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The study of accident in history implies that of necessity.
—LEROY LADURIE (1977: 115)

Hazards are everywhere. Somewhere in the world there is always some kind of environmental disaster at hand—and often many at once. Most of these disasters have an obvious social dimension: a forest fire in the suburbs gets a lot more attention than one in the wilderness.

Less obviously, the kinds of impacts a “natural” disaster has on a society depend not only on the nature of the disaster but also to some extent on how that society is organized, as several of the preceding chapters point out. In this chapter I provide a necessarily brief and selective review of the ways the organization of societies has changed over very long spans of time (I’m an archaeologist, after all!). The typical hazards societies confront probably affect these long-term processes of social and political evolution, since—over long enough periods of time—well-functioning societies tend to change in directions that neutralize the hazards they currently face.

Archaeologists and other anthropologists have long recognized that societies have become more complex in a variety of senses, as the majority of the earth’s peoples moved from hunting-and-gathering ways of life in the Pleistocene (a 2.5-million-year epoch of repeated glaciations ending about 12,000 years ago) to become mostly farmers and herders in the Holocene
and eventually the kings or slaves, craftswomen or shopkeepers, and priests or police of the highly differentiated societies that began to appear a little over 5,000 years ago.

Simply identifying these general trends was a major preoccupation of mid-twentieth-century anthropology, aided considerably by a worldwide increase in the quantity and quality of archaeological research fortified by the then-new chronological framework provided by radiocarbon dating. Archaeologists, especially in North America, began to see themselves as scientists. As various regional chronologies became firmly anchored in time and their gaps filled in, archaeologists could more clearly perceive regularities through time within the huge diversity of the worldwide human experience.

Their fellow anthropologists could also recognize some similarities between ethnographically known societies and various past societies investigated by archaeologists. This allowed them to propose models for the social and political organization of ancient societies, based on the organization of various small-scale societies that were still, or recently had been, in existence. Among the most widely used of these models were the stages proposed by Elman Service (1962, 1975) and Morton Fried (1967). Related influential work was also published by Leslie White (1959) and, more recently, by Allen Johnson and Timothy Earle (1987).

**THAT OLD-TIME CULTURAL EVOLUTION**

Anthropologists in this “cultural evolutionary” or “neo-evolutionary” tradition saw their work as a way of putting social and political flesh on the bones and stones provided by the archaeological record. Elman Service, for example, suggested that during the long Paleolithic era the world was populated mainly by patrilocal bands made up of a few nuclear families—ordinarily fewer than 100 people overall—with no specialized occupational groups, economic institutions, or classes; no institutions governing political or legal affairs; and no religious system separate from the constituent families. (Since the mid-nineteenth century, archaeologists have used the term *Paleolithic* to describe the societies and technologies of humans during the Pleistocene epoch.)

In the post-Pleistocene world, where plant and animal domestication offered significantly higher productivity and perhaps a “much more stable, consistent productivity than [existed] at the hunting-gathering level” (Service 1962: 112), tribes that were several times larger than bands emerged from bands. Tribes, according to Service, could avoid disintegrating into bands because of the invention of integrating organizations—“pan-tribal sodalities” such as clans, age-grade associations, and societies for warfare, curing, and so forth. Because of these sodalities tribes had many more social statuses than
bands, but these statuses were not marked by differences in influence or power except within very restricted spheres.

Next in this series of idealized social types, which Service unabashedly referred to as levels or stages in a progressive sense, are chiefdoms, which are larger social groups than tribes. They are marked, according to Service, by centers coordinating economic, social, and religious activities within the society. Service considered redistribution of goods and labor to be an especially important central activity of the chiefdom, and he thought the responsibility and judgment this activity required, in turn, gave rise to permanent leadership. Although chiefdoms are commonly larger (usually much larger) in numbers of people and possibly geographic area than tribes, what most importantly separates them from the fiercely egalitarian bands and tribes are the differences in social power within a chiefdom: “chiefdoms are profoundly inegalitarian” (1962: 150). Quite possibly, this was the most important single transition in the evolution of human societies.

Finally, states (whose appearance Service considered in detail in his 1975 book) are ordinarily larger again than chiefdoms, in terms of both population and the space they occupy. They differed from chiefdoms not only in size but more essentially, Service believed, in having increased institutionalization of the centralized leadership and, eventually, a hereditary aristocracy that grew out of that leadership.

In the 1980s and 1990s, stage theories such as those of Service came under criticism for a variety of reasons, including what some considered the implicitly Colonial placement of the Western European or American types of political organization at the apex of a long evolutionary ladder. Then, too, many of us recognized that some archaeologically known societies may be nothing like societies that still happened to exist in the nineteenth or twentieth century. It was widely felt that archaeologists had spent altogether too much time trying to classify their societies using these categories rather than figuring out how societies actually operated and why they changed. Around this time many archaeologists simply became more interested in other topics such as identity, symbols, and meanings. Even archaeologists who remained interested in evolutionary processes were moving on to different kinds of models and approaches (see reviews in Kohler 2008; Shennan 2008). Yet despite all this, some researchers retained (or are returning to) a focus on long-term, comparative social evolution while keeping 1960s-style stage theories suitably at arm’s length. Examples include a recent book by Norman Yoffee (2005) and chapters in books edited by T. Douglas Price and Gary Feinman (1995) and Kevin Vaughn and his colleagues (2010).

Here I want to couple my own interest in long-term comparative social evolution with the materials presented so far in this book. What implications might sudden environmental change have for the long-term processes of increasing social scale identified by Service and many others?
SOCIAL CONSTRUCTION PROCESSES

To answer this question, we have to dig some new foundations. I would like to replace the aged and ambiguous term *cultural evolution* with *social evolution*. Social evolution is the long-term tendency for the largest group with which people identify and regularly coordinate their activities (whether by choice or necessity) to become larger in both numbers of people and spatial scale. Everything important about social evolution centers on groups. I contend that the principal motor for increases in group size has usually been inter-group competition and that those physical and cultural capacities that differentiate us from our various last common ancestors with related creatures, moving backward in time along our phylogenetic tree for at least the last 2 million years, are the result of selection within our lineage for the ability to live successfully in ever larger groups. Full defense of this thesis is not possible here; I outline some of my reasons for this position in Kohler (2004).

Consider briefly how exquisitely designed are just two of humanity’s defining features—culture and language—for communicating knowledge and norms within groups but how, at the same time, both tend to create barriers between and variability among groups. Over the roughly 200,000 years modern humans have existed, our success has been such that our societies have grown in size from family units or bands to states and empires.

The long-term tendency for groups to increase in size is a result of the fact that (all other factors equal) when two groups are in conflict, the larger prevails. (This has been formalized as Lanchester’s Square Law; see Lanchester 1916.) This is most obvious for the case of armed conflict and can result in cultural group selection: the replacement or absorption of one cultural group by another (Bowles, Choi, and Hopfensitz 2003). This is called *cultural group selection* because it refers to a process in which people do not necessarily perish but their culture disappears or changes profoundly. But larger *cultural* groups can also have more subtle advantages and need not be in armed conflict with other groups in order to prevail; larger *cultural* groups have an advantage over smaller *cultural* groups that would not be as true for, say, groups of other animals in which cultural traditions are of little or no importance. This is the case because larger human societies can accumulate and maintain more complex technologies and skills (Henrich 2004; Powell, Shennan, and Thomas 2009), including social technologies and skills; to the extent that these technologies and skills improve the group’s quality of life, this in turn will make them a target for selective immigration from other groups (Boyd and Richerson 2009), further increasing their size. This process (also called payoff-biased migration, or voting with your feet) can even cause group-beneficial behaviors to spread so long as there is acculturation and certain other conditions are met. Cultural group selection probably explains why practices that should be discouraged by individual-level selection—such as young males’ willingness to risk their lives for their social groups—can instead proliferate.
So if larger groups have such great advantages, why weren’t there any Paleolithic empires? Of course, there are very important counterbalancing forces that prevent groups from becoming ever larger. Robert Carneiro (1987) and, more recently, Matthew Bandy (2008) have emphasized the strong tendency for groups to fission because of “scalar stress” (internal dissension) once they reach a certain size. Bandy, for example, suggests that “only the development of novel institutions of social integration at a suprahousehold level [Service’s intra-tribal sodalities come to mind] could make possible the emergence of villages larger than [a] critical population threshold” (2008: 341). Small-scale human societies have a strong egalitarian ethos that also resists the development of internal inequalities, and they regularly employ public opinion and “punitive moral sanctioning to ensure that alpha-dominated hierarchies could not form” (Boehm 2000: 213). Also, large groups that are relatively sedentary may degrade their local environments more than did their less numerous, more mobile ancestors. Environmental degradation surrounding newly large and sedentary populations therefore presents another common barrier to growth, as Meredith Matthews and I (Kohler and Matthews 1988) argued for some early villages in the US Southwest.

I use the phrase social construction work for all those social and political processes that allow groups to grow larger, overcoming the probability of fission and the difficulties of cooperation and communication within ever larger groups and avoiding the environmental degradation that accompanies growth. Social construction processes are what people use to build and maintain groups.

What do these processes include? Very small groups can be built on the ancient biological (genetic) logic of inclusive fitness (or kin selection): we are genetically programmed to be biased toward helping those to whom we are related. But such motivations decay quickly as groups expand beyond just a few households; I’m much more likely to help my brother than help my third cousin. Kim Hill and colleagues (2011) have recently shown that contemporary hunter-gatherer bands are composed of mostly unrelated people, though they often include adult brothers and sisters. If this was also true of ancient foragers, then the logic of inclusive fitness plays only a very small part in the story of how we evolved to cooperate so efficiently.

As we move in size beyond the scale at which inclusive fitness can be effective, myriad potential mechanisms for inducing and sustaining cooperation within groups have been suggested by economists, political scientists, physicists, and game theoreticians. A partial list includes mutualism, conditional reciprocity, indirect reciprocity, “strong reciprocity,” and reputation management, including signaling. These mechanisms interact in complicated ways with levels of selection, for example, if they rely—as many do—on the existence of underlying norms or institutions whose existence might depend on prior (cultural) group selection or if they tend to be effective in stabilizing those same norms.
So much work is being done in these areas that we are beginning to see a series of papers just trying to systematically define and classify the competing and often partially overlapping approaches and disentangle the difficult semantic issues that accompany them (e.g., West, Griffin, and Gardner 2007). I strongly suspect that this multiplicity of pathways represents real complication in the social world and that within-group cooperation can be achieved in a variety of ways. It is also important to remember that all of these are mechanisms for bottom-up coordination and that they may play only minor roles in societies in which one subgroup has achieved an ability to coerce the behaviors of other subgroups. In what follows I refer to cooperation induced from the top down as political coordination, using coordination as a general term to include both bottom-up and top-down processes. Political coordination may be structured in a variety of ways, but in the early states most of interest to archaeologists, ritual, ceremony, and ideology (i.e., “symbolic capital” in Yoffee’s [2005: 197] nice phrase) may have been especially important, though they must have been backed up by the possibility of coercion.

SOCIAL EVOLUTION, HAZARDS, AND RESILIENCE

The mid–twentieth-century cultural evolutionists generally believed that greater productivity per unit land area and also stable, consistent productivity were important in creating conditions conducive to transitions between stages. This implies that a general absence of hazards or disasters might have been important. The foregoing chapters, however, suggest to me slightly different conclusions. First, the effects of hazards on social evolution depend on their spatial severity, areal scope, and predictability and the way these interact with the most prominent mechanisms stabilizing within-group coordination. Moreover, some kinds of hazards might have been more or less irrelevant. Finally and most significant, I argue here that the evolutionary tendency toward increase in scale and hierarchy is in itself a response to the hazard of being absorbed or displaced by neighboring groups.

Ben Fitzhugh (chapter 1) finds that the unpredictable and occasionally large but highly localized volcanic eruptions in the prehistoric Kurils had little discernible effect on the human societies in the area. More or less the same thing can apparently be said about earthquakes and tsunamis, although in that case site locations at higher-than-expected elevations may be attributable to a defense against these hazards. Perhaps this apparent lack of impact results from the fact that the spatial scope of these events was small compared to the size of areas across which specific groups would commonly range. It is interesting that Fitzhugh considers one of the greatest hazards for the small-scale societies in this area to have been incorporation into the states that eventually bordered them on both sides.
Fitzhugh’s chapter left me wondering, though, whether the “failure” of Kuril inhabitants to develop the sort of complex hunter-gatherer societies evident on the North American Northwest Coast or in southwest Florida or even nearby on Honshu is a result of the relatively high periodicity of the disruptions he notes or whether it is more a matter of the relatively low productivity that limited human population sizes. Or, to put it another way, if the Kurils existed without volcanoes and tsunamis, would complex hunter-gatherer societies have developed there? A science of comparative human social evolution wants to know. I’ll have more to say about such counterfactual speculations later.

Payson Sheets presents a well-developed framework for understanding the differential consequences of volcanic impacts on ancient societies of Central America and Mesoamerica. In my view it is particularly important to try to quantify the degree of disruption of various impacts—as he can do with volcanoes using the Smithsonian’s Volcanic Explosivity Index—but, of course, such quantification is not the whole story, since small VEI events can still cause a great deal of damage locally and even large ones may have beneficial effects for people who are far enough away.

I also think Sheets is absolutely right to emphasize population density and carrying capacity considerations in his explanations for why some disruptions had no apparent effects and others did. About Arenal he says, “In spite of carefully examining the cultural inventory (artifacts, features, architecture, subsistence, economy, political organization, and pattern of settlement) for any evidence of volcanically induced change, we could find none. And that was not because of a paucity of eruptions.” Sheets contrasts this with the (admittedly much larger) eruption of Ilopango, which brought down the entire Miraflores branch of Maya civilization. Presumably this was not only a result of the size of the event but also of its impact on all levels of the social and political hierarchy, not just on one corner of a polity.

In general, in assessing the effects of disasters, it would be helpful if we could predict what the population and settlement patterns in an area affected by such disasters might have been in the absence of these hazards. In the case of Arenal, for example, is the absence of change an effect of interest in a larger framework of comparative social change? Such questions are at present idle speculation, but it is not impossible that we will someday have the means to address them. Computer simulation models will be necessary to advance this strategy of investigating “counterfactuals.” Margaret Nelson and her colleagues, in their chapter, discuss a model of “what might happen” in systems of mixed maize and agave cultivation as rainfall changes in its annual mean and variance. Similarly, with a group of colleagues, I have been developing models of subsistence and settlement for the northern US Southwest (e.g., Kohler et al. 2007) that ask, for example, what the population size through time in our
study area would have been if only high-frequency (annual and decadal) variability in temperature and precipitation affected productivity and people had no ability to immigrate into or emigrate from our study area.

I’ll restrict my specific comments to one more chapter—the only one for which I can claim any specific expertise. Nelson and her colleagues present a number of provocative thoughts and suggestions in their discussion of potential hazards to ancient societies within the southwestern United States and northern Mexico and their differential vulnerability to those hazards. For example, they contrast the Mimbres and Hohokam, on one hand, and the Zuni, on the other, in their investigation of the role of irrigation infrastructure in vulnerability. The first two cases led to collapse, at least in some senses of that word, whereas the Zuni showed remarkable robustness and continuity. The authors suggest that these differences resulted from, at Zuni, a smaller-scale and “more dynamic” water-delivery system, an absence of population growth after the late AD 1200s, and what might have been in some sense a more appropriate pacing between the development of social institutions (what I’m calling social construction) and the appearance of large-scale aggregates (the late prehispanic towns).

This all seems plausible to me, but the points also beg for explanation. We know that one of the difficulties for the Hohokam system was the arrival between AD 1250 and 1350 of large numbers of Pueblo immigrants from the north, exacerbating social tensions and contributing to a shift to aggregation (Hill et al. 2010). The Zuni area appears to have grown across this century as well, but why were the ultimate results so different? Coupling these questions in terms of “vulnerability” and “resilience” has the advantage of helping us identify interesting contrasts, but it doesn’t necessarily get us any closer to explaining the differing underlying causal chains that appear to have different ultimate results.

Much the same can be said for the concept of rigidity traps. My own analysis of the causes for the depopulation of the thirteenth-century Mesa Verde region in southwestern Colorado (Kohler 2010) emphasizes the importance of high-frequency drought and low-frequency cold conditions in depressing maize production, as well as the generally depleted nature of the landscape after two centuries of very dense populations. But there are also important senses in which something like “rigidity traps” probably contributed to the depopulation (Bocinsky and Kohler in press). First, depletion of deer populations meant that most people were getting their high-quality (meat) protein from turkey, which was fed corn (Rawlings and Driver 2010), so populations were ultimately dependent on the success of maize farming for both their calories and their protein.

Second, Mesa Verde region societies appear to have been hanging on to a style of social organization in which households and lineages (or clans) retained a great deal of power, likely including landownership. Given the structure of
potential production across these landscapes, the most consistently productive agricultural lands were likely monopolized by senior and more powerful lineages residing in some of the most advantageously placed villages. Despite this, walled, densely packed villages with new forms of ceremonial structures were constructed during the last three decades of occupation, suggesting that at the same time, increasing interdependence existed among lineages and sodalities within the largest villages. Such interdependence probably means that all the occupants of a village had to be pulling together to make a village work properly.

The regional context also needs to be considered: these societies were in contact and competition with societies in the northern Rio Grande, which were developing social organizations in which lineages and households apparently held less power. Some of this power seems instead to have been given up to pueblo (town) elders, and agricultural lands in particular may have been held and distributed at the level of the pueblo, as was common in the last century in these societies. The social chaos that Nelson and her colleagues allude to, which accompanied the final year(s) of occupation in the Mesa Verde region, probably pitted haves against have-nots, within villages but especially between them. Given the general untenability of local agriculture, especially in the suboptimal fields available to them, the have-nots were on their way south. I think they made sure that the haves weren’t coming along; we know of at least two village-wide massacres from this period (Kuckelman 2010).

So, was the Mesa Verde area depopulated because of rigidity traps? If this analysis (which includes informed intuition and some speculation) is correct, that is indeed part of the story. And yet, while calling the particular binds confronting these societies “rigidity traps” may be useful as recognizing a class of problem, it is no more explanatory than calling a society a tribe. I am certainly not accusing Nelson and colleagues of mere taxonomizing; many explanatory details for the studies they summarize here are presented in other publications. I do worry, though, that just as past archaeologists reified the ideal types of the neo-evolutionary sequence a little too enthusiastically, thereby concealing their internal heterogeneity, future archaeologists may become so enamored of these concepts that their analyses stop with classification. (Paulette makes similar points in his chapter.)

CONCLUSION

Societies grow in size in part to overcome hazards, and one of the principal ones—little discussed elsewhere in this volume—is the danger of being absorbed, displaced, or destroyed by neighboring societies. Inevitably, this growth requires social construction work, and new hazards then present themselves as a result of the greater populations now to be supported and the possibility that the new
social roles will cause internal schisms to develop within the group along lines of contrasting interests.

In this chapter I emphasize how societies evolved toward larger sizes and increasing hierarchical control because in more hierarchical societies the effects of disasters seem to be related to which social and political levels are affected, as well as to how widespread destruction is within a territory. As Paulette demonstrates in his discussion of the world’s first cities and states in Mesopotamia, it is not simply a matter of saying that states are more (or less) resilient than smaller-scale societies. States vary in their institutional forms and in their capacity (and willingness) to respond to suffering caused by disaster among segments of their populations. Polities may even exploit environmental disasters to spread or cement their control; for example, the rapid growth of Teotihuacan around AD 200 may be linked to the eruption of Popocatepetl (70 km to the south), as noted by Emily McClung de Tapia. Paulette demonstrates boom-and-bust cycles in early Near Eastern states, but Nelson and her colleagues demonstrate these cycles for the smaller-scale societies of the US Southwest as well.

Another reason these long-term trends in social evolution are important to a volume such as this is that they emerge precisely from the response to the hazard of being overtaken by one’s neighbors. But this response eventually raises other kinds of hazards by requiring social construction work that may not be entirely successful in alleviating intra-group stress that, in turn, can be greatly magnified by disasters. At the same time, ignoring for the moment the way humans divide themselves into groups, regional populations also tended to grow as new and more productive subsistence systems were developed or in response to temporarily better production as a result of climatic variability. Population growth packs the landscape with ownership rights and therefore limits mobility, which was humanity’s original and best response to local environmental downturns. Under these conditions a disaster—or even locally deleterious climatic variability—might lead to partial emigration if that is possible (which might look like collapse if the remaining, smaller groups abandon the social construction mechanisms they no longer need), complete emigration, or death.

As long as population levels were sufficiently low, death (by this mechanism at least) should have been rare and mobility common. Therefore, as Payson Sheets notes in chapter 2, population growth has been, and continues to be, one of humanity’s greatest traps, since it quickly absorbs the fruits of previous technological advances and social construction efforts and hungrily demands new ones—which, in turn, invariably present new hazards whose spectra can be only dimly glimpsed.

Most of us archaeologists study periods when the world was inhabited by only a few million people. Even if there were hundreds of millions, they were so disconnected that effectively their numbers were much lower. Only archae-
ologists working on the nineteenth and twentieth centuries have had to think about the possible effects of 1 billion increasingly connected people. Today the world has nearly 7 billion people, and most estimates call for at least 2 billion more by 2050. Is anything archaeologists have learned about how societies work over long periods, in what today looks like an almost empty world, still valid for understanding the pressing issues we face today?

The fundamental things apply. There will always be competition between social and political groups. Humanity will never escape some limits on its numbers and its achievements, imposed by the natural world. But can archaeologists draw more precise lessons from the long sweep of history by finding the level of abstraction appropriate to that task? We need to try, and I hope that books like this are a step in that direction. More than three decades ago Emmanuel LeRoy Ladurie wrote of the dangers of doing social science without considering the effects and lessons of (pre)history:

History . . . surprised the social sciences at the swimming hole and made off with their clothes, and the victims had not even noticed their nakedness . . . Everybody, by now, has been forced to admit the obvious: it is not possible to construct a science of man without a temporal dimension any more than it is possible to construct a science of astrophysics without knowing the age of the stars and the galaxies. (1977: 135–136)

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NOTES

1. Since we have been discussing them, I should acknowledge that some neo-evolutionists recognized the centrality of inter-group competition. Service (1962: 113), for example, proposed that “the competition of societies in the neolithic phase of cultural development seems to have been the general factor which led to the development of integrating pan-tribal societies.” Or: “The fact that there is frequently a contiguous belt of chiefdoms suggests the possibility that the cycle of expansion by incorporation and subsequent disintegration is a common cause of the origin and spread of many chiefdoms. It is also possible that just plain warfare between a chiefdom and adjacent tribes could lead tribes to remake themselves politically, copying the salient features of a chiefdom, in order to resist incorporation more effectively” (ibid.: 152).

2. This position implies that human groups themselves became important units of selection. For selection at this level to be important, however, these groups would also have had to have been fairly homogeneous internally but quite variable from group to group.
3. The relative underrepresentation of anthropologists in this literature has had the result that for groups of intermediate size, the role in social construction of one of our group’s favorite topics—kinship—has been slighted. Early on, Marshall Sahlins (among many others) recognized that kinship provides the model for political relationships in small-scale societies (e.g., 1961: 333)—indeed, it is the political system. But kinship, too, eventually fails as a construction principle as groups increase in size.

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