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# The New Past: From Region to Macroregion

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## ABSTRACT

*Archaeology has produced a record of a past that was not known to scholars of the nineteenth and early twentieth centuries. Archaeology's excavations, surveys, and better chronologies contribute actual cases of societies changing over long periods of time, quite different from the fragmentary or shallow histories and comparative inferences from present-day cases that earlier scholars had available. The scale of archaeology's new cases has been regional. Theory-building has been slow to catch up to these empirical contributions. In the last decade, archaeology has begun producing sequences covering much larger geographical areas composed of multiple, adjoining regions. This richer, more detailed 'new past' is uncharted territory that requires a third generation of conceptual tools.*

## THE PAST WE USED TO KNOW

What do we know and what do we not know about the human past? Let us restrict the question to the last 30,000 years, but still adhere to anthropology's broad mandate to comprehend all the world's societies or cultures. Let us simplify matters by saying that we are not demanding to know very much, just a few basics from an ethnographic schedule like Murdock's *Outline of World Cul-*

Social Evolution & History, Vol. 3 No. 1, March 2004 81–105

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*tures* (1963). For each society suppose we would like to know

approximately how many people there were, where they were living, and some fundamentals about their economic, political, and social institutions. To further simplify, we will not ask for a continuous record, but only for a snap-shot every few centuries. If this is what we want to know about past human experience, then unfortunately we know very little.

It is easy to fall into time-depth myopia. There is more of the past than we sometimes are willing to perceive. Assume that the ethnographic record covers the last 150 years. One hundred and fifty years represents 1/200th of the time since 30,000 years ago, and 1.5 % of the time since the end of the Pleistocene. Add in a few areas for which history provides information in addition to ethnology and we might claim to know from these sources 2 % or 3 % of the past. That is, even when all we require is a simple list of facts about the world's societies, history and ethnology give us knowledge of only a small slice, because the present is dwarfed by a much longer past. Our 2% or 3% is not a representative sample of human experience, it is heavily biased toward recent times and toward those places that have written records.

In all honesty we know little about our past, and what we do know is very spotty and unrepresentative. There are few regions or smaller localities in the world for which we have in hand the basic population and ethnographic evidence for the last 30,000 years, or even the last 10,000 years, and perhaps no regions, depending on how satisfied one might be with what is often quite sketchy information.

How do we know about past human experience? Study of the past has relied on a combination of four sources. We know about the past from what we are told (oral history); from history and the primary texts from which history is written; from recent human biology, language, and society (i.e. comparative studies in genetics, linguistics, and ethnology); and from archaeology.

Historical linguistics and historical biology reconstruct temporal sequences from present corpuses. But if we only had this information we would be missing a lot. Today's languages and genetic combinations are but a small selection of those that have preceded in the evolutionary process – by definition. Also, in historical linguistics and historical biology the methodologies for sampling, for dating branch-

ing events, for studying non-branching processes, and for understanding how demographic processes impinge on historical reconstructions are all rather problematic. Perhaps most limiting, languages and genes are not populations, institutions, society, or culture. Where the question is social evolution, linguistics and genetics are an angular, not a direct approach, because the central problems are social and cultural.

Textual history is an important way of building theoretical understandings of the past. But documentary and oral history have limited time depth. Written and oral texts are also far from universal, either in terms of all the world's societies or in terms of the relevance and completeness of information.

With increasing time depth, documentary and oral history recede, and archaeology becomes more important. Archaeology does not match the richness in social fabric and individuality that oral and documentary history sometimes provides. Archaeology is relatively expensive, so it has relied on very small samples. It is also limited by the degree of refinement in its chronologies.

Because ethnology supplied information about societies unknown to conventional history, it has shared a prominent place with the discipline of history in shaping our concepts of the past. Comparative ethnology has breadth of coverage but it lacks time depth. It attempts to compensate for lack of time depth by making two assumptions. One assumption is that all relevant variation in human experience is expressed in recent societies. The second class of assumptions in comparative ethnology involves premises about ordering the existing variation in time, *e.g.*, progress, advancement, linearity, multilinearity, etc. These premises about progress and the completeness of the ethnographic record are central to comparative ethnology. To the extent that they are incorrect, then the contribution of comparative ethnology to knowledge of the past is restricted.

Comparative ethnology has asserted a theoretical knowledge of the past. Our terms, types, concepts, and explanations have traditionally come from the comparative ethnology paradigm. In this paradigm the role of archaeology has been to furnish case studies for ideas that came from comparative ethnology. Archaeology is a younger discipline than comparative ethnology. When archaeology began producing more detailed data in the twentieth century, the explanatory

frameworks had long been established by comparative ethnology. Archaeology provided examples, not new frameworks; it filled in an existing order, it did not create the categories. The major American history of anthropological theory (Harris 1968) mentions archaeology in only 20 of 687 text pages (3%), yet Harris was by no means hostile to the archaeological contribution. Carneiro (2003: 276) says that the role of archaeologists is to 'accept and apply' cultural evolutionism. But Carneiro also admits, 'the task of advancing the study of cultural evolution is today largely in the hands of archaeologists. Ethnologists have, for the most part, abdicated from this endeavor, or, at best, quietly retired from the scene' (2003: 277).

### **THE BEGINNING OF THE NEW PAST: REGIONAL RESEARCH DESIGN**

If there was a moment that began to alter how we acquire the past, I think it was when some of the New Archaeologists formalized the regional research design. An early advocate of regional archaeological research was Struever (1971), who wrote that to reconstruct cultural systems, archaeologists needed to cast aside normative definitions of culture and jettison all that normative thinking implied for field methodology. The emphasis was to be on variation. He pointed out that non-normative approaches called for more and better quality data than normative, culture-history archaeology was accustomed to. If archaeologists were to master variation at a higher level of data quality and quantity, they needed larger-scale projects, with more staff and more funding. Looking back over the last 35 years since Struever's statement, I think that he was prescient. Archaeological projects of the kind he called for did in fact produce a new kind of knowledge about the past.

A key element was the settlement pattern survey at the scale of a whole region (several hundred to several thousand kms<sup>2</sup>). It had its beginning only in the 1950s (Willey 1953; Billman and Feinman 1999). This movement was hived from Steward's and White's comparative ethnology, but as it developed it opened up new pathways. Regional surveys in archaeology map the distribution and abundance of human settlement and other cultural and environmental features at

time intervals set by artifact chronologies. These surveys cover large areas, many orders of magnitude larger than excavations. The surveys are methodical, systematic (Fish and Kowalewski 1990). They generate information valuable in its own right, not just sites to be excavated. They can be used as a time-space framework on which to attach other kinds of information, such as excavations, history, ethnographies, or censuses (Kowalewski 1997).

The regional surveys moved the emphasis in archaeology from the study of sites or tombs to the study of regional societies. They provided systematic data that could begin to approach the simple requirements proposed above: the distribution and abundance of people, and an outline of ethnographic description, at least as seen through the long lens of archaeology. The case, or unit of analysis, was not the site, the community, or the city, it was the broader social system, whole sets of interacting settlements, communities, or cities.

As regional-scale archaeological information became available, normative concepts of culture lost their relevance, as Struever and other New Archaeologists had foretold. Regional studies showed that past social systems had too much variation – at one time and across periods – to be comprehended by the archetypes and peremptory labels of normative culture-history. Regional archaeologists had to look elsewhere to other disciplines or within their own data for concepts. Fortunately, some theoretical and methodological tools for analyzing regions already existed. Archaeologists borrowed freely from ecology and geography, modifying and broadening those models to apply to human populations in the case of ecology, and non-industrial situations in the case of geography. They found analytical utility in ecological concepts such as catchments (Flannery 1976), patchiness (Blanton *et al.* 1993), predictive models of resource use (Jochim 1976), and landscape modeling (Peters and Blumenshine 1995); and geographical concepts such as rank-size (Whalen and Minnis 2001), central place hierarchies (Blanton *et al.* 1982), cost surfaces (Varien 1999), and indices of urbanization (Adams 1981). It is interesting that regional archaeologists often found these approaches more useful for analyzing their data than they did the concepts from normative culture-history or cultural evolution. However, the ecological and economic models often have little social or cultural content.

Wedded to ethnology by bonds of society and culture, regional archaeology nevertheless cast doubt on the two fundamental assumptions of comparative ethnology discussed above, the assumption of completeness of the ethnographic record and the premises of time-ordering. Archaeology (as well as recent historical studies) shows that the past had many types of societies not represented in the ethnographic record (see Wobst 1978 on 'the tyranny of the ethnographic record'). Archaeology also showed that the past was more dynamic, i.e. there was more change than anticipated by the present ethnographic record. For example, in my area of the southern United States, archaeologists recognize a minimum of nine major periods in prehistory since the Paleoindian colonization. Each of these nine periods represented a significant transformation of society from the previous period. During eight of these periods the societies would be termed 'hunter-gatherer' or 'bands and tribes' in conventional parlance. Yet the cultural changes between periods when societies would be called bands were often as great or greater than the shift between band and tribe (if that could in fact be pinned down). The southern U.S. is hardly special – the same unanticipated dynamic variation is characteristic of most world areas when investigation is sufficient to produce a reasonably detailed sequence.

There is no theory in orthogenetic cultural evolution to explain Mesopotamia's secular decline in urbanization (percentage of population in cities) between 2600 and 600 BC, as discovered in Adams's surveys (Adams and Nissen 1972; Adams 1981). Comparative ethnology did not anticipate that sedentism among hunter-gatherers would have been the norm not the exception, but archaeology has shown that nomadic hunting and gathering is the exception. Regional archaeology provides the basis for other trends not anticipated in the orthogenetic time-ordering of the comparative method: increasing complexity by decentralization (Kowalewski 1990), periodic chiefdom cycling (Hally 1996), the rise and demise of the Chaco Phenomenon on the Colorado Plateau (Vivian 1990), the rise and collapse of Maya city-states (Culbert 1988), core zones that become peripheries and then cores again, like the Basin of Mexico (Sanders *et al.* 1979), etc. Initial comparison of scattered regional sequences from several civilizations demonstrate considerable variation (Wright

1986). If archaeology were only offering up sites or cities as cases to test comparative models, its cases could be dismissed as exceptions. But sequences involving all the settlements over whole regions, some of which have been major foci of civilizational development, are not easily dismissed.

The regional surveys in archaeology join with recent skepticism about orthogenetic cultural evolution emanating from other scholarly traditions. For example, a comparative, historical ethnology current in Russia has brought 'alternative pathways' to the forefront (Kradin and Lynsha 1995; Bondarenko *et al.* 2002). In the North American Southwest archaeologists have been active in the search for new concepts in cultural evolution. One recent example is titled *Alternative Leadership Strategies in the Prehispanic Southwest* (Mills 2000). Among Africanists non-orthogenetic theory-building is active (McIntosh 1999).

By the late twentieth century scholars had thus mounted a serious, unrefuted challenge to the assumption of the completeness of the ethnographic record, on the grounds of the types of societies and the dynamic variation missing from ethnology. Regional survey has played a central role in this movement. Archaeology in the last few decades has created a new past. Archaeology is now in a much different and potentially more creative position than it had been vis-a-vis anthropological inquiry and theory-building.

## LIMITATIONS OF REGIONAL SURVEYS

Since most human experience lies in the past, and since most of the past is the terrain of archaeology, one might conclude that archaeology has replaced comparative ethnology as the major source for theory-building about the past. Not so. There is a lag between innovations in data and conceptual development. Modern regional research is only in its fourth decade.

Archaeology and the regional surveys have some inherent disadvantages. Chronologies are sometimes stubbornly difficult to refine, and archaeology cannot divine thought and speech. There are material limitations. Archaeology is a small discipline. In the United States, for example, there are fewer archaeologists than there are trombonists

or pet groomers. Under ideal conditions regional survey can be carried out in many places, but in this world archaeology is relatively expensive. As Struever had said, effective archaeological projects are large in scale, and costly. A regional survey of the kind we did in Oaxaca might cost \$50,000 to \$100,000 or more (roughly equivalent to a large archaeological excavation project).

Aside from some of archaeology's inherent limitations and the economic difficulties, there are theoretical limits to the regional research design. As used here, a region is relatively small, a few thousand square kilometers. If we had hoped that questions about major changes at the society level would be answered simply by attaining regional-scale information, such a hope was naive. (It may also be noted that many of the ethnographically known societies used as 'cases' in cultural evolution are also relatively small.)

Regional studies in archaeology are able to address the ecological and economic fit between population, social groups, and resources at the local level. Such studies can capture local and regional polities and exchange systems. But the long-term dynamics of political economy, that is, the great phase changes of archaeology, involved more than the single polity – these transcended regional boundaries and involved many participant polities. Periodic movements in macro-scale political economy turn out to be quite important for regional and local adaptations. It is the larger-scale transformations that set many of the parameters for local social groups and their economic and ecological adaptations. Regional surveys provide control over some endogenous factors, but major societal transformations are often due to larger, exogenous factors not controlled at the scale of a single region.

One response by archaeologists has been to look to the macroregion, a combination of multiple, interacting regions, up to the scale of the culture-area or large part of a continent (Blanton *et al.* 1993). In the last two decades, archaeology has begun to embrace areas much larger than the single survey region. Progress has been by accretion, one region at a time. Now, in several world areas, multiple, adjacent surveyed regions form blocks of over 10,000 square kilometers. If regional studies were a second generation of analysis and theory-building, macroregional analysis is the third generation, a new past



not well understood even by the scholars who have seen the data. The implications of this new past barely have been glimpsed.

## **MACROREGIONAL RESEARCH**

By the term macroregional analysis I mean something more demanding than having a broad geographical perspective. The term I am using is like Carol A. Smith's 'regional analysis' (1976), writ much larger in space and time. The data requirements for macroregional analysis are daunting. As I see it, macroregional studies require these foundations:

- Large size, tens or hundreds of thousands of square kilometers.
- Contiguity, few spatial gaps in coverage.
- Time depth, the full range of social evolution and sequence of transformations.
- Refined chronology.
- Multiple regions, polities, environments, languages.
- Comparability of methods and data between regions.
- Full coverage, samples approach 100% of settlements.
- Settlement data enriched by systematic cultural information.
- Long-term curation and improvement of data and supporting collections.

No place in the world meets these criteria – yet. But several places come close enough for us to know what the new past will look like. In the next sections I discuss two macroregions I know about where recent developments in research are opening up new possibilities.

### **Mesoamerica**

Studies in the last few years in Mesoamerica offer one prototype of the macroregional potential and illustrate the conceptual and methodological challenges. The first study, from Oaxaca, is fairly primitive; the second includes Oaxaca as a part of a much larger combined data set that also takes in the core of the Aztec area of Central Mexico.

Oaxaca is central to Mesoamerica, lying midway between the Aztec and the Maya areas. In Oaxaca, fourteen regional study areas cover approximately 8,000 km<sup>2</sup>. There are some spatial gaps isolating

several of the projects, but coverage is generally contiguous. The data run from 1500 BC to AD 1520, the course of precolumbian Mesoamerican civilization. In some places archaeologists can recognize 10–12 temporal subdivisions in this 3000-year span, but in practice usually fewer, and some of the phases are too long. Full-coverage archaeological settlement surveys, which are more or less comparable, excavations, and ethnohistoric studies provide the information. The combined block is composed of different physiographic and behavioral regions: small mountain valleys in the west where most of the human habitation was between 2100 and 2300 m above sea level; high, rugged mountains; the Valley of Oaxaca, the largest expanse of flat land in the highlands; the smaller Ejutla and Sola de Vega valleys at 1500 m asl, downstream from the Valley of Oaxaca; and the Cuicatlán Cañada, a narrow band of irrigable land in the hot country at 500–800 m. There were at least four languages spoken, Chocho, Cuicatec, Mixtec, and Zapotec, and the latter two had several distinct dialects. As of yet there is no institutional arrangement for long-term curation of data or integration of the on-going local and the foreign-led projects.

In spite of their environmental and linguistic diversity, central Oaxacan regions shared a common but not identical history. The term I use for macroregional change that is due to common, underlying processes is ‘concordance’. Concordant change at the macroregional scale is exhibited by simultaneous movements, in constituent regions, that are inferred to be linked to a common cause. The effects of the underlying causes may be different in the various constituent regions, because regions may have played different roles in the macroregional system. For example, a common shift toward intensifying staple production might result in greater household reproduction in core regions but more emphasis on large estates on the peripheries. Or, insecurity due to state fragmentation might result in settlement abandonment in some regions and concentration in nucleated towns in other regions. In both hypothetical cases, the underlying causes are the same, there are roughly simultaneous consequences in the various constituent regions, but the regions vary in their responses. Concordant change expects some similar responses to new conditions, and some different responses.

Comprehending multiregional concordant change is not easy, it is a multivariate problem with the added dimension of time. One way to simplify the mental task is to focus on indices for the states of the component regions. Figure 1 illustrates what I mean for just two regions in Oaxaca, the Valley of Oaxaca and a large part of the Mixteca Alta that lies 50 km to the west of the Valley of Oaxaca. The horizontal axis represents time, 1500 B.C. on the left and A.D. 1500 on the right. The first graph of the three shows the sheer number of archaeological sites by period for both regions. The curves for the two regions generally move in the same directions, although their values and rates of change are different. The center graph uses the summed occupied areas of all the sites in each period. This graph is different from the previous in that the sites in the Mixteca Alta tended to be larger on average than those in the Valley of Oaxaca.

Settlements having the same surface area in hectares may differ substantially in their numbers of inhabitants, i.e. the population densities might vary. The third graph takes different population densities into account by using our population estimates, which are based on assessments of within-settlement population density. It is in this variable that the two regions track each other the most closely.

The example of concordant change in Figure 1 is a fairly simple one. The Mixteca Alta and the Valley of Oaxaca moved in similar directions in most periods, in numbers of sites, total occupied area, and total population. There were differences. The Valley tended to have more small settlements and the Mixteca Alta tended to have more large, densely nucleated sites. The two regions diverged most in the middle periods, Late Formative to Epiclassic.

This case can be pursued a bit further, with more cultural content. Figure 2 is a visual representation of settlement structure for the same two regions, plus several other contiguous regions in central Oaxaca (it is based on Balkansky 1997; Balkansky *et al.* 2000; Feinman and Nicholas 1990; Finsten 1996; Kowalewski *et al.* 1989; Spencer and Redmond 1997). The drawings are not settlement maps, but graphic simplifications designed to highlight major characteristics of scale and complexity in settlement. The next six paragraphs describe the sequence of transformations in Figure 2, reading from bottom to top.

– Human groups were interacting and interdependent over this whole area, from the beginning in the early Holocene. The transition from nomadic gathering-hunting to sedentism and farming occurred no later than during the second millennium BC. After that time, in the earlier Formative represented by the lowest row in Figure 2, settlement consisted of local variations on a pattern of a head town and surrounding cluster of villages and hamlets. This pattern lasted for a thousand years.

– About 500 BC the clusters in the Valley of Oaxaca added a new paramount capital, Monte Albán, initiating urbanization and a major cultural transformation in the Valley of Oaxaca (Blanton *et al.* 1999). Within 200 years, a tide of militarism affected all of central Oaxaca, especially in the west, the Mixteca Alta, where all Formative settlement clusters were abandoned and replaced by fortified hilltowns (Kowalewski *et al.* 2001; Balkansky *et al.* 2000). In Figure 2 notice the proliferation of the hilltown symbols in each region after 300 BC. The Monte Albán state conquered the Cuicatlán Cañada (Spencer and Redmond 1997), but its effects in the Mixteca Alta were probably less direct.

– By the first centuries AD rural abandonment and consolidation had resulted in only three large urban centers, Monte Albán, Yucuita, and Huamelulpan. Large expanses of the Mixteca Alta, including virtually all the fortified hilltowns, were abandoned. By the third or fourth century AD, the Early Classic, the whole area was recolonized with small cities, towns, hillforts, and dispersed settlement. This was a time of peak population and economic integration, coinciding with the florescence of Teotihuacan in Central Mexico.

– In the Late and Epiclassic, A.D. 600–900, this fairly integrated urban system in Oaxaca broke down. There was another episode of major regional abandonments, loss of integration, loss of population, and consolidation at a few nucleated settlements. Elsewhere in Mesoamerica this time period saw the collapse of urbanism in the southern Maya lowlands, and the fall of Teotihuacan.

– Massive population growth and reintegration took place in the Postclassic (the top row in Figure 2), with strong urbanization in all regions, intense economic activity, and competition and alliances among multiple small states. This was the time of the Mixtec king-

doms or city-states (Spores 1967), and in its last century, the expansion of the Aztec empire.

In this illustration, each component region behaved somewhat differently, yet they tended to change together, concordantly, due to underlying political and economic processes that we do not yet fully understand. Figure 2 represents a graphical attempt to preserve some of the variation, while highlighting a few major trends in scale, complexity, and settlement types.

This Oaxaca case does not meet all the requirements of macroregional analysis outlined above; in fact, it is rather small and simple. But I think that even at this stage of research one can appreciate how the explanation of regularities with multiregional variation is a different, more challenging enterprise than the pigeon-holing of archaeological cases into predetermined boxes of culture-historical identities or band-tribe-chiefdom-state.

One of the scholars working at the macroregional frontier of the new past is Charlotte A. Smith, who recently completed a meta-data synthesis of all twenty systematic regional surveys in highland Mesoamerica (2002). Smith's study combines all the surveys from central Mexico to Oaxaca. In this broad area the regional surveys are all in a single methodological tradition, that begun by William T. Sanders in the Basin of Mexico, the Aztec area. Smith compiled a dataset of 14,800 archaeological sites. She studied inter-regional patterns using indices of population, settlement patterns, and civic-ceremonial architecture. Smith's results suggest that highland Mesoamerica cycled back and forth between periods of greater and periods of lesser economic integration and inter-dependence between regions. Mesoamerican civilization grew in scale because of the intensive economic interdependence of its constituent regions. However, not all regions responded in the same ways. Regions functioning as cores had higher population density and more urbanization than regions that functioned as peripheries to the cores. Mesoamerica is a civilization with multiple cores and peripheries. It tended to grow as a whole when there was a high degree of interdependence between core zones; it tended to stagnate as a whole when individual cores were more isolated from one another. Smith's study also demonstrates that hierarchical complexity could be built in two ways, through highly central-

ized primate systems or through more intensive development at the middle range of the hierarchy, in its secondary and tertiary centers. A synthesis like Smith's has never been done before, but her work opens up entirely new realms of inquiry.

### **Southwest**

A major development toward macroregional capabilities has taken place in the archaeology of the U.S. Southwest. Here, an early vision of comparing different regional project areas studied with similar systematic methods dates from the 1970s in the form of a cooperative known as SARG (Gumerman 1971). SARG was an interesting program that did not come to fruition; it was surpassed by other developments. In fact, the basic Southwestern macroregional analysis was not built from cooperating research projects, as SARG had intended and how it happened in Mesoamerica. Instead, what made macroregional analysis possible in the Southwest was the site registries maintained by museums and state governments. These databases, which began under the auspices of museum research, now contain tens of thousands of archaeological sites. Official site registries have grown tremendously in the last several decades due to the explosion of contract archaeology and legally mandated site inventories. By the late 1990s several scholars had published settlement distributions for large parts of the Colorado Plateau (Adler 1996; Duff 1998; LeBlanc 1998).

In completeness of coverage, breadth, and systematic rigor, the successful ascent of the macroregional peak in the Southwest was led by David R. Wilcox (2002a, b). Wilcox and his collaborators have assembled data on nearly all archaeological sites larger than 13 rooms (rooms are the conventional way of describing settlement size in the Puebloan Southwest), for an area of 600,000 km<sup>2</sup>. This macroregion covers the whole of the Anasazi/Hohokam U.S. Southwest; some is also known about the adjoining Mexican states of Sonora and Chihuahua. This is the largest, most complete, and best chronologically controlled macroregional dataset anywhere in the world. At present the time span is 400 years, but there are plans to extend it back in time. What makes this macroregional project interesting, in addition to the huge area, is the chronological refinement. Archaeologists can

divide the 400 years into 50-year intervals – just two or three human generations – and correctly assign most of the sites to these sub-phases.

The Southwestern macroregion certainly qualifies as heterogeneous. Environmentally, elevation differences are over 3000 m, and vegetation communities range from desert shrub to montane forests. In terms of paleoclimate, it is one of the best studied areas in the world. Linguistically there were at least three high-order language families represented. Beyond the settlement information in the databases, the Southwestern case is enriched by detailed information on architecture, many classes of artifacts including textiles, petroglyphs, painted pottery, trade items, ethnohistory, a rich ethnographic record, and importantly, the living Native American communities.

Wilcox's research finely documents the emergence of Hopi, Zuni, Acoma, Laguna, and the Río Grande pueblos (the Native American pueblos at the time of the Spanish conquest), from an earlier social landscape that was much different. In terms that are too preliminary and too general to do justice the variation, one could describe this as a 400-year process of coalescence (Kowalewski 2001) of towns as social groups out of earlier, more dispersed social units. Large towns replaced small hamlets and villages as the dominant settlement type. This happened, concordantly, in quite different physical environments. Some communities disappeared or were consolidated into larger ones. All of this happened during a secular trend of overall population decline. There is evidence of violence in some places, not much in others. There was intensified production of key commodities and more evidence of regional and long-distance trade. Coalescence into larger towns and alliances of towns was accompanied by increasing investment in integrative ritual.

In standard cultural evolution, none of this was evolutionary change, it was all 'tribal', all one stage. But the new data suggest profound changes in how societies were constituted, differentiated, and integrated, that is, profound changes in complexity. The conceptual language for describing this 'rearrangement of complexity' has yet to be worked out. The mechanisms of change – how similar but not identical things happened over a vast area – are not well understood.

The Southwest is one of the world's more studied places, ethnographically and archaeologically. Some of the many anthropologists whose work in the Southwest contributed to general ethnological theory include A. L. Kroeber, Fred Eggan, Julian Steward, and Leslie White. Their research on cultural development, social organization, cultural ecology, and cultural evolution is still important to us. But, if by some quirk of history, these scholars of the mid-twentieth century had had access to the macroregional archaeology of 2003, I strongly doubt that their ethnological theories would be the same as what they had written in the mid-twentieth century.

### **THE NEW FUTURE OF THE PAST**

In a way the Mesoamerican and Southwestern cases represent a bottom-up, demographic, settlement approach to civilizational studies. In the inductive stage of our methodology, one does not know what is coming, there are still new discoveries, many surprises, and significant nuances. In the tandem journey of theory and data, the latter has leaped ahead a bit. But one must make theoretical contact with the mass of detail.

The new past poses cognitive and conceptual challenges. Comprehending concordant changes such as these in Mesoamerica and the Southwest is one of the major intellectual challenges. It is difficult to hold in mind multiregional variation for a single time period, and even more so to keep track of changes in each constituent region over time. One needs to retain the variation as long as possible in study and analysis, because this is how one learns in new branches of research. We need more population thinking and less typological thinking. Yet to comprehend and to reach a higher comparative and theoretical level, at some point variation needs to be simplified and reduced to a concept. Graphics may help to enhance comprehension. Whereas regional method and theory were readily available in other disciplines, macroregional or multiregional method and theory are more challenging. From the present perspective, there are at least four different analytical approaches, none of which is well developed in archaeology:



1) Macro models, or the ‘big thinking’ of V. Gordon Childe or world-systems approaches (cf. Kristiansen 1998 for Bronze-Iron Age Europe);

2) Simple extension of ecological and geographic models to cover the larger areas;

3) Comparisons, with separate regions being the units of analysis (*e.g.* Blanton 2000);

4) Modeling and simulation to predict flows and behavior of components given the state of the whole and vice versa.

Macroregional study requires conceptual innovation. Regions vary functionally in ways not comprehended by culture-history labels, evolutionary stages, or core-periphery dichotomies. We need ways to describe multiple dimensions of complexity. We need to know more about the flows of goods, people, force, and information, *i.e.*, the interactions that make regions and their neighbors interdependent. We need to know more about the institutional basis of inter-region interaction. Once we have macroregional datasets from different world areas, the next obvious step will be to compare their trajectories. In effect, at that stage we will be re-doing general cultural evolution, this time from the ground up.

In review, this paper argues that archaeology since the 1960s has slowly produced new, regional data and the potential for new ways of thinking about the past. These data cover certain fundamentals of demography, ecology, economy, and social organization. Systematic regional surveys were the first step. As more regional surveys were carried out, archaeologists were able to conceive of systematic data coverage for multiple, adjoining regions.

In the new past, civilizations were more dynamic, that is, they changed more over time as well as across space, than envisioned in comparative ethnology. Regional and macroregional perspectives show how civilizations are built by intensifying integration, how complexity is not the same as centralization, how complexity can be built by development of the top or the middle of urban hierarchies, how waves of violence retard growth, and how new collective orders are constructed to control widespread violence and disruption.

The past has a new future. Heretofore, knowledge of the past has been dominated by the paradigm of comparative ethnology. The po-

tential exists for empirically-based theory-building independent of the comparison of cases from the ethnographic present. But building appropriate macroregional method and theory is still a major intellectual challenge. Whether the models of the conventional paradigm are upheld or not, it is always advantageous to be able to test ideas with independent data. That independence may turn out to be one of the durable contributions of the new regional and macroregional studies.

### **ACKNOWLEDGEMENTS**

I presented an earlier version of this paper in 2002 at the conference 'Hierarchy and Power in the History of Civilizations', in St. Petersburg, Russia. The Central Mixteca Alta Settlement Pattern Project was funded by the National Science Foundation and authorized by the Instituto Nacional de Antropología e Historia. I sincerely thank my Oaxacan and Southwestern colleagues, especially Richard E. Blanton and David R. Wilcox, for their help, advice, and criticism, but they will not necessarily agree with what I say here.

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