Chasing the Will-o’-the-Wisp of Social Order
Steven R. Simms

If there are connections everywhere, why do we persist in turning
dynamic, interconnected phenomena into static, disconnected things?
Eric Wolf (1984:4)

Concepts like “nation,” “society,” and “culture” name bits and threaten
to turn names into things . . . the concept of the autonomous, self-
regulating and self-justifying society and culture has trapped anthro-
pology inside the bounds of its own definitions . . . just as the soci-
ologists pursue the will-o’-the-wisp of social order and integration in
a world of upheaval and change.
Eric Wolf (1982:3, 18)

Reflecting a catharsis in the humanities and social sci-
ences, messages such as Eric Wolf’s from his book Eu-
rope and the People without History (1981b) are often
dismissed as the whining of postmodernists. Such mat-
ters may strike the positivist, materialist world of most
Basin-Plateau archaeology as incidental or even vulgar,
yet at the same time we indeed remain attached to the
habit of casting admittedly “dynamic, interconnected phenomema” as “autonomous, self-regulating and self-
justifying” entities. We thus place great stock in bound-
aries, and as part of our quest to define them, we re-
egate discourse on past behavior and connections among
people to vague notions of affiliations, interactions, or
influence, among otherwise autonomous entities. We
thus forget that boundary formation among cultures is
as much about shaping the nature of connections as it is
about defining difference. This lapse probably obscures
the exploration of much past behavior archaeologists
would find important, especially the search for why
culture changes under different circumstances, often in
spite of tradition. Our habits of discourse also inhibit
our ability to convey a sense of the past to a funding
public in terms other than familiar stereotypes. Perhaps
a consideration of his message is useful when cast in
terms of topics familiar to Basin-Plateau archaeologists.

My message is not one of semantics, a quibble over
terms such as Fremont culture, Lovecork culture, Numic
peoples, Late Prehistoric, Paleoindian, or the Archaic
versus some other, “better” designatory; nor do I plea
for all of us to cease using taxonomy as a tool. I wonder
however, if our habits of discourse and taxonomy might
benefit from a perspective intended to contrast with our
habits—that is, our own cultural baggage and myths
about ancient times. It is necessary to state at the outset
that I do not make this suggestion to denounce culture
history (the critiques are well-established in archaeo-
logical method and theory). Rather, I wonder if we are
capable of telling prehistory in a more behaviorally re-
alistic way, and in a way that helps rather than hinders
explanation while complementing rather than repeating
our own culture’s received wisdom about the an-
thropological “Other.”

CHANGE VS. TRADITION: A FALSE DICHOTOMY

The opposition between stability or continuity vs. persis-
tence or tradition is one of the oldest dilemmas in social
science. After acknowledging that both exist, we slide
into ideological cliché where those who attempt to iden-
tify change, diversity, and variation are said to ignore
“social factors,” or to not give adequate import to “cul-
ture,” or discount the search for “pattern” (e.g., Jasielski
et al. 1997:9). This reaction however, implies that only
one facet of culture is important—the existence of tra-
dition and the functional, stabilizing attributes of cul-
tural systems. It also fails to consider culture as poten-
tially transformational, and because of this unnecessary
limitation, culture change is conceptualized in terms of

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migration, abandonment, or extinctions, and tradition is granted immortality in the zeal to connect living individuals with those of the past. On the other side of the ideological rift, claimants for the importance of "culture," or "social factors" are often dismissed with the ever-dimming accusation that they are mere culture historians—luddites unworthy of further consideration. The reality of cultural diversity and culture change does not, however, mandate the abolition of social or cultural "factors," or patterns. It does not deny the existence of notions of weak and fluid as they may be), nor does it require artifact and cultural types to be thrown out. Our cherished projectile point styles and ceramic types are safe (at least from the critique at tempted here), but once we know they "exist," is it not logical to ask what else we might want to know? For instance, a recent attempt to explore the relationship between regional ecology and various dimensions of mobility (Simms et al. 1997) does not invalidate ceramic types, nor the chronology known for the region. Nor does it remove an understanding of ceramics from the realm of culture. Indeed behavioral analyses are about explaining culture, rather than using bounded types to explain culture away.

The argument that we are preoccupied with boundedness and autonomy can transcend theoretical polesmises. For instance, Wolf's point about connections and the value of a perspective that deemphasizes, for the purposes of analysis, the autonomy of culture, is suggestive of the interest among evolutionary ecologists in identifying the interrelations of culture within cultures, not just between them. This is important because conflicts of interest, and thus, competition and cooperation may only occur within cultures, between sexes, between parents and offspring, and between generations to name only a few. These forces tug at the cultural rules and the tides of tradition, and lead to the spread of some behaviors through behaviors at the expense of others (see Zealah and Simms, Chapter 1).

A form of this message also resonates with postprocessual archaeology, although specifically, studies based on "practice theory" (see Ortiz 1994 for a theoretical overview). Practice theory holds that the organizational principles and aims of behavior are constructed through the routines of daily life—the little things. Rather than autonomous and bounded, culture through this lens is contextual and interrelated, with cultures being "made" and "transforming themselves through daily behavior, not between monolithic classes of the categories per se. A recent study by Lightfoot and Horsman (1998) makes precisely as practice theory is applied to the archaeology of cultural pluralism in the case of Fort Ross, California, where Russians, Native Alaskan, and Mexican/Native Americans are reflected not as "mixture," of three cultures, but a culture, through the organization of their daily lives.
homogenize the very real lives of past people. Further, adaptations tend to play out over the lives of individu-
als, and a useful perspective to help archaeologists engage behavioral plasticity (e.g., Hill and Hartundo 1996:3-37). People join and leave marital relationships, adopt new identities, trading relationships, and social groups as the tyranny of cir-

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unfolds. Boundaries among cultures are prob-
able, shifting, and fluid, and many behaviors, cera-

ities, or the behaviors of some people, or the behav-
or of males vs. females, parents vs. offspring, young vs.

ol. In order to see how boundaries emerge from the

production and selection of variability, it is neces-
sary to describe the range of variability upon which se-

lection might operate, then clarify the conditions under

which certain strategies might spread or fade. I know we

have all heard something like this before, but sug-

gest one reason we have trouble implementing it is

the implicit assumption that the boundaries of bound-

aries—by chasing the will-of-the-wisp of social order.

Here is an example that transcends the idiom of

boundaries and leads us to implications for every-

day archaeological interpretation. The concept of adap-
tive diversity stands apart from historicist notions such as

migration, depopulation, etc., to account for pattern-

ning in the material record. It does not allow for divi-
sions in lifeways within cultures, but changes over the

life history of individuals. As such, it is a potential tool

for gaining insights into boundaries to the morphogen-

tic processes that form the health and survival of peo-

ples. Thus, an interesting point to consider is how

the Great Salt Lake wetlands (Coltrain 1997; Simms 1999), as well as a study nearing comple-

ition of the wetlands (Byth 1995), supports the notion there was a constant dynamic among Fremont foragers and farmers. In the Great Salt

Lake Basin, I am particularly interested in examining the study of cul-

tural ties via stable carbon isotopes, the evidence indicates that these differences were expressed in some individ-

uals during their lifetime (Coltrain 1997; Simms 1999).

Using ceramics, we explored behavioral issues not

previously addressed. Our approach assigns a formal classifica-

tion and chronology. We analyzed thousands of plain wares sherds for morphological variability, many

from dated contexts, in light of expectations about in-

cipient boundaries in the Fremont period in two rem-

dant regions of mobility (Byth et al. 1995; Simms et al. 1997). We

found patterns that cross-cut many of the traditional ceramic types. Does this mean that these boundaries are

not detectable? No, it means they were designed for purposes other than an exploration of variation. They were de-

voted to identity and shared cultural traditions, which they
doubtfully was. However, we want to know about differ-

cences in mobility among these people with shared cul-

tural traditions, as well as similarities and differences in

mobility and people with different shared cultural traditions. Our explicit intent was to confront the tradi-
cional categories because we think this might move us beyond the bounded char-

acterization of a Fremont people.

There are implications here for archaeological prac-
tice. Adaptive diversity implies that limited activ-
ty sites may not always be extensions of farming sys-
tems, even though we typically assume they are because we

label them as "Fremont " even if they are part of a

longer period. Upon inspecting most limited activity sites, the activities conveyed by the

term "Fremont" are not particularly evident. Nor is it

readily apparent that such sites were created by farmers

camping away from their pithouses. Neverthe-

less, a bounded designation is applied to all sites of the

period because we assume the autonomy of the catego-

ries we select—in this case the autonomy of the farm-

ing, "Fremont" Fremont.

Recent study of the boundary between the Fremont

and the Anasazi, another example that moves under-

standing beyond the assumption of autonomy. David

Madsen observed that the mixture of traits at some sites in

south-central Utah (Madsen 1982c) could lead to an

ascription of a new type, the Anamont or, if not that "Anasazi" (Madsen 1982c, 1985). Madsen was sug-

gest that since the boundaries had long been drawn in

this case, circularity in reasoning about any behavior

among the two is encouraged by mere habit of discourse.

Phil Geb (1996:1-9) has thoughtfully revisited this issue and the parallels with the task of identifying

boundaries among archaeological cultures. Realizing the

shortcomings of extending boundaries among archaeo-

logical cultures and the limits of detecting them, I sug-

gest that the utility of the practice, Geb specifies his search as one for social and ethnic boundaries. He argues that it is

useful to attempt to identify those traits and explores ways

that boundaries may be detected.

Geb considers projective points, and suggests that

existing categories for point set parameters. He also consid-

ers that the idea of a "boundaries" may refer to other

boundaries. Cerami-


cies may also be useful, but probably not via the most

commonly employed criteria of pace and point set parameters. These like these that are relatively amorphous and hence

mobility (Geb and Lienes 1996). On the other hand, vessel

shape, design, or rim forms may be important for

detecting boundaries in a way that allows Geb to use the word "boundaries." Geb also considers

ceramic art as a realm of potential, but with no

fundamental archaeological problems. Similar to Geb,

Lienes points out the importance of making sense of the

way boundaries are used in everyday life, and the

ways they are used in a particular social context.

I am not proposing that we must reconstruct indi-

vidual life histories. I am suggesting that consideration of

mobility has implications for fluidity, dyna-

mism, and variability that is not captured in the con-

cepts of archaeological cultures or in the identification of

ethnic and social boundaries per se. Then there is the

fact that the specific character of boundaries can change over

time. The boundary between the Anasazi and the Fremont in the Glen Canyon region should be associated with the Anasazi in the North on what Anasazi (1982c) and Geb (1996:1-9) makes a foray in this direction as well. How might these different contexts of selection shape the expectations for the movement of males, females, points, pots, or architectural attributes at different times? As an alternative to cultures and periods, D. Madsen and Simms (1982c) used the concept of "social selection" over time to understand Fremont behavior.

Again, rather than seeing the ethnic and social as the ultimate causes shaping behaviors, it may be more re-

worker to the behavior to the formation, maintenance, and dynamism in social and ethnic boundaries. The assumption that physical characteristics or cultural presence, as well as mem-

bership in a cultural group, plays a role in the formation of a culture? In this example we may benefit from the research sponsored "Lingering across these boundaries. We could begin with a concludes of projective points (men and) and ceramics (women) without presuming we already know about these things simply because we have always assumed "the autonomy of cases selected for study"—the Fre-

mont and Anasazi.

Thus, it is right when he reflects: "Writing it has been an internal struggle between a gut-level belief that material culture groups actually do define people in some nonsocial sense and the apparent truth that, while this sometimes might be the case, it is more likely not the case, or at least not in such a simple, straightforward manner as the old adage of sharpe equals people."

From my perspective the problem is bigger than this, however, and it has to do with exploration across the boundaries, in addition to refining our abil-

ity to find them.

POSTSCRIPT

I often hear archaeologists say we can move to an ex-

amination of process and attempt explanation only after

the culture history is worked out, and this is a terminology that this is a myth. To the extent that the problems raised here are some-

ething that culture history seems unable to stretch be-

cause, then perhaps the problem is the one we have.

It is the way of doing much new. This issue goes to the

heart of our reason for being, that is, learning about the
guest and conveying something about it that tran-

scends the commonplace perceptions of our modern

Archaeology exists as a beneficiary of an affluent society and its culture it is knowledge not only within the profession, but to wider audiences via print and TV media, popular books, college classes, and school programs. It is the question sponsored "Lingering of the Past." To the extent we are as slaves to our idiom, we submit that history is really prehistory and consists of a progressive sequence from the Paleolithic people, the folk, to a Formative period. We write about our categories with an unstated adherence to the myth that back when these cultures existed, the boundaries were clear, people did not move from the teachings of
their elders, and fluidity was driven only by the actions of cultures as units. We then speculate on the relationships between these ancient entities to contemporary Native peoples. In the process we atomize the past and mirror the division characteristic of our cultural present, even as we embrace the rhetoric of multiculturalism. Like multiculturalism, we have taken the first step of appreciating cultural diversity, but have unwittingly left our stories about the past in terms of boundaries and division, thus inhibiting any real crossing of those multicultural boundaries.

A bitter irony ensues when archaeology is mainstreamed to wider audiences in this well-worn idiom. Archaeology becomes the lackey of a debased popular culture, rather than a social commentator or educator stimulating change in how we understand the past. Perhaps all this is just too difficult and too external to our own cultural legacy—grapple with. Perhaps it simply requires too much energy to deal with in the workaday world of archaeologists and other educators. On the other hand, we cannot rely forever on our oft-repeated retelling of history. As we continue to limit ourselves, we will only foster the creation of what Eric Wolf (1982b) calls “the people without history.”

ACKNOWLEDGMENTS

This paper was originally presented at the 1994 Great Basin Anthropological Conference, Elko, Nevada. More reference to the work of Eric Wolf in the body of this paper is not enough. As a student of Julian Steward, Wolf holds a kinship to those who work in the Basin-Plates region. As one who transcends the demarcation of ideological encampments, he serves as a reminder that we can and should bend the questions in new directions, if for no other reason than because we can. Kudos to Nicholas Blarton Jones for the coinage “the tyranny of circumstance.” Editorial advice from Marina Hall was of immense value; the persistent autonomy remains mine alone. Many thanks to Charlotte Beck for a great deal of work and astonishing patience. I dedicate this editorial to graduate students in anthropology—the ones who will frame the questions of the future.

What we can learn about the past is conditioned by the models we use, the data at our disposal, and our efforts to bring each of these to bear on the other. In the last few decades archaeological models have commonly employed the wonderful ethnographic data that Great Basin archaeologists have at their disposal (e.g., Bettiger 1977; O’Connell 1977; D. Thomas 1973). Whether or not one subscribed to Jennings’s Desert Culture concept, it is clear that ethnography strongly conditioned the way that we see prehistory in the Great Basin. And why not? The nineteenth-century Paiute and Shoshone were better exemplars of life in the intermountain deserts than foraging peoples from other parts of the world.

But there are two problems here. First, the obvious one: imported diseases, the horse, plains raiders, new trade relationships, fur companies, California-bound immigrants, farmers, cattle, and miners forever changed crucial pieces of the social, economic, and natural landscape long before ethnographers arrived (C. Beck and Jones 1992; R. Kelly 1997a). This means that the nineteenth-century Shoshone, Paiute, and Ute are not direct analogues for the prehistoric inhabitants of the Great Basin. By and large, archaeologists are aware of this issue.

A second and more important problem is that as more and more archaeology is done, we see more and more variation in prehistoric lifeways. As our sample size of Great Basin archaeology increases, so does the diversity we see within it from small pithouse camps to lakeside pithouse villages, foraging to maize horticulture, Paleoindian sites near ancient lakeshores to late Prehistoric alpine villages. And we see significant differences over time in the role of large game, wetland, alpine, and horticultural resources, in house shape and size, the importance of seeds, rock art, site locations, and so forth (R. Kelly 1997a). When Jennings published the Danger Cave report in 1957, vast areas of the Great Basin were terra incognita. Of course a simple descriptive model based on ethnography seemed useful.

That is no longer the case. Thanks largely to CRM-supported archaeology, we know a lot about the archaeology of the Great Basin, that is, we know what occurs where and when throughout much of the Great Basin and for much of the time span of human occupation there (although the interpretation of these “facts” often remains elusive or debased and there are still some holes that need plugging—the late Paleoeocene and early Holocene are perhaps the most glaring gaps in our knowledge). Today, many Great Basin archaeologists rely upon theoretical models to circumvent the problems inherent in ethnographic analogy and to explain differences in lifeways implied by the observed differences in archaeology. Recent models have been based on a neo-evolutionary perspective and employ behavioral ecological models (e.g., Barlow and Metcalfe 1996; Barlow et al. 1995; Broughton and Grayson 1993; Elston and Raven 1994; K. Jones and Maiden 1989; J. Kelly 1981, 1986; Larsen and Kelly 1995; Lindström 1996; D. Maiden and Kirkman 1988; Raven 1992; Raven and Elston 1988, 1989; Raymond and Sobel 1990; Simms 1984b, 1987, 1990; Simms et al. 1997; Zanah et al. 1995) that place in the foreground the natural environment, subsistence pursuits, and population density (an approach that has a long history in Great Basin anthropological research, a matriminist, be that as it may, a local

Thinking about Prehistory

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