (ku)-pa-ri-so, which may be locatable.

(ii) The use of sets or files, rather than individual tablets, as the basic units for defining place-name connexions.

(iii) Complicating the matrix by introducing long-range metrics defining connexions between places not linked directly (e.g. if A links with B, and C links with B, then A has a second-order connexion with C).

(iv) Application of local - rather than global-order scaling (Sibson 1972), which tends to place "bad" points haphazardly without disturbing the arrangement of the rest of the points.

(v) Introduction of moderately certain pieces of topographic information derived from deductions internal to the tablets as geographical constraints on the plot (e.g. fixing the positions of pu-ro or ri-jo in advance, or demanding that the (?) coastal towns me-ta-pa, a-ke-re-wa and ti-mi-to a-ke-e fall along some defined "edge" in the picture representing a coast.

This last suggestion raises an important methodological question of general significance. The analysis discussed here deliberately restricted itself to one minimal class of information (a place-name's nearest neighbour on a tablet), yet it was able to yield a picture of some interest and complexity in its own right. This information, of course, is not new, but it is used exhaustively and in isolation; only at this point does it become appropriate to modify the picture by introducing complementary data of other kinds. There is heuristic value, in other words, in isolating a body of data and following it to its logical conclusion before evaluating it in the context of (and ultimately relating it to) other information. McNeal (1972) has outlined the importance of such an approach to Aegean prehistory as a whole: we must spin out separately our archaeological, textual, mythological and other strands of information before we think of weaving them together into a composite picture. Dr. Chadwick's book The Mycenaean World (1976) represents an explicit attempt to do this from the textual point of view.

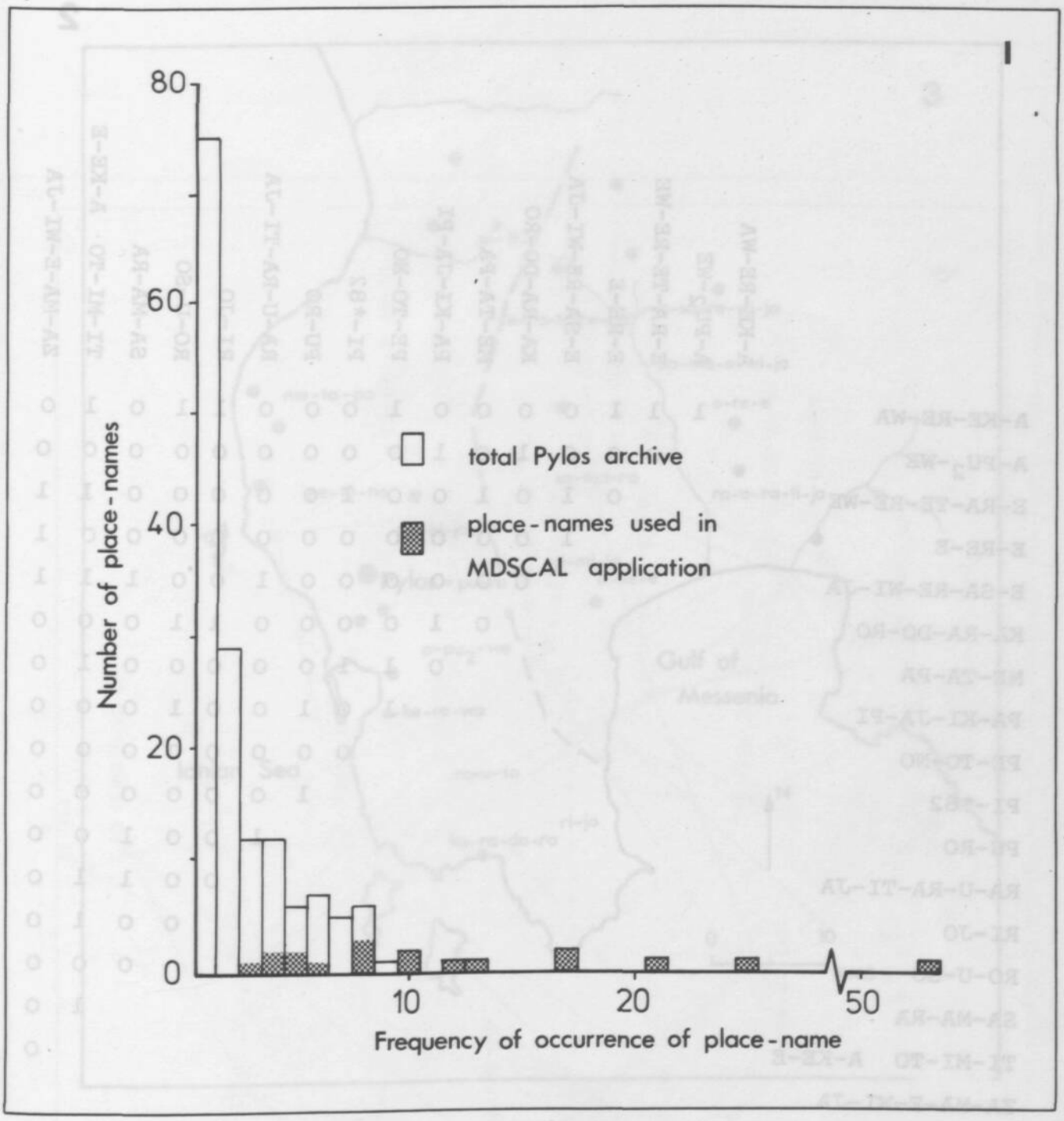
Certain Mycenaean philologists may, on principle, find the kind of analysis described here unpalatable, but they must recognize that, unless new archive discoveries or substantial tablet joins occur, the data set is finite and bounded: the only unbounded element is our own ingenuity in devising ways to extract fresh insights from familiar data.

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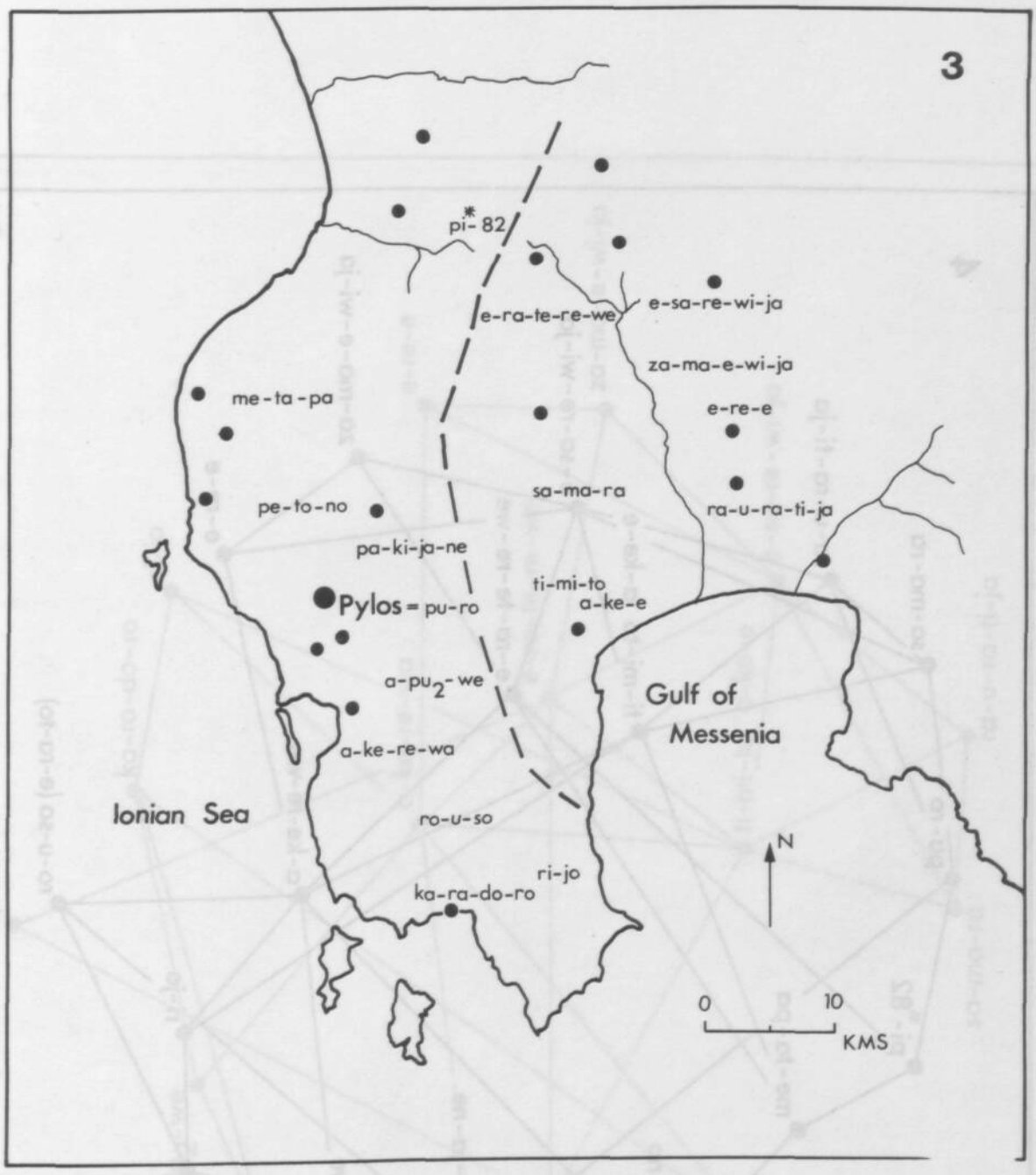
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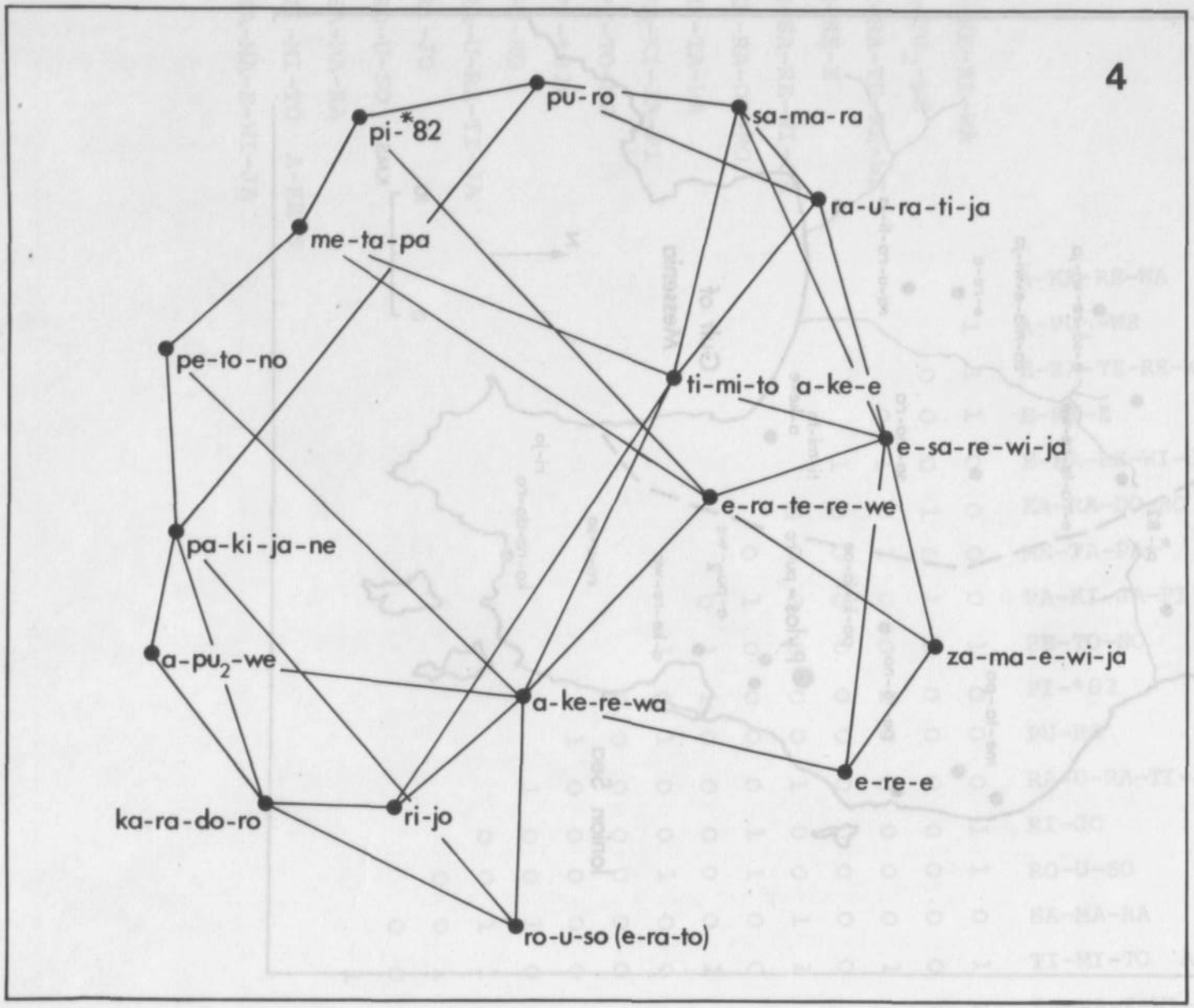
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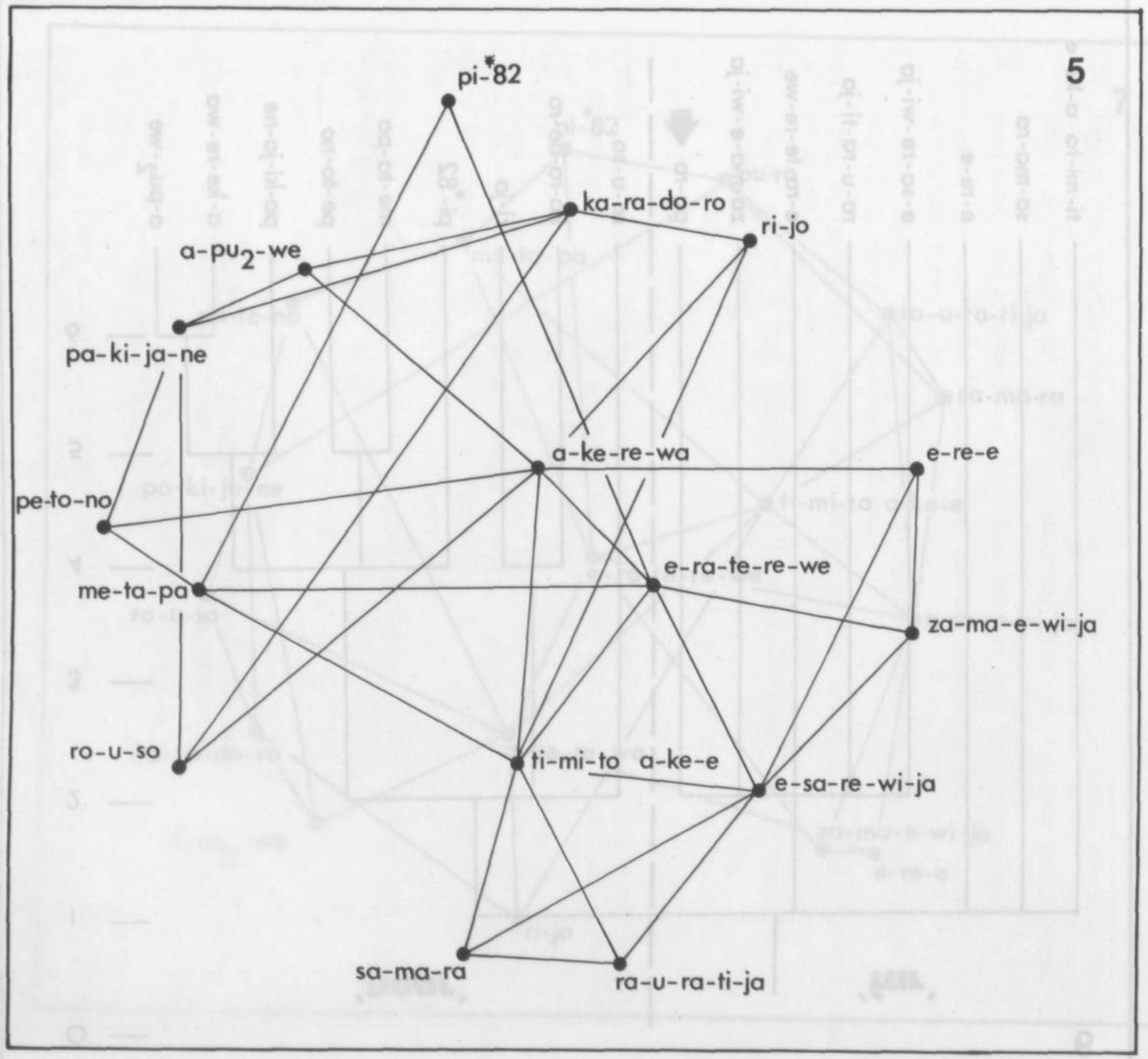
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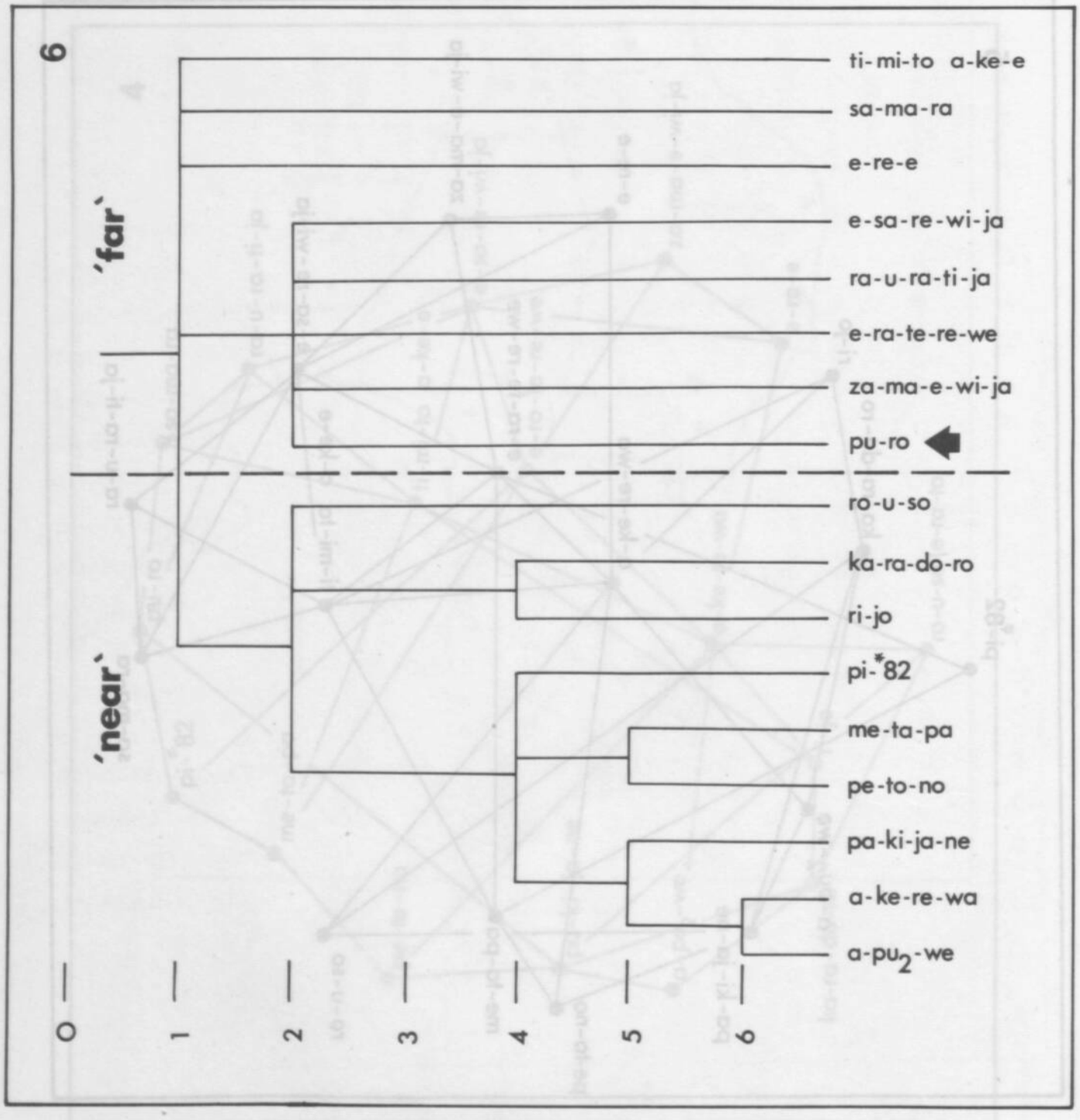

80
60
40
20
0
Number of place-names
total Pylos archive
place-names used in
MDSCAL application
10
20
50
Frequency of occurrence of place-name

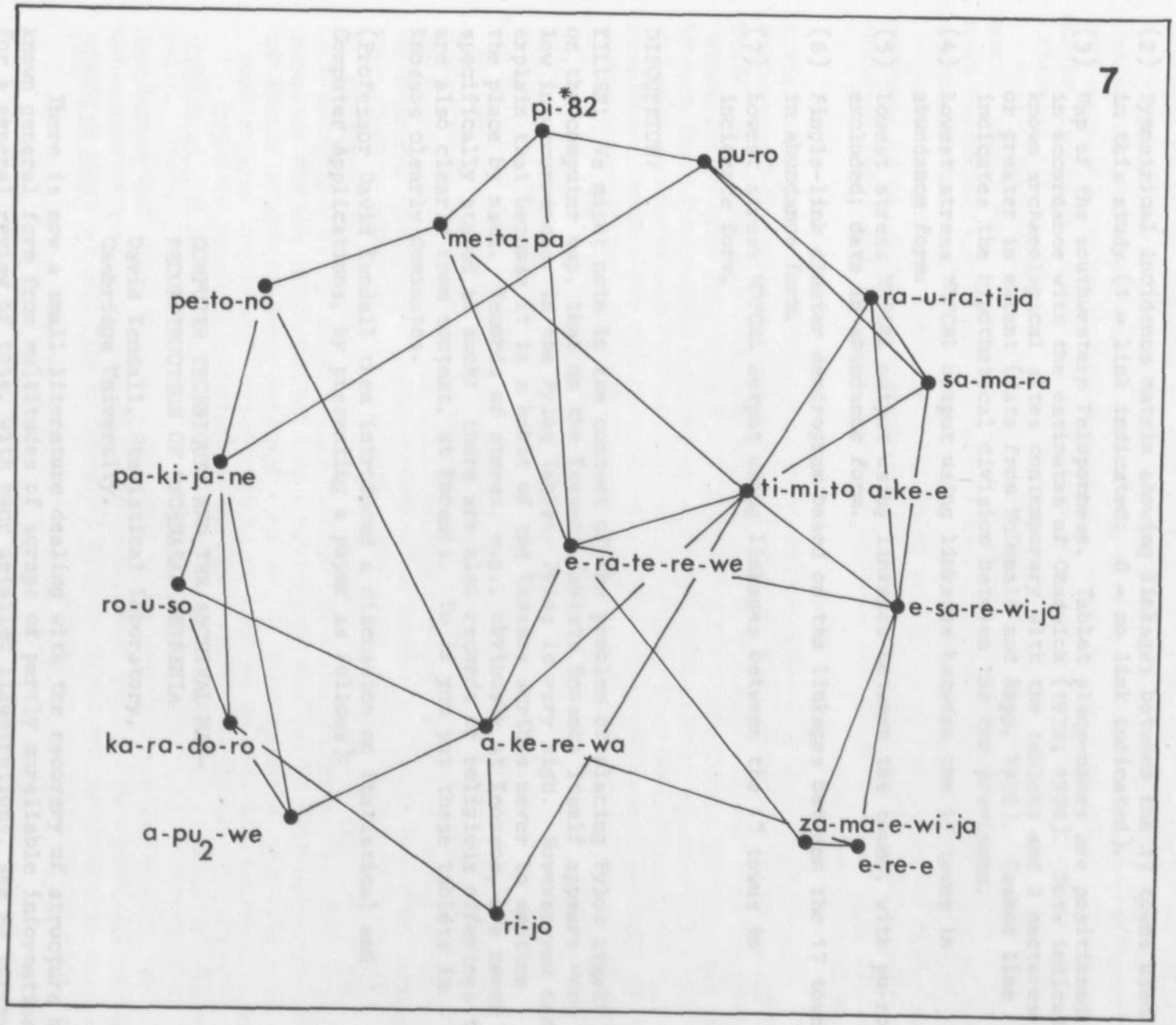
	A-KE-RE-WA	A-PU$_2$-WE	E-RA-TE-RE-WE	E-RE-E	E-SA-RE-WI-JA	KA-RA-DO-RO	ME-TA-PA	PA-KI-JA-PI	PE-TO-NO	PI-*82	PU-RO	RA-U-RA-TI-JA	RI-JO	RO-U-SO	SA-MA-RA	TI-MI-TO A-KE-E	ZA-MA-E-WI-JA
A-KE-RE-WA	1	1	1	0	0	0	0	0	1	0	0	0	1	1	0	1	0
A-PU$_2$-WE		0	0	0	1	0	1	0	0	0	0	0	0	0	0	0	0
E-RA-TE-RE-WE			0	1	0	1	0	0	1	0	0	0	0	0	0	1	1
E-RE-E				1	0	0	0	0	0	0	0	0	0	0	0	0	1
E-SA-RE-WI-JA					0	0	0	0	0	0	0	1	0	0	1	1	1
KA-RA-DO-RO						0	1	0	0	0	0	0	1	1	0	0	0
ME-TA-PA							0	1	1	0	0	0	0	0	0	1	0
PA-KI-JA-PI								1	0	1	0	0	1	0	0	0	0
PE-TO-NO									0	0	0	0	0	0	0	0	0
PI-*82										1	0	0	0	0	0	0	0
PU-RO											1	0	0	1	0	0	0
RA-U-RA-TI-JA												0	0	1	1	0	0
RI-JO													0	0	1	0	0
RO-U-SO														0	0	0	0
SA-MA-RA															1	0	0
TI-MI-TO A-KE-E																0	0
ZA-MA-E-WI-JA																	


3
pi-82
e-ra-te-re-we
e-sa-re-wi-ja
za-ma-e-wi-ja
e-re-e
me-ta-pa
sa-ma-ra
ra-u-ra-ti-ja
pe-to-no
pa-ki-ja-ne
ti-mi-to
a-ke-e
Pylos = pu-ro
a-pu2-we
Gulf of Messenia
a-ke-re-wa
Ionian Sea
ro-u-so
ri-jo
ka-ra-do-ro
N
0
10
KMS


4
pu-ro
pi-*82
sa-ma-ra
ra-u-ra-ti-ja
me-ta-pa
pe-to-no
ti-mi-to
a-ke-e
e-sa-re-wi-ja
e-ra-te-re-we
pa-ki-ja-ne
za-ma-e-wi-ja
a-pu2-we
a-ke-re-wa
e-re-e
ka-ra-do-ro
ri-jo
ro-u-so (e-ra-to)


pi-*82
ka-ra-do-ro
ri-jo
a-pu2-we
pa-ki-ja-ne
a-ke-re-wa
e-re-e
pe-to-no
e-ra-te-re-we
me-ta-pa
za-ma-e-wi-ja
ro-u-so
ti-mi-to a-ke-e
e-sa-re-wi-ja
sa-ma-ra
ra-u-ra-ti-ja


6
'far'
'near'
ti-mi-to a-ke-e
sa-ma-ra
e-re-e
e-sa-re-wi-ja
ra-u-ra-ti-ja
e-ra-te-re-we
za-ma-e-wi-ja
pu-ro
ro-u-so
ka-ra-do-ro
ri-jo
pi-*82
me-ta-pa
pe-to-no
pa-ki-ja-ne
a-ke-re-wa
a-pu2-we
0
1
2
3
4
5
6


pi-*82
pu-ro
me-ta-pa
pe-to-no
ra-u-ra-ti-ja
sa-ma-ra
pa-ki-ja-ne
ti-mi-to
a-ke-e
e-ra-te-re-we
ro-u-so
e-sa-re-wi-ja
ka-ra-do-ro
a-ke-re-wa
a-pu2-we
za-ma-e-wi-ja
e-re-e
ri-jo

CAPTIONS FOR FIGURES IN CHERRY: INVESTIGATING THE POLITICAL GEOGRAPHY OF AN EARLY STATE BY MULTIDIMENSIONAL SCALING OF LINEAR B TABLET DATA

(1) Histogram showing frequencies of occurrence of identifiable place-names in (i) the total archive at Pylos (ii) the subset used for this study.

(2) Symmetrical incidence matrix showing linkages between the 17 towns used in this study (1 = link indicated; 0 = no link indicated).

(3) Map of the southwestern Peloponnese. Tablet place-names are positioned in accordance with the estimates of Chadwick (1972; 1976). Dots indicate known archaeological sites contemporary with the tablets and 2 hectares or greater in extent (data from McDonald and Rapp, 1972). Dashed line indicates the hypothetical division between the two provinces.

(4) Lowest stress MDSCAL output using linkages between the 17 towns in abundance form.

(5) Lowest stress MDSCAL output using linkages between the towns, with pu-ro excluded; data in abundance form.

(6) Single-link cluster dendrogram based on the linkages between the 17 towns in abundance form.

(7) Lowest stress MDSCAL output using linkages between the 17 towns in incidence form.

DISCUSSION:

KILLEN: We might note in the context of the problem of placing Pylos itself on the computer map, that on the Knossos Tablets Knossos itself appears very low in occurrences, in the Pylos Tablets Pylos is very high. However you can explain that because it is a habit of the Knossos scribes never to mention the place by name. Records of stores, e.g., obviously at Knossos, are never specifically stated as such; there are also records of religious offerings that are also clearly from context, at Knossos. So if you put these tablets in Knossos clearly dominates.

(Professor David Kendall then introduced a discussion on Statistical and Computer Applications, by presenting a paper as follows):

COMPUTER TECHNIQUES AND THE ARCHIVAL MAP-RECONSTRUCTION OF MYCENAEAN MESSENIA

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There is now a small literature dealing with the recovery of structure of known general form from multitudes of scraps of partly unreliable information. For a general review of this, with many detailed illustrations, see my survey article (Kendall, 1975)*. (For one or two later developments, see also Kendall (1976), and the Appendix to Rivett (1976)). Here we are concerned with the situation in which the structure "of known general form" is the map showing the relative locations of the towns of the realm of Pylos, while the "scraps" of information are derived from the associated Linear B tablets containing lists of such towns. It is argued very persuasively by John Cherry in the preceding

* I have no spare offprints of this, but copies are still available at (and can be purchased from) the Royal Society, 6 Carlton House Terrace, London SW1Y 5AG.

paper that from the adjacency of the names of two towns in such a list we might tentatively infer some kind of spatial proximity. The dangers in this inference are obvious. One cannot sensibly convert a spatial layout into a linear arrangement, and so it is to be supposed that at some crucial places in the list we may be presented with adjacent names which refer to towns not quite so proximal as might be inferred. On the other hand, a town in such a list should have at most two "adjacent neighbours", yet might spatially possess other equally proximal neighbours. Thus the data is by its very nature imperfect, and other imperfections might be added to these. We must therefore employ a type of analysis which will stand some chance of revealing its spatial relevance, or the lack of it. The best way to achieve this is to hold back some of the relevant information, and then to see if our procedure recovers it independently from the remaining portions which are presented to the computer. What I have in mind here is the "Chadwick Line" drawn by John Cherry and separating the "near" from the "far" province. The relationship of the sites to this line, and indeed their assignment to such provinces, will be held back; we shall then watch with interest to see if the automatic analysis rediscovers it. It is to be expected that it will do so, because the Chadwick Line was itself in great part derived from data of this sort.

I understand that it is expected of me that I will give a very brief sketch of the sorts of procedure I have developed for so "re-discovering" topographical relationships; one of these is, of course, the procedure now used by Mr. Cherry, and described by him already in fair detail, so that it will only be necessary to add a few remarks to his, together with some visual illustrations of some of my earlier exercises of this kind. (In the present (written) version these latter will be suppressed, as it will be preferable that the reader should refer directly to the survey article mentioned above, and to some of the other references given by Mr. Cherry, especially Tobler & Wineburg (1971).

A good starting point is provided by Mr. Cherry's presentation of the computer AA mileage map (see Cherry, this volume). I should like to direct your attention to the "map of the UK" in the top right-hand corner of that diagram (Ed: not reprinted in this volume). This map was presented by Mr. Cherry in good faith as if it were a true geographical map, and I noticed that though he happened to leave it on the screen for a considerable time, no-one objected to it as such. In fact, however, he was mistaken, and it is nothing of the kind. It is a direct reproduction of a part of Figure 14 on page 557 of my survey article, and as the legend which was omitted makes clear, it is a computer reconstruction made by me, and not a true map. The fact that you all accepted it as a true map, although most of you know the area perfectly well, should in itself convince you that my methods do in fact work! Now if you were to look at it again, you would notice that Hull is presented as a suburb of York, although in fact it lies very much farther to the east.

Mr. Cherry has outlined the broad principles of such mapmaking procedures, and yet experience of earlier discussions makes it seem necessary to stress, at the risk of labouring the obvious, what information was, and what was not made available to the computer programme.

The programme only had access to the following list of fifteen facts:

Hull-York is the closest pair;

Cardiff-Gloucester is the next closest pair;

.....

Cardiff-York is the most remote pair.

From wuch slender information the computer then made the map which deceived you all.

Of course such an exercise is not very impressive because we cannot forget that the distances were known to the experimenter, even if not used by the computer, and from such distances a map can always be made, though it may not be a good one. It is a matter of record that the surveyors of Ulster in 1609 produced excellent maps of the escheated territories, which however, were less useful than they might have been because East and West were interchanged. The "reflected" maps can be seen in Andrews (1974); I do not know if there is any record of the confusion they produced in the minds of those who tried to reconcile them with what they actually found on the ground.

This is a helpful anecdote because it illustrates the important fact that such "reflection errors" (as well as orientation errors, and errors of scale) must be expected in any maps we produce by the techniques in view here, unless indeed we are provided with at least 3 known and non-collinear reference points. We do, of course, have one such reference point (Pylos = pu-ro), but that is not enough. In any case the identification of pu-ro is another of those things which we shall withhold from the computer.

Why do we bother to use computers at all in exercises of this sort, when the number of items to be placed is really rather small (17 in this example)? One reason is that we might have in mind further extended analyses involving hundreds rather than tens of sites, and then the computer's speed and unflappability will indeed be needed. But there is another and a more important reason. Such analyses as these must always be repeated several times, from many different (random) starting positions, in order to eliminate sub-optimal solutions. It is just not possible for the human operator to carry out even a second analysis, and completely to disregard the results of the first. And even during the first analysis, it is impossible for him to ignore completely the fact that he knows that Pylos = pu-ro. So we use computers principally because of their lack of prejudice.

The use of adjacencies in lists, which lies at the root of Mr. Cherry's analysis, is very similar to my use of adjacencies in the reconstruction of the map of France from the abuttings of Departments, and of the map of the manor of Whixley from the abuttings of the common-field units. These last exercises of mine in fact suggested to Mr. Cherry his present ambitious and admirable project. It is therefore especially gratifying to me to find that he has so clearly demonstrated that the adjacency data do indeed, as was hoped, by themselves hold topographical information, and that to a certain extent at least this information can be extracted.

When looking at his maps it is useful to bear in mind that one is just looking at the data itself from a novel and topographically informative standpoint. Indeed the actual data is visible all the time in the line-segments on the maps, which link the sites assigned to two townships when and only when their names occur adjacently on at least one tablet. This is a very weak requirement, yet in a sense it has been shown to be enough.

Mr. Cherry made use of a local (IBM-Winchester) variant of the basic Shepard-Kruskal MDSCAL algorithm which is central to the present approach, and he used only one (and perhaps not, I think, the very best one) of the numerous options which should be available on a good version of this algorithm. So I will show you what can be done by making the most of the options possible, by putting before you, in my Figure 1, the result of 200 iterations using local primary scaling and a similarity coefficient based on the Wilkinson metric (this last has the advantage that it links sites via other sites as far as may be necessary). (It is also possible to use unprocessed, or only mildly processed, raw adjacencies, combined if so desired with my "Tertiary Treatment of Ties" (Kendall, 1976); for the results of this see my Figure 2.) Figure 1, typical of many such, shows

the final configuration together with segments linking all immediately "adjacent" pairs. You will notice that there are fewer flagrant "crossings" of segments than in Mr. Cherry's pictures, and this reflects in one way the greater measure of success achieved here.

How are we to react to this picture? It is based on data that we may consider unsatisfactory, and it will necessarily inherit any faults that the data may possess. It is however a fair and unprejudiced representation of the data, and we can now inspect it with all of whatever else we currently know or believe about the realm of Pylos explicitly in mind. For example, we can consider the location assigned to Pylos itself, and so on, but I leave such discussions to experts qualified to conduct them. One thing which even the amateur can do is to ask, how far are Chadwick's "near" and "far" provinces discriminated by the computer map? The best way to assess this is to pick out the townships of the "near" province and to draw round them the tightest fitting convex polygon, and similarly for the "far" province. This is done in my Figure 3, and you will see that the degree of separation is extremely striking. At the same time we must remember that the Chadwick Line is itself largely derived from such adjacencies.

We must above all take careful note that a technique which scores a bull's eye in one respect may miss the target wildly in another. Just because Chadwick's two provinces have been separated by the computer, we must not fall into the facile error of supposing that we have anything so precise as a geographer's map, even after the inevitable errors of scale, orientation, and "handedness" have been allowed for.

It will be found instructive to compare Figure 1 and Figure 3 on the one hand with Figure 2 and Figure 4 on the other; these last use unprocessed raw adjacencies (instead of the Wilkinson metric), and local primary scaling. Once again the "near" and "far" provinces are separated, but the two "maps" are by no means identical.

In Figures 2 and 4, the only requirement imposed on the computer is that, if X is any site, and if Y_1, Y_2, ... are those sites which are somewhere named adjacently to X, while Z_1, Z_2, ... are those sites which are never named adjacently to X, then:-

> in the map, all the Y's are to be closer to X than any of the Z's.

This requirement is demanded for every possible choice of X, and conflicts are resolved by compromise. Such conflicts do occur. Thus in Figure 2, K is not linked to P, but is placed closer to P than is G, which is linked to P. Because all the adjacency-links are shown on the map, we can if we look for them detect all such inconsistencies.

It is interesting to observe that some sites (such as G) are linked to many other sites (in that case, to seven). From this it follows that the adjacency data are certainly not derived from a standard linear order. Again, G (in the "near" province) is linked to K, M, and P in the "far" province. Thus the relationships are more subtle than those of a mere "listing", and of themselves suggest a 2-dimensional rather than a 1-dimensional layout.

What should now be done, by those qualified to do it, is a careful item-by-item checking of these computer maps against the individual tablet-lists which generated them. In this way one may discover something about the provenance of the lists themselves (the way in which they were drawn up and the purpose they were intended to serve), as well as an understanding of which parts of the

supposed "maps" have geographical significance, and which have not. And while doing this, let us remember the fate of the genealogist who thought that he could put all the former inhabitants of his village into the houses they lived in in 1851, by following the census-taker's route from house to house, until a friend reminded him that the census lists were very possibly made up over a pot of ale in the local public house.

I wish to thank Professor Robin Gibson of the University of Bath for making available to me his elegant and flexible variant of MDSCAL. I am also grateful to Mr. John Cherry for allowing me access to the Linear B data, to the University of Cambridge Computing Service for computing facilities, and to Miss Nell Hughes who re-drew the illustrations.

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(This paper contains an Appendix summarising (Kendall, 1976)).

Legends for Diagrams

Figure 1. Local primary scaling of Wilkinson distances derived from adjacencies; 200 iterations.

Figure 2. Local primary scaling of the unprocessed adjacencies themselves: 200 iterations.

Figure 3. The two "provinces" in Figure 1.

Figure 4. The two "provinces" in Figure 2.

Table 1. Code for diagrams

"Near" province	"Far" province
A = PI-*82	K = TI-MI-TO A-KE-E
B = ME-TA-PA	L = SA-MA-RA
C = PE-TO-NO	M = E-RA-TE-RE-WE
D = PA-KI-JA-PI	N = RA-U-RA-TI-JA
E = PU-PO	O = ZA-MA-E-WI-JA
F = A-PU_2-WE	P = E-RE-E
G = A-KE-RE-WA	Q = E-SA-RE-WI-JA
H = RO-U-SO	
I = KA-RA-DO-RO	
J = RI-JO	

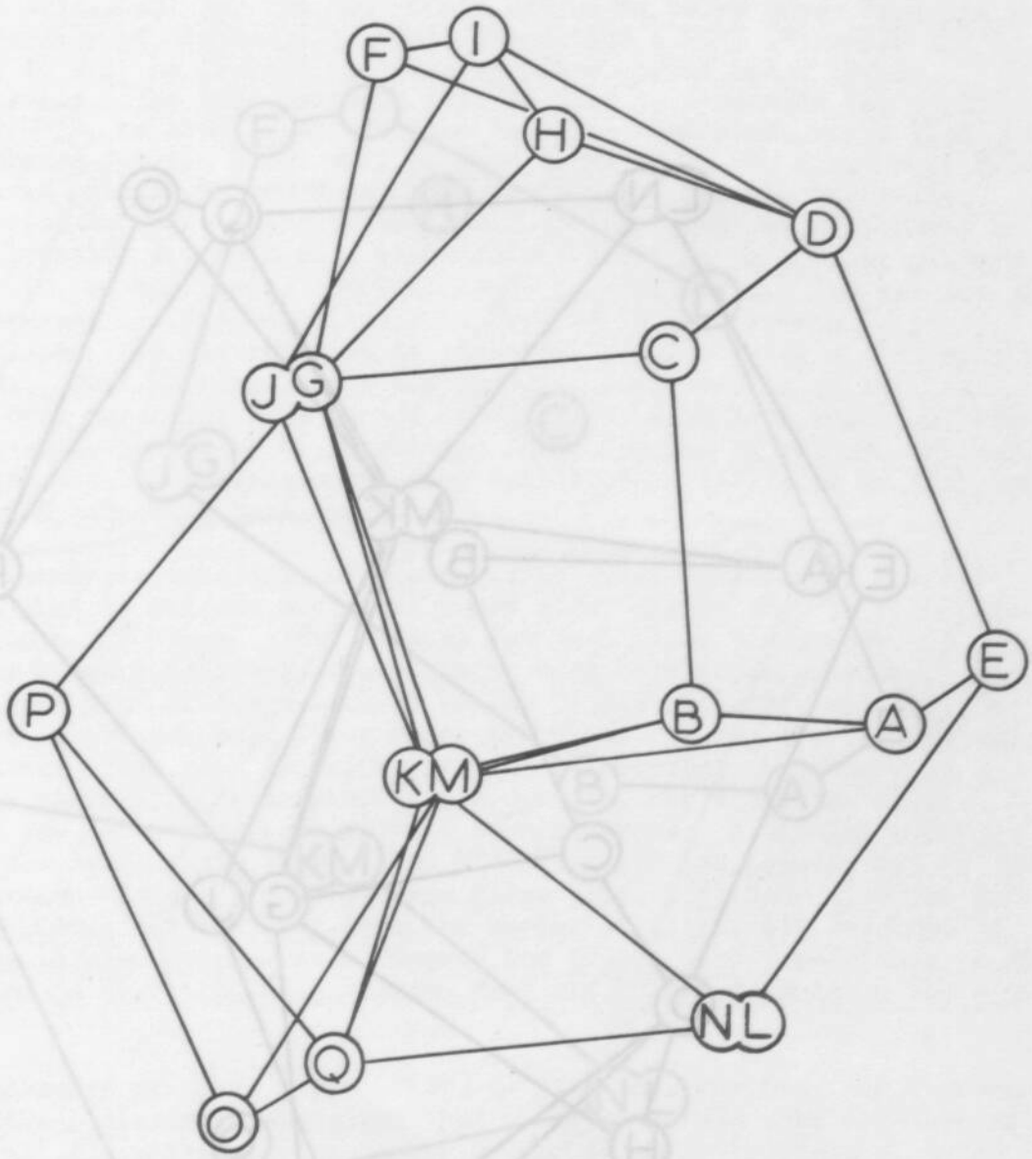
DISCUSSION: (Taking part: Paul Halstead, John Cherry, Professor Brown)

HALSTEAD: Professor Kendall, have you tried using some of these measurements to see if they compare to those of a real map?
KENDALL: No, and I think it is a mistake to try and do it.
CHERRY: Looking at the three examples that I know of where these kinds of techniques have been used for this type of archaeological archive data - the Pylos data, some similar work I've done on the Knossos Tablets which gave almost identical results to those of Dr. Wilson, and the study of Tobler and Wineburg on the Anatolian Kultepe Tablets: I think there are some interesting correspondencies in the characteristics of the input data and the degree of success that's achieved in the output. What we're really dealing with is the average number of points with which each point is connected, the index of connectivity for the entire data set. And with the particular data set which both Professor Kendall and I were using, that has been about 4, that is on average each place has about 4 linkages. In the case of the Knossos data the value falls below 4, with the Kultepe texts that value is 3. Kendall has shown, using information theory arguments, that if the value falls below 2 the problem is automatically insoluble; if it falls between 2 and 3 it's very difficult to devise very sophisticated algorithms to solve this sort of problem. When it's got above 3 it's quite likely to show successful applications. So if we were to compare success levels, we do seem to have picked up undeniable patterns from these Pylos texts, but I had much less success with the Knossos set, and on the Kultepe texts there was zero correspondence between any of the previously published estimates of the location of these merchant towns and the output of the multidimensional scaling programme. That's not necessarily to say that structures aren't existant in that data set, because another worker, a French mathematical archaeologist, was able to use the same data expressed in the same formulae of the Kultepe Tablets, and using network techniques to isolate cliques of merchants and their interaction.
BROWN: Professor Kendall, are we then to understand that the maps that you have produced do not represent distance?
KENDALL: If there are enough connections, and internal evidence supports a consistent grouping, then I think we might expect an element of real spacing to be recorded by our analysis.
BROWN: Well, you're not really answering the question. I think we must be precise about the type of interpretations feasible and the limits on their validity, as in this case, especially if the processes of calculation cannot be followed with appropriate caution by the archaeologist using such results.

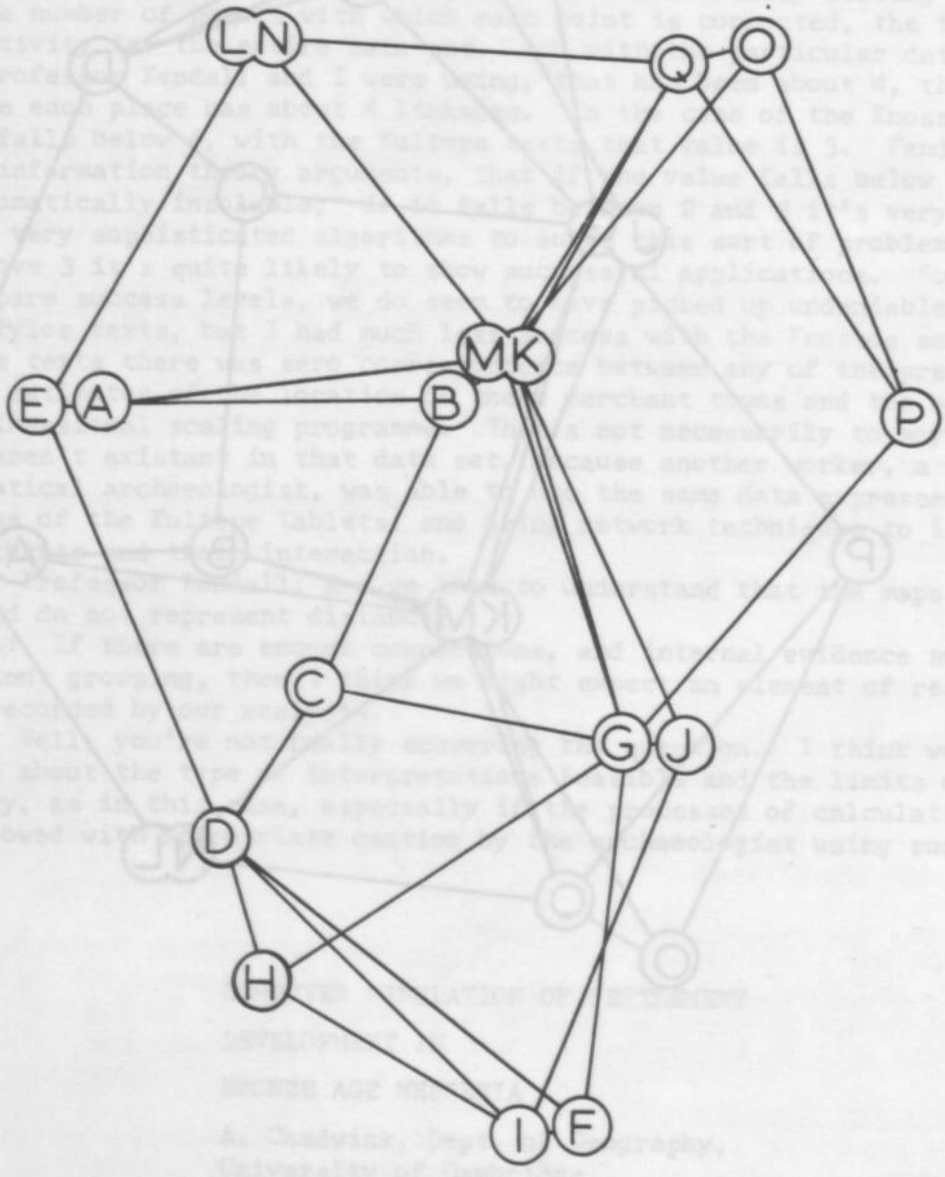
COMPUTER SIMULATION OF SETTLEMENT DEVELOPMENT IN BRONZE AGE MESSENIA

A. Chadwick, Dept. of Geography,
University of Cambridge

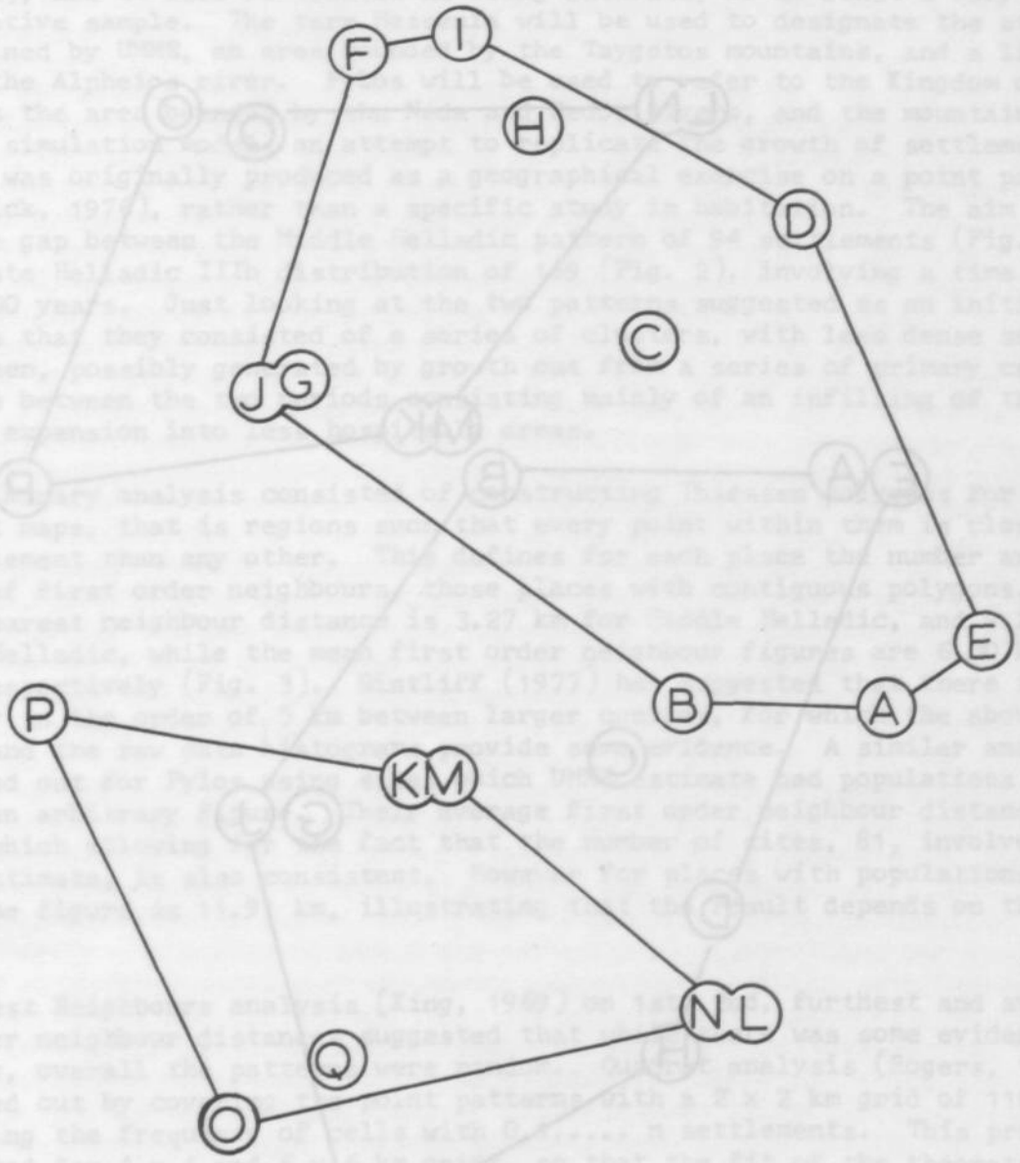
It has been said of simulation that it only reproduces the investigator's ignorance - this is true. It is only a technique that allows experimentation with, and refinement of, some theory about reality. This involves building some form of working model, which requires both simplification and a rigorous formulation of the hypothesis involved. The major problem with it is that just because a good statistical fit can be obtained, difficult enough to test for in many cases, there is no guarantee that the process postulated is correct. The same end result can be produced by more than one process, and in the absence of



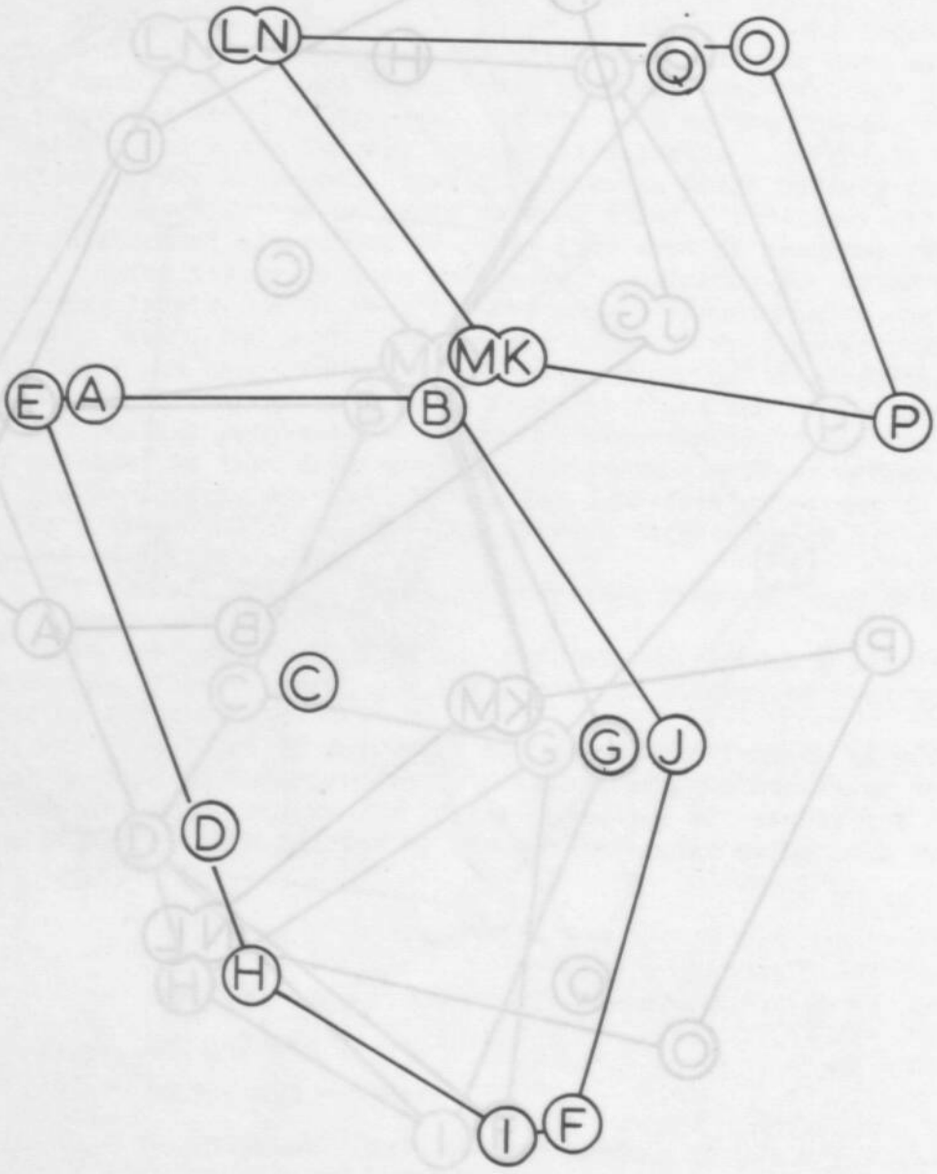
THE REALM OF PYLOS (WLP200; DGK 451)



THE REALM OF PYLOS (M1LP200; DGK 683)



THE PROVINCES OF PYLOS (WLP 200; DGK 451)



THE PROVINCES OF PYLOS (M1LP 200; DGK 683)

specific information there is no easy way of discriminating between the options. The computer is simply a device for solving at high speed a set of equations, using a suitable algorithm, and allowing the production of a population of results. The technique is used here to examine Mycenaean settlement, the aim being to suggest its uses and limitations, rather than to answer specific problems.

The data on settlement for the two models discussed below comes from the work of the University of Minnesota Messenia expedition - UMME (McDonald and Rapp, 1972), and it will be treated as being accurate, or at least a very representative sample. The term Messenia will be used to designate the study zone examined by UMME, an area bounded by the Taygetos mountains, and a line just north of the Alpheios river. Pylos will be used to refer to the Kingdom of Pylos, defined as the area bounded by the Neda and Nedos rivers, and the mountains. The first simulation model, an attempt to replicate the growth of settlement in Messenia, was originally produced as a geographical exercise on a point pattern (A. Chadwick, 1976), rather than a specific study in habitation. The aim was to bridge the gap between the Middle Helladic pattern of 94 settlements (Fig. 1), and the Late Helladic IIIb distribution of 169 (Fig. 2), involving a time span of some 300 years. Just looking at the two patterns suggested as an initial hypothesis that they consisted of a series of clusters, with less dense settlement between, possibly generated by growth out from a series of primary centres. The change between the two periods consisting mainly of an infilling of the gaps, with some expansion into less hospitable areas.

Preliminary analysis consisted of constructing Thiessen polygons for both settlement maps, that is regions such that every point within them is closer to that settlement than any other. This defines for each place the number and location of first order neighbours, those places with contiguous polygons. The average nearest neighbour distance is 3.27 km for Middle Helladic, and 2.21 km for Late Helladic, while the mean first order neighbour figures are 6.00 km and 4.40 km respectively (Fig. 3). Bintliff (1977) has suggested that there is a regularity in the order of 5 km between larger centres, for which the above averages and the raw data histograms provide some evidence. A similar analysis was carried out for Pylos using sites which UMME estimate had populations of 100 or more, an arbitrary figure. Their average first order neighbour distance is 5.44 km, which allowing for the fact that the number of sites, 81, involved is an underestimate, is also consistent. However for places with populations of 500 or more the figure is 11.91 km, illustrating that the result depends on the data used.

Nearest Neighbours analysis (King, 1969) on 1st, 2nd, furthest and average first order neighbour distances suggested that while there was some evidence of clustering, overall the patterns were random. Quadrat analysis (Rogers, 1974) was carried out by covering the point patterns with a 2 x 2 km grid of 1109 cells, and counting the frequency of cells with 0,1,.... n settlements. This procedure was repeated for 4 x 4 and 6 x 6 km grids, so that the fit of the theoretical distributions could be tested for scale dependency. The Poisson model was fitted first, implying a random pattern, where each cell has the same probabilities of independently receiving 0,1,...n settlements. The fit of the Poisson broke down on the larger lattice sizes, however the negative binomial gave a much better fit at all scales, and for both time periods. The latter model can be generated in a number of different ways, of which two are relevant to settlement studies (Cliff and Ord, 1973). Either the point patterns could have been generated by contiguous growth of offspring from randomly located centres, or else they could have come from a Poisson model, but one in which the probabilities vary from cell to cell. Analysis showed a firm preference for the latter, a result also obtained for Pylos using places with populations over 100. Cliff and Ord suggest that this result can be interpreted as showing that a settlement pattern is random, but

biassed by the potential of the underlying environment.

On the basis of these results an environment surface was constructed, using the 2 x 2 km grid. This represented a synthesis for each cell of the dominant geomorphology, the availability of surface water, and the resources offered by the sea. An initial simulation model was constructed by converting this surface into a set of probabilities, and using random numbers from a uniform distribution to locate the 75 new settlements required. The result was judged a failure, which is not surprising as the model assumed that the location of a settlement is independent of the existing pattern. Settlements need some form of 'territory' to exploit to provide each with a livelihood, which does not have to be a contiguous area as it is defined by economic rather than spatial factors (Hudson 1969). In the case of a mainly agricultural community the 'territory' would be mappable as a contiguous geographic space, because distance is the main organising factor involved. For operational purposes the Thiessen polygons already drawn around each site provide a clearly defined surrogate, and these were used to transform the environment surface by means of a form of potential model. For this the distance decay term was produced by fitting a curve to the frequency distribution of the distances to the sides of the Middle Helladic polygons. A 'settlement field', to modify a term of Hägerstrand (1967), was generated by fitting a curve to the Middle Helladic first order neighbours histogram. Values were then computed from the resulting equation for the mid-points of 2 x 2 km cells forming a 17 x 17 lattice, and then converted to probabilities. Each figure represents the chance of finding a first order neighbour that distance from the central cell. The resulting symetrical 3-dimensional shape is rather like a volcano with a large caldera (Fig. 4).

After some experiment a final model was evolved (Fig. 5) that operated by placing the 'settlement field' over each cell with one or more settlements in it, multiplying by the underlying transformed environment surface, and scaling the resulting values to sum to an index value between 1-6. This is an inverse index compounded of the environment surface value, and the number of sites in the cell under consideration, acting as a surrogate for population. These fields were added up, and a set of probabilities produced for the whole map. Then 10% of the existing number of settlements were located using random numbers, actual entry to a cell being more difficult as the number in it rose. The probabilities were then recalculated, and the procedure cycled until the Late Helladic total was reached. The 10% figure represents a compromise between the cost in computer time, and the ideal of recalculating the probabilities after every new settlement. A residual map was formed by taking the simulated away from the actual Late Helladic pattern, and this was tested to see if they were randomly located, implying no systematic deviations. The test was carried out using the 4 x 4 km lattice rather than the 2 x 2 km used for the simulation as it was felt unreasonable to expect the model to be accurate to 2 km without more detailed information. At a 99% significance level it is just possible to say that the Late Helladic pattern could have been a member of the simulated population of 49 (Cliff and Ord, 1973).

The model suggests that the key factors controlling the growth of the settlement pattern between the two time periods are the environment, the spacing out due to a need for a 'territory', population size, and a stochastic element. The effect of summing the 'settlement fields' is to favour the infilling of the settlement pattern, which agrees with what was initially hypothesised. The histogram of distances to nearest Middle Helladic neighbours of sites new in Late Helladic shows a flat peak in the 1-4 km range, compared with the very peaked distribution of Late Helladic nearest neighbours in the 2 km range. This gives some support to another Bintliff (1977) hypothesis that as well as infill, fragmentation was also taking place. The notion being that offspring were emerging within the 'territories' of existing sites whose location was independent of

other settlements. If this had happened on a large scale the quadrat analysis should have been picked up, even if ambiguously, for Late Helladic, which it did not. The flat peak shows that new places in the gaps in the Middle Helladic pattern were just as important. The population element, if the crude index can be so called, was included, because without it the model would not produce a satisfactory result. It allows a greater chance for remoter areas to be settled, and prevents sites from piling up in already well settled areas. There is obviously a limit to the amount of infill that can take place due to the pressure placed on resources, especially when emptier areas are available. The role of population could be better evaluated if it were explicitly built into the model, along with a consideration of time, and making the whole thing dynamic.

The stochastic element in the model represents the net effect of the large number of individual decisions which in aggregate at this scale can be regarded as random. This is the argument behind Curry's (1964) concept of the 'random spatial economy', which put another way says that location in space will be random, subject to constraints. The constraints are important otherwise all places are equally likely, the difference between the Poisson and the negative binomial in the quadrat analysis. This framework is a much more fruitful way of looking at settlement than rigid and static central place theory, though the latter does have some useful concepts. The UMME population estimates already mentioned are available for 131 sites in Messenia, on the basis of area and a constant multiplier. If these are ranked from smallest, and plotted on a graph the result if an exponential shaped curve. This can be compared with that produced by a random splitting model (Cliff et. al., 1975), which gives the proportions that would be expected if the population were split at random into 131 pieces. A modification of the basic Whitworth model by Cohen to allow for a minimum threshold generates a close fitting curve (Fig. 6), and the random splitting hypothesis is supported by a statistical test. This result supplies further evidence for the 'random spatial economy' hypothesis. Given the observed minimum settlement size of 26, and an arbitrary population total of 75,000 this model suggests that Pylos should have in the order of 215 sites, compared with the 138 found by UMME.

The tablets provide a list of the 17 regions in the Kingdom of Pylos, split between 8 in the Further Province, and 9 in the Hither. From the order of listing and other internal evidence it is possible to produce a rough map of their relative locations (J. Chadwick, 1976), and an attempt was made to put some geographic flesh on these within the Kingdom as previously defined. Frontier delineation is a contentious issue, but looking at the Late Helladic IIIb Messenian settlement pattern the obvious place for a break is in the general area of the Neda river, not along the Alpheios. The tentative resulting map (Fig. 7) is a synthesis from cluster analysis on physical distance, the line of the various watersheds, and a large amount of guess work, using the Late Helladic Thiessen polygon boundaries as a basis. The Further-Hither province boundary is essentially a watershed, the greatest problem being in the area of the Kyparissia and Soulima valleys where the pattern has no clear breaks in it to support this. Within the Further province the basic four way split is relatively easy to make from the cluster analysis, but the further break down is arbitrary, as is the location of names. In the Hither province the area around the palace is difficult to split up, so again a lot of guess work has been used. If the output figures for an unknown agricultural product listed on the Ma series of tablets (Bennett and Olivier, 1973) for the regions are ranked, and compared with the results of a random splitting model, a statistically significant fit is found (Fig. 8). A similar result is obtained from the areas of my regions. While this does not prove anything, it does suggest that in line with the 'random spatial economy', any regionalisation that does not conform to the random splitting hypothesis should be treated as suspect.

The second model is an attempt to simulate population growth from Middle to Late Helladic, something which is very much a leap in the dark, and involves making a great number of arbitrary assumptions. For this purpose the 17 regions were collapsed into 9, which can be used with more confidence; the random splitting hypothesis still being acceptable. The Late Helladic populations were assumed to be proportional to the tablet values for the new regions, with an overall total of 75,000. The starting populations were arrived at by calculating the average number of people per Late Helladic site for each region, deflating this by 50%, and multiplying by the number of Middle Helladic sites, to give a total of 20,570. The model is a cohort survival one, with an allowance for internal migration (Wilson, 1974). The population is aged in cycles of 10 years using survival rates derived from data for England and Wales in 1801. The birth rate was made up, being equivalent to each female having an average of 6 surviving children to age 9, which is rather high. The actual birth or survival rate used on each occasion was found by drawing a random number from a normal distribution with a small variance. The migration element used a production constrained gravity model (Wilson, 1970) to distribute the flows, which derives its theoretical justification from entropy maximisation with constraints. Each cycle, repeated until the population total exceeded 74,000, consisted of the operation of the birth and survival rates, followed by the subtraction of emigration, the addition of immigrants to each region, all subject to random variation.

For what it is worth the supposed Late Helladic population is a member on the basis of a statistical test of the population of simulations generated, however actually interpreting the model is not easy. Not only was the required population growth generated in 120 years, rather than the desired 300 or more, but also the level of internal migration used was much too high to be plausible in so small an area. From the growth in the number of sites in the Late Helladic the 120 year period is not that unrealistic, but the model does not explain why this did not happen earlier, though the reason need not be directly demographic. The high birth and migration rates seem to be acting as an external source of migrants, which suggests that the model should be recalibrated with those rates lowered, and the introduction of external migration. Further research would be needed into the actual Middle and Late Helladic populations from archaeological evidence, the regionalisation used, and the parameter values found in communities living today at a similar technological level. The model clearly illustrates the use of simulation in clarifying ideas, allowing experimentation, and suggesting new problems, but it also emphasizes the need for a strong theoretical and empirical base. Without that base the whole exercise degenerates into a computer programming problem, and nothing else.

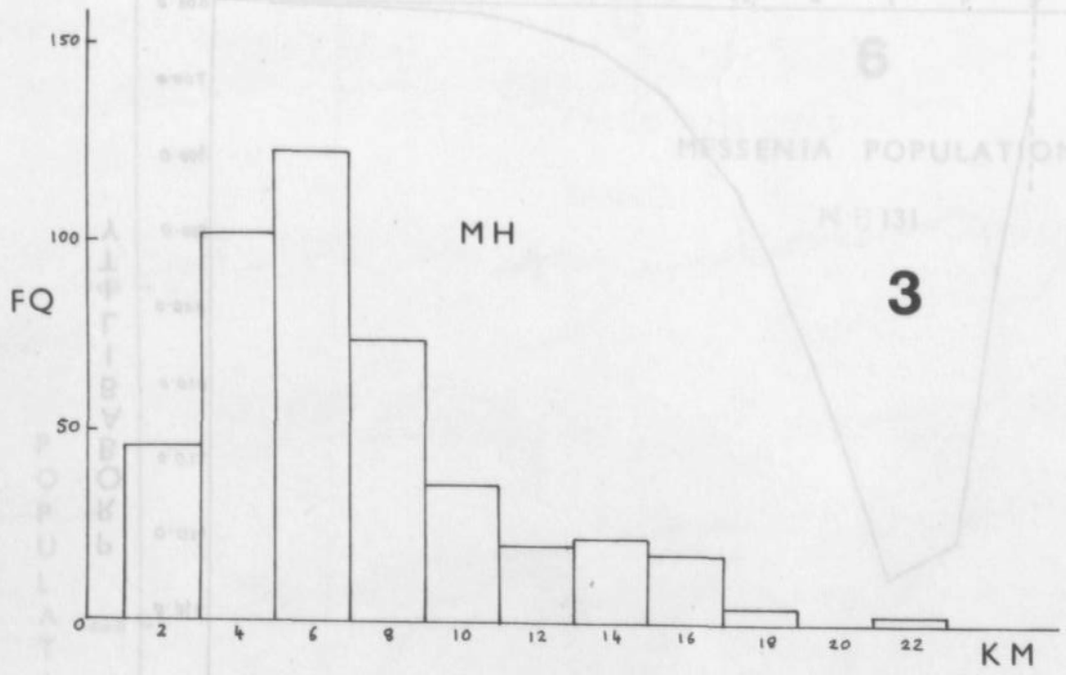
It has been demonstrated that settlement in Messenia appears to be a function of the potential of the environment, and the spacing out effect due to a need in an agricultural community for sufficient land to feed the population. Both these factors could have been hypothesized a priori, but simulation and the analysis required for it has helped to provide some justification for them. Ignorance may have been replicated, but it is at least possible to get some idea how ignorant one is. The models have also shown that settlement is better viewed in a probabilistic framework, rather than something as inflexible as central place theory. Simulation cannot provide all the answers, but it is a useful technique to have available.

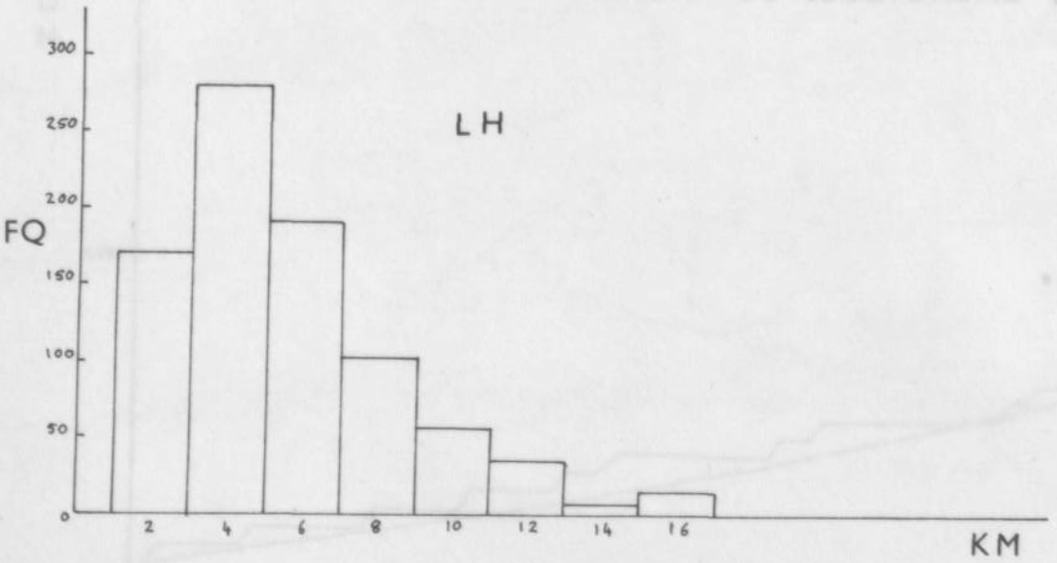
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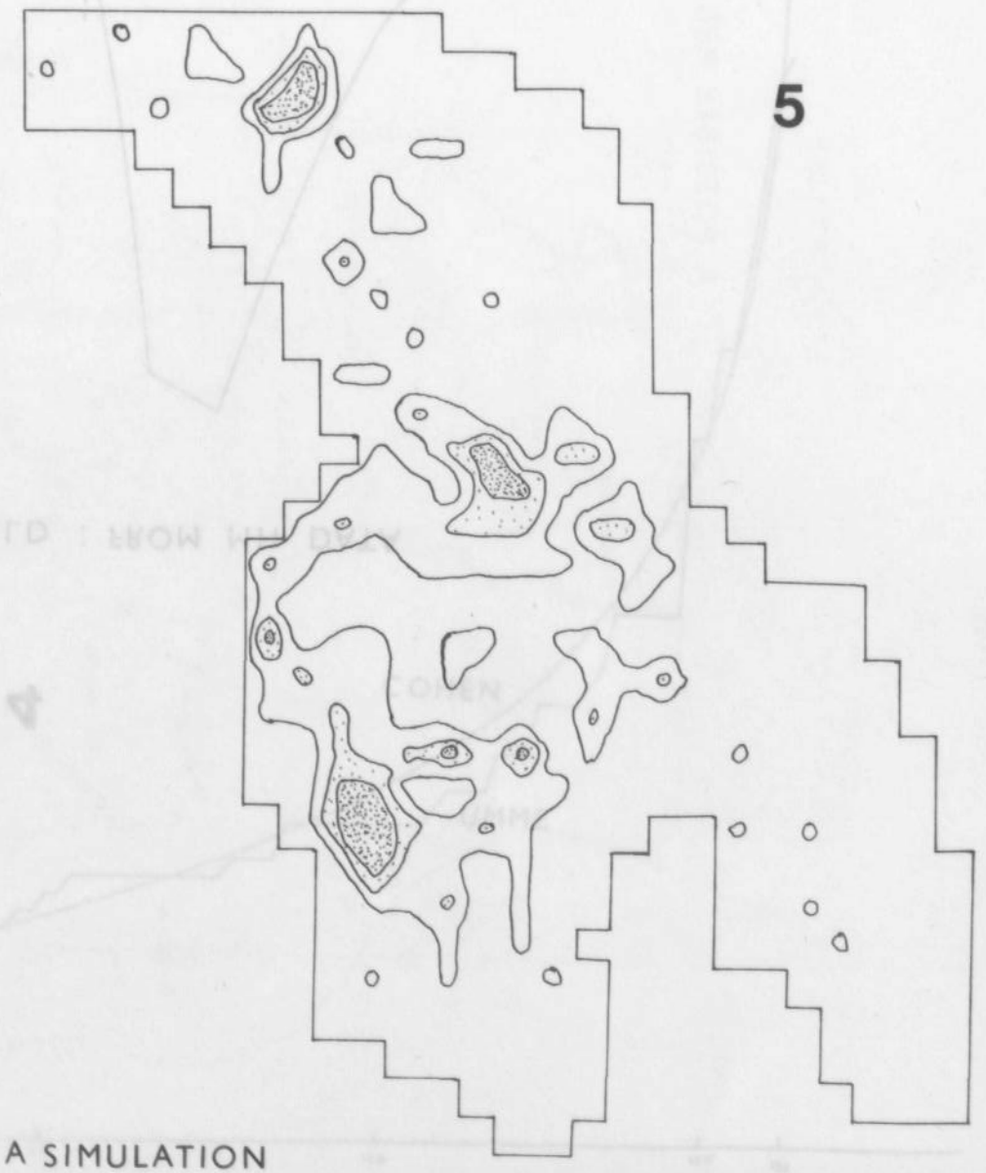
Bintliff, J. L., (1977). Natural Environment and Human Settlement in Prehistoric

2
LH
1
MH





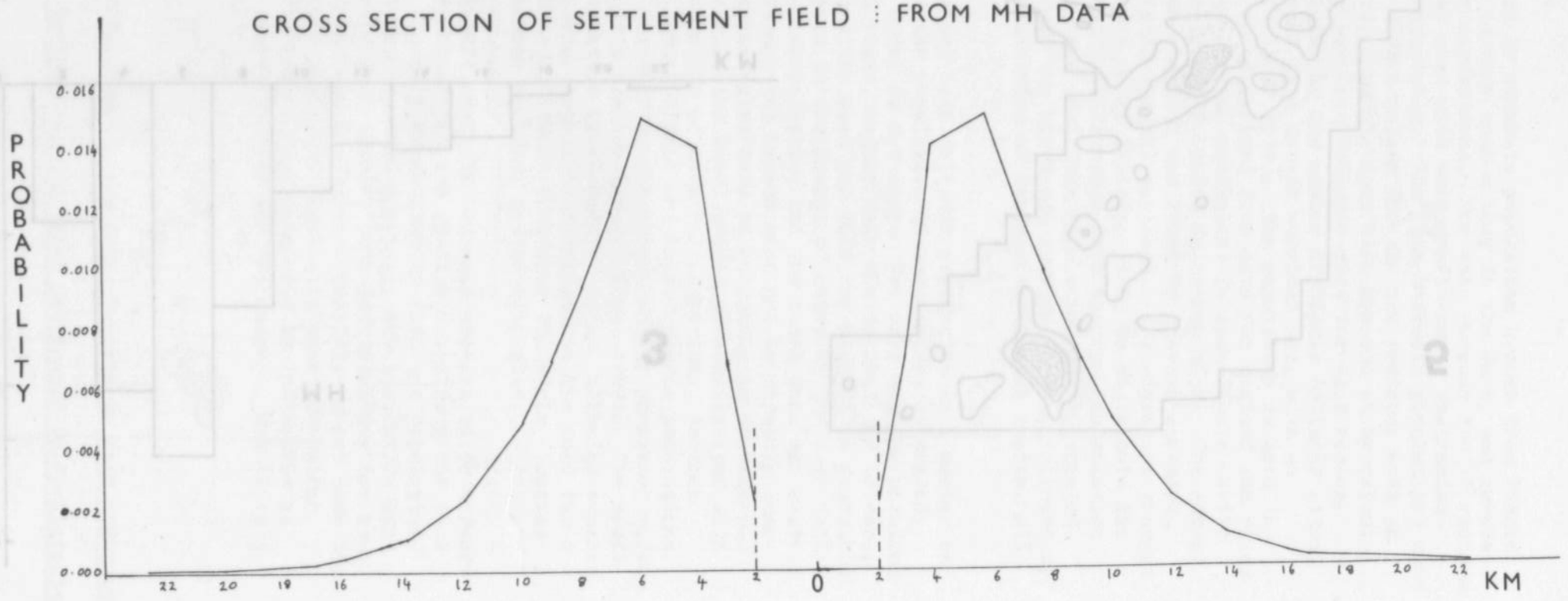
MESSENIA: ALL NEIGHBOURS



A SIMULATION

4

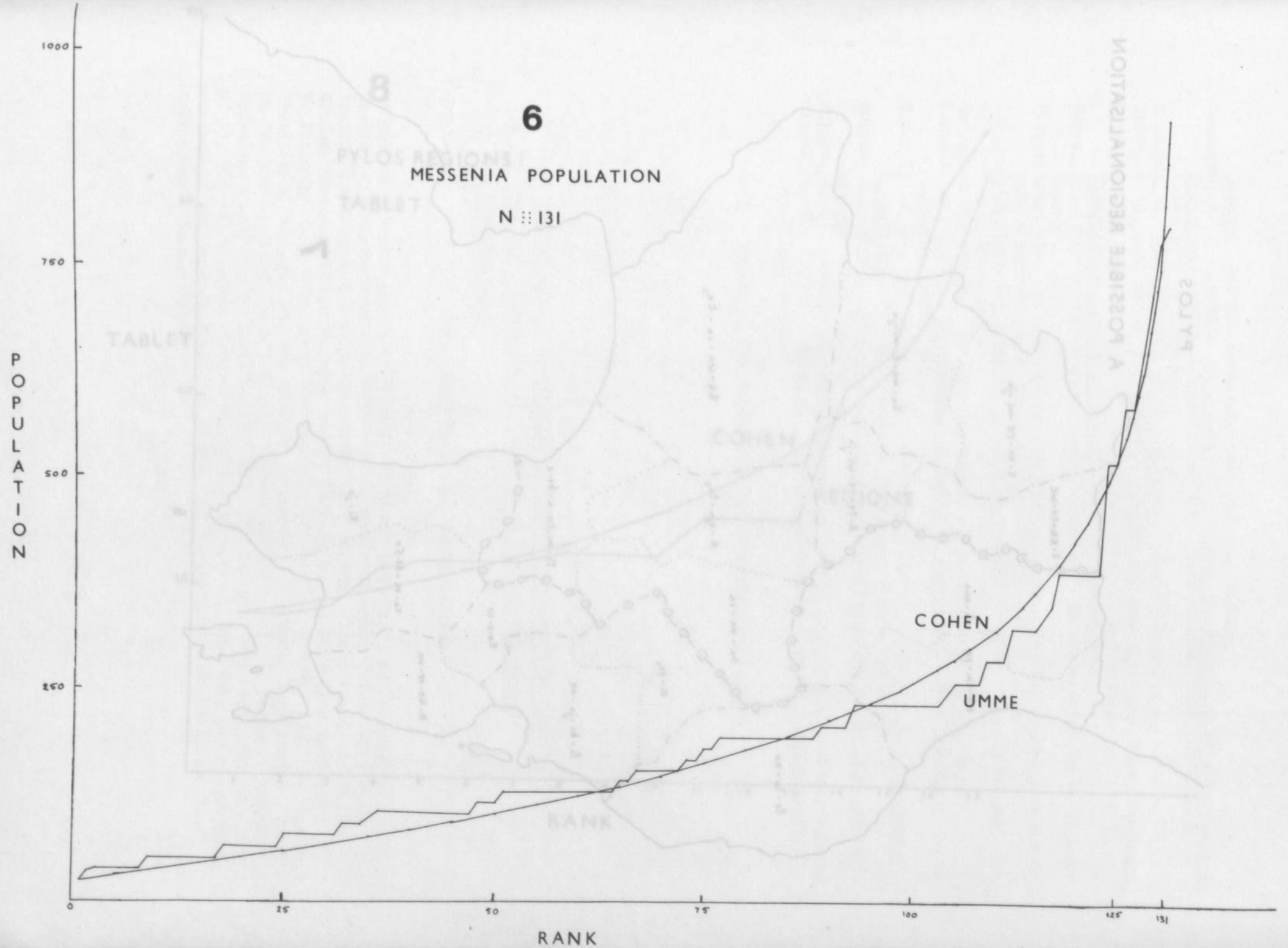
CROSS SECTION OF SETTLEMENT FIELD : FROM MH DATA

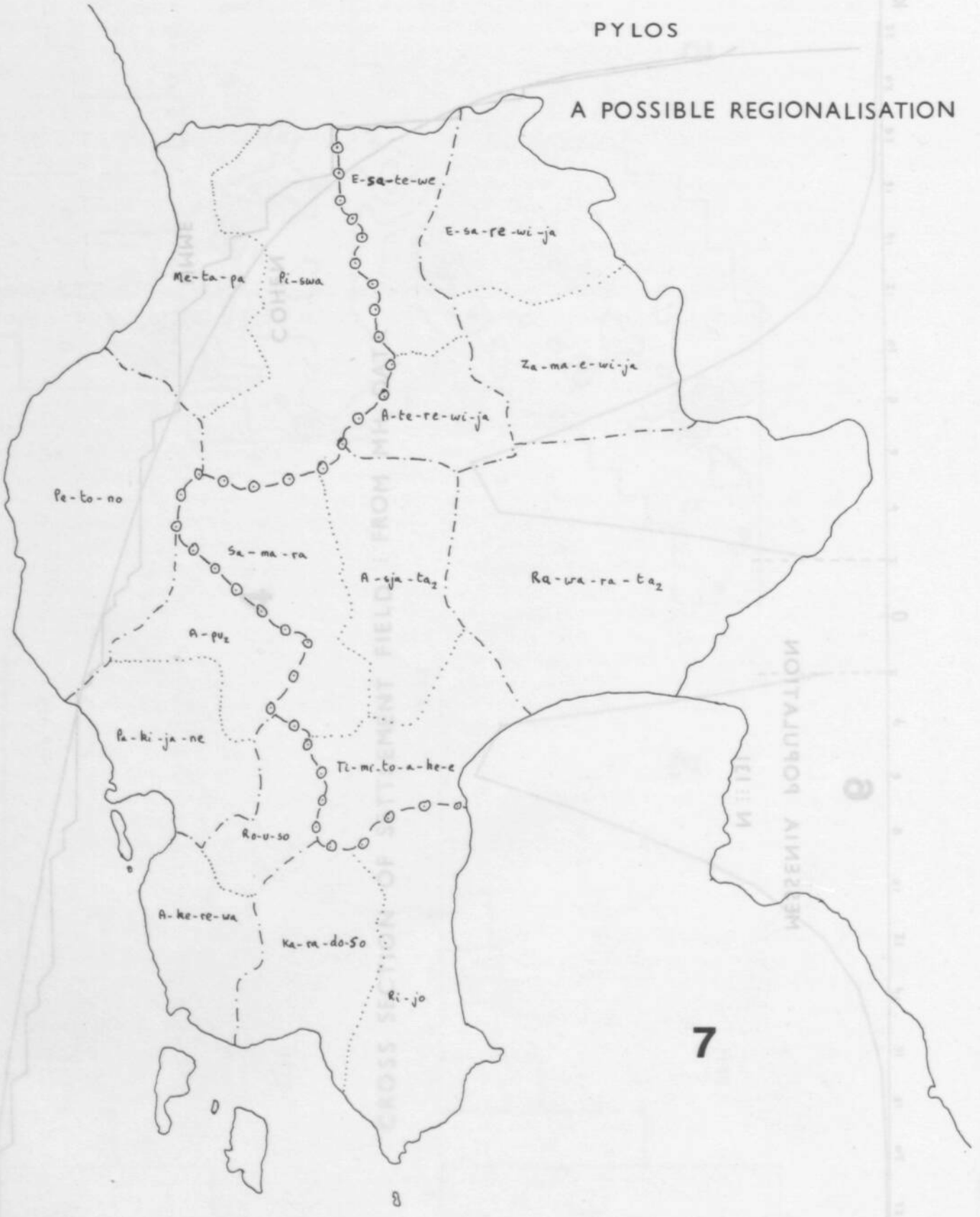


6

MESSENIA POPULATION

N = 131

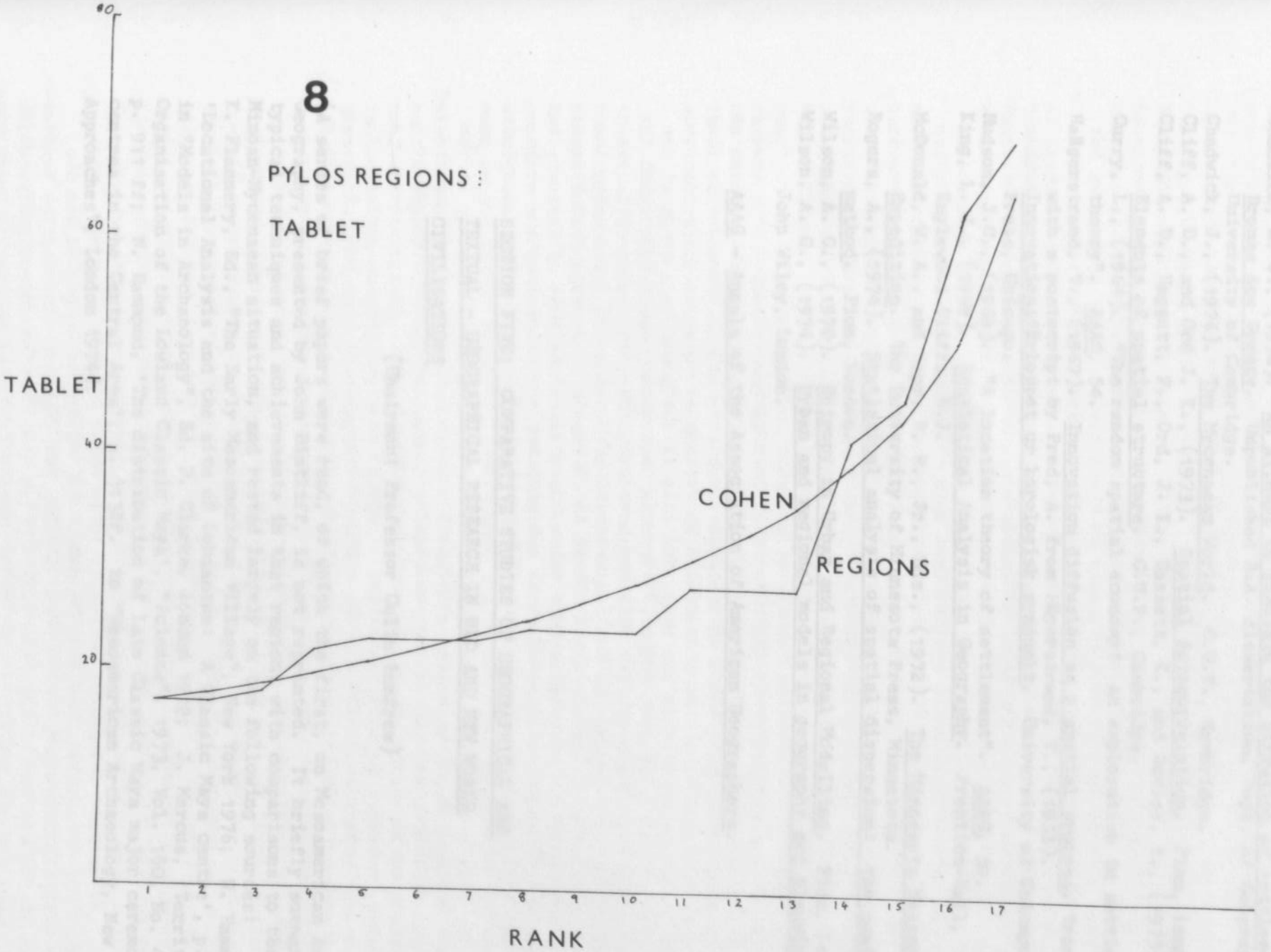



PYLOS
A POSSIBLE REGIONALISATION
E-sa-te-we
E-sa-re-wi-ja
Me-ta-pa
Pi-swa
Za-ma-e-wi-ja
A-te-re-wi-ja
Pe-to-no
Sa-ma-ra
A-sja-ta2
Ra-wa-ra-ta2
A-pu2
Pa-ki-ja-ne
Ti-mi-to-a-he-e
Ro-u-so
A-ke-re-wa
Ka-ra-do-so
Ri-jo
7

8

PYLOS REGIONS :

TABLET



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AAAG - Annals of the Association of American Geographers.

SECTION FIVE: COMPARATIVE STUDIES OF GEOGRAPHICAL AND TEXTUAL - GEOGRAPHICAL RESEARCH IN OLD AND NEW WORLD CIVILISATIONS

(Chairman: Professor Colin Renfrew)

(A series of brief papers were read, of which the first, on Mesoamerican Archaeo-Geography, presented by John Bintliff, is not reprinted. It briefly summarized typical techniques and achievements in that region, with comparisons to the Minoan-Mycenaean situation, and rested largely on the following sources: K. Flannery, Ed., "The Early Mesoamerican Village", New York 1976; N. Hammond, 'Locational Analysis and the site of Lubaantun: A Classic Maya centre', p.757ff. in "Models in Archaeology", Ed. D. Clarke, London 1972; J. Marcus, 'Territorial Organisation of the Lowland Classic Maya', "Science", 1973, Vol. 180, No. 4089, p. 911 ff; N. Hammond, 'The distribution of Late Classic Maya major ceremonial centres in the Central Area', p. 313ff. in "Mesoamerican Archaeology, New Approaches", London 1974).

EGYPTIAN GEOGRAPHY AND SETTLEMENT PATTERNS [1]

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Egypt is perhaps unique in the degree to which its geography has been determined by a single feature. The river Nile and its annual inundation have exercised a decisive influence not only on the distribution of population but also on the approach and capabilities of those seeking to establish ancient settlement patterns. It is inevitable, therefore, that the basis of such enquiry should be the documentation of the behaviour of the Nile - of fluctuations in its course, in the level of inundation, and in the extent of the floodplain. It now seems that the present degree of aridity in the desert on either side of the valley already existed by c. 2500 B.C. and the abandonment of desert margin settlements by that date emphasizes the future concentration of population within the floodplain. The specific significance of short and long term variations in the level of inundation has only recently been realised. A series of detailed analyses by Bell of the incompletely preserved records of flood levels has stressed the effects of such variations on the social, political, and administrative structure - effects which are most strikingly apparent in the First Intermediate Period.[2]

The former topography of the Nile valley can be reconstructed with some confidence for the Graeco-Roman period - in the section of the valley north of Sohag a general eastward shift in the Nile since then can be deduced from the fact that the remains of towns described by Ptolemy as being on the river now lie up to 5.5 kilometres to the west, and this is confirmed by a study of the area just north of Sohag by Butzer.[3] Even in the century and a half since the French cartographic survey, the Nile has moved significantly eastward in the region of Qau, destroying the Ptolemaic temple of Antaeopolis and causing the relocation of the village of Qau.[4] There is no reason to suppose that changes in the course of the river have only taken place since the time of Ptolemy, but the dating of more ancient meanderings can only be accomplished by the archaeological exploration of the abandoned levees which characterize the floodplain.

The Nile's influence on settlement can also be seen quite clearly in the Delta. Geological evidence suggests that the present coastline had been created by about 4000 B.C. and that subsequent marine transgression has been offset by periods of increased alluviation from high Nile floods. The settlement of the Delta, however, seems to have been a gradual process with two periods of accelerated expansion.[5] Old Kingdom settlement appears to have been clustered in the central Delta and it was not until the Ramesside period (c.1300-1100 B.C.), when a number of important towns are first attested, that the desert margins were substantially settled. The Mediterranean coastline seems to have been uninhabited until the Saite-Ptolemaic period, when a north-westerly movement of population is reflected in the founding of cities such as Naukratis and Alexandria. The Ramesside expansion was partly necessitated by the threat of invasion by the Libyans in the west and the Asiatics in the east, but was perhaps also a response to changes in the distributary system of the Nile in the Delta. A knowledge of the ancient courses of the numerous now-defunct branches is clearly a prerequisite for an understanding of changing settlement patterns there. Subsurface topography indicates three major original branches - the Rosetta, the Sebennytic, and the Damietta. By the Ramesside period the two outer distributaries had forked to create two new branches reaching the Mediterranean in the regions of the classical Canopus and Pelusium respectively, which may in part explain the fringe settlement in the Ramesside period. A re-examination of the literary evidence for this period by Bietak and the use of modern

topographic maps on which the levees which indicate former Nile courses are marked have enabled him to reconstruct the topography of the Eastern Delta at that time and to illustrate the excellent strategic position of Tell el Dab'a-Qantir, already known as the capital of the Hyksos in the seventeenth century B.C. which is now certainly to be identified with the site of the Ramesside capital Per-ramses.[6] Further changes are apparent from the descriptions of the classical geographers, but without archaeological excavation these changes cannot be dated or related more closely to settlement shift.

In considering questions of historical topography, the Egyptologist has at his disposal a rich textual record which is, however, heavily weighted in favour of Upper Egypt on the one hand, and the New Kingdom, and especially the Ramesside period, on the other, and an archaeological record rendered uneven by differential preservation and the interests of earlier archaeologists.

The evidence can be divided into two groups. Firstly, there are the monuments found in situ which enable us to locate the sites of ancient place-names with some precision. Secondly, there are a number of toponym lists which all, insofar as they are preserved, follow a south-north order.[7] These are sufficiently numerous to enable us to detect individual errors in sequence, and although most of the lists are Ramesside, the preservation of a single Late Middle Kingdom example and of a number of Ptolemaic lists does permit some conclusions to be drawn about changing settlement patterns. The result is that the sites of most of the important Middle Kingdom, New Kingdom, and Ptolemaic centres are known with certainty while many lesser centres can be located in their correct relative order and to within a few kilometres on a map, although we cannot always be sure on which bank of the river a particular town lay.

This knowledge is theoretical, however, and to some extent illusory. With the exception of Amarna, the city of Akhenaten, we have no idea of the size or extent of a single dynastic Egyptian town. The differential preservation which has determined the material available to the archaeologist has not been kind to settlements. Many habitation sites are buried beneath the silt deposit of centuries and are rendered inaccessible, where they still exist, by the high water table or continuing occupation of the site, so that most work has been done on the better preserved desert edge - i.e. the temples and cemeteries.

Even so, there are town mounds in Egypt - these have been pillaged in the hunt for papyri, they have been gradually removed by local peasants in search of fertilizer, they have been eroded by the Nile, they have been destroyed by the population explosion of the last one hundred years, but they have not been excavated. Urban archaeology is therefore in its infancy.[8] Most of the urban sites which have thus far been excavated are those which lie on the desert edge - short-lived, artificial foundations, atypical in situation and functionally specialised in character, e.g. fortresses in Nubia, and government villages for workmen and priests employed in mortuary cults in Egypt. The nearest we have to an "organic" urban site sufficiently extensively excavated to be susceptible of detailed analysis is Amarna, arbitrarily selected by Akhenaten and aberrantly situated. There is no real reason to suppose that its layout or its economic structure differed from that of any other large Egyptian centre of the New Kingdom, but its uniqueness should serve as a warning against generalising from it.

In the absence of data for the study of individual sites, some scholars have of late concerned themselves with the spatial distribution of population. In Upper Egypt, significant settlement has, at least in the dynastic era, and probably since the palaeolithic, been confined to the river valley. The narrowness of the floodplain (maximum modern width 27.5 kilometres)[9] has created a settlement pattern oriented on an approximately south-north axis. Within the

valley there is a contiguous good soil zone with equal water supply and communications at all points, the only variable being the width of the floodplain. This is reflected in the fact that the capital was constantly shifting, apparently according to the origin of the reigning dynasty and/or external factors and not geographic conditions. The available data on urbanisation is quite inadequate, however.[10] The multitude of excavated cemeteries cannot serve as a substitute for settlements, of whose extent, function, and population we are quite ignorant. A study of relative population size and spatial distribution along the valley, as reflected in the cemeteries, has been made for the Qau area.[11] Such a study is only feasible where an area, inevitably small, has been exhaustively excavated, and can only be of value in optimum conditions - it assumes a constant burial practice, a continuous, well-dated and complete or randomly depleted record. The toponym lists provide us with the names of many dynastic towns but it is clear from the Wilbour Papyrus,[12] which mentions 416 settlements of varying size within an area of 136 kilometres of Nile frontage, that the smallest hamlets are not mentioned, and that an analysis based on the toponym lists alone would reflect only the distribution of relatively more substantial population aggregates. The provincial capitals are among those listed but they are not differentiated from other towns that had no administrative function that we know of. We cannot use a rank-size model to determine their relative importance since we are quite ignorant of their extent. In a recent attempt to assess relative status, K. Butzer has selected a series of functional attributes, allotting a point score to each and classifying settlements, according to their cumulative totals, into four groups - large village, small centre, large centre, and city.[13] The results are inconclusive, for reasons which Butzer discusses, but seem to tally with what we know from the fragmentary textual sources.

The unique Wilbour Papyrus provides the opportunity for a detailed study of some aspects of the landholding pattern in one area in the Ramesside period. It deals with only a small percentage of the total cultivable area, but it might be worthwhile to explore the possibility of using a computer programme to determine the topography of the land-holdings, an operation which has been attempted by hand, so far unsuccessfully.[14] The data is incomplete but it is, at least in part, geographically arranged and can be related to known sites at a number of points.

I have tried briefly to indicate the ways in which the Nile has influenced population distribution in Egypt. The greatest desideratum is the dating of changes in its course. Before "modern geographic techniques" can profitably be applied to Egypt, more data must be collected and, above all, more settlement sites, such as Mendes, Armant, and Edfu, where the exposed stratification reveals a continuous occupation from the Old Kingdom to the Christian era, must be excavated.

NOTES:

1. This paper is based on the recent work of other scholars and contains nothing original. In particular, it owes much to Early Hydraulic Civilization in Egypt by Karl W. Butzer. (University of Chicago press, 1976).
2. B. Bell, "The oldest records of the Nile flood", Geographical Journal 136 part 4, Dec. 1970; "The dark ages in ancient history I : The first dark age in Egypt", AJA 75, 1971, 1-26; "Climate in the history of Egypt : the Middle Kingdom." AJA 79, 1975, 223-69.
3. Op. cit. p.34.
4. G. Brunton, Qau and Badari I, (1927), p.3 and pl.1.

5. K. Butzer, "Delta" article in Lexicon der Ägyptologie, ed. W. Helck and E. Otto, vol. 1, fascicle 7, 1043-52.

6. M. Bietak, Tell el Dab'a II : der Fundort im Rahmen einer archäologisch-geographischen Untersuchung über das östliche Nildelta. (Vienna 1975). See also A. Sneh and T. Weissbrod, "Nile Delta : the defunct Pelusiac branch identified", Science 180, 1973, p. 59-61.

7. A. Gardiner, Ancient Agyptian Onomastica, (Oxford 1947) p.40-63.

8. A collection of papers on this aspect appeared in Man, Settlement and Urbanism, ed. P. Ucko, R. Tringham and G. W. Dimbleby, London 1972. See in particular B. J. Kemp "Temple and town in ancient Egypt", p. 657-80, and D. B. O'Connor, "The geography of settlement in ancient Egypt", p. 681-98.

9. J. Wilson, "Buto and Hieraconpolis in the geography of Egypt", JNES 14, 1955, p. 209-36.

10. The notion of Egypt "as a civilisation without cities" put forward by J. Wilson in City Invincible ed. C. H. Kraeling and R. M. Adams, (Chicago 1960), 124-64, is based on a complete absence of evidence and is therefore unjustified.

11. D. B. O'Connor, "A regional population in Egypt to circa 600 B.C.", in B. Spooner (ed.) Population growth : anthropological implications (Cambridge, Mass., 1972) p. 78-100. See also O'Connor, "Political systems and archaeological data in Egypt; 2600-1780 B.C.", World Archaeology 6, 1974, p. 15-38.

12. A. Gardiner, The Wilbour Papyrus, (Oxford 1948). The information it contains on population distribution has been most recently analysed by O'Connor in the paper cited in n. 8.

13. Butzer, Early Hydraulic Civilization, p. 57-80.

14. O'Connor, op.cit., p.690.

GEOGRAPHY AND INDIAN ARCHAEOLOGY

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Cambridge University

In the brief remarks that follow I shall attempt a consideration of the extent to which archaeological work in the subcontinent has included analytical techniques that recognize the importance of modern geographical methods, and try to outline in very broad terms something of the relationship between Indian civilization and the physical geography of its environment.

Before continuing with this line of thought some idea of the general analytical character of archaeology in the subcontinent may be of interest at this point. For the most part, archaeology in this area is still at what may be described as a fact finding stage. The immense area involved coupled with problems of resources available for this kind of work have militated against the most thorough coverage of the archaeological potential of the place. A vast amount of work has been done, almost all of it with the intention of clarifying in chronological terms the sequence of cultures comprising the development of Indian civilization. To this extent, sites are excavated with a view to broadening the scope of knowledge of the variety of early cultures, relations among

these cultures, and their place in a continuous chronological sequence. With notable exceptions, even at the largest sites, excavations are of an invariably very limited area. In pre-Independence times, or pre-Wheeler times it should be said perhaps, excavations were often of much greater area but a generally less sophisticated approach to technique often cancelled any advantage accrued in that way.

Briefly stated, it may be said that the intention of archaeology in India up to roughly the present, has been and largely remains to establish a secure chronological foundation for the development of culture. This intention coupled with the usually limited area of most excavations has restricted considerably the introduction of analytical methods taking the meaning of data into non-traditional realms. It should be sufficient to state at this point that the kinds of techniques being discussed here at this colloquium are restricted in the Indian context largely to the work of pure geographers. In the Indian subcontinent, as until recently elsewhere, a truly interdisciplinary approach to the analysis of problems common to both archaeologists and certain geographers has not yet been achieved. The recent work of Bridget Allchin in collaboration with geographer Andrew Goudie and chemist K. T. M. Hegde in the Rajasthan desert, where they have been able to place a complete stone age sequence in the context of a stratified series of ancient sand dune formations, is a rare exception to the general lack of interest in this sort of co-operation.

Surveys and excavations by Suraj Bhan in Haryana State have suggested a connection between changes in the course of the Jumna River and the movement of people out of that area in the late Harappan period.

On the face of it, the Indian subcontinental environmental system does not appear to show a particularly complex or unusual relationship between settlements and the land. As elsewhere in the world sites are usually located in places favourable in terms of proximity to resources, water, agricultural land, natural raw materials, defensive potential and so on. The great civilizations of the Indus and Ganges drainage areas were both largely riverine in orientation and based their subsistence and growth on the fertility of the endless flat plains through which these rivers flow. As might easily be expected, rather more sites are located on these plains than on the slopes of the mountain ranges to the north. The detailed relationship of these sites one to another in economic or political terms has still to be worked out in detail. The relationship of individual sites to their local environment has yet to be explained in anything but the most general terms.

Evidence concerning the nature of settlements as a phenomenon is at best scanty in this context. Although a vast number of sites exist, in the absence of such techniques as aerial photography for archaeological purposes and a concerted programme of site mapping, the sample of sites available must be relatively small. Site distributions are, therefore, largely unknown on any large-scale detailed way. There has never been any concerted effort to map and publish an accurate site distribution survey from any single area with a view to drawing some conclusion concerning the nature of the pattern. Distribution maps that do exist are invariably of the most general kind suggesting no more than the most obvious relationships, that for example of the relation of major sites to rivers. The scale of these maps is usually of such large size that detailed conclusions are impossible. The relationship of sites to sites is understood entirely in cultural terms, with reference to artifact sequences and so on. The relative importance of one site to another is discernible by comparison of site size only.

Large urban centres are spaced more or less regularly throughout the Indus and Ganges valleys and beyond. They represent centres of political, economic,

and ceremonial importance, none of these functions being necessarily more important than any other. These sites are largely restricted to the great plains of the Indo-Gangetic region by reason of the easy availability of fertile farmland and rivers for irrigation, trade, and transport.

As far as can be detected at the present, the spacing of archaeological sites in limited local areas does not correspond to any formal pattern. During the early Gangetic iron age some clustering of sites in some areas is apparent, notably in those more obviously thoroughly explored. This grouping is no regular thing and exhibits no particular pattern. The limited areas we are speaking of here are very little less than whole districts in size. Fieldwork designed specifically to tackle these problems is a prerequisite to the continuation of study in this area.

Centres of population in ancient India vary in size, as may be expected, from small villages to large urban sites. During no archaeological period is the relation of these sites to one another understood in precise terms. By comparison with the modern situation and other parts of the world, it may be inferred that sites are linked to each other by a variety of ties and that the relationship of village to town in ancient India was probably complex rather than founded simply on one factor, the ceremonial importance of a centre for example. In the Indian context, the home of Buddhism and Hinduism, the religious importance of a site becomes traditionally part and parcel of the political and economic significance of the centre. At the modern city of Allahabad yearly bathing festivals can attract several million pilgrims, all gathered at the confluence of the Ganges and Jumna Rivers close to the ancient site of Jhusi. The impact locally of such a gathering can easily be imagined.

However important the ceremonial aspect of any given centre may be, the city functions invariably as an administrative and trading place as well. The rigid standardization characteristic of the Indus Civilization of the second and third millennia B.C. suggests a heavily political and administrative bias in functional terms for their large centres, Harappa, Mohenjo-daro, and Kalibangan although not at the expense of the ceremonial importance of these sites, the precise nature of which is not yet properly understood.

By the middle of the first millennium B.C., the Gangetic Civilization is characterized by the presence of a series of large cities serving a growing population both urban and rural in the ways characteristic of any large urban centre. Again, the precise relation of these cities to smaller sites is not understood. No survey work with this end in view has ever been completed. Concentration of interest on the internal structure of the large sites with a consequent restricted sample of small sites make any distribution map devised from present evidence next to useless for these detailed purposes.

These large sites of the north Indian iron age are scattered throughout the great plain of that region. For the most part they figure in ancient literature as political capitals of the kingdoms and republics of the mid-first millennium B.C., many being associated with events in the lives of such religious leaders as the Buddha and Mahavira. They are the direct descendents of the villages characterizing the early Gangetic iron age of the first few centuries of the first millennium B.C. and do not find obviously their origins in any single factor.

Geographical data derived from ancient Indian texts is concerned almost exclusively with physical features in the ancient landscape. Much work has been done in the area of ancient names of rivers, mountains, geographical and political subdivisions, and cities. The identification of sites in historical terms has been based upon this kind of study. Ancient names have been attached to

known sites and new sites discovered based upon textual evidence. These texts, both Brahmanical and Buddhist, provide no definitely usable information about settlement patterns, however. In some later texts, notably those of the Chinese pilgrims Hiuen Tsang and Fa Hien, distances between various major Buddhist centres are given. The units of measure in these cases are vague and variable and are broadly useless for serious geographical purposes. Data concerning ancient natural environments is likewise very thin. There is no tradition of written history or record of natural conditions in India much before the Muslim period, as texts are almost entirely of a narrative or philosophical character.

In the works of Pāṇini and Kauṭilya considerable detail is used in description of ancient polity as well as the explanation of the words used to refer to various types of settlements in functional terms. Some settlements are said to be the centre of 800 villages, some of 400 villages, and so on down to about 10. These large numbers are almost certainly an exaggeration. It is sufficient only to notice that a definite hierarchy of settlements is recognized in the literature of the late first millennium B.C. although none of this data has ever been attested to archaeologically. This latter problem has been overcome only from the point of view of place name studies when inscriptions have supported textual data, or in the identification of sites with events in the lives of the great religious leaders in like fashion. Occasionally, the comparison of the physical geography of some sites with traditional or textual accounts has made possible precise identification of the site, Rājgir for example.

Some texts such as the Śilpa Śāstras deal in part with settlement planning from an internal point of view but are only vague in stating reasons for site positioning and do not deal with relative situations. The comparative lateness of these texts (early centuries A.D.), coupled with the fact that they seem to have been consulted largely only in the construction of temples, places them outside the present interest. From such texts as the Buddhists Jātakas snippets of information can sometimes be found, none of it, however, tieing in with any archaeological data. Villages of artisans of specific types are described as being situated outside the gates of a major city. These are largely idealized descriptions and include no usable data.

Historical geography in the Indian subcontinent goes its own way, largely ignoring archaeological data in the same way that archaeologists ignore much of what is happening in geography. Plenty of work is done by geographers using modern theory and technique, never, however, in the archaeological context. Geographers interested in the development of settlement patterns seem impatient of archaeology and set about their own research into modern conditions, largely ignoring the past. It is more than just arguable that conclusions drawn in this way concerning the nature of pre- and proto-historic settlement patterns and ancient environments in the absence of archaeological data must be seriously affected. Until some reconciliation of these two fundamentally important methodologies is achieved there can be no progress in this field in the context described here.

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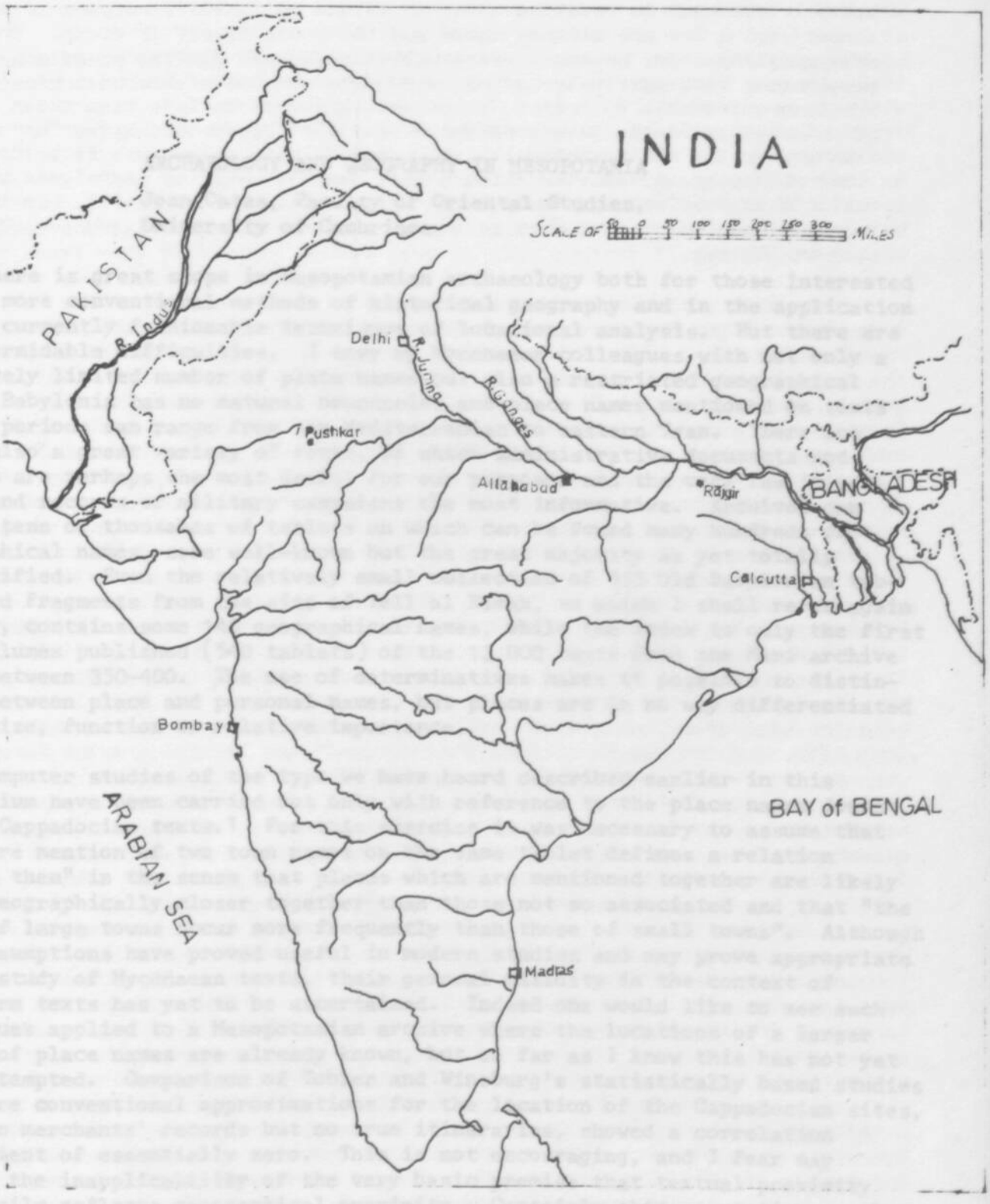
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ARCHAEOLOGY AND GEOGRAPHY IN MESOPOTAMIA

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There is great scope in Mesopotamian archaeology both for those interested in the more conventional methods of historical geography and in the application of the currently fashionable techniques of locational analysis. But there are also formidable difficulties. I envy my Mycenaean colleagues with not only a relatively limited number of place names but also a restricted geographical area. Babylonia has no natural boundaries and place names mentioned on texts of all periods can range from the Mediterranean to eastern Iran. There are found also a great variety of texts, of which administrative documents and letters are perhaps the most useful for our purposes and the very few itineraries and records of military campaigns the most informative. Archives can number tens of thousands of tablets on which can be found many hundreds of geographical names, some well-known but the great majority as yet totally unidentified. Even the relatively small collection of 335 Old Babylonian tablets and fragments from the site of Tell al Rimah, to which I shall refer again shortly, contains some 140 geographical names, while the index to only the first five volumes published (540 tablets) of the 13,000 texts from the Mari archive lists between 350-400. The use of determinatives makes it possible to distinguish between place and personal names, but places are in no way differentiated as to size, function or relative importance.

Computer studies of the type we have heard described earlier in this colloquium have been carried out only with reference to the place names found on the Cappadocian texts.[1] For this exercise it was necessary to assume that "the mere mention of two town names on the same tablet defines a relation between them" in the sense that places which are mentioned together are likely to be geographically closer together than those not so associated and that "the names of large towns occur more frequently than those of small towns". Although such assumptions have proved useful in modern studies and may prove appropriate to the study of Mycenaean texts, their general validity in the context of cuneiform texts has yet to be ascertained. Indeed one would like to see such techniques applied to a Mesopotamian archive where the locations of a larger number of place names are already known, but so far as I know this has not yet been attempted. Comparison of Tobler and Wineburg's statistically based studies with more conventional approximations for the location of the Cappadocian sites, based on merchants' records but no true itineraries, showed a correlation coefficient of essentially zero. This is not encouraging, and I fear may reflect the inapplicability of the very basic premise that textual proximity necessarily reflects geographical proximity. Certainly this assumption can be shown to be invalid with respect to place names on many Mesopotamian texts where geographical data are more informative. Indeed on such texts the association of names would appear to reflect no more logical sequence than the totally random order in which the recording scribe picked up various relevant documents. Perhaps the best illustration of this point is a Mari letter, once assumed to

have been an itinerary, but which would have represented a remarkably circuitous route from Babylon to Mari, had this been the scribe's intention.[2] The letter in fact records a list of messengers who have arrived in Mari from various parts of the Near East and the lack of any logical geographical order is readily apparent. Itineraries do exist but they are rare. They provide invaluable guidance in locating sites to within quite small margins of error, at least when a few are already known and the route itself is clear. Even such highly specific data, however, are usually inadequate for the precise association of names with particular sites, which must be one of the historian's ultimate requirements.[3] Indeed Tobler and Wineburg conclude that their average error of roughly 50 km. in attempting to map the Cappadocian sites "may seem too large for the field archaeologist, especially in an area as rich in mounds as central Turkey."[4] It seems likely that computer mapping techniques have specific data requirements that Mesopotamian texts may not fulfil, but it would be very interesting to see them tested on a better-documented archive than that from Kültepe.

Yet another problem encountered in Mesopotamia arises from the fact that even when written documents have been recovered from a site, its name may still remain uncertain. Perhaps the classic example is provided by the dispute over the identification of Chagar Bazar in northeastern Syria, originally thought to have been a town called Til-Ša-annim but now believed by many scholars to be Šubat-Enlil, a major administrative centre of the kingdom of Šamši-Adad.[5] An even more striking conflict of evidence can be found at the site of Tell al Rimah, excavated by the British School of Archaeology in Iraq in the 1960's.[6] In the first season a number of Middle Assyrian administrative and economic texts were discovered, on which the most frequently mentioned local name was Qatara. One such document concluded with the words witnessed "before the king in Qatara" and the gentilic "of Qatara" identifies some of the major participants in the transactions recorded. This evidence led some to suppose that Rimah was in fact Qatara.[7] In the fourth season a number of Old Babylonian tablets were found, including an archive belonging to a female official, Iltani. Iltani was clearly a woman of some distinction and importance. Not only was she the daughter of a king of Karana, a city known from the Mari letters to have been the seat of kingship of a small state of which Qatara was a nearby dependent city, but she was married to the "diviner" who led the army of Karana and who appears to have succeeded - or deposed - her brother as ruler of that city. The possibility that Rimah was Karana had now to be considered, a possibility heightened by the fact that the excavations had revealed there an impressive and clearly important monumental temple and a large palace or administrative complex. The evidence from the archive itself, however, was equivocal. A letter to Iltani says helpfully, "You used to live in Ešnunna.... now you are living in Karana", and another asks her to send out the people of Karana to help with the harvest. This seems convincing if circumstantial evidence of identification, yet one letter from her husband gives pause for thought: "Speak to Iltani, thus says Aqba-hammu. I shall travel to Karana tonight. Tomorrow I shall leave Karana... You are to go to your house with the tablets."[8] Not only would it appear that Iltani and her house were not in Karana, but that she carried her tablets with her - a distinctly disquieting revelation from the viewpoint of historical geography. The moral of this tale should be obvious, that in Mesopotamia even tablets found on a site do not necessarily provide certain evidence for its identification. Nor can one assume that the name which occurs most frequently is likely to be relevant; indeed on economic texts the name of the site of origin rarely appears. Unequivocal evidence for site identification will be found in monumental inscriptions, and on inscribed bricks or foundation deposits discovered <u>in situ</u>, yet even here Rimah provides problematic evidence. In the fifth season a foundation plaque was discovered apparently <u>in situ</u> below a wall in the palace area; this recorded the construction of a palace in the city of Razama. Now the one fact that is clear from the rest of the epigraphic

evidence is that Rimah was not Razama, a rival kingdom whose ruler does not figure in the Rimah archives. Yet had this last piece of evidence been found alone, we would unhesitatingly have identified Rimah as Razama. Clearly some souvenir-hunter, superstitious soldier or fifth column agent had brought the tablet back from the siege of Razama which is well-attested in the Mari letters, and re-interred it at Rimah.

I have been dwelling on the difficulties inherent in the texts themselves. On a more constructive note, one might mention the possibility of identifying unknown and unexcavated sites using techniques of neutron activation analysis in order to compare the clay of tablets which have originated from a known but unidentified site and clay samples from specific sites in the possible area of its location. This is already being attempted in the continuing search for the Mitanni capital, Waššukkanni, on the assumption that the letters of its king Tušratta originated there. Such an application of neutron activation analysis seems very promising, though even in this context Iltani's peripatetic habits give some cause for concern.

To turn to matters of field research, archaeologists working in Iraq are making increasing use of geographical models in studying problems of spatial relationships, both within and between individual sites. But it must be emphasized also in this context that the Mesopotamian data by their very nature place many constraints on the application of such models, and that, however sophisticated the technique, the results obtained by its application can never exceed the quality of the data employed, a fact we would be very foolish to forget. It would be equally foolish to pretend that Mesopotamian survey data are adequate for the purposes to which they are being put by some of our more enthusiastic colleagues. You will say that the fault lies in the field work, but this is not entirely fair in an area where relatively little survey or excavation has been carried out, and where in recent years the ideal combination of sounding and survey has not been permitted under the existing antiquities law. Our archaeological ignorance in an area that is superficially so well known may surprise you, but there are many periods in Mesopotamian history and prehistory for which no reliable ceramic sequence exists, and it would be a rash archaeologist indeed who would claim to be able to date even well-known ceramic types to within a period of 500 years or to recognize any significant percentage of sherds from long-occupied sites. You may notice, for example, that survey reports fail effectively to identify sites in the south of the mid-second millennium or, throughout the country, of the early first millennium B.C. The reason is simple: we have no clue whatsoever to the differentiation of pottery of these periods from earlier or later phases, with which for the present such ill-defined periods must inevitably be coalesced. An examination of existing reports will demonstrate that attributions of date are made for the most part on the basis of a small selection of "type fossils" but even for these, dating criteria are often far from satisfactory. Although gross trends in settlement variation are usually clear, survey data in general lack the precision essential to the refined application of, for example, central place theory.

Probability sampling has not yet been attempted on any Mesopotamian survey.[9] For the most part this is not the result of ignorance or backwardness on the part of the archaeologists concerned, but of deliberate decisions, taken by archaeologists working in uncharted areas in which there are literally hundreds of unsurveyed mounds, to forego systematic coverage for knowledge of the larger historical and geographical setting. A number of excellent surveys have been carried out in recent years, particularly by Adams, but the lack of good excavated sequences for comparative purposes, the difficulty of obtaining such essential aids as air photographs, the impossibility of obtaining sounding permits, and so on, has meant that work of this sort cannot, at least for the moment, proceed in any ideal fashion. The evidence available at present is

without question of enormous value, but cannot be manipulated in ways which require data of which the statistical validity can be ascertained. These are, of course, solvable problems, though whether money would ever be available on the necessary scale is highly doubtful.

But there are other difficulties inherent in Mesopotamian settlement pattern data that are seldom understood except by those well-acquainted with the area. Techniques of locational analysis require quite specific knowledge of site size for which accurate estimates are difficult if not in most cases impossible, even on settlements of relatively short duration. The reasons for this are clear to anyone who has observed modern patterns of settlement. Mud-brick houses have a relatively short life span. Rarely is the whole of a village site occupied at the same time. When an individual house begins to decay beyond repair, it can be levelled and rebuilt in situ or a new house can be built elsewhere in the village. There is, in our experience, no lack of space in modern villages that might attach a high site value to the location of a particular house or extended family compound. On some prehistoric sites, e.g. Choga Mami, there is evidence for the reconstruction of some buildings on or within the outline of their predecessors, but we cannot know whether the reasons were economic - narrowly defined as ownership of the site - or social, some unguessable tradition that required the maintenance of an established location. But even on town or city sites of the historical period we commonly find large, apparently unoccupied areas, which may have been used to house the people of the countryside and their flocks in times of danger, a practice attested by the Mari texts.

Thus in archaeological terms overall distribution of settlement debris is a questionable criterion of site size, and an even more doubtful indicator of population. This difficulty is confounded by what I have called the problem of "walking potsherds". Surface sherds may originate from rubbish removed from a structure that is being levelled for rebuilding and thrown elsewhere, commonly over the edge of a growing mound. On the other hand they may originate in levelling fill brought in from elsewhere, possibly even from deposits of radically different date. Undoubtedly the most striking example of this phenomenon comes from Ur where a large number of the early fifth millennium B.C. 'Ubaid sherds published by Woolley come not from a village of that date but from a secondary deposit of levelling fill brought in for the rebuilding of the temenos area some 4000 years later. We know neither the size nor the precise location of the original village. Major sites like Ur, occupied for long periods of time, create special problems since evidence of settlement in the earliest periods tends to be obscured, at least to any informative degree, by later occupation. At Nineveh, for example, surface survey yields no information whatsoever about such important periods of settlement as Ninevite V or Agade, or indeed any of the prehistoric periods we know to have been represented there. It is probable that Nineveh was always the major "central place" in the upper Tigris valley, yet the data to support such an assumption are simply not obtainable, by probability sampling or any other survey technique.

The most serious problem of all, however, and this is not one that is unique to Mesopotamia, is that of contemporaneity. Dating criteria are not now, nor in the foreseeable future are they likely to be, sufficiently precise to distinguish between a group of small sites occupied at one and the same time or occupied in succession by essentially the same group of people. Anyone acquainted with the modern scene knows that villages tend to move about the countryside, often returning after some years to a site occupied at some earlier time. Since we can rarely date archaeological materials to within a few hundred years, the detection of such shifting patterns is completely beyond our present competence. I need not labour these difficulties, but here is a crucial and indeterminate variable which must inevitably affect the applicability of locational theory in

archaeology.

The most recent, and undoubtedly the most carefully considered, application of locational theory in Mesopotamian studies is that of Johnson, using Late Uruk data from the Warka survey carried out by Adams and Nissen.[10] Adams himself made extensive use of geographical models in his own analysis of the survey materials, but observed that "in the Late Uruk period, save for Uruk itself, all sites whose size can be determined (ital. mine) seem to fall within a unimodal distribution... a regular hexagonal pattern is not readily apparent in the available data".[11] With the single exception of Uruk itself Adams estimated all sites of this phase to have been less than 15 hectares in area, though he noted that the heavy overburden of later occupation debris at certain major sites, especially on the eastern side of the area surveyed, may have tended to minimize the apparent areal extent of earlier remains. Johnson, using the same field data, has produced significantly different results indicating a four-level site hierarchy. His article provides a fascinating demonstration of the application of locational theory, but one cannot help but notice certain discrepancies between his assumptions and the original evidence, at least as presented in the survey report. Estimates of size for four first order sites (large centres) significantly exceed Adams' 15 ha. limit, including site 230, recorded by Johnson as a Late Uruk settlement of 42 ha. but described in the original publication as having only "rare" evidence for Late Uruk occupation,[12] while no allowance is made for the possibility of significant Late Uruk settlements at such major sites as Fara (Shuruppak), Umma and Larsa, which were deliberately omitted from the survey. (Fara lies only 2 km. north of the "small centre" 020, while Umma is situated approximately midway between sites 168 and 198, Fig. 2; both are very likely to have been important Late Uruk sites.) Moreover, in "testing" the validity of the very interesting hypothesis that so-called "small cone sites" were specialised administrative centres and functioned as "nodes on transport lattices", Johnson includes the decorative cones, by which this category of site was specifically identified, as one factor in a test for artefact diversity as an indication of "trading volume". Such a procedure is statistically highly irregular and indeed gives falsely positive results in differentiating these sites from other Late Uruk villages. A significant correlation between wall cones and spindle whorls is established at these sites, however, but Johnson's preoccupation with "local exchange systems" and Christaller's "Marketing Principle" leads him rather blindly to the unfortunately ludicrous conclusion that "spindle whorls would seem to have been the item most consistently involved in the Late Uruk exchange network."[13] Common sense suggests many alternative explanations for these data, for example the possibility that these small sites around Warka provide the first evidence for the association of a manufacturing activity (spinning and weaving) with temple institutions. It is also instructive to compare Johnson's settlement system (Fig. 2) with that of Adams and Nissen (Fig. 1). Adams in commenting on his failure to find a "uniform superimposed grid of hexagons" remarks, "Lines of communication, rather than following the shortest, most economical path presupposed by the theory, must have been subject to severe warping or distortion by the numerous channels of the Euphrates." It will be obvious, in fact, to anyone who has travelled in southern Iraq that the routes between settlements in irrigated country usually follow the raised banks of canals. Adams also observes that "population, rather than being uniformly distributed over a land surface of constant agricultural potentiality, was concentrated in enclaves interspersed with swamps or arid steppes."[14] Johnson indeed notes that access to "differentially distributed water" was an important determinant in settlement location, but believes his hexagonal settlement lattice to be totally compatible with the observed linear distribution.[15] Perhaps the most striking feature of Fig. 2, however, is its failure to reproduce in any way the obvious linear patterning of Fig. 1.

Two further assumptions of most locational theory should be questioned in

the light of Mesopotamian evidence. One, that all central places develop in similar ways and perform similar functions, and second, that relative functional size is a valid basis from which to determine settlement hierarchies, i.e. that there is a close relationship between areal size and the number of "service functions". Assyrian capital cities, from which documentary evidence is available, provide a nice example of the idiosyncratic and unpredictable factors that can affect the history of urban centres. The situation of Assur, for example, on a relatively ill-favoured site at the edge of arid steppe, would be very difficult to explain in the absence of such written information.[16] The early second millennium B.C. site of Tell Harmal, on the outskirts of modern Baghdad, with a maximum area of 1.7 ha., that is, smaller than the early Hassuna village of Yarim Tepe, would be recorded in any survey as a small farming village. Yet excavation has revealed it as the administrative centre of a part of the kingdom of Ešnunna[17] and clearly more important with respect to "service functions" than (excavated) contemporary towns nearby of much greater size. Even more striking is the "village" site of Drehem near Nippur, which in the late third millennium B.C. was an administrative centre without parallel in the ancient world. Thousands of tablets from the site, through which passed all the livestock destined for the main shrines of Sumer, record an extraordinarily highly organised bureaucracy, centred on this small settlement. The existence of Tell Harmal and Drehem alone must raise awkward questions about the general applicability of the concept of "functional size". This is certainly not to question the general usefulness of locational theory as a mechanism for generating testable hypotheses, and indeed as a valuable framework within which to order settlement data. I do wish, however, to argue for a realistic assessment of the results obtained, especially in view of the fact that their graphic presentation tends to lend them a spurious sense of precision and authority.

FOOTNOTES:

1. W. Tobler and S. Wineburg, "A Cappadocian Speculation", Nature 231, 7 May 1971, pp. 39-41. I am very much indebted to Dr. Ian Hodder for this reference.
2. ARMT vi, 23.
3. A. Goetze, "An Old Babylonian Itinerary", JCS 7, 1953; "Remarks on the Old Babylonian Itinerary", JCS 18, 1964; D. Oates, Studies in the Ancient History of Northern Iraq, 1968, p.35, n.3.
4. Tobler and Wineburg, 1971, p.41.
5. C. J. Gadd, Iraq 7, 1941, p. 25; Goetze, JCS 7, 1953, p. 58; D. Oates, 1968, p. 37, n.2; Hrouda, MDOG 90, pp. 29-33.
6. D. Oates, "Excavations at Tell al Rimah", Iraq 27, 1965, onwards.
7. H. W. F. Saggs, "The Tell al Rimah Tablets", Iraq 30, 1968, p. 156.
8. S. Dalley, C. B. F. Walker and J. D. Hawkins, The Old Babylonian Tablets from Tell al Rimah, 1976.
9. Some experimentation with sampling techniques has been carried out on surveys in eastern Turkey, cf. inter alia C. L. Redman and P. J. Watson, American Antiquity 35, 1970; C. L. Redman, Archaeological Sampling Strategies, 1974.
10. R. McC. Adams and H. J. Nissen, The Uruk Countryside, 1972; G. A. Johnson, "Locational Analysis and the Investigation of Uruk local Exchange Systems", in J. A. Sabloff and C. C. Lamberg-Karlovsky, (eds.) Ancient Civilization and Trade, 1975.
11. Adams and Nissen, 1972, pp. 17-19.
12. Ibid., p. 228. Johnson's 8 second order sites (small centres) include site 460 where Late Uruk material is "limited to the north-east end of the site" (Adams and Nissen, p. 238) and site 387 where evidence for Late Uruk occupation is very minimal (Adams and Nissen, pp. 88-89); the possibly very

large site 198, on the other hand, is reduced by Johnson to an "arbitrary 5 ha." (p. 311).

13. G. A. Johnson, 1975, p. 331.
14. Adams and Nissen, 1972, p. 19.
15. Johnson, 1975, p. 324.
16. D. Oates, "The development of Assyrian towns and cities", in P. J. Ucko, R. Tringham and G. W. Dimbleby, eds., Man, Settlement and Urbanism, 1972, pp. 800-02.
17. T. Baqir, Tell Harmal, Baghdad, 1959; Sumer 2, 1946.

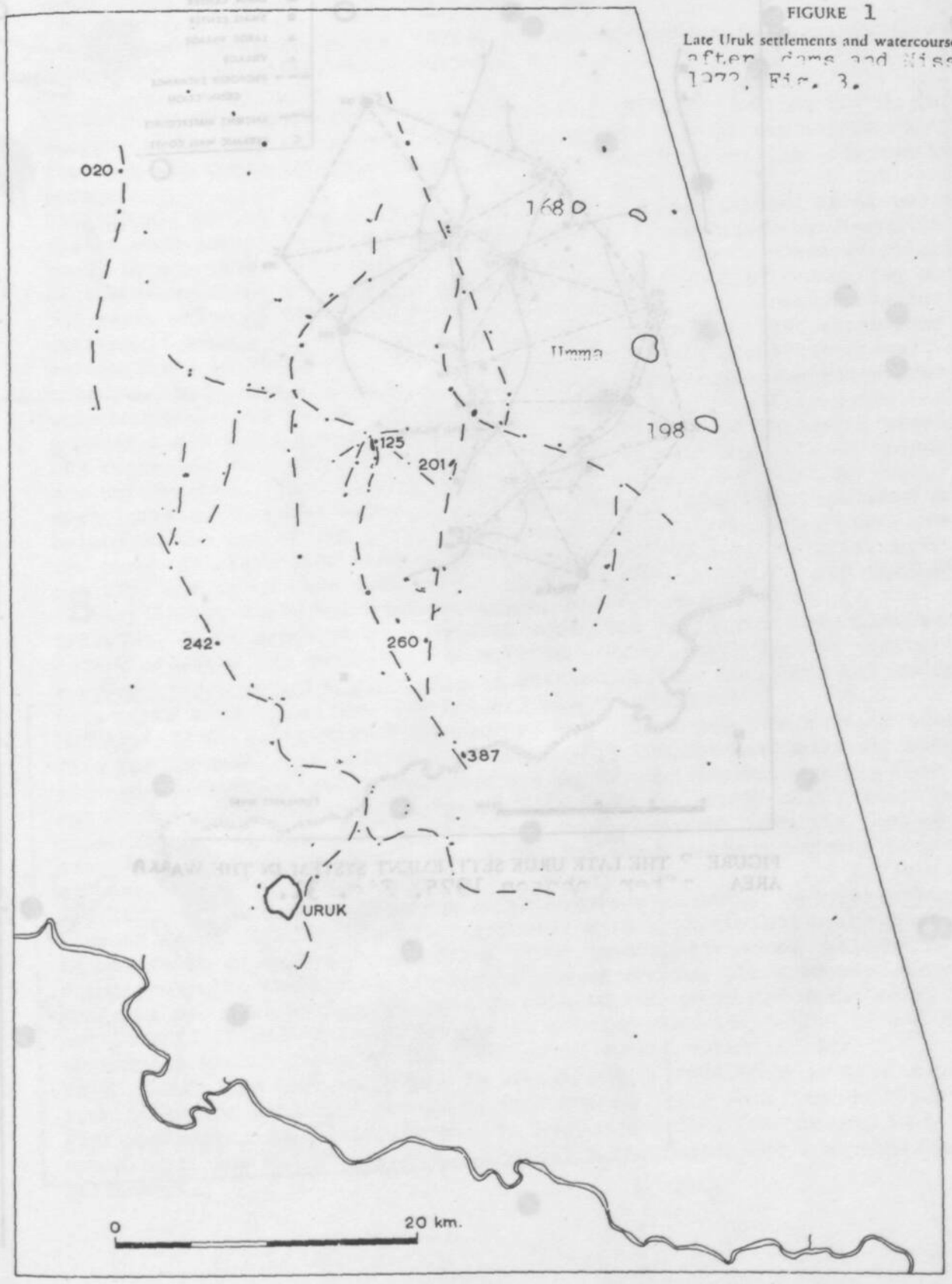


FIGURE 1

Late Uruk settlements and watercourses after Adams and Nissen, 1972, Fig. 3.

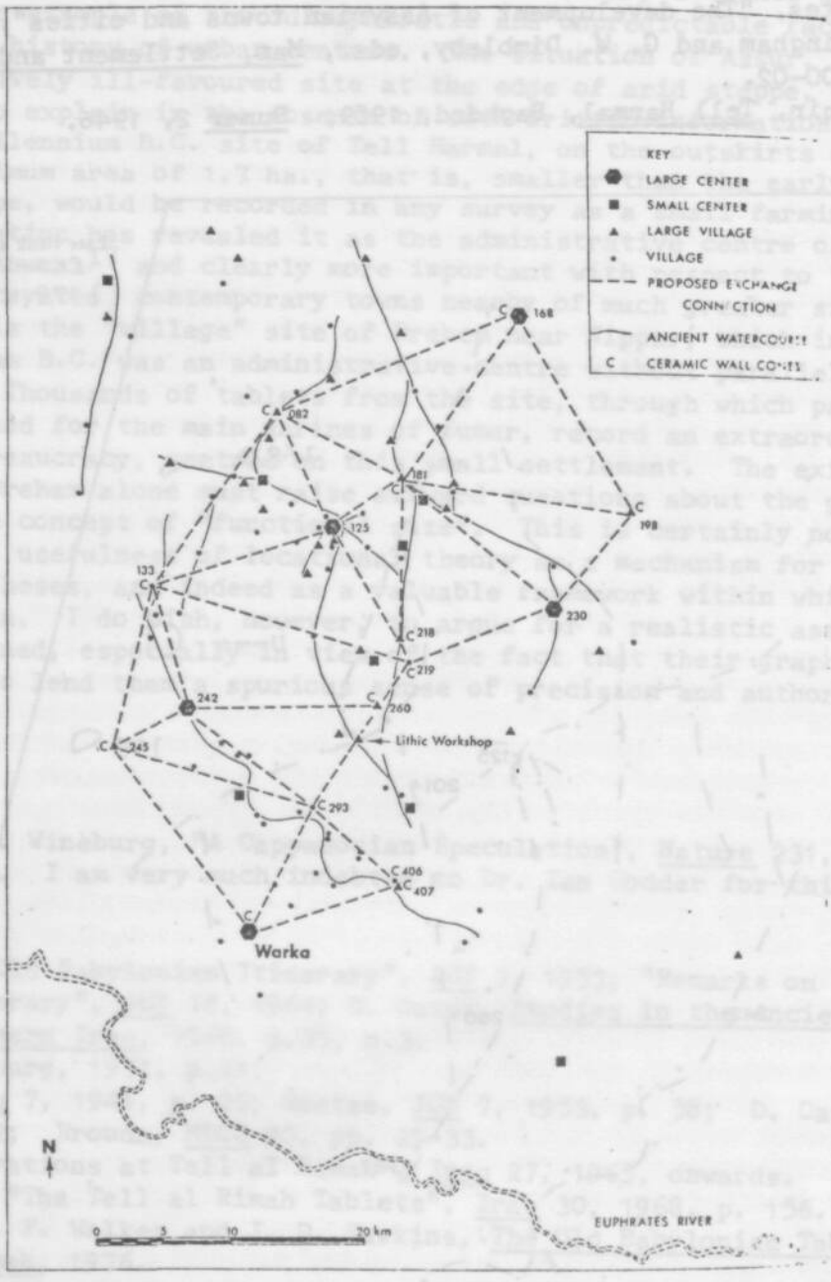
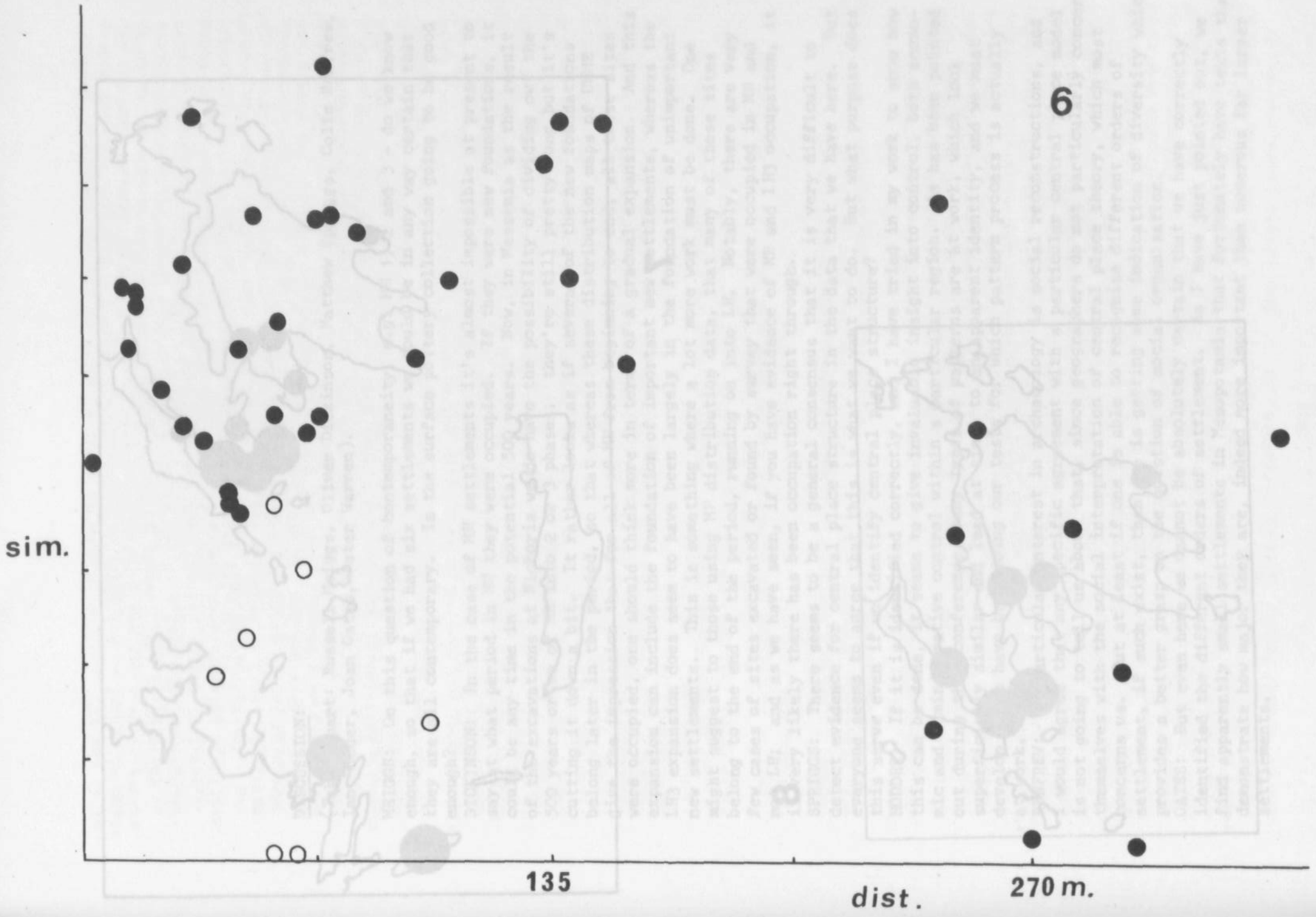
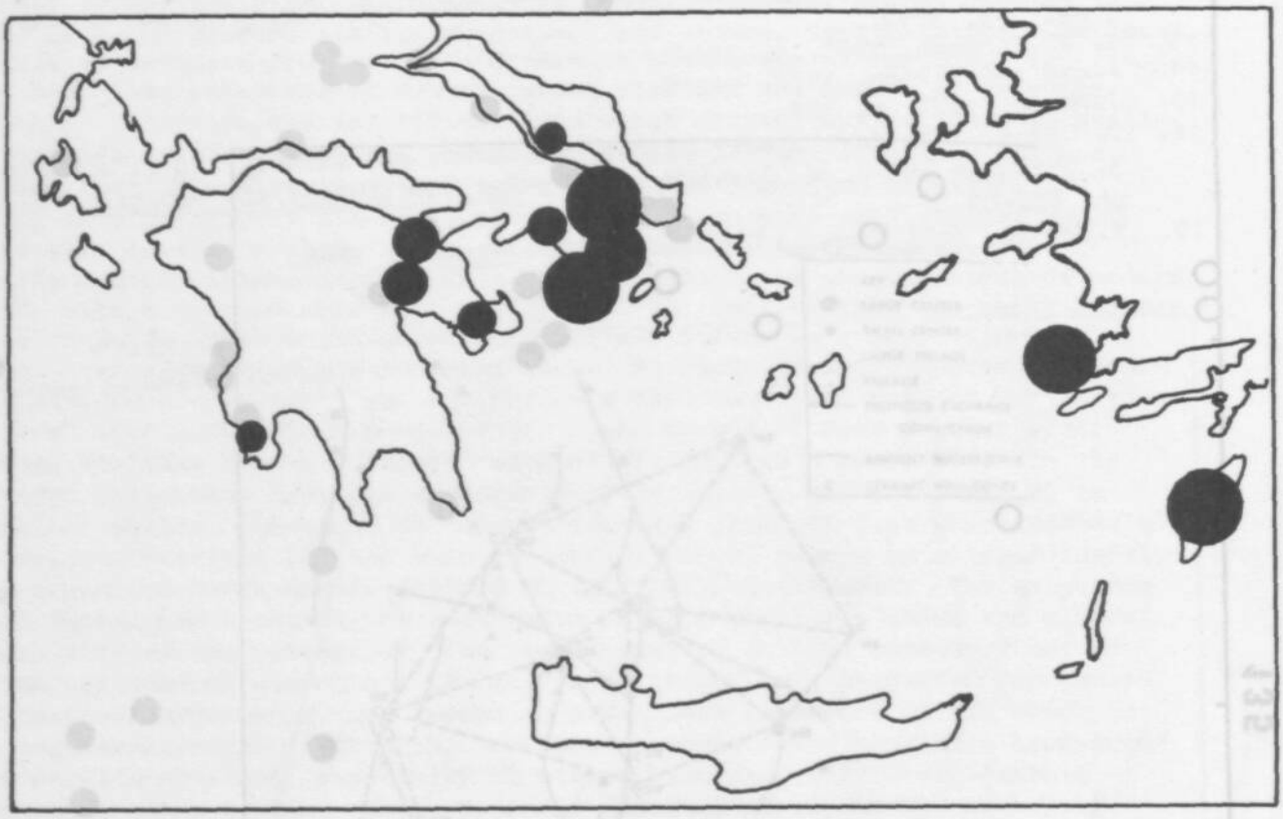


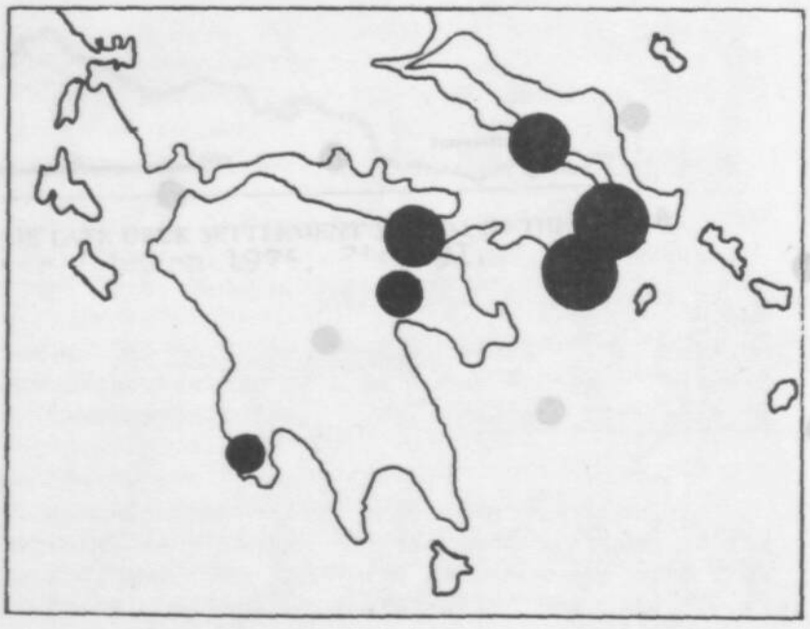
FIGURE 2 THE LATE URUK SETTLEMENT SYSTEM IN THE WARKA AREA after Johnson 1975, Fig. 31.


6
sim.
135
dist.
270 m.



7

8



DISCUSSION:

(Taking part: Russell Meiggs, Oliver Dickinson, Matthew Spriggs, Colin Renfrew, Ian Hodder, Joan Oates, Peter Warren).

MEIGGS: On this question of contemporaneity, e.g. MH 1, 2 and 3 - do we know enough, so that if we had six settlements we could be in any way certain that they are all contemporary. Is the surface pottery collection going to be good enough?

DICKINSON: In the case of MH settlements it's almost impossible at present to say at what period in MH they were occupied. If they were new foundations, it could be any time in the potential 500 years. Now, in Messenia as the result of the excavations at Nichoria we do have the possibility of dividing out the 500 years or so of MH into 2 or 3 phases; they're still pretty long but it's cutting it down a bit. It rather looks as if several of the new foundations belong later in the period, so that whereas these distribution maps of UMME give the impression that for all of MH from beginning to end, all these sites were occupied, one should think more in terms of a gradual expansion. And this expansion can include the foundation of important new settlements, whereas the LH3 expansion does seem to have been largely in the foundation of unimportant new settlements. This is something where a lot more work must be done. One might suggest to those using MH distribution data, that many of these sites belong to the end of the period, running on into LH. Notably, there are very few cases of sites excavated or found by survey that were occupied in MH and not LH; and as we have seen, if you have evidence of MH and LH3 occupation, it is very likely there has been occupation right through.

SPRIGGS: There seems to be a general consensus that it is very difficult to detect evidence for central place structure in the data that we have here. But everyone seems to agree that this is what we want to do. But what purpose does this serve even if we identify central place structure?

HODDER: If it is identified correctly, and I have tried in my work to show how this can be done, it seems to give invaluable insight into control, both economic and administrative control within a particular region. As has been pointed out during this conference, many types of patterns are at work, which look superficially similar and lead at times to an apparent identity, and we must develop as we have been doing our tests for which pattern process is actually at work.

RENFREW: My particular interest in archaeology is social reconstructions, and I would agree that any specific agreement with a particular central place model is not going to tell us about that, since geographers do not particularly concern themselves with the social interpretation of central place theory, which most concerns us. But at least if one is able to recognize different orders of settlement, if such exist, then one is getting some indication of diversity which provides a better grasp on the question of social organisation.

OATES: But even here we cannot be absolutely certain that we have correctly identified the different orders of settlement. As I have just pointed out, we find apparently small settlements in Mesopotamia that fortunately have texts that demonstrate how major they are, indeed more important than numerous far larger settlements.

RENFREW: Obviously one has to think in terms of more dimensions. If one is in a very complex situation - I think the Mesopotamian situation is larger in scale and for that reason, if for no other, more complicated - one has a whole series of urban functions, and we have to think of all of those. It's no surprise at all when you say that a very important centre can be a very small community. This is a property of the modern world, though most capital cities that one knows of are very large. It's only when you get a very sophisticated situation, particularly with federal centres, then you suddenly find Washingtons or Canberras - much smaller than the great cities you expected to find. So these may not be aberrant, they are simply due to further processes operating which arise from the greater complexity of the system you are dealing with. So I don't find this example depressing. On the other hand Johnson's work was unsuccessful not just because of these factors. He did not fully perceive the nature of the archaeological data, and made this spurious exercise in drawing a supposed central-place network - then completely arbitrarily deleting a number of points. He then said that what is left compares with what is left on the ground - well so it ought if you deleted half the points!

WARREN: If I could take up one of Joan Oates' points. It is the extreme importance of the quality of conversation which goes on between the archaeologist and the statistician or geographer, or whatever, before some course of action is arrived at. Let us recall John Killen's earlier comment that there are some associations of names on the Knossos Tablets, for which there is very good reason to suppose that they did not involve any geographical proximity at all, e.g. the 'missing women' case. Yet these it would seem have just been fed into the programme as basically suggesting proximity. If a rather more careful conversation had taken place between archaeologist, textual scholar and the computer or statistical expert, this need not have arisen. This is just a warning note on the sort of control we need, and as Professor Brown was saying - we must monitor closely the likely suggestions that come out at the end of the line.

RETROSPECT AND PROSPECT

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Surveying the tantalisingly imperfect data available, and the numerous problems in the Aegean bronze age for which no solution is available, we should first count our blessings. It is now precisely a century since Schliemann inaugurated Mycenaean studies with his excavations at the Citadel of Mycenae, and we can reasonably claim that since that time we have come to know more quantitatively about the Minoan-Mycenaean civilisation than of any of the world's other early and complex societies. Decades of site survey in both Crete and Mainland Greece have produced lists of prehistoric sites whose coverage, while it may not be as intensive as for some area surveys in other parts of the world, has the merit of embracing the entire territories occupied by the civilisations in question. A relatively high proportion of these sites has been investigated by excavation so that, without the advantage of written historical narratives, we have a clear outline of the prehistory of the region. And through the great work of Furumark, and of his successors, the chronological development of Mycenaean pottery offers a firmer base for dating, both relative and absolute, than is available from the artefacts alone for any of the world's early civilisations.

Yet at a deeper level, in many ways a serious understanding of the Minoan and Mycenaean civilisations still eludes us. No clear picture yet emerges of the social organisation of Crete in the Middle Minoan period, the early phase of the first palaces. In particular the origins of Mycenaean palace society are altogether obscure: the wealth of the Shaft Graves dazzlingly outshines anything that we could have predicted from the remains of the earlier Middle Helladic period. And too little is known of the period between that time and the developed palaces of Late Helladic III A for us to explain what has happened. The old controversy between Evans and Wace on the dependence or autonomy of the Mycenaean civilisation vis à vis the Minoan is not dead yet. Indeed a recent study of Early Mycenaean metal working (Tripathi 1976) suggests that there is more to be said. Above all, the discussion too rarely goes beyond the realm of description to that of explanation: we have not yet systematically formulated models which would set the available data into a broader framework and allow us to begin to say why the changes observed occurred, that is to say how their occurrence could have been predicted as possible or even likely in terms of the contemporary environment and culture history.

The most encouraging feature of this Conference has been the sustained effort by most contributors to set Mycenaean studies within a wider context than is sometimes the case, that is to say the context of the early civilisations in general. It was appropriate, in the final session, to hear of the problems facing scholars with analagous tasks to our own in Mesoamerica and Mesopotamia, and in the valleys of the Nile and the Ganges. For in these areas too there are often real difficulties in bringing together the archaeological and the epigraphic evidence, and of relating the prehistoric archaeology to the prehistoric geography, and above all of seeking to apply a nomothetic approach to culture history. This conference has done something to further progress in all these directions.

If we were to formulate the broader aims of our research - and perhaps it is about time that we did - I would state them as follows:

First, to understand the nature of Minoan-Mycenaean polities as functioning entities, and

Second, to explain how they came to take on the form which they had. When we say 'explain', this must imply an effort to predict some of the salient features of that civilisation, of those polities, by reference to more general principles.

THE GEOGRAPHICAL APPROACH

The focus of the Conference has been geographical, and the prospects for progress are encouraging in at least five directions new to Aegean archaeology. These take us beyond the progress made in the past fifteen or twenty years in the study of settlement distribution and environmental adaptation. I suppose that the energetic search for new prehistoric sites, best exemplified today in the first volume by the University of Minnesota's Messenia Expedition can in fact be traced back through the travels of Richard Hope-Simpson and of David French, back even before those of John Pendlebury and of Sylvia Benton to Wace and Thompson's Prehistoric Thessaly, published in 1912. But the environmental aspects of these sites have only been investigated in more recent years: I believe Robert Rodden's excavations at Nea Nikomedeia (1962) were the first to apply in the Aegean the full range of techniques, including archaeozoology, palaeobotany and palynology, which Robert Braidwood applied nearly a decade earlier in the Near East. It should however be recalled that Winifred Lamb's work at Thermi (Lamb 1936) was a pioneering work in systematic interdisciplinary study, bringing to fruition some of the earlier insights of men like Schliemann and Sir Arthur Evans. Already therefore, in the past twenty years, systematic

site survey and an environmental approach to excavation have become well established. So too has palynology: Judith Turner's work, with James Greig, initiated in the framework of the excavation projects at Saliagos and Sitagroi, has a scope and relevance that goes far beyond those sites (Turner 1968; Greig and Turner 1974), laying the basis for a vegetational and climatic history of the Aegean (cf. also Wijmstra 1969, Bottema 1967, Wright 1972) to set alongside less well-founded climatic speculations tending as much to mislead as to illuminate (Carpenter 1966; Bryson, Lamb and Donley 1974).

The new developments, each in its different way, focus less upon the environment itself than upon the relation of human settlement to that environment.

1. The Younger Fill

The results of John Bintliff's work are nothing short of revolutionary for our understanding of settlement distribution. It was Vita-Finzi (1969) who first indicated that the alluvial deposits in the plains and valleys of Greece may be divided into an Older and a Younger Fill, and that the latter is of relatively late formation, perhaps Late Roman and mediaeval in date. Whether this phase of alluviation occurs simultaneously in different areas remains an open question, and Vita-Finzi's climatic explanation may yet be superseded by one laying more stress on the effects of human activity on vegetation and erosion. Indeed work on Melos (Davidson, Renfrew and Tasker 1976) suggests that the formation of the Younger Fill there may have begun already by the end of the late bronze age. (But see Bintliff, this volume, Note 1.)

One of the most important implications of all this is that the landscape of Greece today is very different in a number of ways from the scene in Mycenaean times. Whole new plains have been formed by alluvial deposition, and upland areas and sites extensively eroded. Many areas which may have contained traces of prehistoric occupation have been buried. In these circumstances the geomorphological and pedological background is of much more urgent interest than in more geomorphologically stable environments.

2. Tablet Toponyms

Already the Linear B tablets have offered rich rewards to the prehistorian, many of them insights of an economic nature. It is the promise of further progress in this direction which makes the problem of equating tablet place names with locations on the ground very much more than just a fascinating puzzle. For the prize is not simply the assignment of place names of the fourteenth or thirteenth centuries B.C. to actual locations in Messenia or the realm of Knossos. It is the definition of a real geography, open to modern research, for the world of the tablets. For the great value of the inventories, payments and returns recorded in the Linear B records is that they are quantified: we know how much wheat, what size flocks, how many smiths. If place names could be related firmly to the archaeological topography we should be on the threshold of an economic geography of the Knossian and Pylian kingdoms, where modern crop yields and agricultural studies (making due allowance for geomorphological change) could be related again to the records. This is of course the approach which Dr. Killen has used already with such rewarding insights for the Knossos wool tablets, where a whole aspect of the economy stands revealed.

The great value of the formal analyses of tablet toponyms undertaken by John Cherry and Professor Kendall is that they isolate one particular class of information, simple adjacency of names on the tablets, without drawing on the wealth of further information which a familiarity with the tablets brings. Using this very restricted class of information a pattern is produced from the data

which we may then contemplate. Now this pattern will only approximate to a topographic map if certain assumptions hold, principally that adjacency on the tablets implies adjacency on the ground. It is quite conceivable that the tablets would frequently list place names grouped by class of produce, so that our adjacency clusters would be offering some kind of 'map' of economic activities which would not correspond directly to surface topography. Such notions underline the virtue of analysing the adjacency data separately from other considerations.

In the event it has been encouraging that Dr. Chadwick's very persuasive assessment of Messenian topography, drawing on a much wider range of information, yet without a formal multidimensional scaling, and Dr. Killen's very interesting presentation of the Knossos data, have not been contradicted by the formal analyses discussed at this conference. Yet it seems likely that our knowledge of the archaeological site distribution in the areas is still inadequate for any serious attempt at matching toponyms with specific locations.

3. Intensive Survey

Again and again during the conference speakers stressed our good fortune in possessing the excellent site survey records for both Crete and the Peloponnese, and in particular for Messenia and the Knossos region. The very real pay-off made possible by all the energetic, time consuming and often laborious work in the field is seen in the very title of this conference. If we can begin to speak of a Minoan or a Mycenaean geography, it is field survey as much as the decipherment of the tablets which has laid the foundations.

Yet I would argue that, if we are really to exploit the potential of this inviting theme, a new phase of field survey must now be initiated. Hitherto the aim of fieldwork has very properly been the discovery of as many new and important sites as possible. The strategy has thus been to visit the most likely locations and to follow up all reports of antiquities offered by local informants. This may be described as extensive fieldwork, where one seeks to locate the most prominent sites in as wide an area as possible.

What is required now, given that this extensive approach has been applied over much of the Peloponnese and of Crete, is the application of intensive fieldwork techniques. Several different approaches could be employed, but two elements are common to each: a formal and rather rigorous sampling strategy, and the field-walking of selected areas by survey teams who will cover the entirety of the terrain within those areas.

The aim of intensive survey is to do two things: to discover different classes of site, including the smaller, less obvious but no less important ones which are often overlooked in extensive survey, and to provide a basis for the informed estimation of settlement number and density in areas not yet intensively surveyed. There are of course dangers here in premature extrapolation, and Professor Warren was critical in discussion, I think with some justice, of my own efforts in this direction (Renfrew 1972a; 1972b chapter 14). But my extrapolations and estimates had perforce to be based on non-intensive survey, since none had yet been carried out in the Aegean.

The application of sampling strategies is a commonplace in the sciences (e.g. Conway 1967, Blalock 1960) and is now becoming an important feature of archaeological method (Mueller 1975, Redman 1975, Gummerman 1971). It implies the recognition that there is not the time or the resources to study intensively all the potentially available data - you cannot always dig an entire site, you cannot sieve all the soil from an excavation through a 2 millimetre mesh, you cannot fieldwalk intensively a whole country.

Intensive fieldwalking is time consuming and demanding: on rough terrain it may take a day for a group of seven or eight to cover an area of just two square kilometres. But the outcome may radically alter existing patterns. In Melos during the summer of 1976 John Cherry led an intensive survey, a part of the environmental project organised in conjunction with the British School's excavations at Phylakopi, which changed the picture of settlement through time there in a number of different ways.

The rewards will be commensurately greater in Messenia and the Knossos region when the results can be brought into relation with the geographical understanding discussed above, and where very good extensive surveys have already laid the foundations. The value of these has been amply demonstrated throughout this conference, and I believe that richer rewards are to be gained. The increasing mechanisation of agriculture in Greece over the past two decades makes intensive survey a particularly urgent task. It is only now being realised in Britain that rescue archaeology faces more formidable problems in the country than in the town, and this must apply with still greater force to Greece.

4. Social Landscapes

The environmental implications of locational analysis are now being assimilated successfully to archaeology. The ecological approach pioneered over two decades ago by Grahame Clark was the precursor of methods such as 'site catchment analysis', advocated by Eric Higgs and Claudio Vita-Finzi, much in the spirit of work like Chisholm's Rural Settlement and Land Use. It is however the social interrelations of settlements which remain fully to be explored, and some of the techniques of locational analysis are appropriate here also.

Some measure of settlement hierarchy can certainly be discerned in the Aegean later bronze age, indeed that of prehistoric Crete has been compared with the modern hierarchy of settlement (Renfrew 1972b, 259). What is clearly needed now is some more systematic and formal analysis to test the extent to which Hierarchies of settlement can be discerned in the Mycenaean social landscape, although the data may not yet respond directly to some of the techniques worked out elsewhere (Hodder and Orton 1976). It may not follow that size of settlement is a direct measure of its place in a hierarchy, and further means are needed to assess this.

In prehistoric archaeology a spatial approach will often be the most effective available for distinguishing the number and size of independent polities within a region (Renfrew 1977), and even applied rather simplistically to the existing Mycenaean data it is not uninstructive (see fig. 1). But this is a field where the tablets can tell us far more, as indicated below.

5. Simulation

The fifth approach which offers great scope for the enlargement of our understanding of Aegean prehistory is that of simulation. Anthony Chadwick's paper to this conference indicates a number of opportunities. Simulation means nothing, of course, without an appropriate predictive model by means of which a phenomenon can be simulated. And the assessment of the simulation, the comparison of the simulacrum with the real data, present problems of method. Yet already we can discern, for instance, that using Bintliff's insights about the location of major and minor settlements in location to terrain and soil, and employing certain simple rules about settlement hierarchy, patterns of settlement can be predicted, and can be simulated using real data about the terrain.

It has long been argued that the geography of the Aegean world imposes certain constraints on history (Myres 1953, Cary 1949), and a similar insight illumines the more sophisticated approach of Braudel (1972). And it is, ultimately, by simulation that this can most effectively be investigated. For it is ideally suited to follow through the logic of rather simple principles (which can be expressed in algorithmic form) applied to a rich and complicated specific landscape, allowing (by the use of stochastic processes) in fair measure for the effects of time and chance.

If there are regularities to be sought underlying the richness and variety of Aegean prehistory and history - and the recurrence of city states and local regions at different periods suggest that there are - this is the approach which may allow us to distinguish between the contingencies of the local environment and the influence of those underlying basic processes.

LITERACY AND THE AEGEAN BRONZE AGE

1. Literacy

To what extent were the Minoans literate? The question, whose answer is far from obvious, has some bearing on the social organisation, and indeed on our understanding of the political geography of the bronze age Aegean. To illustrate the extremely limited extent to which writing was used by the Minoans, and especially the Mycenaeans, it is sufficient to compare the position with that in Mesopotamia as set out by Oppenheim, quoted by Goody (1968, 21):

The Uses of Writing in Mesopotamia

(1)	Recording data for future use	(a) administrative purposes (b) codification of law (c) formulation of a sacred tradition (d) for annals (e) and ('eventually') for scholarly purposes
(2)	Communication of data on a synchronic level	(a) letters (b) royal edicts (c) public announcements (d) texts for training scribes
(3)	Communication with gods	secret texts, amulets, etc.

The Aegean case suggests a further use, no doubt also seen in Mesopotamia, namely the marking of products and commodities with an inscription indicating their origin, nature, destination or ownership. Such is presumably the function of many of the clay sealings, and perhaps of the Linear B inscriptions on the Thebes stirrup jars.

It is indeed remarkable that the only texts of any length from Crete or the Mainland fall within class 1(a), although the poor quality of one tablet has led to its (perhaps uncertain) assignment to class 2 (d). In addition the various short inscriptions in Linear A script on altars and sealstones can be assigned to class 3, and these may testify to a wider literacy beyond that of palace scribes. It is noteworthy, then, that such documents are commoner in Linear A than in Linear B, where the stirrup jars offer the only important case of inscriptions on portable objects. So one or more pot - painter was literate too. It seems, then, that literacy may have been confined to the palace itself, and this point may itself be of some interest.

In many ways the most useful thing which the Pylos tablets have to tell us is the size and extent of the Messenian kingdom (although I am not yet sure that the evidence has been assessed with conclusive finality). There is no suggestion that Pylos is subservient to any higher order centre in a hierarchy: it is an autonomous political centre. Disappointingly perhaps the tablets have not yet yielded any information about relations with neighbouring polities. But at least we shall be able to infer the area of this autonomous kingdom, and by comparison infer that other literate centres such as Mycenae and Thebes were the centres of equivalent autonomous realms. In this way the tablets allow the construction of a political geography of Mycenaean Greece which accords well with the archaeological record (and does not conflict, incidentally, with the picture painted by Homer).

The same approach is clearly possible for Crete. The Linear A archives up to the end of the Late Minoan I period already, without their decipherment, suggest a similar approach. We may at least suggest that each place centre with a Linear A archive functioned within an autonomous polity of the kind which we see in the LH IIIB Peloponnese. But is this certain? Is there a possibility that the Knossos Linear A tablets will one day reveal that in the Late Minoan I period Knossos had control over Crete as a whole? It is here that the discussion above of the extent of literacy is relevant. For Knossos to have overall control, yet for records to be kept at Phaistos, or Chania or Mallia, would suggest that subsidiary centres in a political hierarchy with Knossos at the top were also literate to this limited extent. Such a suggestion at first sight seems unlikely (although one would not necessarily use the tablet find from Myrtos Pyrgos to class that site as the administrative centre of an independent kingdom).

A similar approach to the Knossos Linear B tablets should presumably allow us to decide whether or not Knossos ruled the whole of Crete in the LM II and IIIA1 periods. Obviously the lack of any rival palace of the period is a strong argument, but one nonetheless ex silentio. Dr. Killen's interesting study of the place names of the Knossos tablets provides stronger evidence, and it is clearly significant that Phaistos and Kydonia (Chania) figure in them. It would, however, have been useful to have seen one or more recognisable place names from east Crete as well. At present it would not be impossible to see Knossos as ruling the centre and west, but for the east to be again an autonomous polity, as no doubt it was in LM IB times, when Zakro and perhaps other centres were functioning, and as presumably it became again in the fragmentation which followed the destruction of Knossos around 1380 B.C.

2. The Sacred and the Economy

If the restricted extent of literacy is one curious feature of Aegean bronze age civilisation, by comparison with that of the Near East or Egypt, the very modest scale of its investment in religious monuments is another. The rooms specifically set aside for religious functions in the Minoan palaces are distinctly mean (although of course other rooms and indeed the central courts may have been used as well). And the peak sanctuary, as the only specifically religious site-type in Crete, is not impressive. On the mainland the few Mycenaean shrines, although full of interest, are not themselves unduly impressive, and the religious structures of Aghia Irini on Kea or Phylakopi in Melos scarcely bear comparison with the ziggurats of Mesopotamia, the temples of the Nile or the pyramids of Mesoamerica. This I find one of the most surprising features of the time: that its archaeology is predominantly secular.

It comes as a surprise, therefore, that the tablets should make such full and frequent reference to deities. Moreover they are extremely well provided for in the lists of offerings. Had we archaeological evidence for temples, as

in Mesopotamia, it would be easy to interpret these offerings to deities as in practice offerings to the temple and hence to the priesthood. These contributions would support households and craft specialists as on the Mesopotamian model.

So where is this tribute going? What becomes of the golden vessels dedicated to this or that goddess? Naturally we would not expect to find much precious metal in a temple or sacred precinct today, but the archaeological evidence for such a sanctuary should nevertheless be substantial. This is a question which may need clarification before a good overall understanding of the Mycenaean economic system could be reached.

3. The Catalogue - an Alternative Geography?

In conclusion I cannot resist reference to the geographical implications of a very different literary source: the Homeric Catalogue of Ships. This subject has of course been the focus of work by Hope Simpson and Lazenby (1970), and was a starting point for Hope Simpson's comprehensive survey (1965) of Mycenaean Greece. The interest here, however, is not upon the location of specific sites listed, but rather upon the general political geography which arises from an analysis of the Catalogue. This does not, I think, conflict with the impression given elsewhere in the Iliad as well as the Odyssey of a patchwork of autonomous small states, governed by independent rulers (often related by kinship or marriage) sharing a common language and customs but united politically only on occasion and in a loose associative way, under the charismatic leadership of the ruler of Mycenae. His authority over the inhabitants of other states and their leaders was valid only while they were associated in a great military expedition, and there is no suggestion that he had any territorial jurisdiction over their own states at home, but simply over their soldiery while on the expedition overseas. Of course we must be careful, as Finley has consistently indicated (1954), and as John Chadwick has stressed from the experience of interpreting the Linear B tablets, not to assume that The World of Odysseus (Finley 1954) can be equated with The Mycenaean World (Chadwick 1976) at any period.

My approach to the question is from quite a different quarter, entirely without reference in the first instance to the Catalogue of Ships. I have suggested (Renfrew 1975, 12-21) that a spatial pattern can be discerned in early civilisations of a specific kind:

"1. In most, perhaps all, early civilizations there function a number of autonomous central places which, initially at least, are not brought within a single unified jurisdiction. It is such autonomous territorial units, with their central places, which together constitute what we would all term a civilization. They may be recognized as iterations of what I propose to call the early state module (ESM).

If the territorial extent of any early civilization is marked on a map, the higher-level organization pattern will take a configuration of fairly evenly spaced autonomous central places set in territories which may notionally be indicated by means of Thiessen polygons. At the level of the early civilization or early state, these are the largest central places found. And when archaeologists claim to speak of the origins of early civilizations, they are usually found to be speaking of the development of the ESMs and of the less permanent and less active central places which preceded them. A central place, as considered here, is not, of course, merely an agglomeration of population; indeed its population may be small.

2. The early state module apparently falls within a restricted size range.

Frequently the modular area is approximately 1500 sq. km. with a mean distance of about 40 km. between the central places of neighbouring modules. Special environmental or social factors may reduce this distance to about 20 km., while intervening parcels of uncultivable land may increase it to at least 100 km.

3. Many early civilizations comprise, before subsequent unification, about 10 of such early state modules, although the number may vary by a factor of at least 2, and cases are known where the number is higher."

Following the procedure adopted, plotting simply the fourteen palaces and major fortresses listed by Hope Simpson (1965), and making the assumption (which is certainly open to discussion) that Gla, Mideia and Argos were not permanently functioning central places comparable with Mycenae, Tiryns, Thebes or Orchomenos, the map seen in fig. 1 results, without further assumption. Now it is important to stress that no-one is claiming that this is a good approximation to the political geography of the Mycenaean world: it is simply an analysis in terms of presumed central places. Any autonomous areas without a well developed palace organisation (and indeed any areas with a palace which has not yet been discovered archaeologically) will not figure on the map. Arcadia, one imagines, is such a case, and if I were using the map as the basis for a proposed political geography, that is one region which would be inserted. Indeed the convergence of six territorial boundaries in a narrow area in the middle of the Peloponnese itself hints at a missing territory there, without further analysis. In producing such a map, Euboia would need consideration, and one might well suggest autonomy for some of the Ionian islands. The palaces of Late Bronze I Crete could firm the basis for the construction of an analogous map.

It is not the details which are crucial here, however, but the general picture which can be produced from a simple general principle using hard archaeological data, as listed by Hope Simpson. The picture emerging from the Pylos tablets, as discussed above, harmonises well with this notion of a number of small autonomous states, and indeed the scale of the kingdom suggested in fig. 1 (where the mean distance between centres of adjacent territories is 76 km) if of the same order as that put forward by Chadwick for the Messenian kingdom. Insofar as his analysis is based on a consideration of place names, it is largely independent of that data used to establish fig. 1.

Comparison of this map with those by Page (1959) and by Lazenby and Hope Simpson (1975) based exclusively on the Catalogue of Ships given by Homer is very instructive. Figure 2 follows closely Page's two maps for Mainland Greece, with which Hope Simpson and Lazenby's Map 1 is in close agreement. Naturally no account is taken of the contribution of Crete and the Dodecanese to the Greek forces.

At first sight, I think, the similarities between figures 1 and 2 are very striking. Even more so if allowance is made for Arcadia, as suggested above. The landscape is divided into a number of autonomous states, of which there are six (including Arcadia) in the Peloponnese. Attica, Boiotia, Phokis and the Orchomenos district constitute autonomous states in both cases. It is only further north that discrepancy arises, where the realms based on Iolchos and Petra on figure 1 do not reflect the complexity of figure 2. Page (1959, 126) has indeed pointed out that the picture of the kingdom of Achilles as implied in the rest of the Iliad, outside the Catalogue of Ships, leaves no room for the kingdoms assigned by the Catalogue to Protesilaos, Philoctetes and Eumelos, which would bring the picture into close conformity with the kingdom of southern Thessaly seen in fig. 1. Similar problems pertain to the kingdom of Odysseus, and may arise in part from the prominence in the Epics of heroes who were not masters of any large territory at home. It is clear also that the compiler of

the catalogue laid particular stress on Boiotia (Page 1959, 125), so that the treatment of central Greece is particularly detailed.

The correspondence between these two maps, compiled from entirely independent sources, is impressive. And my first reaction in contemplating it was to feel that it lent weight to the claims made by some scholars for the Catalogue as a description of Greece in the LH III period, handed on with little change through oral tradition, and set down in the eighth century B.C., some five centuries later. Such an explanation is essentially historical, by implication stressing continuity, and asserting that the archaeology of the LH IIIB period and the Homeric Catalogue of Ships are giving us a picture of what is basically the same reality.

It is very relevant to the more general ideas underlying this conference, however, to approach the similarity in another way. The outlook is suggested already by Chadwick (1973, 415): "Since 1955 my view on the authenticity of the account of Mycenaean Greece preserved by Homer has changed, and I now believe the Homeric evidence to be almost worthless for this purpose. The only way out of the impasse into which Homer leads us is to cut the Gordian knot, and reject Homer as containing more than a distant and confused recollection of the Mycenaean age". His views are no doubt coloured by the lack of correspondence between the place names of the Pylos tablets and those of Nestor's kingdom as listed in the Homeric catalogue.

An alternative approach, then is to reject the validity of the supposed historical continuity, and seek an explanation which is processual rather than historical. Again Chadwick (1973, 405) indicates a solution, when we are pondering apparent correspondences between Mycenaean archaeology and the Homeric epics: "The similarities are all too often those which are imposed by the nature of a monarchical society, operating a none-monetary economy in a small area, rather than individual details unlikely to recur by chance". Here there is an insight, similar to that which led to the formulation of the concept of Early State Module, where analogous processes with analogies in the resulting material culture are predicted for societies which hold a number of fundamental circumstances in common. Moreover, when the societies in question were occupants of the same terrain, albeit at different times and perhaps without any element of continuity whatever , then close analogies in the use which they make of that terrain, and of some aspects of the spatial organisation are bound to follow. Of course there can be no doubt that there was indeed a measure of continuity between the Mycenaean world and the world of the eighth century B.C. But the underlying idea here is that even without such continuity we would predict a segmentary pattern of the kinds seen on figures 1 and 2 for societies possessing a measure of centralisation, as chiefdoms or kingdoms. Moreover, given the particularities of the terrain, such a segmentary pattern at approximately this scale is likely in many cases to result in approximately the same territorial units at one time as at another. Elis, Laconia and Messenia for instance form natural units of this order of size.

Such an interpretation of the similarities between the maps offers a far greater potential for further research than does the first, framed in terms of historical continuity. For in reality it is less an explanation than an invitation to develop an explanation in terms of the regularities underlying the two cases, already hinted at by Chadwick. Ultimately the aim should be (a) to reach a clear understanding of the organisational principles at work in the societies involved (in the Mycenaean case largely with the aid of the tablets), and (b) to give a detailed description of the environment in the manner discussed by Bintliff so as (c) to produce a simulation of the spatial pattern and of the settlement distribution of the societies in question, and of the location of their

central places. Using such a research strategy we would regard figs. 1 and 2 as real (or at least independent) products of the same process, available to compare with those which the simulation would yield.

All of this naturally seems a little ambitious at present, in view of the imperfections in the data, our relative ignorance of several aspects of Mycenaean society, and the undeveloped state of simulation methods in archaeology. Yet, as this conference has shown, the foundations are well and truly laid. The tablets have already yielded and are continuing to yield information of direct archaeological relevance in the field of Mycenaean economy and geography, so that archaeology and epigraphy are interacting fruitfully. Detailed environmental work is now changing our understanding of settlement distribution. Social as well as ecological factors are being evaluated. Intensive survey in the Aegean is now beginning, capitalising on the rich results of the extensive surveys already undertaken. Simulation studies are now demonstrating the potential of a method where simple general principles can be applied to the intricate complexities of the real data. The prospects for further progress are encouraging.

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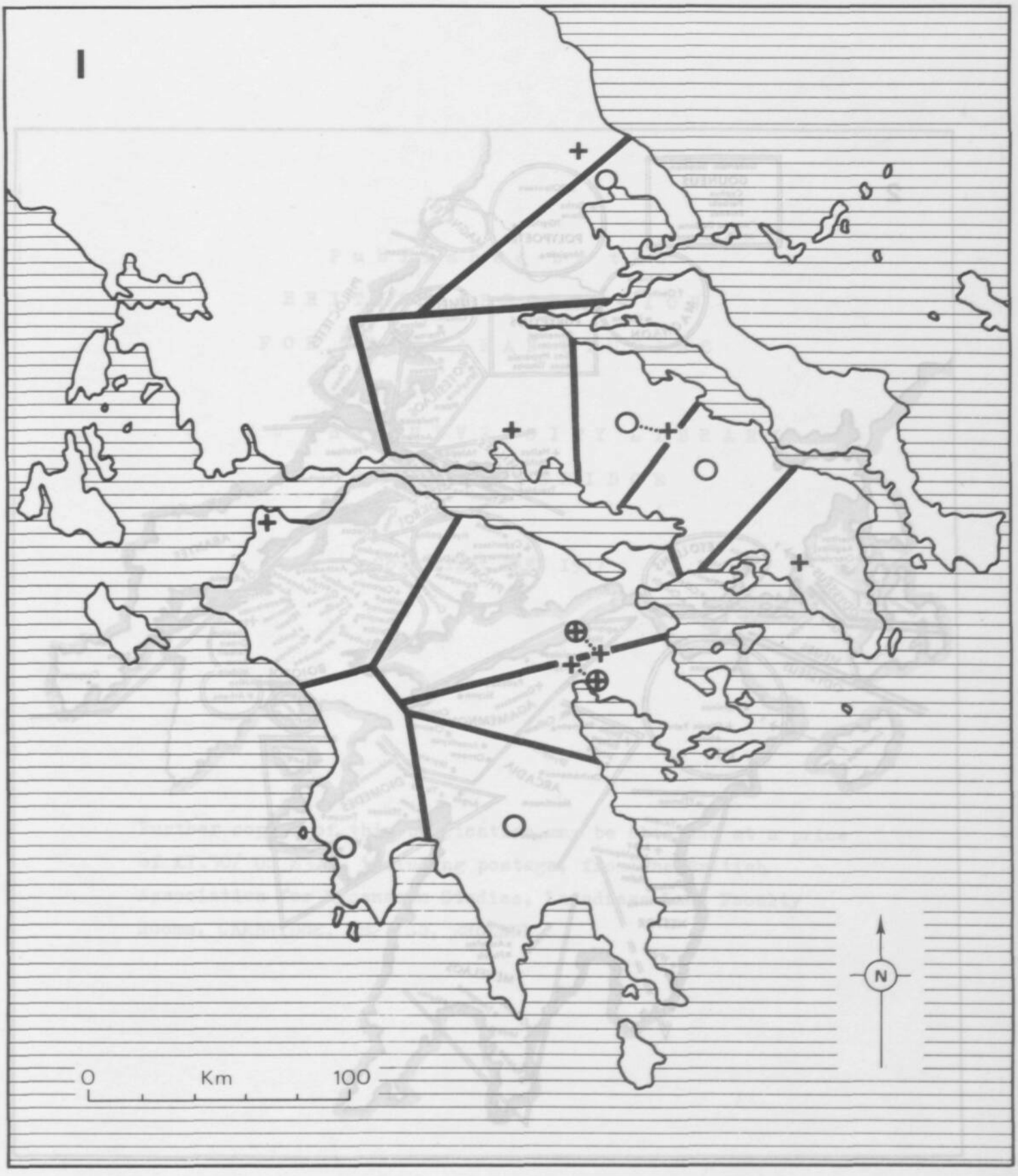
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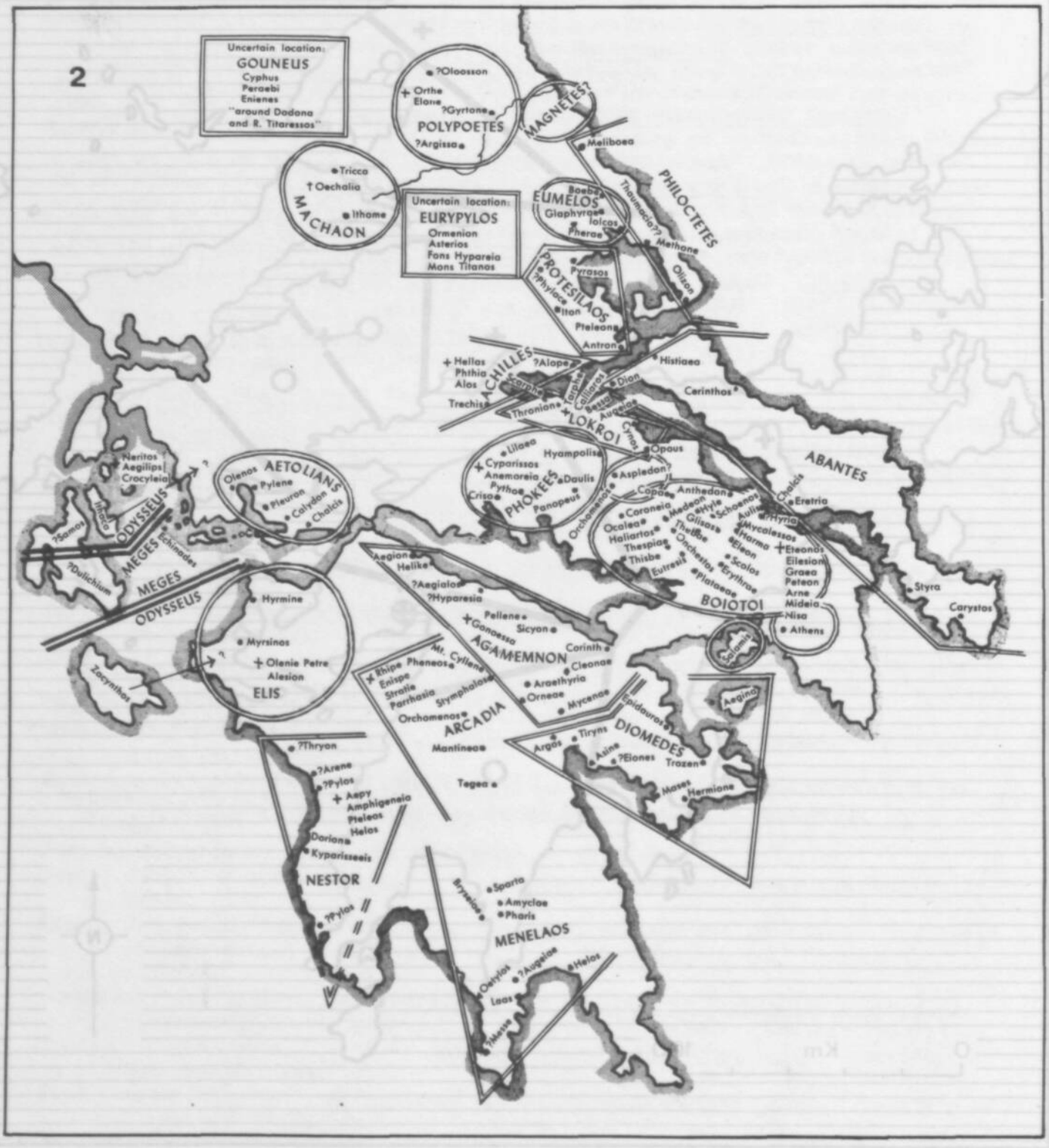
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Captions for Figures

Fig. 1. The early state module in Mycenaean Greece. The social landscape emerges when Thiessen polygons are drawn using palaces and fortified sites listed by Hope-Simpson. (A more flexible procedure would allow of modifications including a territory for Arcadia.)

Fig. 2. Political geography for the Greece of the Iliad from the Catalogue of Ships (based upon Page 1959, reproduced by kind permission of Sir Denys Page).


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0
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2
Uncertain location:
GOUNEUS
Cyphus
Peraebi
Enienes
"around Dodona
and R. Titaressos"
?Oloosson
Orthe
Elone
?Gyrtone
POLYPOETES
?Argissa
MAGNETES?
Meliboea
PHILOCTETES
Tricca
Oechalia
Ithome
MACHAON
Uncertain location:
EURYPYLOS
Ormenion
Asterios
Fons Hyperia
Mons Titanos
EUMELOS
Boebe
Glaphyrae
Iolcos
Pherae
Thaumacia??
Methone
Olizon
PROTESILAOS
?Phylace
Pyrasos
Iton
Pteleon
Antron
ACHILLES
Hellas
Phthia
Alos
?Alope
Histiaea
Cerinthos
Trachis
Thronion
LOKROI
Cynos
Opous
Lilaea
Hyampolis
Cyparissos
Anemoreia
Daulis
Python
Crisa
PHOKEES
Panopeus
Aspledon?
ABANTES
Chalcis
Eretria
Hyria
Neritos
Aegilips
Crocyleia
Ithaca
ODYSSEUS
?Samos
Echinades
?Dulichium
MEGES
AETOLIANS
Olenos
Pylene
Pleuron
Calydon
Chalcis
Orchomenos
Copae
Anthedon
Coronea
Haliartos
Thespiae
Thisbe
Eutresis
Ocalea
Medeon
Glissas
Thebae
Orchestos
Platacae
Harma
Eleon
Scolos
Erythrae
Mycalessos
Eteonos
Eilesion
Graea
Peteon
Arne
Midea
Nisa
Athens
BOIOTOI
Styra
Carystos
Aegion
Helike
?Aegialos
?Hyparesia
Pellene
Gonoessa
Sicyon
Corinth
AGAMEMNON
Cleonae
Araethyria
Ornese
Mycenae
Hyrmine
Myrsinos
Olenie Petre
Alesion
ELIS
Zacynthos
Rhipe
Enispe
Stratie
Parrhasia
Phenecs
Mt. Cyllene
Stymphalos
Orchomenos
ARCADIA
Mantinea
Tegea
Epidauros
Aegina
Argos
Tiryns
Asine
?Eiones
Trazen
Troezen
Hermione
DIOMEDES
?Thryon
?Arene
?Pylos
Aepy
Amphigenaia
Pteleas
Helos
Dorion
Kyparisseeis
NESTOR
?Pylos
Sparta
Amyclae
Pharis
Bryseiae
MENELAOS
Oetylos
?Augeiae
Helos
Leas
Thalame

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