
TOPICAL REVIEW

MAYA ARCHAEOLOGY 1958-1968, A REVIEW

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THIS PAPER IS A SURVEY OF MAYA ARCHAEOLOGY OF THE LAST TEN YEARS. A brief historical examination of the years prior to 1958 is made. There follows an intensive look at fieldwork from 1958 to 1968. An examination is made of the state of the field with respect to research design and analytical techniques along with a consideration of data contributed by collateral fields. On the level of explanation and synthesis, there is an examination of the major problems of Maya culture-history to the solution of which recent research has contributed. The concluding sections summarize the characteristics of recent work and attempt to sketch the critical areas for future investigation. I may have unfairly slighted some investigations and workers because much recent work is still in unpublished form. I have depended not only on preliminary reports and the standard publication sources, but also to some degree on the "bush telegraph" of anthropology in assessing the field. Thus the following can only be regarded as one (albeit active) worker's perception of his own specialty.

Maya archaeology as a discipline is an integral part of that of Mesoamerican prehistory. To use a concept more often applied to the data than to the discipline, Maya and Mesoamerican archaeology are parts of a vast diffusion sphere, or what we might term the co-tradition of New World archaeology. Maya archaeology has thus followed many of the general trends and stages of development of the general field. As in stages of archaeological cultures, the developmental events in the discipline have not been mutually exclusive in time. At any one time, most or all of the activities characteristic of an earlier period may be carried on in conjunction with those investigations and methods which started later. This parallels the well known phenomenon in prehistoric cultures of cumulative cultural elaboration. Therefore, the reader need not be

surprised if the sequence of events noted below has an often familiar aspect; they have happened elsewhere although in distinct permutations. However, it will be noted that many events within Maya archaeology happened earlier than elsewhere or are unique to the field. This is because many of the innovators of New World archaeology have specialized in Maya studies, and because of the nature of the data available.

Historically, Maya archaeology can be divided into the three classic stages. These were the "Great Explorer" Period, the "Carnegie" Period, and the recent past, or what might be called the "Multi-Institutional" Period. The main burden of this paper is to examine work done in the third period, but a brief examination of the earlier stages is desirable in order to set the context.

The "Great Explorer" Period lasted approximately from 1839 to 1924 and was characterized by extensive survey of architectural remains, sculpture and an intensive study of the hieroglyphic system. The work was largely financed by museums and private individuals. The Peabody museum of Harvard and the British Museum were especially active and such persons as Charles Bowditch contributed much. Bowditch, not incidentally, financed most, or all, of the Peabody's early work in the Maya area. John Lloyd Stephens, an earlier explorer, had used his diplomatic post as a vehicle for research, and this device was followed by E. H. Thompson in Yucatan. Maudslay, Maler and Charnay all had institutional, as well as private, backing for their work. Persons without any such post or institutional backing made few contributions to the field in this period. The somewhat later work of Tozzer, Merwin and Morley included some excavation and there was an obvious influence on the Maya area in techniques and research concepts from North American archaeology. This was because most of the workers in the Maya area were North Americans and therefore were also trained in anthropology. Most were therefore intellectually influenced by the dominant anthropologist of the time, Franz Boas. As has been frequently noted, Boas favored an approach to fieldwork and processing of data which emphasized the descriptive aspects. This was because he felt that it was in many ways premature to attempt explanatory theories about culture and this attitude included culture history. It has also been noted that this attitude was a reaction to the more extreme evolutionary schemes of the 19th century. In this general atmosphere and in attempting to gather as much data as possible without imposing a theoretical framework upon it, it is not surprising that American archaeology became very much oriented toward rigorous technique and description. Nor is it surprising that there was a rejection of explanatory theory as being unwarrantedly speculative. After all, if the ethnographers who dealt with whole and operating cultures avoided theorizing, why should the archaeologist who dealt with not only part cultures, but also with an infinitely more complex and diverse range of cultures? When explana-

tion was advanced, it was in the nature of multiple factor theories, or particularistic and historical. Maya archaeology, then, began to reflect the increasing rigor of fieldwork techniques and descriptive presentation of the early 20th century. This was a trend which intensified and became one of the predominant professional characteristics of the field during the next period.

The second period might be called the “Carnegie” Period, being dominated as it was by the activities of the Division of Historical Research of the Carnegie Institution of Washington. The period can be said to run from about 1924 to 1958 (Pollock, 1958). Other research institutions attempted some emulation of the Carnegie approach, notably the Middle American Research Institute and the University Museum of the University of Pennsylvania, but their activities were more sporadic and had less continuity than those of the Carnegie research group which worked together for about 35 years. As always, however, individual scholars, notably Franz Termer, made substantial contributions (cf. Termer, 1936, 1941). At about this time, the Peabody Museum of Harvard entered a period of symbiotic relationships with the Carnegie group. Many of the most influential members of the group were trained at Harvard under A. M. Tozzer who had become inactive in fieldwork although not in research. A. V. Kidder, as chairman of the Division of Historical Research (1930–1950), dominated the period in Maya archaeology as indeed in large measure he dominated the general field of American archaeology.¹ Kidder reflected the before mentioned attitude of extreme skepticism toward theorizing combined, on the other hand, with great innovation in applying new techniques; that of stratigraphic excavation, for example. Kidder also brought to the Maya field an overall integrated research plan which was no doubt a product of both his and S. G. Morley’s anthropological backgrounds. These men conceived of Maya research as properly embracing all the collateral and supportive fields which could furnish data to archaeology as well as the other main fields of anthropology. The Carnegie Institution during this period supported not only archaeology but also the linguistic studies of Andrade, the ethnology of Redfield, Chamberlin’s historical investigations, as well as Scholes’ ethno-historical studies. Medical and environmental studies were important parts of the program. The main emphasis was on archaeological fieldwork, however, and the aim to gain a greater and more diverse sample of Maya prehistoric culture by intensive excavations in selected areas and sites and extensive reconnaissance. An immense amount of work was done considering the resources allotted, the manpower involved, and the complexity and size of the problems. High quality reports were published in great quantity. However, in 1937 Tozzer published an article which criticized the general field of archaeology for preoccupation with technique and description and for its seeming lack of interest in theoretical explanation. C. Kluckhohn followed in 1940 with a similar but

more intense attack on Maya archaeology in general and the activities of the Carnegie Institution in particular.² This was a prelude to the general and more polemical assault of Kluckhohn's student, W. W. Taylor, made in a full length monograph in 1948. In all of these critiques of American archaeology the theme was that prehistorians were neglecting their duty as anthropologists to advance explanatory theory and that archaeology was in danger of becoming a sterile exercise in systematics. Indeed, the attack continues today in the polemics of the adherents of the "new" archaeology. As in so many endeavors, World War II intervened and acted as a watershed in the development of Maya archaeology. The Carnegie Institution, beginning in about 1938, was strongly reoriented by its president, Vannevar Bush, toward the "hard" sciences. A systematic pruning off and exclusion of humanistic and "soft" science programs began. World War II, with the increase of prestige of the physical sciences, reinforced the attitude. The Department of Historical Research came under increasing pressures to conform to more of a physical sciences approach. However, the final major project mounted by the department reflects little of these pressures and is a logical continuation of the previous long term research plans. The late period site of Mayapan was excavated in the hope of linking the archaeological record with the chronicle material which mentioned Mayapan as being an important political entity immediately before the Spanish conquest. A. V. Kidder retired in 1950, and H. E. D. Pollock was left as director to close up the operations of the department, this finally taking place in 1958 just before the retirement of Bush himself.

It has been argued that the Carnegie program was a victim of culture lag, and that it attempted to carry out an essentially 1930's type of archaeology, descriptive and technique oriented, into the late 40's and 50's, when the emphasis in new world archaeology was beginning to shift to cultural ecology, a concern with culture process, and all that that implies.

There are no differences between the Carnegie program and present day research, however, in fundamental orientation. The program embraced nearly all of the activities characteristic of Maya archaeology today, cultural ecology (environmental studies), settlement pattern studies (housemound surveys), and so forth. Indeed, it was quite innovative in character. The major differences seem to lie more in the realm of theory. Kidder, and later Pollock, speak of the difficulties of integrating the diverse and disparate information gathered by their program (cf. Kidder, 1936:113, 1939:239; Pollock, 1958:443). The Carnegie program lacked a comprehensive body of explanatory theory to integrate its data. It can hardly be faulted for this, however, since in no case was there a really adequate body of such theory widely accepted in archaeology prior to World War II. Julian Steward's theoretical work was still largely without wide influence in anthropology at that time. Another difference is that the

present research has a larger mass, sample, of information to deal with. It will be noted that more work has been done in the last ten years than in the preceding thirty. In one sense, the old Boasian adage of "waiting until the facts are in" to theorize was correct. The nature and the mass of data now available enables us to ask questions which were unsuspected by the investigators of the earlier periods. Finally, there was an emphasis on the problems of defining the nature of Classic period civilization. As will be seen, the concern with origins, expressed in a consuming interest in the Preclassic, has become extremely important in Maya archaeology recently. Since the Preclassic is the period in which so much that is crucial to culture change takes place, it may be seen that the Carnegie group also worked under something of a handicap of an imbalanced sample of information. Again, the earlier workers can hardly be faulted on this count. Indeed, many of the principal problems of the Carnegie group seem not to have been professional at all in substantive terms, but a matter of what we might call "image." The image was created to some degree by the exaggerations of the Kluckhohn-Taylor attacks. Another of their problems was simply the internal politics of the Carnegie Institution and the basic hostility of its administration to the anthropological aims of the Department of Historical Research.

In 1958 Proskouriakoff discerned the new style of research which would replace the old order under the Carnegie group. The new style had developed concurrently with the last years of the Institution's dominance of Maya prehistory, and indeed was somewhat fostered by the Institution's program of graduate traineeships. Proskouriakoff saw a shift from object-oriented research institutionally sponsored by museums to research in the hands of academic institutions. "In recent years the participation of students and faculty members of American universities in Middle American research has been greatly facilitated by the availability of short grants-in-aid provided by various foundations. The effect of this has been to reverse the trend toward specialization and to bring to bear on archaeological problems many varied interests." . . . "The rapidly changing organization of research is providing greater opportunities to younger investigators to gain a first-hand acquaintance with Middle American archaeology, and research is passing from the hands of a few experts who devoted their lives to a study of a single region to a far wider group of investigators with many varied interests" (1958:460, 462).

The third and present period of Maya archaeology may be characterized as university dominated, more anthropologically oriented, and more open to feedback from other parts of Mesoamerican archaeology. There is also more integration of the ideas and techniques of cultural ecology into research design. The remainder of this paper is an examination of the work which has been done in Maya prehistory in the last ten years and the general trends of both

theory and method, with an examination of important literature and delineation of some crucial research areas. Willey and Phillips have observed that all scientific endeavor may be divided into the three levels of operation: observation, organization, and explanation (1958:4). We will use this scheme as an organizational principle for an assessment of the state of the field.

OBSERVATION: FIELDWORK 1958–1968.

Fieldwork in the Maya area has fallen into three major categories during the recent past: A. Intensive site excavation, B. Limited excavation and testing, and C. Exploration and survey. The other overlapping categories of work are, D. Reconstruction, and E. Special. Three major ecological zones make up the Maya area, the lowlands, the highlands, and the Pacific coast. Culturally, these major areas seem to be largely contrastive and the following survey of fieldwork is made within this framework. Reference should be made to Table 1 for temporal distribution of projects.³

Referring to Table 2, it will be noted that considerably more fieldwork of all kinds has been done in the lowlands than in either of the other two areas. Furthermore, the geographical spread of work within the lowlands has been greater. The preponderance of the lowlands in intensive site excavations (Category A) is especially crucial, because this is the type of project producing the most detailed and greatest quantity of data. Thus the lowlands are not only better known in quantity but also qualitatively. In the number of projects involved, the disparity is not so apparent, but in the resources devoted to each area, there is a significant order of difference. The large and well financed project at Tikal has in many senses dominated the thinking of Maya archaeologists because of the sheer mass of information pouring from it. In the northern lowlands, the Dzibilchaltun work has achieved transcendental importance for the same reasons. Shorter, but relatively no less intense and important, projects at Palenque, Altar de Sacrificios, and Seibal have contributed to the quantum jump in data available to the Maya prehistorian. Limited excavation and testing programs (Category B) which were not necessarily connected to the Category A connection with the site excavations have filled in some of the gaps around these sites. Again the lowlands dominate the recent research scene. Only four projects were done in the highlands, and two on the Pacific Coast, compared to seven from the lowlands. On the other hand, the contributions by the highlands and Pacific Coast projects seem out of proportion to the effort expended there because of the lack of any kind of previous work in many regions within these two areas. Exploration and survey work is still quite an important and significant activity in Maya archaeology judging by the fact that at least ten known projects of this kind were carried out in the last decade. Unexplored

regions are by no means a thing of the past, and the work of Ian Graham demonstrates that gifted and dedicated individuals can still make significant contributions from outside the institutional framework (Graham, 1967).

The bulk of intensive work (Categories A and B) is now in the hands of academics overwhelmingly associated with U. S. universities (see Table 1 for institutional sponsorship). Of a total of 26 A and B projects, the pure research institution is represented by only two projects. Five projects were museum sponsored, and another was largely Mexican government sponsored, making a total of eight non-university projects, compared to 18 university sponsored investigations. In contrast, from 1924 to 1957, of 22 A and B projects, 13 were Carnegie digs, two by other research institutions, three museum sponsored, two university sponsored, one Mexican government sponsored and one by a business corporation.

On the other hand, it is interesting to note that while institutional continuity has been somewhat broken, the field is still dominated by persons trained in a relatively few schools and in some cases, by the former members of the Carnegie group. As examples of the latter, E. M. Shook was the initial project director of the Tikal Project while A. L. Smith has been, successively, field director of the Altar de Sacrificios and Seibal projects. R. E. Smith, J. E. Thompson and T. Proskouriakoff have all continued productive work. Domination of the field by Harvard trained individuals has lessened to some degree with the entry of the University of Pennsylvania into the field and its use of the Tikal Project for graduate student training. Scholars from other schools drawn by individual interest represent a third group from the point of view of training, and include such archaeologists as R. Matheny, and D. M. Pendergast. Alumni of the National School of Anthropology and History of Mexico are active but usually in administration and only Alberto Ruz has been active in fieldwork in the past few years. Considering that continuity of personnel and of training seems to insure continuity of theory and goals, it is not surprising that the Maya field shows an essential unity of interests and orientation among the present group of specialists.

A final point to be noted is that Tables 1 and 2 roughly demonstrate that as much field work has been done in the Maya area in the last ten years as was done in the preceding thirty.⁴ In actuality, improved field techniques and more massive organizational efforts have led to greatly increased production of data in relation to effort expended. It is to innovation in systematics and organization that we now turn.

THE LEVEL OF ORGANIZATION: FIELD SYSTEMATICS AND ANALYSIS.

In actuality, there is no hard and fast division among the levels of opera-

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tion which are here being used as an organizational device. Research design and field systematics to a great degree are organizational techniques which have a large influence on what form the data will take. The influence is even more substantial if analytical techniques are pre-selected and tied into a feedback system in which analysis goes on simultaneously with the fieldwork. Newly introduced and innovative approaches include the following:

TABLE 1

Temporal Institutional Distribution of Archaeological Fieldwork in the Maya Area

Categories of Projects	"Carnegie" Period		"Multi-Institutional" Period
	1924-41	1945-57	1958-68
A. Intensive excavation projects	*Chichén Itzá *Uxactun *Copan †Kaminal juyu (4, all CIW)	*Mayapan, CIW *Palenque, INAH *Barton Ramie, PM †Zaculeu, UF †Mixco Viejo, MH (5)	*Dzibilchaltun, MARI *Tikal, UM *Altar de Sac., PM *Seibal, PM *Altun Há, TM *Palenque, INAH *Comalcalco, AM †Iximché, IDAEH, Indep. †Kaminaljuyu, PaSt Bilbao, MPM Monte Alto, PM Izapa, NWAf (12)
B. Limited excavation projects	*Benque Viejo, CIW *Baking Pot, CIW *San José, FM *Lubaantun, BM †Zacualpa, CIW †Atitlan, CIW †Tajumulco, SAR Cotzumalhuapa, CIW (8)	*Tulum, CIW *N. Yucatan Gen'l, CIW *Caracol, UM †Nebaj, CIW †Tonalá, SAR (5)	*Jaina, INAH *Lake Petén, PM *Yaxhá, PM *Topoxté, PM *Makanché, PM Baking Pot, ROM San Estevan, ROM *Aguacatal, BYU †Chiapas highlands, UChi †Cotzal sites, UMinn †Acul, CNRS †Chajcar, UMinn La Victoria, PM Salinas la Blanca, Yale (14)
C. Survey and reconnaissance projects	*N. Yucatán, gen'l *Cobá *SW Campeche *Petén, gen'l	*Aké, CIW *Chacchob, CIW *Chiapas lowlands, Indep.	*Tikal, UM *Ikil, MARI *Campeche, MARI *Petén gen'l., Indep.

TABLE 1—(Continued)

Temporal Institutional Distribution of Archaeological Fieldwork in the Maya Area

Categories of Projects	"Carnegie" Period		"Multi-Institutional" Period
	1924–41	1945–57	1958–68
	†Central Highlands	*Tabasco, CIW	*NE Petén, PM
	†S. Highlands	†Chiapas highlands,	*Río Azul, PM
	Pacific Coast	UChi.	*Dos Pilas, IDAEH
	(all CIW)	Pacific Coast	*Pomona, INAH
			*Polochic, Yale
			†N. Quiché, CNRS
D. Recon- struction projects	*Chichén Itzá, INAH	*Uxmal, INAH	*Dzibilchaltun, MARI
	*Quirigua, CIW	*Kabah, INAH	*Tikal, UM, Guate, Govt.
	*Copán, CIW	*Sayil, INAH	*Seibal, PM
		*Labná, INAH	†Iximché, Indep.
		*Edzná, INAH	
		†Zaculeu, UF	
E. Special projects	*Hieroglyphic survey, CIW	*Bonampak, CIW	*Balankanché, MARI
			*Chichén well, INAH
			*Xutilhá, UM
			*Comalcalco, UOre
			†Amatitlán, MPM

1) Symbols:

- * Lowlands
- † Highlands
- Pacific Coast

2) Institutional Abbreviations:

AM	American Museum of Natural History
BM	British Museum
BYU	Brigham Young University
CIW	Carnegie Institution of Washington
CNRS	Centre National de la Recherche Scientifique, France
FM	Field Museum
IDAEH	Instituto de Anthropología e Historia de Guatemala
INAH	Instituto Nacional de Antropología e Historia, Mexico
Indep.	Independent
MARI	Middle American Research Institute, Tulane University
MH	Musee de L'Homme, France
PaSt	The Pennsylvania State University
PM	Peabody Museum, Harvard University
NWAF	New World Archaeological Foundation
ROM	Royal Ontario Museum
SAR	School of American Research
UF	United Fruit Company
UM	University Museum, University of Pennsylvania
UChi	University of Chicago

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UMinn University of Minnesota
 UOre University of Oregon
 Yale Yale University

3) This table does not represent an exhaustive listing of projects carried out in Categories C, D, or E. Certain Category A and B projects also included reconnaissance and reconstruction, for example, although I have not listed them under the separate categories. Further, many small research ventures, especially for the purpose of obtaining epigraphic material, are not listed. These were often matters of two or three days or less, and involved considerably less effort than those listed. The intention is to cover those projects which probably involved 95% or more of the time and effort of Maya scholars.

Settlement pattern studies. The reintroduction of settlement pattern studies into Maya archaeology was apparently sparked by G. R. Willey's innovative and successful use of them in the Virú Valley project in Peru. At any rate, at about the same time that Willey was himself applying the method to the Barton Ramie site in British Honduras, the Carnegie group incorporated the approach into their Mayapan work (A. L. Smith in Pollock *et al.*, 1962). It will be noted that the Barton Ramie project also marked the re-entry of the Peabody Museum into active fieldwork in the Maya area. Willey (1953: xviii–xix) has pointed out the ultimate roots of the concept in Julian Steward's theory. It has become one of the most effective instruments available for articulation of archaeological data and general anthropological theory. For example, the social structural implications of the distribution of settlement systems in a particular ecological setting have allowed archaeologists to get at much more of the non-material culture than was previously possible. Settlement pattern studies can also be regarded as the most culturally meaningful kind of cultural ecology since, as Willey defines them, they involve “. . . the relationship of

TABLE 2

Spatial Distribution of Archaeological Fieldwork in the Maya Area

Categories of projects	1924–41	1945–57	1958–68	
A	3	3	7	Lowlands
	1	2	2	Highlands
			3	Pacific Coast
B	4	3	7	Lowlands
	3	2	4	Highlands
	1		2	Pacific Coast
C	X	X	X	Lowlands
	X	X	X	Highlands
	X	X		Pacific Coast

aboriginal occupation to natural environments; the nature and function of buildings composing habitation communities; and the form, size, and spacing of these communities with reference to each other and to ceremonial centers' (Willey *et al.*, 1965:15).

The major published work now in print is the final report on Barton Ramie (Willey *et al.*, 1965). However, the techniques have been widely adopted in all of the major work carried on in the last ten years in the Maya area. Dzibilchaltun, Altar de Sacrificios and Seibal have all been sites where excavations in major architecture have been heavily complemented by extensive survey and excavation in domestic house structures. The Tikal project has expended a significant amount of its time and energies on such matters and has added immense amounts of ecological data in studying the sustaining areas around Tikal (Coe, 1962:502-504). These matters have led Maya prehistorians into detailed considerations of demography, class structuring, economic systems, the nature of social integration mechanisms, and other matters previously thought to be beyond detection in the archaeological record.

Ceramic technology studies. The innovative use of the theories and techniques of Anna Shepard by Robert Rands has at last operationalized this approach. Rands has largely dealt with the Maya ceramics of the Tabasco-Chiapas lowlands regions. His aims have been to detect in detail the major trading patterns *within* a major site's area of influence, to define their boundaries and therefore presumably define the political boundaries also. Rands has been most concerned with the major site of Palenque as a focal point and has used technological characteristics of domestic as well as of non-utilitarian pottery as his data. The major features of this work have been summarized in recent papers (Rands, 1967a, b). Economic systems information seems to be the major class of data directly derivable from this sort of study. There are implications, of course, for other parts of the culture.

The Type-Variety system of ceramic analysis. A direct importation from its origin area of the Southwestern U.S., the Type-Variety system has revolutionized ceramic analysis in the Maya area. James Gifford is largely responsible for the theoretical refinement of the Type-Variety concept and the operationalization of it in the Barton Ramie work (See Gifford in Willey *et al.*, 1965, and R. E. Smith, G. R. Willey and J. C. Gifford, 1960). Subsequent applications of the system to material from Mayapan, Altar de Sacrificios, Seibal, Tikal and from other smaller operations owe much to the flexibility and adaptiveness built into it by Gifford. The major aim and accomplishment has been to produce from the immensely complex Maya ceramic universe comparable analytical units. While largely a technical achievement in this sense, the system also thereby allows much broader syntheses of vast amounts of archaeological material (cf. Willey, Culbert and Adams, 1967) with all of the

culture-historical and processual implications that this allows. At the least, Type-Variety analysis allows an easier grasp of the data, and at the most, it affords great insight into cultural process. This last achievement comes through the sensitive and relatively secure comparisons made possible. An excellent example of this is the recent detection of essential identity of domestic pottery between the Maya lowland sites of Barton Ramie and Altar de Sacrificios and very early material on the Pacific slope of San Salvador (Sharer and Gifford). Since the complexes involved are the earliest known in the Maya lowlands (Xe and Early Jenny Creek) and presumably are associated with the earliest agricultural colonists of the Maya lowlands, this ceramic identity may well indicate one of the sources of the early Maya and of their cultural beginnings.

Field systematics at Tikal and Altar de Sacrificios. Systematic organization of notes, observations and of various kinds of data collection is an old problem in archaeology and one to which many more or less complex solutions have been applied. However, the Tikal project has faced a problem which is greater in magnitude than that probably faced in any other excavation in the new world. The problem was that of a long term excavation at a high culture site by many different people over a long period of time and of how to achieve comparability of excavation, observational and analytical units. As already noted, Tikal has adopted the Type-Variety system as part of the analytical problem. However, the project, largely in the persons of Linton Satterthwaite and Wm. R. Coe, has developed a field systematics which in its own way is as flexible and adaptive as the Type-Variety system. Briefly, the system builds on an excavation lot which is anything the excavator chooses to make it. This is the most flexible unit and occurs within a sub-operation and operation. An operation is the area or mound within which excavation takes place. A sub-operation is a specific trench, pit, or other unit. Lots are contained within the sub-operations. All units are numbered and lettered. Thus Operation 12 might be the excavation of structure 5D-16, Sub-operation A, a test pit in front of the structure, and the lots (say 1-23) the 25cm. levels within the pit. A single lot would carry the whole number, 12A23, and all material not unusual in interest or significance would be assigned the lot number. This is the basic organization of the Tikal system and these units are then manipulated and built into hierarchies of associated units which may have various sorts of implications. Thus, the observational unit may also become an analytical unit. At Altar de Sacrificios modification and simplification of the system was made, largely in dispensing with much of the ferociously elaborate accounting by cards, catalogues and charts used at Tikal. The principal advantage is again a system applicable with modification to all conceivable archaeological situations. It will allow adequate and full recording of materials for any sort of known analytical operation. At the same time, however, there is a danger inherent in

this intricate field system, and that is that the systematics will become reified. Compulsive theoretical symmetry can often stifle imaginative and pragmatic approaches to data without compensating advantages.

Willey and Phillips' standard archaeological units. Willey and Phillips (1958) have attempted a systematization and standardization of units of time, space and content in such a manner that they are not inextricably welded together. Such units as the region, component, and phase are examples. Again the large aim has been to produce comparable units and to facilitate communication among archaeologists about their common concerns. An important aim has also been to attempt to settle much of the preoccupation with systematics by producing a flexible system and thereby allow archaeologists to spend more time on explanatory level matters, those of culture process. The application of this system to the Maya area has not been universally accepted without grumbling. However, the criticisms of the system have been largely in terms of what the system did not attempt and was not, rather than in terms of what it did (see W. R. Coe's 1962 critique of the phase concept). In general, the benefit has been to shift attention to matters less of technique than of culture process.

In sum, most of the organizational level innovations in the Maya area are derived from the general field of archaeology, although the applications and refinements have been much more sophisticated than originally conceived of. The aim has in both cases been the eliciting of data susceptible to explanatory interpretations. This has been attempted through the effort toward more standardized, flexible, and comparable analytical and observational units. Somewhat paradoxically, therefore, immense concern with systematics and observation has led to a situation in which the archaeologist can free himself more easily from this level and concern himself more with culture process. Before turning to activity on the explanatory level, however, we must consider the impact of recent work in ethnology and collateral fields.

ETHNOLOGY AND COLLATERAL STUDIES' CONTRIBUTIONS.

Ethnology. Recent major projects in the Maya highlands of Chiapas sponsored by Harvard University and the University of Chicago have had influence on Maya archaeology. Most of the significance is based on an assumption of cultural survivals in ethnographic cultures from prehispanic times. Summarization of the results of these still continuing projects has appeared in various preliminary papers (cf. Vogt, 1964) and more recently in several monographs (cf. Vogt, 1966; Cancian, 1965). A major debate has developed around at least one aspect of the ethnographic material. Carrasco has pointed out the importance of the civil-religious hierarchy in Middle American cultures

generally (1961). The highlands of Chiapas studies have produced more finely detailed information on the local manifestations of this than is available for any other part of Mesoamerica. Cancian's study is the major published result, and Vogt (1961) and Holland (1961, 1964) especially have attempted to apply these data to the archaeological problem of social integration among the Classic Maya. Given the dispersed settlement pattern of the lowlands, the question is: how did the ceremonial centers command the loyalties of the sustaining population? It has been suggested that something along the lines of the civil-religious hierarchy and the cargo system operated also in ancient times. In such a system today, most or all of the men of the society participate in one way or another in a series of religious duties associated with a series of hierarchically ranked offices. One must fulfill the obligations of the lower offices in order to qualify for the higher ones. Thus, theoretically, a man can rise in his lifetime from the status of simple corn farmer to the most respected and influential individual in the community. These duties must be performed in the ceremonial center. This is a partial answer to the present day problem of social integration of dispersed hamlets and population with the ceremonial center, and it is suggested that it was at least part of the ancient solution to the similar problems. While perhaps over-enthusiastically accepted by archaeologists at first (Willey, 1956) the idea has merit. However, the system seems inappropriate to the Classic period of the lowlands for which there are strong indications of the presence of an aristocratic class, if not caste (Proskouriakoff, 1960; Kelley, 1962). In addition there is evidence for class ranking by occupational specialization (R. E. W. Adams, in press a). Others have argued similarly from other evidence (Haviland, 1966). Thus, while the Chiapas material may well explain certain prehistoric patterns in pre-classic times (Bullard, 1964), it is at the most only a partial explanation for Classic period and thereafter. Even so, it is a great contribution if only as a stimulus to further collaboration between archaeologists and ethnologists. It has also furthered the interest in reconstruction of social structures by archaeologists (cf. Haviland, 1966).

Charles Erasmus has carried out some interesting experiments with monument building as a means of estimating the amounts of man-days involved in construction of Maya ceremonial centers. Erasmus hired peasants in Mexico to excavate and carry rock and dirt. They also did some sculpting under his direction. Having determined labor represented by metric units of fill, sculpture and masonry, these figures were divided into the estimates of the architectural elements at Uxmal, yielding a total of $7\frac{1}{2}$ million man-days. Uxmal was occupied about 250 years and the annual man-day investment computed. This figure, compared with population density estimates, could help fix the number of man-days per year invested by each household. The annual household labor

contribution was compared with those known for communal projects in societies of varying degrees of social organization to estimate the extent of associated political development. Erasmus concluded that the Maya ceremonial centers could have been achieved with a level of social organization approximating that of the chiefdom as defined by Service, and that no greatly centralized authority need be postulated to account for the Maya monuments, (Erasmus, 1965).

The conclusion drawn from the experiments is interesting and I have no objections to it per se. However, the study takes no account of the following data which imply a higher level of political organization. There are records of dynastic succession in the hieroglyphic texts which would have a strong correlation with a ruling caste or class. Secondly, there are, as noted above, strong implications of occupational specialists of high skill and arcane knowledge who were involved in the construction of the ceremonial centers. Third, the occupation estimate for Uxmal is pretty speculative. Finally, it must be observed that Erasmus himself in his experiments acted as the repository of skills and knowledge and as the directing agency, roles which would ordinarily be fulfilled by specialized classes. Craft specialists and dynasts taken together are more typical of states than chiefdoms. However, it is flattering that a social anthropologist is interested enough to concern himself with prehistoric data and this is certainly a trend to be encouraged.

Ethnohistory. At the moment, the Maya field lacks the specialists in historical studies that it once had in the persons of Scholes, Roys, and Miles. Through relative inactivity or death these persons have been removed. However, more scholars than before seem to be taking an interest in historical data and its applicability to questions of prehistory. Occasional papers by specialists in other topical areas, such as M. D. Coe (1965b) contribute stimulating ideas to the interpretation of prehistoric data. Ruben Reina has engaged in studies in the Archives of the Indies. Alfredo Barrera Vasquez (1965) and Robert Carmack (1968) have produced recent work in the field. Only Carmack's, however, has immediate applicability to archaeology. There is little doubt that the resources of the Spanish historical documents and the native documents still have to be exploited to their full potential. Probably one reason that they are not has been the relative lack of archaeological work in the areas from which much of the historical material derives, i.e., Northern Yucatan.

Agronomic and palynological studies. The Carnegie program emphasized information on modern crop yields, cultivation methods, labor investment, etc. This interest has carried on in Maya studies. The most important recent studies done in this field have been in the southern lowlands and by an agronomist (U. Cowgill) and an ethnologist (R. Reina). Cowgill's findings that, using the swidden system of cultivation, the southern lowlands could indefinitely

sustain a population of 100–200 per square arable mile is an extraordinarily important conclusion (1962:277). This bears directly upon the question of the failure of Maya classic culture since it seems to refute the probability of ecological disaster as a single cause. Combined with the recent evidence from the Pasion River sites (Altar and Seibal) of cultural and historical reasons for the collapse, it makes the problem more susceptible of solution. However, Cowgill's findings (1961, 1962) have been questioned by Reina, not so much on the productivity of milpa agriculture, as on the amounts of time that a man engaged in it has left for non-subsistence activities. Reina argues that disruption of the milpa cultivation cycle, a delicately timed sequence, could lead to catastrophic results (1964).

Cowgill (1961, 1966) has also introduced for the first time in the Maya area the authentically new technique of palynology in reconstruction of previous climates and ecology. Her conclusions, based on a core from the southern lowlands, was that the ecological situation is not distinguishably different now than in ancient times. In other words, no ecological shift of any large consequence was detected by this method. Again, the primacy of ecological factors in the matter of the collapse in the terminal classic period of the lowland Maya seems to be ruled out. Obviously, the sample is inadequate, but the initial effort shows the way, as well as the possibilities and the enormous importance that the information may have for culture-historical problems of the Maya.

Tsukada and his associates at Yale have done some coring in the Guatemalan and Salvadorean highlands (1967). The results are somewhat equivocal in that, simply, investigators are not yet well enough acquainted with what the shifts in the exotic flora mean. However, grass pollen sharply rises at one point and is surely indicative of maize cultivation. An unpublished core from the vicinity of Nebaj in the north highlands is still in the limnological laboratory at the University of Minnesota with only preliminary analysis done. The value and pertinence of information yielded by agronomy and palynology are indisputable. The problem now is to build the sample of information to achieve adequacy of spatial distribution and interpretative control.

Other physical analysis techniques: Radio carbon dating, neutron activation and obsidian hydration. Although some disquieting doubts have been cast upon the radiocarbon technique, it is still generally accepted as the most reliable independent method of placing archaeological material in time. An effort at solution of a major problem in the Maya area, that of correlation of the Maya and Christian chronologies, has been attacked by this means. The Tikal project systematically sampled wood which was highly likely to have been cut and used at the time of construction of some large buildings. The wood was used in the buildings and the structures included Maya texts giving what are presumed to be the dedicatory dates of the buildings in the Maya system. A large number

of samples were run in order to guard against idiosyncratic circumstances (W. R. Coe has termed these "placement history") influencing the dates undetected. The decision was overwhelmingly in favor of a correlation equating 11.16 in the Maya system with 1539 A.D. in the Christian calendar (Satterthwaite and Ralph, 1960). Falling as it does with the weight of other evidence, most Maya archaeologists have gratefully accepted this as the resolution of a disquieting, thorny, and arcane problem. However, the question is still an open one due to the fact that new information from Tulane's Dzibilchaltun project has led E. W. Andrews to seriously reconsider the Spinden correlation (12.9 = 1536 A.D.). This matter will be discussed at more length below.

Undoubtedly the use of the new physical techniques has aided archaeology. The dating of cultural material by radiocarbon, however, has often raised nearly as many questions as it has answered. There is dissatisfaction with it primarily because one is not directly dating the cultural material but organic substances perhaps associated with it. The possibility of dating potsherds themselves by a cheap and simple method is a dazzling one indeed (MASCA newsletter, Dec., 1965, Vol. 1, No. 2, p. 2). In the end, however, the evaluation of the importance of the data depends strictly on the control that the archaeologist has of other more conventionally derived data. As an example, Altar Group Fine Orange pottery has been found in stylistically identical form at both Kixpek in the Guatemalan highlands, and at Piedras Negras in the southern lowlands. Neutron activation of the pottery from the two sites showed that it is identical in trace element composition. At this point the investigators made the interpretation that the lowland pottery had been introduced from the Kixpek area because Fine Orange pottery is quantitatively rare at Piedras Negras (Sayre *et al.*, 1958, and cited in Rainey and Ralph, 1966). In actuality, the correct conclusion was that the pottery at both sites had been manufactured at the same place. However, the archaeological record shows that this particular pottery was manufactured at neither Kixpek nor Piedra Negras, but probably in the Gulf Coast area of Tabasco (Berlin, 1956). Unfortunately, Fine Orange pottery from Tabasco, where it is present in overwhelming quantities, was not analyzed. This case shows how erroneous conclusions may be drawn from sophisticated analyses if the interpreter lacks sufficient expertise in control of the standard literature.

In summary, nothing revolutionary comparable to the effect of the initial introduction of radio carbon dating into Maya studies has occurred. The apparent new possibilities of physical analytical techniques would seem to be something in the line of thermoluminescence or obsidian hydration. The former is apparently still unreliable and the latter is highly suspect due to large anomalies in dates (Evans and Meggers, 1960:533).

THE EXPLANATORY LEVEL.

At this point it should be stated that the writer views the explanatory level of operation as being at least a two step process. The first and indispensable step is explanation on the specific level (reconstruction of a burial event, for example). The second step is generalization by comparative means from the specific reconstruction. Obviously, this investigatory continuum may be broken down into more than two steps, but for our review purposes the two are regarded as sufficient. We shall treat the major problems of Maya prehistory and recent progress toward explanation of them roughly in a temporal order. Obviously, this list is not exhaustive of the problems or their implications, but it is a framework within which we can touch upon all of the significant work of the past ten years. Necessarily, most of the review touches upon work of the first step since implications of the second level are of culturally broader scope than our hints allow here.

1. *The origins of settled life in the Maya area.* It seems clear on the basis of work by MacNeish and his colleagues (1967) that agriculture did not originate in the Maya area but in the central Mexican highlands. Recent work by M. D. Coe and K. V. Flannery (1967) have found what may be the Maya area's earliest settled communities on the Pacific coast of Guatemala, dating from ca. 1200 B.C. Although probably partly agriculturally based, a great deal of food must have been derived from local wild resources. This would tie in with MacNeish's suggestion that sedentary life was initially based on non-agricultural resources along the coasts of Mesoamerica. Only after agriculture was developed in the highlands and spread to the lowlands did the lowland interiors become inhabitable by sedentary life (MacNeish, 1966). Robert Sharer's recent work (1967a, and Sharer and Gifford, in press) has demonstrated early occupation of the southern edge of the Maya highlands. However, work by R. E. W. Adams in the northern highlands of Guatemala and by the larger University of Chicago project in the Chiapas highlands (R. M. Adams, 1961: 342-4) indicate an essential vacancy of much of the Maya highlands until relatively late in the preclassic period. In some cases no occupancy is indicated until perhaps as late as the time of Christ. Derivation of the Pacific coast ideas in ceramics appear to be from the Mexican West. The earliest settlements of the highlands are physically adjacent to these coastal zones and seem to be derived from them. The initial populations of the Maya lowlands are a different problem. On the other hand, a strong ceramic linkage between the early Salvadorean material of about 600 B.C. and the Xe material of the Maya lowlands indicates contact. If the adjacent highlands are largely vacant at the time of the earliest complexes now known in the lowlands, then we cannot derive cultural ideas nor people, nor agriculture from these same vacant areas. The

Xe-Salvadorean contact probably indicates one source. However, there are some material culture ties also to the Chiapas lowlands which might indicate the Mexican gulf coast plain lowlands as a source region. No aceramic sedentary settlements of any kind are known from the Maya lowlands. The earliest remains from the northern lowlands are not ceramically close to those of the southern lowland (Xe complex) nor to any other complex in Middle America. Several problems therefore cluster about the origins of sedentary life in the Maya area. The demonstrable assumption that is made here, of course, is that the later Maya civilization was based upon agricultural sedentary life. Derivations from various areas on the basis of material linkages are explanations on the first step.

It is noteworthy that the known early settlements of the southern lowlands are located along rivers (Barton Ramie, Altar de Sacrificios and Seibal). Sanders and Price in their cultural ecological explanation of the evolution of Mesoamerican civilization argue that this was no accident. They point out the agricultural advantages of alluviated flood plains versus the interfluves in swidden agriculture. They argue that chiefdoms with their higher density populations and economic advantages would develop first in these ecological niches (1968:131). This is a second step explanation since it is tied in with a very complex and more wide ranging theoretical system. It must be considered, but as yet our data seem to be inadequate.

2. *The origins of civilization in the Maya area.* Two major groups of theories of the rise of high cultures in the Maya area dominate this question. The first group argue basically for the donation of at least part of the content of Maya civilization from elsewhere in Mesoamerica. It might be argued that these explanations are akin to those advanced more than a century ago in which various old world cultures were regarded as the originators of New World high cultures. In any case, content similarities between the early preclassic lowland cultures and those of the highlands of Mexico and Guatemala led in the 1930's and 1940's to an assumption of a developmental connection. This seemed reinforced by the stylistic similarities of the earliest sculptural styles in the Maya southern highlands and lowlands, and the apparent temporal priority of sculpture in the highlands. Recent research in the Maya highlands already mentioned above has indicated that most of the highlands adjacent to the lowlands were vacant or culturally feebly developed when the lowlands was already selecting and developing patterns to be fully exploited in the Classic period. Considering the outside areas which would be most likely to influence Maya culture, we begin with El Salvador. Sharer's sequence in El Salvador seems to be largely reflective of the southern Guatemala highland cultures with little indication of contact with the Peten. However, a rather sudden burst of Usulután related pottery and subsequently polychrome pottery in the Maya lowlands about the

time of Christ does seem to indicate a resumption of strong interaction with Salvador again (R. E. W. Adams, in press b). Usulután is definitely earlier in Salvador than in the Maya area proper and polychrome may well be. However, the basic patterns of Maya high culture, and the ceremonial centers may well be in operation by this time. This last assertion is largely based on the Tikal project's findings (W. R. Coe, 1965a) and those from Altar de Sacrificios and Seibal.

A strongly argued origin theory for Maya high culture has been advanced by M. D. Coe, who would make the Olmec (Tenocelome) culture ancestral to that of the Maya. Coe has argued that indeed Maya sculpture is derived from styles themselves derivative from the older Olmec (1965a:773). However, there is as yet an insufficient body of Maya sculpture and stucco modeling from this period to judge whether this is really a tenable view or not. Most recently Coe has suggested that the Olmec themselves were Maya speakers and that they displaced into the Peten upon the breakup of the Olmec state or states. "What had once been Olmec civilization eventually transformed itself into the Maya civilization" (1968:118–121). Legacies of at least the art style and the calendrical system came from the Olmec. Data from both Altar de Sacrificios and Tikal seem to be against this postulated cultural origin. The earliest pottery from the Maya area, the Xe complex, dating around 800 B.C., shows few or no ties with the ceramics of the Olmec heartland. The later Maya preclassic pottery shows even less linkage with Olmec pottery (Willey, Culbert and Adams). The Tikal data would suggest, on the other hand, that there is a large possibility of at least some regional variants of Maya culture developing many of its sophisticated traits in situ (W. R. Coe, 1965).

The other class of theory concerned with the problem of cultural development is that which suggests a largely autochthonous basis for Maya civilization. Both the Tikal and Dzibilchaltun material has been used to argue this point of view. Andrews sees little connection of any sort between his early material and any area outside the Maya lowlands. At Tikal, W. R. Coe argues from his massive but localized body of data that Maya lowland civilization is largely an in situ development. This argument is based mainly on the lack of any strong connectives between late preclassic Tikal and extra-Maya cultures. However, it is in this late preclassic period that Altar de Sacrificios, Seibal and Barton Ramie show strong connections with Salvadorean ceramic traditions.

All things considered, it seems likely that much more complex mechanisms and processes are at work rather than just donation-reception, or in situ evolution. There is no question but that the Maya area as a whole was differentially affected when it was influenced from outside. In turn, the Maya regions may have entered into a differential interaction and development pattern. This would fit into G. R. Willey's (1962) and Sanders' and Price's (1968) con-

ception of the origin of Mesoamerican civilizations as largely a result of interaction of cultural diversity at high intensity and frequency. Sanders and Price have integrated this view with a total explanation of the evolution of Mesoamerican civilization based on cultural ecology. A major conceptual tool used in this study is that of the symbiotic regions, which are combinations of interacting and contrastive ecological zones. Whether or not this complex explanation is eventually generally accepted, the fact is that single cause explanations have proven unsatisfactory. In this case, and in others to be considered, a multi-factor explanation of some form would seem to be demanded.

3. *The problem of the nature of Classic period society.* This is in reality a vast closet full of problems. However, major foci of the past ten years have been on the relationships between settlement pattern, demography, and social structure. There has also been an attempt to delineate possible class related occupational specialities. All of the recent major projects have included in their aims and methods attempts to estimate population, its distribution across the landscape, and its relationships with the nucleated centers called ceremonial centers. Major advances have been made in these areas. If exact figures are not correct and indeed may be precisely unattainable, it seems likely that we now at least know the correct dimensions of populations. In other words, we can argue with some confidence that we are dealing with less than 10,000 persons in the case of a middle sized center such as Uaxactun (R. E. W. Adams, ms.). Certain difficulties are inherent in population estimates. These are discussed in detail in the final Barton Ramie report, but they include the assumptions that all housemounds are known, that each mound represents a biological family, and that ethnographic household sizes are approximately the same as those of ancient times. In addition, there is the unmentioned difficulty that the assignment of housemound occupations is on the basis of ceramic dating. Most ceramic complexes as now known in the Maya lowlands seem to represent long enough periods of time for several generations. In other words, an assumption of contemporaneity of occupation of several housemounds on the basis of similar pottery content might be lumping together what were actually sequent occupations thus increasing the population estimates by as much as two, three, or even four times the actual figure. However, all of these difficulties can be overcome with present archaeological techniques, and some approximation arrived at. It seems comforting that the archaeological estimates are at least falling within the limits set by agronomic studies.

Willey, et al., (1965) estimate a peak population of about 24,000 for the Belize River Valley in Late Classic times. One point that has never been made explicit is that Belize Valley, while representing an ecological situation unusual in the Maya lowlands, is for this reason an unusually favorable region in which to associate sustaining population with ceremonial centers. In most of the Maya

lowlands, dispersal over all available arable land is the norm with the result that it is difficult to define boundaries of influence of particular ceremonial centers.⁵ In the Belize area, however, the housemounds are at least nucleated into a ribbon-like strip. This strip can be segmented according to proximity to the ceremonial centers in the Valley and estimates arrived at as to how many people would go with a particular center. The Belize centers seem to have averaged around 6,000 persons as supporting population. In the case of large scale centers, such as Tikal, it may be that we are dealing with less than 40,000 in terms of a population which had intense interaction with the center. E. W. Andrews has argued that Dzibilchaltun evidence indicates a total population for that site of perhaps as many as 250,000 persons, or about 1,000 per km². Andrews is unhappy about this high figure, however, and argues simply that a high and urban level of density is present in the north during the period equivalent to Classic in the southern lowlands (1965:36–38). And so on. The order of magnitude of a population obviously has some relation to the sophistication of social structure. The density also has considerable effect, argue Sanders and Price (1968, Chapter 4). However, these rounded off figures are misleading in the sense that places like Tikal, dependent on the political situation, may well have drawn for resources on a much greater population. In other words, tribute and services from Uaxactun may have supported Tikal as well as Uaxactun itself. Centers of the magnitude of Tikal, however, are relatively rare in the lowlands, only about six being known at present. However, intensive survey may well indicate that previously known sites are much larger than thought. Very large sites indeed are still being found, that of Mirador being the prime example (I. Graham, 1967:41).

Bullard's study of the Northeastern Peten (1960) is an example of the extremely useful data to be gained by extensive survey. The data from this study of house structures and their relationships to various dimensions of ceremonial centers fits well with the results of the Barton Ramie, and the recent agronomic studies (Willey, *et al.*, 1965:578). All of the above again emphasizes the complexity and challenges of work in the Maya area. Obviously, population estimates cannot be applied wholesale from one zone to another nor from one region to another. Again, we are confronted with the necessity for careful regional sampling as a prelude to future areal synthesis. The testing of such interpretations, as those Sanders and Price advance, await a larger and more detailed spread of data. There seems little doubt, however, that we are presented with confirmation of ranked importance of the Maya ceremonial centers judged initially by the size of the ceremonial center and now upon relative population size. In most cases it is also apparent that we are dealing with the late segment of the Classic period in survey and that excavation is vitally necessary to arrive at estimates for the earlier periods and demographic

curves, however impressionistic. Note that all settlement pattern work of an intense nature has been confined to the lowlands. Based on my own unpublished work in the Cotzal Valley, it is quite feasible to make domestic residence counts in the Maya highlands. Implications of these kinds of demographic data are large for social structural reconstructions. Quite obviously, if Tikal has a minimum resident population of 12,000 during the Late Classic, there was a still larger sustaining population by several times. Further, these numbers by themselves then imply larger than tribal or even chiefdom-sized societies (Sanders and Price, 1968: Fig. 4). They are involving numbers of people possibly living with a state organization. Other data must be fed in at this point in order to select among the possibilities.

Leaving the matter of population estimates, M. D. Coe has attacked the problem of social structure by ethno-historical means with an examination of a certain set of ceremonies occurring at the end of the Maya year. Shifting of ceremonial duties and supernatural powers from one to another of the four quarters of a Yucatecan town took place at this time. Coe argues that political power may have shifted along with ritual distinction and that this continual rotation system was a means of social integration (1956). Coe further suggests that there is reason to believe that this pattern was an ancient one among Maya.

Proskouriakoff's identification of historical data in Maya writing (1960) is discussed at more length below. However, the elite class genealogies arranged in dynastic successions, and the lists of conquests associated with rulers have far reaching implications for the nature of Classic society. Elite class kinship patterns will no doubt be further elucidated by this means along with indications of alliances among the elite groups of various centers.

An attack on the problem of Classic period occupational specialization has been made by considering the complexity of artifact forms and their quantities. Combined with depictive evidence from sculpture and murals, a series of occupational specializations, full or part-time, have been outlined. These are ranked according to the degree that they demanded elite class knowledge and also considering the degree of intimacy of contact with the elite class demanded by the specialty. It is suggested that this ranking gives a skeleton outline of Maya Classic society although undoubtedly factors other than occupation determined a person's status. It is argued that Maya society was much more complex than just a two or three part society (R. E. W. Adams, in press a).

Reconstruction of an elite class burial at Altar de Sacrificios has a number of implications for the nature of Classic society in the southern lowlands (R. E. W. Adams, in press b). It would appear that Maya political units were tied together on the basis of marriage alliances and kinship ties. High status representatives from the sites of Yaxchilan and Tikal attended the funeral of a middle aged woman at Altar de Sacrificios in 754 A.D. The deceased was me-

morialized by massive renovation of a temple structure. This might suggest that in some cases we have matrilineality in the Classic period in certain zones and in the elite class. If Maya centers were indeed tied together in this ephemeral and shifting manner it would seem well reflected in the regional styles of polychrome pottery, monumental architecture, sculpture and so forth. The fragility of larger than regional sized political units would seem to be another implication.

Wm. Haviland has recently (1968) published a tour-de-force application of ethno-historical, and ethnological evidence and social structural theory to the archaeological remains at Tikal in an attempt to reconstruct ancient Maya lowland social organization. The basic theoretical tools are derived from Murdock's cross-cultural social structure study (1949). Two principles upon which Haviland depends heavily are 1) the principle of limited possibilities of change given the starting point of a particular social organization, and b) the retention of features from former social organizations into later systems. The latter principle allows for reconstruction of past systems by use of the apparent inconsistencies in the system for which most data is available (usually the present day or contract period form). Using the inconsistencies in cousin terminology and residence patterns of the 16th century Maya, Haviland reconstructs the following sequence of social organizations for the lowland Maya: a) Normal Hawaiian, b) Matri-Hawaiian, c) Patri-Hawaiian, and d) Normal Guinea (16th century Yucatan). Unfortunately, there is no archaeological evidence at Tikal for stage a, and little for stage b. Haviland marshals considerable evidence for stage c at Tikal in the form of 1. residence groups which by their nature could be patri-clan loci; 2. the fact that all elite tombs at Tikal contain only men after 25 B.C.; and 3. the fact that only men are depicted on Tikal stelae.

The aims of this study are laudable but the detailed terminology and scholarship are flawed. Haviland has to refute preceding studies by Roys and M. D. Coe which suggest the possibility of double descent among the Maya. The refutation is based principally on the fact that incest taboos are not extensive on the matrilineal side (Haviland, 1968:101). This is not a valid objection inasmuch as it assumes a functional equivalence of both sides. In classic double descent situations the sides are not functionally equivalent. The marriage rules may be handled by the patrilineal side and religious affairs by the matrilineal side. The Maya could simply have assigned distinct functions to the two sides, the matrilineal side not handling marriages and therefore not having any functional necessity for extensive incest taboos. On the other hand the Altar burial noted above seems to suggest a possibility of matrilineality among the elite class members involved.

A cautionary attitude is needed when dealing with elite class kinship arrangements, as Haviland points out (*ibid.*). Where royal lineage is involved,

manipulation of standard rules of behavior may take place in order to assure continuity of power for the established kinship unit. There may also be a more complex kinship system in operation among elite groups than is in operation among other segments of society. In an elite class, kinship complexities have varying functions, but a prime one is to ensure the orderly passage of political power. Proskouriakoff (1960) and Kelley (1963) have shown that the Maya maintained extensive genealogical data for this purpose. However, the manipulations to which such royal lineages are subject should not be taken as indicating general rules necessarily widespread within the society. The stakes were higher at upper social levels and therefore the motivations for more complex rules and for bending those rules were greater. Therefore a large part of the Tikal data may refer only to elite class material and not be congruent with the 16th century Yucatec material.

Haviland says that the Tikal settlement pattern data indicates that the usual residence unit size represents from 4–8 households. Haviland argues that this size unit is more likely representative of a clan kinship unit than of a lineage (1968:109). This argument is absurd. The lineage is a vertically organized group with usually only three generations in existence at one time. Therefore there are limited numbers. Groups of from 4–8 households are excellent candidates for lineages. Clans are numerically much larger units. It is possible that Haviland's use of inherently contradictory terminology leads to some of these tangles. In this most flagrant example, he favors the term "cognatic lineage" (111) which is really a paradoxical conception combining laterality with verticality of organization. What really seem to be referred to are non-unilineal descent groups.⁵

Reference to extra-Tikal evidence is cursory and inaccurate in at least one case. Haviland characterizes Barton Ramie as lacking detectable residential groupings throughout its sequence (1968:114). However, the first two phases of the sequence do show possible groupings of residences (Willey *et. al.*: 562–563).

It is particularly unfortunate that this study is so flawed because it is the first full-scale attempt at such social structural reconstruction from archaeological as well as other evidence. Such an attempt is too important to risk discreditation on technical grounds.

Reviewing the above it would seem that Maya archaeology is on the verge of making a fine scale and longitudinal definition of the nature of Maya society. However, it also has been well demonstrated that expertise in complex matters of kinship, social structure must be brought into the picture. There is little doubt but that more intense collaboration between prehistorian and ethnologist would be of great benefit.

4. *The shift in quality of Maya civilization between Early and Late Classic*

periods. The role of Teotihuacan in Maya culture history. There seems to be little dispute over the distinctiveness of Maya culture between the Early and Late Classic periods, especially in the Peten. Although this is a change in degree, to an undetermined extent it is also a change in quality. G. R. Willey, among others, has suggested that this change in quality is tied in with the phenomenon of selective influence from Teotihuacan. This influence has particularly been noted at the site of Tikal in both sculptural and ceramic forms and styles, but also in architecture (W. R. Coe, 1965b:35–37), and seems to fall into two phases. The first phase is the strongest and may result from politico-military domination of the site by persons from Teotihuacan. The shift in Maya culture from mainly small sized to many large sized ceremonial centers occurs about this time. Architecture becomes more massive and there is more of it. In view of the fact that most of the formal architecture is functionally associated with the elite class, it can also be inferred that there was an impressive expansion of the elite class itself along with the supporting social apparatus; bureaucracy and so forth. The as yet unpublished Tikal data especially bear on these questions. They are crucial to considerations of culture process. Sanders and Price make the suggestion that it was only under the stimulus of a “hydraulic state” that the swidden based Maya state would develop from a chieftain level of social organization (1968:204–6). It is of interest that in other intensive work in the Peten and other parts of the Maya lowlands, Teotihuacan influence has been either absent or feeble (Altar and Seibal preliminary reports).

A project now (1969) under way at the highland site of Kaminaljuyu should shed considerable light on the nature of Teotihuacan influence. Ever since the detection of the strength of the domination of Teotihuacan culture (Kidder, Jennings and Shook, 1946), a series of questions has remained open. A major point to be clarified is that of ties, if any, between the lowland and highland expressions of influence in the highland Mexican classic on Maya culture. Specifically, for example, was there any connection between what went on at Uaxactun, Tikal and Kaminaljuyu? Settlement pattern shifts at Kaminaljuyu from the Miraflores phase to the Teotihuacan dominated Esperanza phase seem to reflect a basic reorientation of society (W. T. Sanders, personal communication, 1969). Does a similar reorientation take place in the lowlands under Teotihuacan stimulus?

5. *The failure of Classic civilization.* Nearly all of the recent work in the Maya lowlands has had to deal with this major problem. Nearly all of the projects have produced greater or lesser amounts of information bearing upon it. Only some of this has reached print in preliminary form and, unfortunately, has produced acrimonious and polemical debate (Sabloff and Willey, 1967; Binford, 1968, and Erasmus, 1968), not all of which has a direct relationship to the problem.

Prior to 1958 most of the explanatory theories of Maya collapse fell into what Elman Service has recently termed the “prime mover” category (1969). That is, cause and effect relationships between a single postulated major event or process such as soil deterioration, or revolt of the peasantry against the elite were argued. (See Thompson, 1965:344–349; and G. L. Cowgill, 1964, for resumés of these theories.) This is possibly because work prior to 1958 turned up little direct evidence about the nature of the collapse; simply that it had occurred. The two outstanding features of the collapse are the abandonment of the ceremonial centers and the depopulation of a large part of the southern lowlands. Lacking much evidence of a cultural nature to explain the event, one of the most important types of explanation has been ecological. However, the agronomy studies of the 60’s throw doubt on these because they have suggested that up to 100–200 persons per square arable mile could be indefinitely sustained by swidden agriculture, possibly over the maximum population figure in ancient times (U. Cowgill, 1961). The settlement pattern studies of all projects, however, leave no doubt that population maxima was reached in all known regions long before the end of the Classic (cf. Willey *et al.*, 1965). Whether or not this eventually strained the ecological limits is difficult to say at present without more data and more refined estimates. Haviland’s physical anthropological data from Tikal would seem to indicate poor nutrition for all classes as the Classic period went on (1967). F. Saul’s studies of the Altar de Sacrificios skeletal material (in press) suggests malnutritional diseases toward the end of the Classic. Recent studies at Tikal, however, indicate that there certainly was more food material in the lowlands than has been previously recognized. Ramon beans and root crops are argued to have been at least major supplements to the ancient diet (Haviland *et al.*, 1967; Bronson, 1966). It may well be that the burgeoning of the elite social segment and its demands produced a superstructure too heavy to bear. However, these are all variants of arguments used before. The latest information opens the possibility of a more complex and plausible theory of the Maya collapse which would incorporate much of the previous evidence and interpretation.

There is increasing evidence for militarism in the Late Classic of the southern lowlands and even from the intermediate area (cf. Piña Chan, 1964). The great ditch of Tikal (Puleston and Callander, 1967) is explainable only as a fortification. The next to the latest ceramic phase of Altar de Sacrificios in the Pasion Valley shows a reorientation toward militarism and a loss of interest in previously absorbing elite class concerns. Activity in production of stelae and monumental architecture at Altar as well as at most Maya lowland sites lapses or becomes feeble (R. E. W. Adams, in press b). There is definite evidence of a takeover by a non-Peten elite group at the site of Seibal, also on the Pasion River (Sabloff and Willey, 1967:323). This is the one major southern lowland site to actually undergo a florescence during this terminal Classic period. All of

these factors combined with the existence of a late classic fortress site at Becan in the intermediate zone (Ruppert and Denison, 1943:54 *et passim*), suggest a militarily competitive atmosphere within the Maya world in the terminal Classic period. Finally, definite evidence of a military intrusion by a non Maya group into the southeastern lowlands and the occupation of Altar de Sacrificios by this group suggests that military pressures from the outside may have been involved. These events have been differently interpreted by Willey and Sabloff (1967) but they accept the implication of extensive military activity. Obviously this activity is not an adequate or final explanation of the Maya collapse. Militarism seems often to have been the result of competition in ancient Mesoamerica (Sanders and Price, 1968:cf. 210). The nature of the competitive situation in this case is not yet clear, but population pressures might have been involved. Judging from what is going on in the rest of Mesoamerica at this time, however, militarism may well be a result of a competitive situation ultimately located outside the Maya zone, in central Mexico. In other words the reverberations of the central Mexican collapse in the form of displaced militaristic groups and militaristic influence on Maya culture may be what we are witnessing here. In any case, it seems likely that combined with militarism, the circumstances of collapse were different and perhaps even unique from zone to zone and even from site to site. G. Cowgill (1964) and Vogt (1964:40–41), reach roughly the same conclusions from somewhat different data. A fuller and more complete examination of the situation in the Pasion Valley sites will be presented in forthcoming reports on Altar de Sacrificios and Seibal. Synthetic papers will no doubt be based on these new data when fully presented.

The Dzibilchaltun information as presently available seems to reflect a lack of disturbance at this period. That is delayed for several centuries, until the Toltec invasion.

A final complexity might be mentioned here. Pollock in a recent article (1965:393, footnote 27, and 1952:238–239) has mentioned the intriguing possibility that the Toltec Chichén period representing the aforementioned intrusion, might actually date from 200 years earlier than generally thought. This would possibly make it coeval with the southern lowland, and suggests an involvement, direct or indirect, of the Toltecs in the Classic collapse. There is little question but that more intense work is indicated in the zones in which there seems to have been military activity and from which the invaders may have come, the intermediate zone and the Gulf coast of Tabasco and Campeche.

Again we are faced with a much more complex and challenging set of problems than originally thought, and again the problems, hypotheses, and explanatory theories are a direct result of new data.

6. *The Post-Classic transformations of Maya culture.* Work in the southern lowlands has touched little upon this period. Most of the culture and popu-

lation seems to have disappeared within a century or so of the collapse. The population lingered on in some regions, and at Barton Ramie there is perhaps a maintenance of the population maximum. In the Pasión Valley, in contrast, there seems to be a relatively rapid and complete depopulation. G. Cowgill (1963) has defined the ceramic sequence for the Lake Peten region for the Post-Classic in an attempt to fill the gap between the Classic culture as known and the historic Itza conquered in 1697. Recent small scale work by Bullard has continued this task. Bullard has also added a great amount of information by excavations at heavily Mexican influenced sites at Topoxté on Lake Yaxhá to the east of Lake Peten. This would seem to indicate some sort of Post-Classic culture in the central lowlands (Bullard, in press).

In the Maya highlands, the marginal nature of the northern area either to the lowlands or to the rest of the highlands has been defined for the Classic period by work of P. Becquelin and R. E. W. Adams. Becquelin has identified Proto-historic (1250–1540 AD) sites which can be directly tied to the Ixil, a highland Maya group (1966). In this region it seems that, unlike other regions of the highlands, fortress sites and hence formal militarism was not so important. Perhaps because of the isolated nature of the region the Ixil ruins show little central Mexican influence. This is in great contrast to the ruins of Iximché, the historic capital of the Cakchiquel, recently excavated and partially restored by G. Guillemin (1967). Murals, architecture, burial modes and so forth show great Mexican influences.

The most impressive work on these problems of Post-Classic Maya cultural transformations has been done at the site of Bilbao in the Pacific coastal plain. There is a substantial body of sculpture which even in the early Classic shows great stylistic affinities with that of Mexico (Parsons, 1965, 1967:15–16).

Recent work has had the effect in this problem area of again not so much revolutionizing the perspective as making it infinitely more complex. We have marginal regions strongly resistant to cultural change and others very open to new ideas, with a spectrum of variation in between in the highlands.

Andrews' work in the northern lowlands has outlined a similar situation in the period of from about 1200 AD on. He suggests that whatever the date of the Toltec invasion and of maximum influence (the Florescent period) that the East coast of Yucatan and Quintana Roo acted as a refuge area for Early Period (Maya) culture. After the subsidence of foreign impact, this refuge zone was possibly that from which were drawn the more traditional Maya cultural norms which make their reappearance in the Decadent period both at Mayapan and Dzibilchaltun (1965:329).

7. *The decipherment of Maya script.* Major advances have been made in the last ten years in terms of definition of content, of methodology, of decipherment and interpretation on several levels. A major figure in this specialized field

continues to be J. E. S. Thompson, whose catalogue of Maya hieroglyphics appeared in this period (1962). Thompson's monumental work along with Zimmerman's earlier catalogue (1956) reflects the great emphasis on the non-calendrical sections of Maya writing which has dominated recent research. The catalogue approaches have been supplemented by a new classification developed by the Electronic Calculation Center of the National University of Mexico (Rendón and Spescha, 1965). This is especially designed for use with computers. The Russian computer center at Novosibirsk pioneered in the use of these machines, in attempting to deal with undeciphered texts (Yevreinov, Kosarev, and Ustinov, 1961). However, their application has been severely criticized by Yuri Knorozov whose theoretical approaches were utilized (1963). The Mexican effort is a collaborative one and is combined with an attempt to correlate the efforts of nearly all of the individuals now working in the field. A conference on Maya writing to this end was held in Mexico City in 1967 (Ruz, Manrique, and Cazes) which resulted in a very fruitful exchange. Modeled more or less on the collaborative effort organized by Michael Ventris to attack Linear B (Chadwick, 1958) the Maya Writing Conference hopes to achieve as much success.

At least as important as the methodological and collaborative efforts, has been the work of several individual scholars in demonstrating the historical content of classic Maya texts. Heinrich Berlin in 1958 identified emblem glyphs which designate specific ceremonial centers. In 1960 followed the epochal discoveries of Tatiana Proskouriakoff that patterned clauses and dates on the Piedras Negras stelae probably refer to birthdays, accessions to power and other events in the lives of temporal rulers. In other words, Maya monumental sculpture and writing were shown to have been functional historical records. Proskouriakoff has continued these lines of inquiry with a series of brilliant papers on the Yaxchilan texts, and Maya women in the texts and art (1961, 1963, 1964). David Kelley has also followed these leads with the identification of dynastic records in the Quirigua and Copan texts (1962a) and Berlin (1965) has found similar data in the Palenque material.

More traditional studies in interpretation of the glyphs by use of iconographic, phonetic, mathematical, and contextual approaches seem to have increased in quantity. Besides Thompson, Barthel has been especially active (1963, 1964, 1965). David Kelley is preparing a very long detailed opus on Maya writing which will synthesize the latest information on the subject as well as incorporating the highly original researches of the author. Kelley has published a useful history of decipherment efforts with a graphic summary of agreement among scholars on non-calendrical material (1962b).

The correlation of Maya and Christian calendars still remains a problem, but as mentioned above the weight of the radiocarbon dates are now in favor of

the Thompson correlation. However, as noted, Andrews' work leads him to question this correlation and it may well be that only much more refined correlations of archaeological data from the northern and southern lowlands will resolve this difficulty.

Calendrical studies, the initial field of decipherment, still continue with such scholars as Satterthwaite and Lizardi Ramos absorbed in the exceedingly complex permutations of the system.

To summarize, the approaches of the last ten years to the decipherment problems have been more varied, and have involved more people from more countries, and has become more collaborative in nature. However, it still would seem that there is no substitute for the individual scholar with his individual unreconstructed inspirations. Proskouriakoff's, Berlin's and Thompson's work all is evidence of this. Emphasis is now definitely on the segments of the Maya writing system which are non-calendrical. This poses certain problems since earlier scholars often did not adequately record these sections, or did not record them at all. Improved techniques of night photography especially developed for the Tikal project by W. R. Coe have made possible the recovery of eroded texts to an extraordinary degree. The writing system is known to have changed stylistically through time and space. This all argues for the necessity for a long term, systematic and thorough recording program of Maya texts. Our samples for some regions and especially for the earlier time periods are inadequate. Unfortunately, such a program is becoming increasingly difficult to carry out as the market in antiquities created by U. S. and European Museums and art dealers has created great demand for Maya sculpture. Much unrecorded sculpture has been removed from sites, without records, and much has been damaged and destroyed by the vandalic antiquities smugglers. The situation has become so acute that it is a desperate race against time if an adequate record is to be made for the future. The situation is a direct result of the U. S. Government's disinclination to stop the receipt of stolen property in this country by museums and art dealers. All exportation of such antiquities from their places of origin, Mexico and Guatemala, is strictly illegal. Fortunately Mr. Ian Graham is doing such exploratory and recording work in an exemplary manner (1965). However, the work is much more than a single individual can manage and Graham should be supported by others.

8. *Topic studies.* In depth essays which collate and compare previously gathered data with new material or simply correlate slowly accumulated information, have been referred to in the preceding sections. Many others have not been mentioned, and yet these are among the most interesting attempts to draw inference from archaeological and supplementary materials and, at times, have great importance for culture process. Thompson's article on trade (1964) is a case in point, as is the Willey and Gifford article on proto-classic pottery at

Barton Ramie (1961). Synthetic articles attempting to draw together and interpret the patterns of Maya prehistory are represented in Willey's and Vogt's articles from the 1961 Burg Wartenstein conference. Warren has also made an attempt at reconstructing Maya origins in a more limited essay (1962). A massive study of funerary customs of the Maya has just appeared (Ruz, 1968). Much of the content of Alberto Ruz's journal, *Estudios de Cultura Maya*, is of this nature. Anders' extraordinary study of pre-hispanic and modern religion of most Maya groups (1963) is a good example of the kind of summarizing and organizational benefits deriving from this kind of work. It seems likely that such studies will increase in the future in direct proportion to the amount of accumulated data. The group of articles on the Maya in the Handbook of Middle American Indians constitute an extremely important summary of descriptive material. The past 10 years has not surprisingly seen an increase in their number.

DISCUSSION

The salient characteristics of the recent work in Maya archaeology are diversity of interest, the quantum jump in the mass of data available, the application of new systematics leading to new syntheses, and the greater use of a body of theory derived from general anthropology which has intense explanatory power. There is also an increase in the number of persons working in the field as well as the growth in the number of active institutions. There is no doubt that there is a direct relation between the quality of our interpretative statements and the quantity of data from which they are derived. The statements today tend to be more Boasian in that they tend to be multiple factor explanations rather than the prime mover style explanations of the neo-evolutionists. However, there is no doubt of the influence of cultural ecology and its theory on Maya archaeology. Nonetheless, in nearly all characteristics mentioned above we are following a pattern set by earlier work, especially that laid down by the Carnegie group under Kidder from 1930 to 1958. As noted before, one of the chief differences between the state of the field at present and before 1958 is in the body of explanatory and synthesizing theory which allows us to deal with the disparate kinds of data that Kidder worried so much about in his annual reports.

Along with these trends Maya archaeology has become involved in the current polemic between the so called "new" and "old" archaeology. The issue has been joined over the specific question of the causes and circumstances of the collapse of Maya civilization. Sabloff and Willey argue that ". . . in the Maya area processual factors, such as the ecological suitability of a great civilization in a tropical forest area, the effects of population increases in a 'type X

environment,' or the long term inviability of a 'theocratic state,' can be understood only after external historical factors are controlled. With a solid historical frame of reference, various hypothetical factors can be treated and tested as independent variables" (1967:312). In so stating they object to the viewpoints of L. Binford as the most visible advocate of the "new" archaeology. Binford, they say, has characterized traditional archaeology as being historically oriented with little interest in cultural dynamics. "However, by redirecting their goals and reformulating their research strategies, archaeologists will be able to contribute to the mainstream of anthropological theory" (Sabloff and Willey:313). By this they assume Binford to mean that process must be considered and given priority in research design. It is to this priority that Sabloff and Willey object. They argue that the priority should be reversed. Erasmus, in commenting on Sabloff and Willey's article, has objected to any priority. He feels that ". . . historical events must be examined from the standpoint of process to be fully explained and that neither has conceptual precedence over the other" (1968:171).

Binford in a rebuttal article (1968) has ". . . maintained . . . that we must continually work back and forth between the contexts of explaining the archaeological record and explaining the past; between the contexts of proposition formulation (induction) and proposition testing (deduction)." With this view of scientific investigation as a feedback process between insight and testing I thoroughly agree. However, this seems to be a shift from Binford's position as stated in his 1964 paper on research design. In that paper he argues for a priority of importance of the materialistic underpinnings of culture; i.e. the ecological context and cultural articulation with it (432). He further sets forth a sampling procedure based on probability theory and using random number tables to select the archaeological sample to be examined. In such a research design the flow of observation toward inference is clearly highly controlled and nearly one way. This seems to contradict Binford's feedback argument that he advances in reply to Sabloff and Willey. Indeed, Sabloff's and Willey's characterization of Binford's strategy seems to be correct. However, everyone is entitled to shift his ground and change his mind.

At this point it is enlightening to consider Abraham Kaplan's ideas about the nature of scientific inquiry (1964). Kaplan says that all inquiry consists of investigatory procedures which can be termed cognitive styles. In turn, these cognitive styles, according to their degree of formality, can be categorized as either logic-in-use, or reconstructed logic. Formal systems of logic are reconstructed logic and an example of such a system is the Aristotelian. The prime present day example of a reconstructed logic is the scientific method or the hypothetico-deductive method. It is this method that the practitioners of the "new" archaeology claim they are introducing into the field (Binford, 1968:

274). Logics-in-use are more or less inductive and individualistic methods. In terms of systematics per se, Kaplan observes that reconstructed logic is not a description but rather an idealization of scientific practice and that logic-in-use may actually precede and be superior to its own reconstruction. Kaplan further states that "To ask for a systematic procedure which guarantees the making of discoveries (and) and as a corresponding procedure guarantees the validity of a proof is surely expecting too much" (1964:15). In short, then, inductive cognitive styles are valid and productive means of doing research and need not be explicitly formulated to produce pragmatically testable results. One only need read *The Double Helix* to become convinced of this (Watson, 1968).⁶

Accepting Kaplan's viewpoint, it appears that Sabloff and Willey and Binford are using varying cognitive styles which are not necessarily in conflict. In part, the unfortunate polemical style of the "new" archaeologists seems to stand in the way of the resolution of what may be largely a semantic misunderstanding. In the case of the Maya collapse the opposing sides rest on fragmentary epistemological bases in the view of science taken by Kaplan. Binford approaches the pragmatic stand of most archaeologists when he accepts a feedback model of scientific inquiry. However, the more dogmatic versions of the "new" archaeology (see for example Flannery, 1968, and Binford, 1964) advocate a rigidity of theoretical position which may be self-defeating. It is curious that while granting Willey a flexibility of theoretical position, Flannery is advocating a reconstructed logic approach of ferocious dogmatism. It is indeed fortunate that few of the present investigators in Maya history are taking extreme positions on either side of this so-called debate. Reification of classificatory and synthetic devices would seem to be one of the greatest dangers Maya archaeology is running in intensifying the systematics of fieldwork and analysis. As long as these devices are regarded as dispensable and modifiable tools, they are useful. When we lose our pragmatic attitude toward them, then there is the danger of rigidity and consequent dogmatism of both inquiry and interpretation. As Donald Lehmer has observed "We must not be beguiled by the cellophane wrapping of the 'new' archaeology's terminology into believing that the contents are much different than those found in the plain brown wrapping paper of the 'old' archaeology."

Suggested Future Research. It is obvious that all of the headings under the Explanation heading constitute fruitful general research problems. The problem cluster approach would seem to be a highly feasible one in archaeology. This is because of the essential unpredictability of the results of investigation. We have data derived interpretations indicating areas of testing and further work, but it is true that one can still not confidently predict even on what problem 60% of his data is going to bear before going into an intensive excavation project. Therefore, a loosely structured research design in which up to 40% of

the project resources might be turned to exploit the unexpected data seems the most reasonable. Considering the characteristics of the most fruitful recent research, one also would emphasize the necessity for systematic mass handling of more data and the need for more balanced data. We have not begun to replicate the results of preceding work except in restricted areas. The regionality of Maya culture is in part responsible for this.

Specifically we need more regional projects. These would combine the intensive excavation of key sites and intensive survey of regions. Such projects are particularly needed in the Alta Verapaz region of the highlands, in the Pacific coast zone of Guatemala, and in the intermediate area of the lowlands. Limited excavation and survey projects are needed in regions which are only lightly known, especially the Chiapas lowlands in the Jatate region, and the Campeche coast and inland zones, and in the Motagua valley.

Intensive excavation is needed at certain crucial sites which seemingly were innovative centers or at sites which are unique in characteristics. A large site of the southeastern lowlands should be dug, along with another site of the southern Maya highlands (Kaminaljuyu is already in progress), and a small Early Classic center of the lowlands. Second stage excavations would be interesting in testing certain culture historical or culture process interpretations. Uaxactun and Altar de Sacrificios jump to mind as especially good prospects. Continued recording of Maya inscriptions is needed desperately by means of rubbings, sophisticated photographic techniques, latex molds and drawings. I. Graham has set an excellent standard in this sort of work. Continued systematic recording of new sites in terms of mapping, limited testing for ceramics, as well as hieroglyphic study is imperative. The sort of surveys carried out by Pollock and Ruppert and Shook in past years is sadly lacking. Vandals and amateurs are first to the sites in more and more cases and data which is exposed and easily destroyed is being lost.

The integrative mechanism of a coordinated research plan for Maya archaeology has been removed by the closing of the Carnegie program. To a great degree there has since been the benefit of more diversity of interest. In addition, the loss of coordination has been made up by ad hoc conferences of Mayanists at various times and on various subjects. However it is time that a yearly meeting is organized along the lines of the Pecos conference. More systematic conferences and symposia are needed in contrast to the topical nature of many papers now. Such a yearly conference could be held in conjunction with that of the Society of American Archaeology and the International Congress of Americanists in alternate years. To some degree the functions of communication and coordination have been fulfilled by the journals, but these are necessarily somewhat behind the times and the formal papers presented therein are no substitute for the informal interchange of personal contact.

The Tikal project is ended, as are the Dzibilchaltun, Altar and Seibal projects, and something of a lull has settled on the field although field work is in progress at Kaminaljuyu, Monte Alto, and is beginning at Becan and Xpuhil. Full reports on recent field work are only beginning to appear. It seems likely that Maya archaeology is due for another explosion of activity and consequent interpretative insights when the present diminished level of field work passes.

NOTES

1. For appreciations of Kidder and the Carnegie Program see Wauchope (1965), Willey (1967), and Pollock (1958).
2. Tozzer's criticism was almost casual in this and an earlier article in which he was oriented toward substantive reviews of Maya and Middle American archaeology. In the 1934 article he quotes A. V. Kidder, "It may, indeed, well be doubted whether it is possible to perceive the nature and to evaluate the action of the forces which have governed the career of any ancient people. But if the attempt be not at least envisaged, archaeology becomes mere anti-quarianism" (1934:19).
3. Bibliographic reference to these projects is complicated by the fact that much of the literature is still unpublished or in the form of mimeographed preliminary reports, informally circulated. The Notes and News (now Current Research) sections of *American Antiquity* dealing with Middle America, and the Middle American archaeology sections of the *Handbook of Latin American Studies* are good sources and guides to what information is currently available. Vols. 25, 26, 27, and 29 of the latter are especially pertinent.
4. In the matter of sample, however, the field is still dealing with a very minute and not necessarily representative body of data. Tozzer estimated in 1934 that there were over 800 sites in the Maya area. No one has since attempted to enumerate them, but undoubtedly the number has grown, probably to over 1000, for all major ecological zones.
5. Rands' current work centered around Palenque aims to define these boundaries by means of ceramic technology. Although mainly oriented toward definition of trade units, this data would no doubt reflect the sustaining population units as well.
6. I am especially indebted to Prof. Eugene Ogan for advice on this section. However, the use that I have made of his critical observations is strictly on my own account.
7. Professor Joseph Michels introduced me to Kaplan's work, and, most importantly, showed me the applicability of Kaplan's ideas to the matter under discussion. I am most grateful for this lead.

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