

Memorandum to: Murray Gell-Mann Concerning: The Complications of Complexity in the Prehistoric Southwest

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The Complications of Complexity in the Prehistoric Southwest

INTRODUCTION: PERCEPTIONS OF COMPLEXITY

(From a memo to Murray Gell-Mann)

Dear Murray:

You'll remember that at the Santa Fe Institute (SFI) conference that George Gumerman and you organized, I was a member of the group commissioned to write an essay on "historical processes in Southwest prehistory." This mandate was, I fear, confusing. Archaeologists have not always been clear about what historians do and, as a student of Mesopotamian historic civilization, the maxim "no writing, no history" has seemed unproblematic. Some archaeologists call themselves "prehistorians" precisely because they work in periods and in places in which there are no written documents. This distinction doesn't always hold, however, because many prehistorians frame inferences about the archaeological record through colonial texts (e.g., *visitas* and *relaciones* in the New World) and especially through ethnographic analogies, namely the texts created by social anthropologists or ethnoarchaeologists. Archaeologists also read "texts" created by other archaeologists

and the past is thus inscribed, revised, and “textualized” in an internal discourse that in history is called historiography. Perhaps archaeologists, increasingly self-conscious about how they know the past, will soon be teaching courses on “prehistoriography.” (Not.)

Our group’s conception of the “history” of nonliterate peoples was largely a reaction to the reliance by so-called “new archaeologists” on adaptation as both a definition for culture (following Leslie White and Lewis Binford) and as an explanation of cultural change. We distanced ourselves from that view by letting Randy McGuire recite the word “contingency” as much he wanted. By this we wanted to imply that ancient people were “agents,” making choices among various behaviors, and that these choices had to be negotiated in the context of previous choices which themselves were the products the cultural tensions and struggle for power. (All this is sounding more poststructuralist than our discussions were. In fact, we behaved as contractees, charged—we felt—to show that local archaeological sequences were “history” and that meaning as well as subsistence could be read from the ancient data.) I can’t claim our chapter is ultimately successful: Haas and McGuire wrote all the substantive parts and Levy and I tried to fit our introduction and conclusion into the extremely rapid review of data in the chapter. Sometimes it’s good to be the victim of discrimination, in my case of implacable alphabetism: I hope our chapter will be cited (if it ever is) as Haas et al.

Since I attended the conference as a minor member of a panel, I was quite surprised when George asked me to write a “commentary” chapter for the volume. Of course, I recognized the syndrome: find an outsider to keep the Southwest experts honest; that is, make them speak to a larger audience than their own brethren and sistren. Outsiders are also thought to be able to identify areal or disciplinary biases that are less visible to the initiated assembly. Sometimes the procedure works well as, for example, in Greg Johnson’s¹⁵ outsider’s commentary on Southwest archaeology, which has stirred up some useful controversy among Southwest archaeologists. However, the trade-off can be that the discussant is so distant from the action that he/she never quite gets the point or writes something so hopelessly mired in his/her own bailiwick that one regrets the entire enterprise.

I’m writing this memo to you, Murray, because, having been assigned my present role, I’ve got more than the usual discussant’s problems and I think you’re just the person to help me. After all, you’re an outsider yourself to Southwest archaeology (though you know a lot about the subject, according to the insiders) and you’re a *real* complexity humanoid in a conference that purports to consider what is complex about the Southwest and what that designation of complexity might mean to future research. Also, you are known as one not happy with cant or the academic posturing designed to make reputations rather than advance knowledge. My problems (as discussant for the papers of this conference) concern what I think might be some misunderstandings between the SFI complexity folk (mainly Stu Kauffman, Chris Langton, and perhaps yourself) and the collected Southwest archaeologists; on a more pessimistic note, I’m worried about one of the assumptions that has justified the conference: if Southwest archaeologists can only learn about “complexity” from the SFI experts, they will become better archaeologists. I write this memo,

then, presuming that you'll tell me where I'm off base on how the SFI investigates "complexity" and also whether my own notions of complexity in archaeology—which incorporate a consideration of Southwest prehistoric cultures—are of any interest to the mission of the SFI.

I do not presume to say anything significant about Southwest archaeological data in this memo. As the participants in this conference demonstrated, the control of data and method in the Southwest is exemplary. It is particularly impressive how the dimensions of geology, geography, climate, bioanthropology, and other specialties are regularly and fruitfully brought into Southwest investigations. Furthermore, Southwest archaeologists like to work together, to talk together, and corporately to advance knowledge in their field. (They also give good parties, if you like cheap beer and tortilla chips.)

It is the openness of Southwest archaeologists to new interpretations, however, that has led to their recent susceptibility to what has become a pandemic archaeological disease. They think good theory lies "out there" in some field other than their own and, if they just read enough and talk to the right people, they'll get an idea that will transform their understanding of their data. This is getting truly weird! When I came to the Southwest in 1972 (well, I was a student at the Vernon field school in 1965, when Paul Martin, Jim Hill, John Fritz, and Mark Leone were my mentors; Fred Plog alluded to this at the conference, but I don't think anyone picked up on it and I didn't want to jeopardize my outsider status by admitting actually to have worked at a Southwest site), my Tucson archaeology colleagues were adamant that the Southwest was where the real action in archaeology theory was. Now in the 1990s, theory seems to be anywhere but in the Southwest! Indeed, the very reason for the conference (for many of the participants) is that an appreciation of what are "complex adaptive systems," especially as they are understood by certified scientists, will turn out to be the key for interpreting ancient social organization and social change in the prehistoric Southwest.

I think you SFIers (that is you, Stu Kauffman and Chris Langton) never really perceived why some Southwest archaeologists are so keen on "complexity." "Complexity" is one of those "now" words being hyped in archaeology: we've got "complexity" in the Paleolithic in Central/Eastern Europe,²⁵ we've got "complex" hunter-gatherers,²³ the Natufian in the Levant is complex enough to be called a "matrilineal chiefdom,"¹² preceramic, pre-(maize) agricultural Peruvian coastal cultures are or are nearly "states,"^{1,24} some chiefdoms are "complex" while others aren't,³⁰ prehistoric Hawaii is a state,¹⁴ and so is Cahokia²¹; thus, it's no surprise that Dave Wilcox²⁷ thinks Chaco deserves to be a state. Naturally, not everyone agrees with these classifications, but it's clear that many archaeologists want their data (and their people) to be "complex."

In light of previous classifications of prehistoric societies, what we see in archaeology is a kind of "complexity inflation" (like "grade inflation" in universities). The term "complexity" also fits well with normative opinions of social/cultural anthropologists (the dominant species of anthropologist in departments in which archaeologists work) who believe that *all* human social and cultural systems are *complex*. Hunter-gatherers, for example, think rich and complex thoughts about

their lives and their environment, speak the most complex of languages, create an incredibly complex array of kinship categories, and perform complex ceremonials.

Whereas all this is right and proper, complexity becomes more complicated when the term “evolution” is introduced into the discussion. Sociocultural anthropologists are not often concerned with social evolution for the good reasons that (1) the term has conjured up notions of progress, changes in cognitive abilities, and the like, all of which are redolent of social Darwinist doctrines that find our (Western) society at the apex; (2) social anthropologists aren’t worried much about long-term patterns of change, say, from the Neolithic, (e.g., at ca. 8000 B.C. in Mesopotamia) in which small villages flourished, to the development of city-states (ca. 3000 B.C. in Mesopotamia). For archaeologists, however, such changes require social evolutionary theory and social evolution, by any other name (development, change) is just as evolutionary.

Unfortunately, the term “complex society”/“complexity” came to be used in social evolutionary theory (starting in the 1960s, I think) to describe those socially and economically riven societies in which there is a politically centralized governmental system, namely states, or to those societies that were nearly states (usually assumed to be “chiefdoms”). One thus spoke of the “evolution of complex societies” or the “evolution of social complexity” while the study of hunter-gatherers and early agricultural societies was excluded from such evolutionary considerations (or relegated to an earlier stage of precomplexity).

In the perspective of sociocultural anthropological insistence that hunter-gatherers were not living in a Rousseauian, egalitarian state-of-nature, archaeologists began to consider some Paleolithic and Neolithic societies as “complex.” Furthermore, as archaeological skills improved and archaeological knowledge grew and, most important, I think, archaeological data accumulated massively, it was far from simple to interpret archaeological remains. Big pit-house villages in the Southwest, for example, presented complex problems of digging, identifying formation processes, classifying data, and understanding ancient inhabitants. Thus, *complexity of modern recovery and analysis became a shorthand for complexity of ancient social organization.*

Furthermore, in the Southwest, the classic investigations of modern puebloan societies that were used to model ancient pueblos began to be reevaluated. The understanding in these earlier studies (see Levy¹⁷) was that the nature of the kinship system more-or-less explained the (limited) kinds of economic and political stratification in pueblos. Thus, Southwest societies (modern and ancient) were not “complex,” i.e., states or on the way to becoming states. That model was attacked, initially by Cordell and Plog,⁵ and now it is generally accepted that ancient Southwest societies (especially Chaco, Hohokam, and Casas Grandes) were more “complex” than modern pueblos. The introduction of European diseases and the incorporation of historic Southwest peoples into a colonial system are the reasons for the reduction of “complexity” in modern pueblos from prehistoric times. However condescending (among other things) to modern puebloan peoples this revisionist view is, when it is coupled with the general anthropological objections to the term “complexity,” the Southwest has clearly joined in the archaeological trend toward

“complexity inflation.” I think you can see from this brief discussion, that Southwest archaeologists brought a lot of baggage with them to the conference on “complexity in the prehistoric Southwest” at the SFI. If you SFI types were only dimly aware of why some Southwest archaeologists are into “complex systems,” it was equally apparent that the Southwest archaeologists had only dim notions of what you (SFI folk) mean by “complexity.”

IS THIS WHAT THE SFI MEANS BY COMPLEXITY, MURRAY?

While I have relied on some recent articles (Holland,¹³ Kelly,¹⁶ Casti,³ Gell-Mann,^{8,9} and abstracts from the SFI workshop “Common Themes in Complex Adaptive Systems,” from conversations with Stu Kauffman and Chris Langton while I was up the road at the School of American Research (SAR) in 1991–1992, and the new book by Roger Lewin¹⁸), I thought I would make sure that I have some of the basic principles of “complex adaptive systems” right, or at least not totally wrong.

A complex system is a network of interacting parts that exhibits a dynamic, aggregate behavior. This system behavior cannot be reduced to the “sum of its parts” because the action of some parts is always affecting the action of other parts so that equilibrium of the entire system is never reached, or maintained for very long. There is no optimum state of system performance and the system can always surprise, as when a small initial perturbation can result in a large outcome. Per Bak’s principles of avalanches, Chris Langton’s “phase transitions,” and Stu Kauffman’s (and others’) “edge of chaos” ideas all account for instances of rapid change, the disproportional output from small inputs, and the nonreducibility of such systems. The system can always perform better or can unravel into new parts that were not the constituents of the old system.

Such complex systems cannot be calculated or predicted through standard mathematical descriptions because their operations are not linear and fixed within a few variables and so controlled by some central process or mechanism. Rather, aggregate—or emergent—behavior derives from interactions, which themselves continuously change internally, and because many systems are in contact with one another and so affecting the landscape of them all. Indeed, it is often difficult to specify the boundaries of a system (or between systems) and the environment that embeds systems is hardly “out there,” providing a standard through which performance of systems can be measured. The natural property of complex adaptive systems is that of self-organization (“self-organized criticality”) and it is the internal dynamics of such systems, not only a response to something external, that can occasion rapid and profound change.

Indeed, complex systems have the capacity to learn from interactions with other systems and the environment (however defined). Complex systems tend to anticipate the future by seeking new interactive ways while also applying old ways

of interaction that have already “worked.” Thus, complex systems are often (or maybe always) inefficient, sacrificing efficiency for the ability to remain flexible.

Complex adaptive systems include turbulence in hydrodynamic systems, pre-biotic chemical evolution, central nervous systems, vertebrate immune systems, political systems, and the global economy. It is one of SFI’s main goals to bring together specialists in these domains to learn if there is some common kernel to all complex adaptive systems.

I hope that the above description, consisting in the main of transcriptions from the papers I’ve read, has not missed the point of what you are doing at SFI. From having heard a number of lectures at SFI by mathematicians, chemists, biologists, and others last year, I can attest that there is a definite feeling that “complexity” signals a new way in which scientists are thinking about things. Of course, I couldn’t understand most of what was being said at the lectures, but I did have a lot of sympathy with those proteins (as complex adaptive systems) that were described as “periodic, disordered, and frustrated”!

What you may not realize, and what several of the Southwest archaeologists at the conference seem to have missed, is that those of us who investigate ancient states (see Paynter²²) have been talking your language (but with a different lexicon) in the last years. We’re well passed the idea that there were “control mechanisms” in ancient states or that we can draw flow diagrams of neatly boxed social institutions or that ancient states are homeostatic systems in the way archaeologists of the 1970s and early 1980s talked about ancient state systems (see Brumfiel²). Rather, ancient states are messes, filled with institutional struggle among classes and ethnic groups and there is conflict within such social groups, too. Leaders half understand what’s happening in their societies. Bureaucracies consist of a mixture of patrimonial retainers and those recruited by ability. Finally, in ancient states, long-term stability is sacrificed regularly for short-term gain; against an old image of timeless rule by beneficent kings who are toppled only by victorious barbarians or more militaristic neighbors, we now investigate internecine strife, the creation of wealth and misery, factional solidarities, and environmental degradation (in no particular order and sometimes all at once).

However, not only are ancient states and civilizations “complex systems” in your terms, so are *all* human societies playgrounds for social negotiation, for the empowerment of the few, in which the parts remain far from some equilibrium with each other and their environment. In SFI terms, it is perfectly obvious that all human social systems, from Paleolithic hunter-gatherers to states are complex systems.

TOWARDS A THEORY OF RELATIVITY OF COMPLEX SOCIAL SYSTEMS

I hope you and I agree that Southwest prehistoric societies are (1) complex systems and (2) by that designation we know absolutely nothing more than we did before so declaring them “complex.” Now we need to get to the real work of social complexity theory. Are some societies more complex than others? In what ways can we measure complexity? Why are there different kinds of socially complex societies? And is there a theory of the relativity of social complexity that can fall within the kinds of complex phenomena SFI is interested in?

Actually, I know from your paper on “the evolution of human languages” that you’ve already said some things about this, for example, that “simplicity” and “complexity” can be evaluated according to “schemata.” A schema is the compression of regularities identified by a complex adaptive system from its surroundings and about itself, including importantly regularities about the behavior of the system. A schema evaluates other schemata so as to anticipate future situations in a fitness landscape. If I understand correctly, a schema is the shortest message that will describe the system at some appropriate level of significance: in human societies such schemata are social institutions, and traditions, and myths. A schema then can unfold to yield predictions of behavior. If the schemata of one system are more numerous than in another system, then system one, by definition, is more complex. The more specialization, the more ranking, the more elaborate patterns of change in a system, the more complex is the system.

If I’ve got all your ideas of schematic relativity right, then I think we can start talking about social complexity and social evolution: for example, we can, proceed to “rank” societies (see next section) according to amounts of social differentiation, economic specialization, and nature of political institutions. But we must make sure that we compare societies only on this set of schemata rather than another possible set of social schemata. For example, I reckon that hunter-gatherers in Australia have a more elaborate set of ritual and ceremonial schemata than scientists at the SFI! Your own example of languages is instructive since, as far as I’m aware, all linguists talk about the evolution of language (and can do so in a variety of ways) but few or no linguists would say that modern English is more or less “complex” as a whole than Proto-Indo-European (PIE). (For example, complex word-order principles in English may replace complex conjugational patterns in PIE.) If complexity, then, is in the idea of the beholder, to some extent, it is the different kinds of complexity that are of common interest to us both. And in this perspective the research into the kinds of complexity that flourished in the prehistoric Southwest might be most productive.

A BIRD'S-EYE VIEW OF NEW SOCIAL EVOLUTIONARY THEORY

At the SFI conference I distributed the draft of a paper (now Yoffee³²) entitled "Too Many Chiefs?" from which I now wish to extract some points that relate directly to a central question about (the kind of) complexity in the prehistoric Southwest. Let me begin this brief discussion by turning my paper inside-out and presenting one of the concluding diagrams on "possible evolutionary trajectories."

As the full paper³² makes clear (I hope), the main purpose of this diagram (Figure 1) is to offer an alternative to the well-known stage-level, stepladder model used by archaeologists in the 1960s–1980s (Figure 2). For present purposes, Figure 1 implies that not all human societies fall along one continuum, in which lower rung examples (like modern pueblo societies) have failed to progress to the top of the ladder (as Figure 2 implies). Although I cannot pause here to discuss other implications of the diagram, I do wish to note that the diagram does *not* intend to claim that there are only four ideal (and epigenetic) "types" of societies, that social, economic, and political relations are fixed within a type, and that change within a type must occur in all relations at the same time and in the same direction.

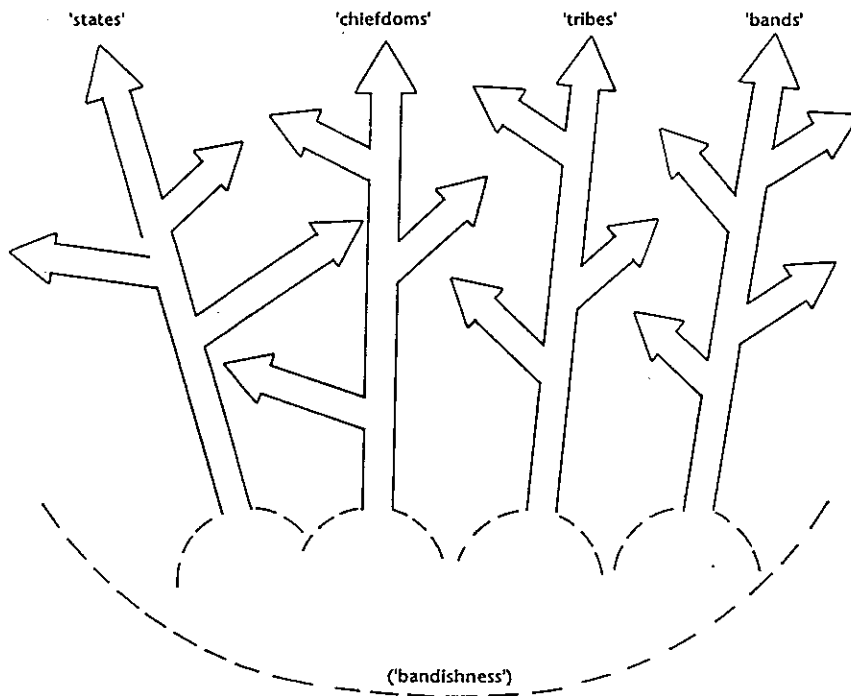


FIGURE 1 Possible Evolutionary Trajectories.

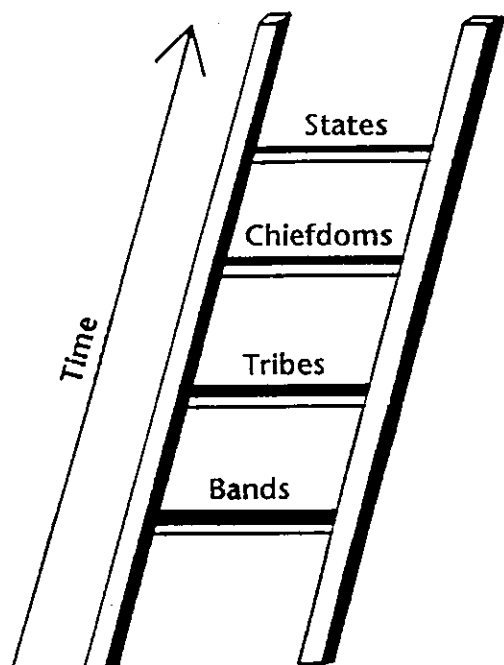


FIGURE 2 Evolutionary, Stepladder Mode.

What I hope the diagram shows is that modern (ethnographic and ethnohistoric) “chiefdoms” may not be good models for those societies that preceded states in the areas of the world in which states evolved. (Of course, chiefdoms could become states through contact with states, as the arrows in the diagram indicate; contact with large states might also reduce small states to chiefdoms.) From our conversations in Santa Fe, I know you can see exactly why Figure 1 should be of interest to SFI theorists of complexity: it implies that there are different *trajectories* (or pathways) in social evolution and that, by identifying specific and differing *initial conditions* (in the time of “bandishness”), we might predict (or, more modestly, begin to understand) what distinguishing qualities there are among the various trajectories.

While I tentatively proffer some possible variables that I have culled from the papers of this conference (in the next section of the memo), I want to spend a moment longer on two (related) issues that are significant to the investigation of Southwest complexity. First, in the perspective of “new social evolutionary theory,” since there are different kinds of complexity, one should be chary of ascribing the Chaco “phenomenon” or Hohokam “complicatedness” (Hallpike,¹¹ see below) as precursors to states. If these Southwest examples of complexity are indeed complex (and they are), one must ask whether they look like those documented prestate societies like Mesopotamian Ubaid, Naqada II Egypt, early Harappan, pre-Shang,

preclassic Maya, formative Teotihuacan, or Chavin Peru societies. (I don't think they do, and I'll come back to the implications of that.)

Second, as Jerry Levy argues in his chapter on analogy,¹⁷ the rejection of modern puebloan society as a direct historical model for ancient pueblos itself raises problems. Since all archaeologists must model the residues of the past upon some other (usually more recent) societies which they reckon are like ancient ones, the abandonment of ethnographic-historic pueblos as models of prehistoric Southwest pueblos *necessarily* requires the substitution of other societies as models. Levy notes that archaeologists (who criticize the direct historic approach) either have not been explicit about what analogies they put in place of the former one or they have facilely equated Southwestern complexity with near-state level complexity. Levy's paper makes the case for kinship-based complexity, as it were, as a model that would explain the nature of prehistoric alliances and conflicts and population aggregations and dispersals in the Southwest—exactly the situations that some prehistorians feel merits rejection of modern puebloan analogues.

THE MAJOR THEME OF THE SFI CONFERENCE ON PREHISTORIC COMPLEXITY IN THE SOUTHWEST: BAD WEATHER

As I sit in my study today (Jan. 8, 1993) in the sunny "Old Pueblo," watching the third day of steady rain, listening to radio reports of street closures (streets that are paved-over arroyos) and noting that the Tanque Verde Wash is only one foot lower than its maximum in 1983 (the year of the 500/1000-year flood that washed away houses nearly a mile from its banks, including the house of Sue Philips, a linguistic anthropologist colleague of mine in Tucson) it is hard to miss the major theme of the papers on prehistoric complexity in the Southwest: bad weather.

The position paper by Alan Swedlund²⁶ and the chapter by Nelson et al.¹⁹ on demography ("sick-o's in the Southwest" according to D. L. M. Martin) repeatedly stress the "clear limiting factors to population growth in a semi-arid environment": "things were good and they got bad"; "on Black Mesa. . .one sees strong evidence for a population that was consistently under marginal nutritional stress, in a marginal environment, and one in which stress markers and disease incidence tend to increase through time"; "agriculture is probably always marginal in the Southwest-somewhere"; and "undernutrition [at Arroyo Hondo and some other sites]. . .became malnutrition and even starvation." Swedlund concludes his essay with a meditation on how "wood limitations present. . .a plausible model for a very serious resource limitation."

Nelson et al.¹⁹ discuss areas that are "environmentally marginal" and suggest that there may be "some sort of absolute threshold of community size beyond which health maintenance becomes increasingly problematic" and that "politically

autonomous groups” were able “to foresee the limitations of their subsistence systems.” Thus, the logic of abandonment of one region and the aggregation in another is one of managing scarcity by population movement. Having cited directly from these papers, it only requires comment that the population numbers of sites and regions, in the perspective of the earliest states, are small (see the various estimates of Nelson, Kohler, and Kintigh²⁰): 5,500 people in Chaco or a maximum of 2,000 in a “humongous” pueblo.

The demography group paper (Nelson et al.¹⁹) represents, I think, something quite new in Southwestern studies and perhaps in archaeology more broadly. Interrelations among diet, resources, population size, health, economy, and politics are evaluated and—even if the data sets are imperfect—hypotheses are invented and tested and health is not simplistically understood as just biology. Deb Martin informs me that on a global scale of agricultural societies, the Southwest seems different because “disease seems so *endemic* to the area from the earliest skeletal remains from Basketmaker times through to historic times.” Deb’s (e-mailed) comments are worth quoting further:

There seems to be a shared profile of health that goes with agricultural economies, but there are certainly key things that put a further strain on health, like food shortages, marginal environment, and lack of good water (like you have in the Southwest) which makes nutrition more of a variable in explaining poor health. In other places in the world, it is the increased density [of population] and increased transmissibility of infectious disease that are the most important issues.

In the context of environmental marginality, it’s no wonder that Joe Tainter finds it “irrational to become complex” (he means politically and economically stratified) in the Southwest. From my outsider’s view, of course, it is amusing that Tainter considers Chaco an “800-pound gorilla” that affects core-periphery relations in his study area. But scale is everything: in a lecture last semester in Tucson on pit-house villages that Pat Gilman excavated in a remote corner of Southeast Arizona, she described the largest set of such pit-houses as “incredibly complex.” Right.

Matters of scale and a comparative method seem to me especially curious in Stead Upham’s paper on “system modeling and evolution.” He lists some of his “biases” about social complexity:

1. “Political systems fundamentally reflect the systems organization of society.”
2. “Political and social systems are really nothing more than solutions to specific problems of management and production.”
3. “Overall population size [is] strongly correlated with . . . centralized political control.”
4. “Economies and production strategies . . . are merely solutions to problems of management.”

As nearly as I can tell, however, most analyses of ancient states ought to cause him to adopt some new “biases.” In the Mesopotamian case there is certainly no overall correlation between population size and political centralization; in neighboring Egypt, on the other hand, which has a small areal population (ca. 2 million in New Kingdom times), there is a vast amount of centralization. Over the populous Maya city-states, there is likewise no overall centralization. In fact, in Mesopotamia (and I think this generally holds in the earliest states), there is no area-wide systemic organization of societies; indeed, within and among various city-states and social segments, individuals contest for power, honor, and privilege. “Problems of management and production” are certainly not reducible to the political or any other single system. If there are no pan-regional politically managed regions among the earliest states (with the exception of Egypt), it is difficult to agree with Stead that there are such “regional and pan-regional organizations” in the Southwest. (I’ll take a couple of paragraphs in the last section of this memo, Murray, to delineate the nature of complexity in ancient states and civilizations and contrast that with the kind of complexity that some argue characterizes the prehistoric Southwest.)

Linda Cordell’s paper,⁶ the panel on explanation (Cordell et al.,⁷) Chip Wills’ paper,²⁸ and his group’s paper²⁹ get at the heart of the complications of prehistoric Southwest complexity. These papers ask what does the trajectory toward social complexity in the Southwest tell us about social evolutionary theory in general and then how does social evolutionary theory fit into studies of complexity in general? I conclude this section, Murray, with a comment on the first topic and I’ll devote part of the conclusion to the second. (Please forgive my *mondo professore* style of telling you what I’m about to tell you.)

The conceit of Southwest archaeology, as is depicted especially in Wills’ paper, is that the Southwest is a laboratory for “develop[ing] general methods that are adequate for identifying their relative effects on human systems, or incorporating them in interpretations.” Cordell et al.’s assessments about developing a comparative archaeology, the necessity of integrating CRM data into larger investigative projects, and the need to know and the need to check facts in the Southwest, however, betrays (I think) a certain wariness about celebrating the achievements of Southwest archaeologists. This cautionary note needs to be juxtaposed with Chip Wills’ comment that “the archaeological record of the prehistoric Southwest is one of the most extensively studied anywhere.” Cordell et al. remind us that it is still to be determined whether these lovely data and analyses are typical of anything outside their own “boundary conditions.” Perhaps the real value of using the Southwest as a case study in building social evolutionary theory is that it may be an example of a trajectory that has been little studied (on a global scale). In this context, the Southwest is truly exceptional and far more interesting than just an example of a lower rung in the great chain of becoming—just like us—a state.

Wills’ statement that “a major trend in the prehistoric Southwest is toward increasing complexity” is both inadequate and incomplete—although it is correct enough in itself. It is inadequate because, as I’ve already boringly repeated, there are all sorts and kinds of complexity. Yes, in the Southwest, population grows, there are degrees of differentiation and stratification, and there is regional interaction.

All of that simply begs the questions, however, of whether population growth had limits, what was the nature of stratification, and what were the kinds of regional interaction. His statement is further incomplete because an equally major trend in the Southwest is "collapse"—abandonment of sites and regions, disaggregation, transportable societies.

The Cambrian phenomenon—an initial explosion of diversity followed by a thinning out of species—is discussed by Wills' group as a useful model of prehistoric Southwest activities. (Lewin does it better in his book on complexity.) In the actual analytical heart of the paper, however, Wills rehearses some of his previous, excellent work on the nature and meaning of agriculture in the Southwest: Agriculture is a risk-avoidance strategy, households are basic units of production, nucleated settlements have more agricultural features than non-nucleated ones, and various patterns of exchange between regions are established because cooperation is a good thing given the bad Southwest weather. "Population aggregation quickly leads to local natural resource depletion in an arid environment like the Southwest."

Levy's paper presupposes much of this and builds upon it. Hopi clans manage the agricultural landscape in ways to ensure that some part of the crop might survive an environmental disaster and an egalitarian ideology promotes areal cooperation. Political confederacies ("alliances" in Southwest-speak) are managed among Iroquois through kinship principles. One is tempted to see the Hopi as wise successors of the cycle of aggregation and dispersal: by rejecting trends towards political and economic power and dominion, they achieve their own richly complex ceremonial and intellectual behavior within a kinship system in which contests over status and leadership are resolved in the Hopi way.

ALTERNATE COMPLEXITIES IN THE PREHISTORIC SOUTHWEST

Judith Habicht-Mauche¹⁰ describes prehistoric Southwest (actually just eastern pueblos) as neither economically and politically stratified societies nor as deeply conservative, egalitarian organizations, but as "complex tribes." In these societies "the fundamental unit of economic and social interaction remains the extended family or lineage...The integration of these units into larger social systems is achieved through the existence of pan-residential corporate social and religious institutions which crosscut these kin groups."

For Habicht-Mauche the expansion of these local social and religious institutions can integrate relatively large geographic regions (which in other views are "provinces"). Interarea exchange and regional specialization are largely decentralized while decisions about agricultural production and craft specialization occur at the level of households and lineages. "Political leadership is vested in a 'council' which represents the various pan-residential social and religious societies. Leaders have respect and status in their individual communities and the regional system

at large, but they do not control access to large quantities of subsistence resources and elite 'status' goods." I quote further, with Habicht-Mauche's permission, her next two paragraphs:

The duties of these tribal leaders can include: (1) the organization and scheduling of religious ceremonies; (2) the supervision of labor for communal projects; (3) the coordination of subsistence activities such as communal hunts, planting and harvesting; and (4) the negotiation of alliances with external groups. Although these leaders may control some coercive force (i.e., war captains and warrior societies might act as tribal police), the real basis for their authority lies in their respect for group consensus and cultural sanctions against uncooperative behavior. Complex tribes, thus, perform many of the same integrative functions of more centralized hierarchical systems, such as chiefdoms. Both can be characterized by regional specialization, inter-regional trade, intensification of subsistence practices, organization of labor for public projects, and the existence of regionally shared religious and social institutions. The two types of socio-political systems differ primarily in the way group decisions are made and enforced, and the degree of control that leaders have over access to natural and social resources.

In her essay, Habicht-Mauche discusses the archaeological evidence for regional clusters of villages, the absence of centralized workshops and storage facilities, and how standardization of crafts is managed as cottage industries by individual craft workers and families, and she states that the majority of exchanged goods are relatively utilitarian objects not limited to any elite segment of society. There is, of course, good evidence for kivas and plazas, religious venues at which inter-pueblo exchange and the distribution of food are reviewed. "Collapse" in the sixteenth century results in the breakdown of ties of economic interdependency and the (retro-)development of self-sufficient villages. In other words, Habicht-Mauche has written the very article Jerry Levy is calling for: while modern pueblo social organization may not supply a 1:1 model for the past, it does offer a relevant set of continuities and discontinuities that can be explained: in a word, puebloan history.

If, as I suspect, there are trends towards social complexity in the prehistoric Southwest that differ from evolutionary trajectories of ancient states and civilizations (and thus, I repeat, the Chaco phenomenon and the Hohokam phenomenon are NOT stages of pre-state societies), I should devote at least a few words on what kinds of complexity are demonstrated in trajectories to states. (I've already written about this in 1979³¹ and 1993³² and in a few places in-between and I'm trying to finish a book tentatively titled *New Rules of the Game: The Evolution of Ancient States and Civilizations*, so I'll only glide over some of the more vexing issues here.)

All over the world in post-Pleistocene times, there evolved increasingly differentiated social groups, organized in relatively specific and autonomous frameworks, with their own symbolic and occupationally specialized orientations, that interacted within the confines of an overarching institutional system. These differentiated and

specialized groups became internally hierarchized and were themselves hierarchically related to one another. New groups, specialized for the purpose, emerged to supply integration among the various forces of production and of interaction in order to ensure the continued existence of the system. In trajectories to states, then, progressive differentiation is resolved in a moment of near-explosive change (in every instance of the evolution of the first states) as a specialized decision-making sub-system began to regulate the flow of goods, people, and information among the various social components. This political sub-system, the state, further maintained itself by use of its power to mobilize resources that were not totally embedded with the various social components.

However, with the exception of ancient Egypt, the earliest states were politically independent city-states, while the regional interaction among them, including the creation of boundary-establishing codes—standardized languages and set of symbols that transcended political borders—we denominate as “civilizations.” (Thus, there are Mesopotamian, Maya, Indus, and pre-Ch’in China city-states/civilizations; pre-Inka Andean city states: Wari, Tiwanaku, Chan Chan; Teotihuacan is a city-state gone pituitary.) This city-state/civilization distinction is not the only thing that can be said about the earliest states, of course, but it is a salient point in the kind of complexity we can contrast with evolutionary developments in the Southwest.

Within the earliest city-states/civilizations, there is a “ubiquity of conflict,” since dominant sociopolitical goals are never accepted by all the differentiated components of the system. Ubiquity of conflict results in a partial, “consensual” resolution in which a legitimation of inequality is effected (that is, inequality is justified as a state of nature). Modes of conflict are partly managed and political struggle is channeled as civilizational values are contested and reproduced. While “collapses” of states (political systems) are normal, so is the ability to reformulate states beyond collapse, since the “idea” of the state to a certain extent, is, independent from the existence of the institution itself (see the papers in Yoffee and Cowgill³³).

In social evolutionary theory and in the empirical archaeological investigations of the development of states and civilizations, it is the process towards socioeconomic differentiation and political integration that is to be traced and explained. Whence come the economically, ideologically, and politically defined social orientations, the arrangements of people not related to one another through rules of kinship, but through the unequal distribution of resources and power? In the evolution of states one sees dramatic changes in the content of people’s roles such that the moral economy of kinship is supplanted, partly by the political economy of power relations: in states the relations between governors and the governed is no longer explained by kinship but through wealth, status, and force.

Hallpike’s distinction¹¹ between “complex” and “complicated” provides a good beginning to describe the differences between trajectories to states and other evolutionary trajectories. “Complex societies” are those with various parts that are interconnected with significant degrees of economic inequality and subordination; social groups maintain separate identities and political rules and full-time political specialists are developed to manage their interaction. “Complicated societies” are those in which social relations are little differentiated, groups look much like one

another, and consequently the societies are “mixed up” and “involved.” Naturally, in the SFI and anthropological views of “complexity,” we must emend Hallpike: all human social interactions are complex and different evolutionary trajectories simply exhibit different kinds of complexity.

THE FUTURE OF COMPLEXITY AT SFI: FINALE

While it’s obvious, Murray, that the term “complexity” as a category that has been used in studies of the prehistoric Southwest, must be unpacked, I think it’s also clear that the scientists at SFI can benefit from SFI’s involvement with Southwest archaeology. Indeed, if I have one policy recommendation, it is that SFI needs to increase its programs in archaeology and social evolutionary theory—if you truly are interested in the subject of “complexity.”

Archaeologists—trained to identify connectedness in social institutions, to measure patterns in social organizations and how these change over periods of time, to delineate the feedback between “natural” and social environments, and to oppose reductionist formulas that ignore sources of variation—have already learned some useful things from the SFI conference, but, as I’ve argued, archaeologists also have some things to contribute to the study of complexity.

Reading the papers in this volume from my distant perch as a Mesopotamianist, it seems most apparent that a social theory of relativity is critical and that we are poised at the edge of a rapid transition in our ability to construct it. The dynamic system of self-organized archaeologists exhibits the emergent property that arises from locally interactive schemata (conferences convened by George Gumerman) in which we can disarticulate kinds of complexity, speak clearly about the need to study initial conditions, and so explain the data in its different outcomes—outcomes that are not themselves ends, but part of historically contingent and continuous processes. Of course, some ideas are fitter than others. Over to you, Murray.

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