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# SURVEYS AND ARCHAEOLOGICAL RESEARCH

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Come what come may,  
Time, and the Hour, runs through the roughest Day.

Macbeth

## INTRODUCTION

Considering the number of surveys done each year and their growing contribution to the field, it is surprising how few attempts have been made in the literature to take the measure of surveys as a means of doing archaeological research. Discussions which turn to the comparative strengths and weaknesses of surveys have usually been developed on a narrow front. Questions of a broader gauge obviously could be asked but have seldom been raised. Part of the reticence here probably stems from the awkwardness of surveys. The stock in trade of surveys is material recovered from the surface of the landscape. Surface remains can be viewed as providing at best only a fragmentary and incomplete picture of what is to be found at an archaeological site. In comparison with the heavyweight business of excavations, surveys stand as a slight enterprise. It is worth recalling that the main function of early surveys, as well as some more recent ones, was that of locating an appropriate site for excavation. In other words, the survey served as a preliminary lightweight bout which preceded and complemented the main attraction. It is not surprising then that the tone adopted in presenting survey results has often been an apologetic one. Even as late as the mid-1960s, a "defense" of surveys apparently was still thought to be necessary (112). From our current vantage point, things have changed considerably: surveys have acquired a more secure position. In some cases they may even

have begun—in the scope of their contribution—to outpace excavation, the heavyweight that if anything only seems to grow more sluggish as field technique is refined. This is seen perhaps most clearly in terms of the conceptual framework recently discussed by Flannery (41) and Adams (4), which would treat social and economic organization as calling for study over a series of multiple, nested levels of analysis. Survey work has made a central contribution to the emergence of this framework, which implies a need for reordering some of the goals and priorities of archaeology.

The number of surveys that have been carried out in various parts of the world during the last 20 years is enormous. It is not the intention of this review to wade methodically through the vast body of literature on surveys, touching base on all corners of the field. Instead, an attempt will be made to trace certain trends in the development of surveys by examining in somewhat greater depth the work that has been done in selected areas of the world. It will be of interest to compare the research problems that different surveys have chosen to explore. Emphasis will be placed on the growth of surveys as a means of obtaining substantive results rather than on more technical aspects of the design and conduct of surveys. Each of the three areas selected—the southwestern United States, the Valley of Mexico, and Mesopotamia—represents a part of the world where surveys have been highly successful. In all three cases, long-term projects have been carried out, and one can trace the evolution of research problems that have been addressed, field methods adopted, and forms of analysis employed. One of the main reasons for selecting these three areas as case studies relates to the recent publication of a book which provides a synthesis or overview of previous work in the area. These are respectively: *Investigations of the Southwestern Anthropological Research Group* (1978) edited by Euler & Gumerman (39), *The Basin of Mexico* (1979) by Sanders, Parsons & Santley (116), and *Heartland of Cities* (1981) by Adams (5). The recent appearance of these three books, which serve as benchmarks for surveying the history of work in the respective areas, makes it an opportune time to attempt such a review.

It is worth noting at the outset that the three areas that have been selected are particularly well suited for undertaking archaeological surveys. The visibility of sites on the surface of the landscape is usually good, and conditions for carrying out survey work are for the most part good as well. The results obtained in such “favorable” areas have tended to encourage high expectations about what can be learned from surveys in general. Such expectations have often turned to frustration, however, when surveys have been extended to other parts of the world offering less favorable conditions. One consequence of this has been the development in less favorable areas of a more critical awareness of the need for improved field techniques and

the better design of surveys. Reference should be made here to recent review articles by Schiffler et al (122) and Plog et al (100), which focus specifically on issues concerned with the organization and conduct of surveys. The recent growth of interest in survey methodology and the tensions raised by the "extension" of surveys to less favorable areas are topics that we shall return to in a later section of this review.

**EARLY SURVEYS** As a means of setting the stage, it is worth looking briefly at early surveys which were, by modern standards, quite modest in their research aims. The turning point in the history of archaeological surveys is generally accepted to be Willey's survey of the Virú Valley, which initiated the study of settlement patterns. Reconnaissance is the term which perhaps best describes much of early survey work. Relatively little was known about archaeological sites and remains in many areas of the world, and major discoveries could be made simply by means of exploration. The initial charting of previously uncharted territories did not require systematic work. But there were also some early surveys which strike a more modern note, such as the one by Spier (129) at the Zuni ruin in 1916, which was concerned primarily with chronology. Over time, a more systematic approach to the discovery and recording of sites was adopted, so that by the 1930s a solid body of survey work was being done.

Alongside the development of the empirical side of surveys, increasing attention was paid to formulating research questions in regional terms. This is reflected in classical surveys of the period such as the work by Braidwood on the Antioch Plain (19), that of Beals and co-workers in Northeast Arizona (12), and that of Phillips and co-workers in the Lower Mississippi Valley (88). Willey's pioneering survey of the Virú Valley of Peru was done just after the war in 1946. The idea of studying settlement patterns, the use of settlement configurations to reconstruct cultural institutions, had been taken up at the suggestion of Steward. It was, in fact, far from clear at the time how such a study should be carried out and even whether or not it would be productive. As Willey (147) mentions in his retrospective account of the Viru Valley survey, he thought at the time that in being given the survey to do, he had been dealt a bad hand in comparison with the other more promising lines of investigation of the wider project. The actual survey was facilitated by the remarkable preservation of sites and by the availability of aerial photographs which made it possible to record more than 300 sites, about one-quarter of the sites in the valley, during a single season. An initial report was prepared in conjunction with Ford (45), and the final report on the survey was published in 1953 (145). The rest is history.

Interest in settlement pattern studies developed rapidly, as is attested by the volume on *Prehistoric Settlement Patterns in the New World*, which was

edited by Willey (146) and appeared in 1956. The chapters in this volume are stimulating but seem in many cases to be premature: the marriage between old data drawn from various regions of the New World and the new perspective does not appear to be an entirely satisfactory one. Another striking feature of the book, at least for the modern reader, is the sparing use of distribution maps in the development of arguments based on settlement patterns. The chapter that deserves special mention is the concluding one by Vogt (141), in which an overview of settlement pattern studies is sketched from the point of view of an ethnologist. It is worth adding as a historical footnote that in commenting on Sander's chapter (113), Vogt looks forward to the time, in an almost prophetic way, when enough will be known to write a full-scale monograph on the sequence of cultural developments in the Valley of Mexico. What is important about Vogt's appraisal is that he touches on several theoretical issues with regard to settlement pattern studies that would only be taken up much later by archaeologists and anthropologists (151).

One constraint on the original Virú Valley survey was that it had been done over a single field season rather than as a multiyear project. The latter would have allowed more of an opportunity for working out some of the implications of the new settlement pattern approach within the context of the Virú Valley itself. It is also unfortunate that this new lead was not actively taken up and refined in other parts of Peru until apparently much later (81). In order to follow the course of development of Willey's own thinking on the study of settlement patterns, it is necessary to turn to the project done on prehistoric Maya settlements in the Belize Valley between 1954 and 1956. The final report, as well as documenting comprehensively the results of the project, includes Willey's reflections on the general aims of settlement pattern studies and the kinds of evidence required for such studies.

Too much reliance on survey data had been one of the criticisms leveled against the Viru Valley study. There is some irony in the fact that the bulk of the fieldwork in the Belize Valley actually consisted of excavations at the site of Barton Ramie. By implication, the goals of settlement pattern studies were presumably beyond the scope of being realized by means of survey work alone. In his discussion of the research problems dealt with by the project, Willey separates them on two tiers: a primary one which would include basic questions such as the form and function of buildings and the relationship between settlement components and the natural environment, and a secondary one involving higher order inference. The latter would include such questions as population density, patterns of land use and agricultural potential, and the extent of urbanism in a region. A common feature of more recent surveys is that they have often set their sights on

questions belonging to the second tier and assumed that questions on the first tier would take care of themselves as a matter of course.

**GREAT EXPECTATIONS** It is only against the background of the wider changes that archaeology has experienced during the last two decades that much of the recent history of surveys can be understood. The changes here would include a greater emphasis on explanation, the use of analytical models, and the exploration of more theoretical issues in general (25, 105). Accompanying the growing ambitions along anthropological lines has been a more rigorous approach to the design of research and a more critical evaluation of research strategies (2, 13, 102, 133). There is no need here to dwell on these shifts in method and theory, since most archaeologists are fully familiar with them. They have contributed on the one hand to what Clarke (26) has referred to as “a loss of innocence” and encouraged on the other hand a new sense of optimism, as noted by Martin (77) and others, about what we can learn about the past. If many issues are still open to debate and unresolved, there can be no question that we now have higher expectations—perhaps exaggerated expectations in some cases—about what archaeology can achieve.

### THREE AREAS

The first half of this section will be devoted to a review of the three books mentioned earlier, each of which provides a synthesis of a group of surveys done in a given region. The selection of these three areas is influenced by the aim of comparing the development of surveys in different parts of the world. Major survey projects in other areas of the world such as Greece and the Mississippi Valley could have been chosen equally well (78, 123). In the limited space available, it will only be possible to outline the main research strategies and findings of the three groups of surveys. An attempt will be made in the second part of this section to compare the three areas and identify some of the common features of the surveys and also the problems that they share. In a final section, some of the issues in current survey archaeology to emerge from the examination of these three case studies will be discussed.

**THE SOUTHWEST** *Investigations of the Southwestern Anthropological Research Group* represents a progress report on a large-scale, cooperative survey project initiated in 1971. The first half of the volume includes a summary of the history and main objectives of SARG and seven chapters which review the results of survey projects conducted in different areas of the southwestern United States (39). These are followed by a series of

chapters which evaluate and summarize what has been accomplished to date and recommendations with regard to the future directions that SARG should take. The original goal of the project was to explain the variability in the spatial distribution of prehistoric population aggregates in the Southwest given the evidence from archaeological sites. As indicated in the 1971 volume edited by Gumerman (50), the decision was made that a broad, common problem should be investigated by the group and that comparable research strategies should be adopted. The goals of SARG were more than simply an understanding of prehistoric populations in the Southwest: there was the further goal of developing or at least exploring laws of human locational behavior. Plog & Hill (96) outline in axiomatic form the three main hypotheses to be tested: (a) sites were located with respect to critical on-site resources; (b) sites were located so as to minimize the effort expended in acquiring quantities of critical resources; and (c) sites were located so as to minimize the costs of resources and information flow among sites occupied by interacting populations. What is meant by a critical resource? "Those without which the system would collapse" is the cryptic answer (96). Agreement is reached on the use of a standard system for recording sites and recommendations are made for the use of comparable sampling designs by those who will participate in this pioneering venture.

In order to see how far surveys had come by 1971, it is worth looking back to the initial report by Longacre (72) on survey work in the Upper Little Colorado, which appeared in the first volume of *Chapters in the Prehistory of Eastern Arizona* in 1962. In the report on the same area that appears 2 years later, we see a growing confidence and readiness on Longacre's part to attack problems such as population growth using survey data (73). The tempo of change accelerated in the second half of the 1960s. Much of the growth was centered on the field station at Vernon under the direction of Paul Martin and the fieldwork conducted in the Hay Hollow Valley, where the main research theme was culture change as reflected in the Basketmaker-Pueblo transition. Plog (90) examined this question by means of a model in which population growth was treated as a central variable. The relationship between population growth and economic growth in the Hay Hollow Valley was also explored by Zubrow (157) in his simulation study of carrying capacity in the region. In both studies, survey data was used to provide the estimates of population size and growth. By the time that SARG was founded in the early 1970s, there was a sense that, productive as individual surveys had been, much could be gained by enlarging the framework and studying patterns comparatively on a broader scale. In the original blueprint for SARG, the problem to be examined by the group remained firmly anchored in the question of population. But there was also a newer and somewhat less well developed concern with the location of sites



and their spatial distributions. There was, in fact, an ambiguity in the definition of research goals and what different members of the group thought they were supposed to investigate. As Dean (36) points out, two different questions can be studied: "Why were Southwestern population aggregates located where they were?" or "Why are prehistoric sites located where they are?" Different approaches are required for answering these questions, and much of the tension within SARG appears to stem from this ambiguity.

One of the striking features of the contributions to the 1976 conference as a whole is the mixture between promise and real gains on one hand and frustration and disappointment on the other. The work done in the Long House Valley, for example, which is used to illustrate new approaches to the display and spatial analysis of survey data, is impressive (37). On the other side, fundamental questions are raised by Judge and others about how well the broad aims and standard recording system actually serve individual surveys and local research problems. The observation is also made that it is probably unrealistic to expect that the research problems adopted by SARG can be solved without evidence from excavations or by surveys alone (70). There is a combined sense of success and failure in the results of Plog's attempt to implement location analysis by means of correlation measures between environmental variables and site distributions (92). One of the problems to emerge with respect to spatial analysis is the need somehow to generate expected patterns of site location if the three original hypotheses mentioned above are to be tested. In addition, the basic approach to site location, which relies upon the law of least effort proposed by Zipf (156) and maximization assumptions, is called into question by Sullivan & Schiffer (134). There seems to be a general realization that the population problems as initially posed may be too broad to serve as productive research questions. If there does seem to be a need to go back to the drawing board, this can be done without an excessive sense of disappointment, however. Research is a dialectical adventure. Archaeological research, although we often tend to forget it, is still in the early stages of its development. It is suggested that only about 10 percent of the prehistoric sites in the Southwest are known at present (93). The tensions and inconsistencies that arise during the course of a complex research project such as SARG and that previously have seldom been well documented in the archaeological literature are positive signs of growth. The searching inquiry for new interpretative keys to be used in further decoding Southwestern prehistory will go on (29, 95) and this is how it should be.

**THE VALLEY OF MEXICO** The growth of knowledge about the Valley of Mexico since the start of surveys in the area in 1960 is truly remarkable.



While several lines of investigation have contributed to the synthesis presented by Sanders, Parsons & Santley (116) in *The Basin of Mexico*, the main one has been a group of surveys which have involved the coverage of most of the region. The early chapters of the book describe the history and aims of the project, the natural environment of the region, the methods and strategies employed in the field, and the preparation of the series of site distribution maps published in conjunction with the volume. The middle three chapters cover respectively the histories of settlement, demography, and resource exploitation in the region. In the final section of the book, the authors turn to broader evolutionary themes and the theoretical implications of their research in the Basin of Mexico. This synthesis of more than 15 years of work is a major achievement, even if the book, as several reviewers have pointed out, is flawed on the editorial side. It is clearly unfortunate that the same amounts of time and energy were not put into the final preparation of the volume as into the original fieldwork.

At the start of the project in 1960, there were four main goals: (a) to trace the development of agriculture, (b) to trace the development of different settlement types, (c) to construct a population profile, and (d) to explore the processes contributing to cultural evolution in the Valley of Mexico. Early work, which included both excavations and a general survey, was carried out in the Teotihuacan Valley on the northeast side of the Basin. Subsequent work on the mapping of the urban center of Teotihuacan itself would be done by Millon and co-workers as part of a separate project (79). In *The Basin of Mexico* there is a candid account of how survey methods and strategies were gradually worked out in the field and progressively refined over the course of the project. The general strategy to emerge would be a regional one with an ecological orientation which would be implemented by systematic location of sites on the landscape. The use of aerial photographs facilitated survey coverage, and by 1967 a system had been worked out by Parsons which made it possible to record directly on photographs a continuous flow of information on the density and period of pottery observed on the surface of the landscape. Longstanding problems with the definition of sites and their boundaries encountered in most survey work were circumvented in this way. In the surveys, priority was given to the comprehensive location of sites over large areas rather than to the more intensive collection of surface material at sites and the recovery of evidence on site function and status. In adopting an extensive strategy and the long-term goal of trying to cover most of the region, there was an acceptance of the limitations that this priority would impose on the development of a settlement typology. In classifying sites, reliance would have to be made primarily on the size of a site. One of the more controversial aspects of the work was the development of a method for estimating population size from

the density of surface refuse at a site. While it is reasonable to expect some relationship between sherd densities and occupation densities to hold at sites, other factors such as duration of occupation and the thickness of overburden at a site will act to make this relationship far from a simple one (137). At their best, surface densities of pottery would seem to provide only a rough index for reconstructing demographic trends.

The results of two of the surveys in the Basin were published in the early 1970s: Parson's work in the Texcoco area and Blanton's work in the Ixtapala area (16, 84). Both monographs identify four main cycles of chronological development which are defined in terms of settlement patterns and demographic trends. Various other publications from this same time reveal a growth in the conceptualization of research problems as well as substantive gains made in the field. This is seen, for example, in the thorough treatment given to the natural environment and contemporary agricultural systems of the Basin (115). It is also reflected in the fuller attention given to population as a variable and in particular the role of population pressure in cultural change (114). There is also a growth in what is meant by a settlement system and by a regional approach to archaeological research (85, 86). The contributions to the 1972 conference on studies in the Valley of Mexico, which appeared in a volume edited by Wolf (149), reveal in addition several contrasting points of view. Logan & Sanders (71) present their general model of cultural evolution in the Basin which focuses on population growth as the main cause of economic intensification and, in turn, increasing cultural complexity. Somewhat different positions on population dynamics are held by Blanton and Cowgill (16, 31). In his introduction, Wolf (149) comments that while the simplicity and directness of the model are commendable, more complex models will no doubt be required when interest turns to an analysis of the critical turning points in the spiral of prehistoric development. There is also a contrast between the emphasis placed on the country and ecology by Sanders and that placed on the town and urban institutions at Teotihuacan by Millon (80). These debates end up being carried on in *The Basin of Mexico*, where some of the deficiencies of earlier models are acknowledged and new arguments are advanced to bolster old positions. But immediate questions concerned with the relative merits and shortcomings of these arguments should not detract from the long-term achievement of the project: some 3500 km<sup>2</sup> of the region have been surveyed; an important series of maps covering some 2500 years of settlement history in the region is now available; the basic contours of research problems in the Valley of Mexico have been drawn.

MESOPOTAMIA *Heartland of Cities* offers an unusual opportunity for tracing the growth of the survey work in a region. This is the third book

by Adams on surveys done in Mesopotamia—the previous two being *The Land Behind Baghdad* (1965) and *The Uruk Countryside* (1972), done in collaboration with Nissen—and it represents an outstanding achievement (1, 5, 6). As a synthesis it examines the dual and interacting histories of settlement and irrigation on the central floodplain of the Euphrates as they unfold over more than 5000 years. Surveys provide the only comprehensive source of information on rural settlement on the Mesopotamian plain, and particular attention is paid to the balance between town and country in the “land of cities.” The book begins with two chapters on the environment (water and land) of the alluvial plain and on the recovery of settlement and irrigation patterns. Adams then examines three historical cycles, each associated with different configurations of the irrigation system and settlement patterns: (a) urban origins (Ubaid-Early Dynastic I periods); (b) integration and fragmentation under successive, contending dynasties (Early Dynastic II-Middle Babylonian periods); and (c) culmination and collapse of an agrarian base and urban superstructure (Neo-Babylonian-Late Islamic periods). The book includes an appendix by Wright (153) on his survey of the area of Eridu and Ur and closes with an overview of demographic, agrarian, and urban development on the Mesopotamian plain.

The origins of the surveys by Adams and others in Mesopotamia can be traced back to Jacobsen, who initiated work in the region in the late 1930s and resumed it in collaboration with Adams after the war (64). The field-work involved visiting mounds visible above the alluvial plain and recording information on a site’s location, size, and periods of occupation as seen in pottery on the surface of the mound. This is the basic approach with subsequent refinements that Adams and other workers (46, 152) have employed in their surveys in the region since 1960. Only a limited area of the alluvial plain can be covered in any one field season, and one by-product of this has been the somewhat artificial definition of survey areas. This is the case with the first survey that Adams published, the lower Diyala survey near Baghdad (1). Here the descriptions of the environmental setting and the field methods used are schematic in comparison with the two more recent books. A basic account of the settlement history of the area is presented and demographic trends are outlined by means of a reconstructed population curve for the area as a whole. The general conclusion reached is that there is a close relationship between settlement and irrigation and institutional structure. In *Heartland of Cities*, these same relationships will be interpreted in a more loosely structured and complex way. With the appearance of *The Uruk Countryside* (6), we perceive changes on a number of fronts. The survey area is a larger one and comes closer to representing a “natural” region. Chronology has been refined. The pottery sequence which provides the basis for dating sites is better documented. A sense of

problem begins to emerge from a closer description of the environmental variables operating on the alluvial plain and an appreciation of their dynamics. A start is made at a more quantitative approach to the analysis of spatial patterns in settlement data. The main substantive concern of the book is that of delineating the spatial and temporal patterns leading to the rise of urbanism. This is interpreted as the outcome of a rapid population shift from the countryside to urban centers rather than as a product of population growth within the region, so that by Early Dynastic I times most of the population of the region was concentrated in a few large urban centers (3).

The basic strategy adopted in the Mesopotamian surveys is an extensive rather than intensive one. As in the case of the Basin of Mexico surveys, priority is given to the broad, comprehensive coverage of a region, and no systematic attempt is made to recover information on site function. Uncertainties over the prospects of conducting a long-term survey project in this part of the world have contributed to the choice of an extensive strategy. Physical conditions also tend to place some constraints on what can be done during surveys in Mesopotamia. Contributing on the other side to the active growth of surveys and their interpretation has been the positive dialogue between Adams and other scholars. A good example here would be the position taken by Wright & Johnson (154) that state formation rather than urbanism should be regarded as the central research problem to investigate. A second example would be Johnson's application of central place models to survey data from the region and adjacent regions (65, 66, 68). Still another would be the questions raised by Weiss (143) about the reconstruction of demographic trends. In all three cases, the dialogue has led to a sharpening of either the questions being asked or the interpretations made.

*Heartland of Cities* takes us another step along the evolution of surveys in Mesopotamia. The regional framework has been further enlarged so that it now essentially corresponds with the natural floodplain of the Euphrates. We see the introduction of new field methods such as the use of sampling and LANDSAT photographs of the region. There is a more critical discussion of site recovery and the general limitations of survey data. Parenthetically, it is worth mentioning along methodological lines the work done by Whallon (144) in the Keban Reservoir area of Turkey, where comparisons are made between intensive surface collections at mound sites and excavation data from the same sites. Close agreement is found in the chronological periods represented in the two sets of data from a given site, supporting the use of surface pottery for making chronological attributions at mound sites. Wider use is also made by Adams of formal approaches to spatial analysis, including rank size graphs of settlement size and the evaluation of the "cultivated area" around sites by means of a simulation study. Concomitant

growth is seen in the conceptual treatment of the environment where emphasis is now placed, for example, on the unpredictability of water as a resource and the keying of human adaptive strategies on this factor. Among the conclusions drawn, one of the more challenging is that population densities over the long period of early occupation of the plain apparently remained below the level where they would have called for an expansion beyond the heartland or nuclear area of the alluvial plain. Another area where there is a shift in interpretation concerns the relationship between the institution of central government and the irrigation system which is viewed as exhibiting only a loose coupling. A fuller account is at the same time given of the rural community: its adaptive strategies and deep-rooted vitality. In developing his new synthesis, Adams takes a broad view of the interplay of many factors. While definite patterns and trends are recognized, he does not try to force everything into a single harmonious pattern, but allows room for discordant elements and diversity.

**COMMON TRENDS** While we tend to view the three groups of surveys in terms of their differences, attention should also be drawn to the common features that they share. All three areas have a relatively warm and dry climate. Pottery is found in quantity on the surface of sites and provides the marker artifact for dating sites. The surveys are much less successful in dealing with earlier, preceramic sites in the respective regions. The economic context in each case is primarily an agricultural one and the occupation of settlements usually takes a sedentary form. In fact, the settlements, especially in the case of pueblos in the Southwest and mound sites in Mesopotamia, assume a compact and coherent form as well. Changes in population size and cultural complexity are observed over the sequence of periods or phases in each area. The main field strategy adopted tends to be an extensive one. The main information that the archaeologist has to work with is the location of a site, its size or area, and the period or periods in which it was occupied. On the other hand, large areas on the map are covered and many sites are recovered. In all three cases, fieldwork has been conducted over a substantial number of years. Major factors in the success of the projects would appear to be the sheer volume of work done and the experience that workers have gradually built up over the years. Neither of these factors is given much credit in the more recent literature on method and theory in archaeology when it comes to the solution of research problems. Perhaps there has been too much devaluation of experience. In terms of trends over time, we observe a more detailed account of how work is actually done in the field and more candid discussions of the limitations of survey data in the three books reviewed than previously. There is a greater awareness of the issue of sampling, which comes as no surprise. We also see a progressively more refined description or treatment of the environment:

for example, the modifications of the SARG environmental variables and the more detailed account of the geomorphology of the Euphrates. Another common trend is the increasing use made of locational models and quantitative forms of spatial analysis.

One of the most intriguing features shared by the three groups of surveys is a concern with demographic trends. The question of population and its role in cultural development has a fairly long history in the case of both the Southwest and Mesopotamia. It is, however, only during the last 15 years that population has surfaced as a central research problem and become the subject of sustained field investigation. The natural affinity of surveys for population questions is not difficult to understand. Population entails an aggregate measurement of a system and offers a way out of the frustrations, inherent in trying to do a study of settlement patterns, that arise as a consequence of the limited information on site function or status obtained from surveys. Surveys often start with the idea of doing a settlement pattern study only to settle somewhat later on a population study as a workable alternative. In the case of all three areas, a tentative start was made in this direction by 1965, and the form that this took was the reconstruction of population curves for areas such as the Upper Little Colorado or the lower Diyala (1, 73). Initially, population was treated as an index whose trajectory was to be traced over time. At the beginning of the 1970s, archaeology experienced its own population boom. This was stimulated by the realization that population was a tangible variable that could be measured in the archaeological record, and also by the appearance of *The Conditions of Agricultural Growth* in which Boserup (18) argues that population growth is the cause of agricultural change. The volume edited by Spooner (130) in 1972 on the anthropological implications of population growth contains chapters on the Valley of Mexico and Greater Mesopotamia which interpret agricultural change and cultural evolution as a consequence of population pressure (114, 128). But just at the time that the population bandwagon was moving at top speed, some archaeologists such as Cowgill (30, 31) began taking a closer look at the demographic side of population change. It was at this time that archaeologists began to enroll themselves in self-taught courses in demographic education. This is seen in the proceedings of a symposium on population studies, edited by Swedlund, which include several contributions concerned specifically with the Southwest and the Valley of Mexico (17, 19, 51, 74, 91). From 1975 onward, population growth and demographic trends would be treated as more complex questions, ultimately linked with birth rates and death rates, and no longer simply as either a cause or consequence of cultural change (55, 104, 158).

In view of the attention paid to population as a variable, it is somewhat surprising in retrospect that the question of estimation itself was not more actively pursued (9, 104, 135). In fact, each area developed its own approach



to estimation, and relatively little effort appears to have been spent on exploring alternative methods which might provide an independent check on estimates of population size or growth rates in a region. In the Southwest, a regression formula was used for converting the area of the surface scatter at a site into the number of habitation rooms occupied at a given time. In the Valley of Mexico, as mentioned earlier, sherd densities observed on the surface were used to estimate occupation densities. In Mesopotamia, the area of a mound was multiplied by a constant representing the number of inhabitants per hectare in order to obtain its population size. Objections to such treatments were obviously raised (58, 137, 143). It is worth noting that in all three cases the approach adopted is a normative one and involves a linear treatment of survey data. When population figures are cited in the more recent literature, it is often done in an almost apologetic way. As an understanding of demographic processes has increased, there has been a tendency to pull back from population size as a guide and to develop new or modified positions where emphasis is placed instead on population dynamics (with allowance made for short-term swings) and on the spatial distribution of population as we have seen in SARG (38, 39, 94, 99, 118). The archaeologist, as Adams (5) remarks, "must avoid unconsciously imposing a 'gradualist' bias upon his findings—for example, the assumption that site occupations were generally stable and population trends consistent over long periods simply because he cannot easily detect volatile, quickly reversible patterns."

**PROBLEMS SHARED** The main problem that surveys share centers on the frustrations encountered in trying to close the gap between the ambitions of settlement pattern studies (23) and what is actually achieved in the way of interpretation of survey data. A clear statement of the problem is given by Sanders et al (116):

Our most basic premise was that the way in which people distribute their residences over the ground surface is a sensitive indicator of how they interact with the natural environment and with other human beings . . . What we failed to do, and what no one has really ever done adequately, is to develop a series of models, by means of which the archaeologist can make reasonable sociological inferences from settlement pattern data.

It is only during the last few years that archaeologists have come fully to realize that field results will not speak on their own and that locational models have to be developed if this is to be accomplished. But part of the problem also stems from the inherent limitations of most survey data. There are four main forms that such limitations commonly take (124): (a) chronology is not refined enough, and sites can only be attributed to time periods measured in terms of centuries; (b) it is difficult to recognize small sites and



they are often missed during surveys; (c) only a rough idea is obtained of the layout or internal structure of large sites or centers; and (d) limited information is recovered on the functions of sites located during surveys. Excavations would seem to offer a means of solving several of these problems. But they can seldom be done at more than a handful of the sites found in a given area, and in this sense excavation does not provide a general means of rescuing surveys from their limitations. While we should continue to strive to attain a greater degree of resolution along these four lines, we should also be realistic and recognize that ambiguity is a common feature of most sets of survey data.

*Overestimated maps* In addition to problems created by a shortage of information, surveys also suffer from problems of wealth. When it comes to the analysis of site distributions, we usually work with maps where all of the sites that can be dated to a given time period or phase, including those that may have been occupied for only part of the period, are represented. In areas of the world where conditions are favorable for the systematic recovery of sites on the landscape, there is a good chance that many of the sites displayed on a map were not all occupied at the same time. The maps that we study are, in fact, composite maps. The problem of overestimated maps has been recognized in the literature in various forms. For example, Sanders et al (116) refer to it as the problem of contemporaneity of surface occupation and note that an assumption commonly made is that all sites assigned to a period are treated as if they were occupied throughout the time period. Another form of overestimation concerns the sizes or areas of sites; only part of the area of a site dating to a given period may have been occupied at any one time. In the Southwest, this problem is recognized in the estimation of the number of habitation rooms at a pueblo, where it is identified as the "correction for the developmental history of the site" (90). In both of these forms, the problem becomes more pronounced as the length of the time span represented on maps increases.

This problem has a number of implications for the analysis and interpretation of settlement patterns. The maps that we examine (assuming for the time being that site recovery is good) tend to give us the impression of being "fuller" in terms of the number of sites and their sizes than patterns of occupation at given points in time actually were. The way in which we look, for instance, at the relationship between land use and population density in an area, as seen through site densities and site sizes, is shaped to some extent by the character of our maps. It may even be that the nature of our maps has played an instrumental role in making population questions of this sort the dominant theme of survey studies. There would appear to be no easy solutions to the problem of overestimated maps. The length of time periods

in most regions of the world is usually measured in centuries and seldom falls below 100 years even for historical periods. As time periods are made smaller, the chances of assigning a given site unambiguously to a given time period also declines. It is worth noting in addition that on a given map the problem may affect sites belonging to different size classes in different ways. For example, it would be worse for small sites occupied on a temporary basis than for larger, more continuously occupied sites. There will always be the temptation to adopt ad hoc solutions to the problem, but before this is done, it would seem useful to spend more time trying to understand the distinctive character of archaeological maps and the implications this may have for their treatment.

## CURRENT ISSUES

**SAMPLING** In concluding this review, it is worth looking forward and commenting briefly on four areas that are likely to become increasingly important for surveys. Questions concerned with the coverage of the landscape during survey work and with the quality of site recovery are obviously fundamental to surveys. Most attention has been focused on the first question or sampling. While a good deal of ink has been devoted to the subject by those taking one position or another, it is by no means easy to pick one's way through the literature on sampling which resembles in some respects a battlefield littered with pieces of machinery that have broken down (32, 76, 82, 97, 101, 136). Probability sampling was originally sold as an uncomplicated panacea for improving data collection and the quality of inferences made in archaeology (13). In fact, the technical aspects of sampling are far from a simple matter. This is well known to those who have tried to read Cochran's classical textbook on sampling techniques, especially the later chapters which deal with cluster sampling, the form that sampling usually takes in archaeological surveys (28). In sampling designs, probability does not operate on a list of sites (initially unknown) but on lists of grid squares, arbitrarily defined spatial units, which cover an area. It is unfortunate that the basic distinction between simple random sampling and cluster sampling was not made clear in much of the literature on sampling in archaeology through the mid 1970s. Confusion is not lessened by the frequent technical and computational errors made in this same literature which have made it a fertile ground for criticism (61, 75, 83, 98, 109, 121). The critiques tend to speak to technical questions, which are lost on the general reader, and have not helped much in clarifying basic issues. It is worth noting that there are other demands than sampling that operate on decisions about survey coverage, such as the utility of working with a contiguous area, if there are plans for doing a spatial analysis of survey data. This is illustrated by the

simulation study of sampling designs done by Santley (117), using maps from the Valley of Mexico. It is also seen in the choice of the Long House Valley, which has a 100 percent survey coverage, to demonstrate the approach to the analysis of site locations and distributions in SARG (39). In their own interests, spatial analysis and probability sampling often pull in opposite directions. If only to make matters worse, changes are afoot within the statistical community on what is meant by a sampling design and on strategies of inference in survey sampling. As Cassel et al (22) note, the sampling design has been an all-important element in classical survey sampling, whereas for newer approaches, it is less important how the actual sample is selected.

One unfortunate consequence of the debate on sampling is that it has drawn attention away from what may represent a more important question, the quality of site recovery. We have been distracted by the technical and more formal questions arising about sampling from asking the basic question: how well are we doing at recovering the sites that were *once* occupied in those areas that we cover? For most surveys, no clear answer to this question can be given. We tend to be optimistic and assume that all or most of the sites in an area can be detected during a single coverage. However, due to factors beyond our own control such as ground cover or geomorphology, many of the sites in an area may go undetected. One way of trying to gain some control over this question is through the repeated, intensive coverage of an area. Our own work in Calabria in southern Italy along these lines suggests that in certain areas only a fraction of the sites can be detected during a single coverage, and several passes are required before a point is reached where few or no new sites are detected. In other cases where conditions are more favorable, it may take only one pass. The point here is that we can no longer assume that a single coverage is all that is required to insure the full recovery of sites during a survey. It is likely that in many parts of the world where surveys have been conducted only a fraction of the sites once occupied have actually been recognized during the course of coverage. It would be useful if some of the energy spent on questions of sampling were redirected to the issue of the quality of site recovery.

**SPATIAL ANALYSIS** There has been a rapid growth of interest in spatial analysis during the last 5 years (27, 60). The issue here is not so much whether spatial analysis should be done but rather which are the best ways to proceed. Since many of the inferences made in archaeology are based on the patterns that we recognize on maps, it is realized that there is a need to go beyond the impressionistic scanning of maps and treat the analysis of spatial distributions in a more rigorous way. One solution has been to turn to methods of point pattern analysis developed by geographers and ecologists.

gists. One of the better known methods that has been borrowed is nearest neighbor analysis which has been employed in a range of archaeological studies (41, 89). Other methods such as quadrat analysis and spectral analysis have been tried out, and a start has been made at the comparison of the results of alternative methods (49). The limitations of such statistical techniques, however, are that they impose unwarranted assumptions on distribution maps and they are highly reductive. The "richness" of maps, and in particular the configurational aspects of their patterning, seems to be lost in the computation of summary statistics. What appears to be needed are heuristic methods which are less reductive and take greater account of the context of a research problem and the pattern-matching abilities and strategies of skilled human analysts (69). There is also a need to develop methods for dealing with situations where point symbols do not provide an adequate representation of settlement patterns on maps. This occurs when occupation extends over substantial areas forming "patches" on maps, as is the case with Aztec settlement patterns in certain parts of the Valley of Mexico. A third line of study in need of development involves the analysis of patterns of continuity and change in site occupation as one moves through time from one period to the next. Much new work remains to be done in the area of the development of methods of spatial analysis tailored to the distribution maps and research problems of surveys.

**LOCATIONAL MODELS** If we turn back to a volume such as *Man, Settlement and Urbanism* published in 1972, we see a wide ranging interest in settlement questions but few explicit uses of locational models (65, 139, 142). When it comes to the use of models for interrupting patterns of settlement, there has been an equally rapid growth over the last 10 years (33, 67, 150). Archaeologists have again been active borrowers turning to geography and closer at home economic anthropology (53, 63, 125). Again, the results obtained to date often reveal an imbalance between the locational models adopted and the problems being explored. A distinction can be made between models which apply to individual sites and those which deal with a group of sites in a regional framework. The former would include site catchment studies which are the subject of a recent review article by Roper (111). Site catchment analysis—the term model is probably more appropriate than analysis—was originally introduced by Vita-Finzi & Higgs (140) as a means of characterizing the economic potential of a prehistoric site so that its economic organization could be understood and comparisons also made between sites. A site's economy was characterized by the composition of soil types represented within a given radius of the site. The attraction of such a model was that it seemed to offer a shortcut to the study of palaeoeconomies without the need for excavation (57). Flannery and others have discussed the limitations of site catchment analysis in its classical form and

have proposed alternative approaches which require, for example, more detailed evidence on a site's economy which can only be obtained from excavations (24, 43, 60, 155). Site catchment analysis has also been adapted for regional studies where it is used in effect as a means of studying how agricultural land is partitioned among the group of sites in a region (20, 87, 131, 132, 155). A major problem here is that the size of the catchment radius is usually taken to be the same for all sites: in other words, the model makes the assumption that the size of a site's catchment is independent of the size of the group living at a site. It would seem to be more realistic for a site's effective catchment area to be some function of its population size and perhaps even its technology. This is incidentally the kind of treatment used by Adams (5) in his simulation study of the "cultivated area" around Mesopotamian settlements.

Extensive use has been made of central place models as an approach to regional analysis in archaeology, following the pioneering work of Johnson and Hodder (60, 66, 68). Crumley (33) provides a review of the burgeoning literature on regional analysis where much of the recent work has been on Mesoamerica (8, 21, 40, 54, 59, 126, 127). One question debated here is the extent to which the ancient economies involved correspond with market economies. Another is whether or not it is appropriate to use central place models when economic systems do not conform closely to the market economies that are assumed by such models. There are no easy answers to these questions at the present time; much basic work still needs to be done in defining the early economies. One alternative open for archaeologists and anthropologists is to develop their own models which would fit more closely the social and economic contexts that are involved. An example here would be the study by Steponaitis (131) of complex chiefdoms in the Mississippi Valley. Renfrew (106, 107) has recently experimented with models and analytical techniques that emphasize the political dimension of spatial organization and also the dynamic time behavior of complex cultural systems.

**BACK TO BASICS** One of the clear trends in survey archaeology is a return to basic questions as they apply to the nature of the surface of a site and the character of surface material. In some ways this represents a return to basics that were skipped over and never originally learned in the first place (7). A major factor contributing to this trend has been the frustrations encountered when surveys have been "extended" to areas where conditions are less favorable for the conduct of surveys. The results obtained by surveys done in more favorable areas such as the case studies reviewed above have encouraged high expectations about what surveys can accomplish. One response to the less satisfying results often obtained in areas with less favorable conditions for survey work has been the refinement of field methods. Another factor contributing to the return to basics has been the growth

of public archaeology where further external constraints are often placed on the design and conduct of surveys (48, 119). Projects such as those in the Cache River Basin and at Black Mesa have demonstrated that it is possible to make significant research contributions within the context of contract archaeology (52, 62, 120). A third factor promoting the rediscovery of basics has been the shift on the part of some archaeologists to more intensive strategies of survey work, where the quality of site recovery and the systematic collection of surface material at sites are considered as important as the actual number of sites found. This is seen in a growing interest in intensive surface collection and the development of techniques such as the replicated collection of site surfaces (10, 14, 42, 44, 103). Another aspect of this return has been a greater appreciation of the role played by geomorphology in shaping the landscape as well as its influence on the visibility of sites on the surface of the landscape (15, 34, 35, 47, 56, 138). Still another sector in which there has been active development involves the mechanics of the movement of material on the surface of sites and the relationships between surface and subsurface remains at a site (11, 108, 110). All of these developments point in the general direction of a better understanding of what the surface of a site represents as a phenomenon in its own right. There has been a tendency to view the surface simply as an extension of the subsurface or the part of a site where excavations are conducted. In terms of the time frame in which we work, the view taken of this part of a site (below the plow zone) is essentially a static one. In excavations we do not expect things to change substantially if the work is done this year or 10 years from now. The situation is quite different for the surface of a site and also the visibility of sites on the landscape. As we have seen in survey work in Calabria, where a small area has been subjected to repeated, intensive coverage over a period of 5 years, what is seen on the landscape changes from year to year. The situation is one in flux. The search for sites goes on within a time frame, and time itself introduces relativity into the relationship between the observer and that which is observed. On a given day in the field, time's arrow conditions in part what we will happen to see. Hence the quotation from Shakespeare at the beginning. The degree of flux will, of course, vary with the kinds and quality of information that we are trying to collect and from one region to the next depending upon local conditions. There is some irony in the fact that in going back to basics we are likely to discover things about survey archaeology that we may not want to know.

## CONCLUSION

There has been a major growth in surveys as a means of doing archaeological research over the last two decades. This is most clearly reflected in the internal growth of the three case studies that we have examined in some



detail. An obvious implication of these three studies from the perspective of research design is the importance of sustained work in an area and long-term projects. The basic questions investigated by the three groups of surveys can be seen as representing extensions of the research problems identified by Willey (148) in 1965: site location with respect to the environment, demographic trends, land use, and patterns of settlement over time in an area. But the conceptual framework for these problems has grown so that now spatial patterns are seen in terms of a series of partially independent hierarchical levels—for example, household, village, and region—each calling for interpretation in terms of its own principles of organization (4, 41). The frustrations often encountered in trying to achieve the higher ambitions of settlement pattern studies would appear to derive both from a lack of models of spatial organization for making sociological inferences from survey data and the limitations of most sets of survey data. Much new work remains to be done on the former. In the latter case, gains can be made by shifting from extensive to more intensive strategies of fieldwork. Active attention has begun to be directed to the development of locational models and methods of spatial analysis during the last 5 years. At the same time, there have been parallel developments in the design of surveys, the refinement of field methods, and an understanding of the nature of the surface of a site. Even if frustrations are regularly encountered when surveys are conducted in areas where conditions are less than ideal for the detection of sites, we have come to hold high expectations about what can be achieved by survey archaeology, which remains a comparatively young field.

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