

Discussion

On Intellectual Property Rights and Archaeology

CLAIRE SMITH

Department of Archaeology, Flinders University, GPO Box 2100, Adelaide, SA 5001, Australia. 26 11 04

This is a seminal paper for archaeology, one that maps out territory many of us will be traversing over the next few decades. The routes we take as individual archaeologists are going to be diverse, and side roads and alternatives to those that emerge from Western mind sets will be identified by Indigenous peoples as they increasingly have input into the research process (e.g., L. Smith 2000) and the numbers of Indigenous archaeologists increase, but the overall direction has been pointed to by Nicholas and Bannister (CA 45:327–50).

A core issue emerging is that of shared intellectual property rights. Much of our work with Indigenous people is informed by their knowledge as well as ours. Often, we could not produce the research that we do without their help, particularly in fields such as ethnoarchaeology. The outcome of such research is shared intellectual property, since neither party could have created the product alone. The rationale behind this approach is perhaps clearer if we think in terms of related disciplines such as ethnobotany, in which the medical “product” can be achieved only by combining the knowledges of Indigenous and Western science. Another way to think of this is as a kind of soup to which different people provide essential ingredients. Though there may be a “chef” (the archeologist), that particular soup could not exist without the full range of ingredients (both Western and Indigenous knowledges), and all the people who provide those ingredients have rights in it.

A related issue is that of direct financial rewards for Indigenous communities from archaeological publications. Archaeologists can get funding from publications in two ways: directly from book royalties and indirectly through these publications’ contribution to the process of obtaining employment, tenure, and promotion. The Indigenous people who contributed to the archaeological research do not receive these indirect forms of payment, but they do have a right to receive remuneration from research. In recognition of this, archaeologists increasingly are sharing the financial rewards that come from publication. When the book emerges from work with one particular community, the royalties may be directed to that community (e.g., C. Smith 2004). When the research

is situated in more general discussions, the royalties may be directed to an Indigenous fund (e.g., Thomas 2000). Along similar lines, royalties from the World Archeological Congress’s Indigenous Archaeologies Series (<http://www.altamira.com/series/indigenous>) are ear-marked for Indigenous attendance at the organization’s meetings and congresses.

One way of recognizing Indigenous people’s rights to financial compensation from archaeological publishing is to pay them an agreed-upon sum whenever an image is published. My own major research is with Aboriginal people in southern Arnhem Land, Australia, and we have reached an agreement whereby I pay A\$100 for each image of them or their land that I publish. Payment is made to the senior traditional owner, the senior traditional custodian, or the individuals in the images, and permission has to be asked any time I wish to republish the image. For an entire book, this can be expensive, but it is possible to apply for publication subsidies to cover these costs or, as a last resort, to pay them from the salary one has obtained from doing research with these people.

Control over publication will become a key issue, one that will be particularly sensitive for North Americans. While archaeologists routinely accept controls over their research from the institutions and authorities with whom their work intersects, when it comes to Indigenous control over research we tend to balk. Yet we have little trouble accepting that a funding body may require first rights in publication or that we have to obtain permits from government bodies to excavate or that landowners may not want us to record sites on their properties.

The question of Indigenous controls over research has not proved difficult in the Australian setting. The question of who owns the past was a focus of archaeological discussion in Australia two decades ago (McBryde 1986). Despite the initial trepidation of archeologists, we have reached an answer, and archaeologists now need written permission by the appropriate Indigenous group to examine Indigenous collections in museums or excavate Indigenous sites and must obtain ethics clearance from universities to work on Indigenous topics and to obtain funding from the Australian Institute of Aboriginal and Torres Strait Islander Studies (<http://www.aiatsis.gov.au>). This process is routine in Australia, and the paper by Nicholas and Bannister indicates that North America is following this path.

Finally, the intellectual property rights issues outlined in this paper in terms of Indigenous archaeology are going to have significant implications for archaeologists working in economically disadvantaged countries. It is going to be an interesting journey for us all.

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Reply

GEORGE P. NICHOLAS AND KELLY P. BANNISTER
Secwepemc Education Institute, Kamloops, B.C., Canada V2H 1H1 (nicholas@sfu.ca). I VI 04

Smith underscores three important topics in her comment: shared intellectual property rights, remuneration, and control over publication. Her soup analogy is appropriate; the products of research relating to Indigenous knowledge exhibit varying degrees of hybridity. As “product[s] of cultural conjugation” (Brown 2004a: 59), they are not just the work of a single individual but the cumulative result of a series of creators that may span the time and space continua. The researcher happens to be the person at the end of the chain of creators—assembling and enhancing the parts provided by others, claiming intellectual credit, and looking to protect his or her investment in the academic merit system and the market economy. Although assessing intellectual contributions is a part of determining intellectual property ownership, the first one to fix the knowledge in tangible form or the last one to add an inventive step is best positioned to claim ownership rights; rarely is this an Indigenous knowledge holder. What kind of system can acknowledge all the “ingredients” in the soup and all the people who have provided them? Exploring this is a key point of our paper and part of the evolving ethical and legal envelope in which we do research, requiring a wider understanding of the conceptual worlds and cultural expectations around intellectual property and Indigenous knowledge of the creators and users (see Brown 2004b for additional comments).

Appropriate recognition of the contributions of Indigenous peoples and other stakeholders is apparent in ethnoarchaeology and ethnobotany. It is more problematic with regard to archaeological materials/knowledge of some antiquity and less certain or less direct ties to living peoples. While archaeologists are seen as the primary creators of archaeological knowledge, the question we have raised concerns the degree to which members of descendant communities many generations removed have some legitimate claim to the archaeological sites and to the knowledge derived from them.

Smith’s suggestion that members of descendant communities should be remunerated for their contributions is one that some researchers may initially object to, but it is not far removed from what many researchers already have to do, that is, seek permission and pay fees to publish information or images “owned” by others. Scholars routinely purchase stock images of people and places or pay subscription fees to news services; is this different from acknowledging and remunerating community members for the knowledge or materials that they provide? Again, the question is how this relates to artifacts and sites associated with “extinct” societies—societies that clearly have no direct ties to living groups—al-

though this may be either complicated or simplified as more genetic material is recovered from archaeological sites.

Directing the royalties from certain types of publications heads off criticism leveled at the discipline by communities that have felt slighted at the lack of recognition paid them by visiting scholars (Brezinski’s [1993] “summer meddlers”). The move by some archaeologists, including Smith, and some archaeological organizations is a positive example to be emulated, providing that it is seen as culturally appropriate by the Indigenous peoples involved.

There are two points to consider in Smith’s comments on control over publication. First, academics are loath to accept any control or censorship of their ideas, but well-crafted research designs and agreed-upon protocols with community representatives may provide acceptable boundaries: “Anthropological researchers must expect to encounter ethical dilemmas at every stage of their work, and must make good-faith efforts to identify potential ethical claims and conflicts in advance when preparing proposals and as projects proceed” (AAA 1998: 3). Second, publication of information derived from archaeological sites or community sources without adequate consultation or permission may run counter to ethical standards.¹ But even when individual researchers want to divest themselves of rights in favor of others, they may nonetheless be obligated by their employers to do otherwise. Most archaeological consultants and archaeologists working for government agencies do not have complete control over work they have been contracted to do and must seek permission to present or publish the results of their research.

We believe that intellectual property rights will be a major factor in shifting current power structures and mind sets toward more equitable models between archaeologists and other stakeholders. Although the territory ahead is unknown, the motivation for exploring routes forward is not.

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1. “Intellectual property, as contained in the knowledge and documents created through the study of archaeological resources, is part of the archaeological record. . . . If there is a compelling reason, and no legal restrictions or strong countervailing interests, a researcher may have primary access to original materials and documents for a limited and reasonable time, after which these materials and documents must be made available to others” (Society for American Archaeology 1996: principle 5, our emphasis).

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On Ritual and Cooperation¹

DAVID J. ROZEN
3125 N.W. 11th St., Oklahoma City, OK 73107, U.S.A.
(jgr62bz4@aol.com). 22 II 04

Sosis and Ruffle's recent paper (CA 44:713–22) demonstrates possibilities for the use of experimental techniques in anthropological research. The theoretical justification of the experiment presents a consensus among social scientists relevant to the structural-functionalist research paradigm of a past generation. The researchers assume that group prayer, a feature of Orthodox Jewish ritual behavior, is more conducive to solidarity than interaction in Jewish secular communities. The study uncritically posits a core axiom of Durkheim and latter-day functionalist theorists—that “unintended consequences of action” have functions, goals, or purposes. Yet, the fallacy of functionalist theory is now recognized to be that “social systems have no purpose, reasons, or needs whatsoever, only human individuals do” (Giddens 1994:7). While social action can have effects not intended or even anticipated by actors, contemporary philosophers of social action assert that humans have agency and therefore create and reproduce societies within “bounded conditions of rationalizations,” for example, historical context, consciousness, and unconsciousness (pp. 112, 250). Therefore, it is perplexing that the report does not mention that the goals of Jewish rituals are reflexive and necessary for the maintenance of self or that failure to follow the *mitzvot* (sing. *mitzvah*) “divine commandments” results in psychic stress (Rozen 2003).

Issues of validity emerge when we attempt to interpret the results of the experiment outside of the context of actual social relationships. The experiment involved a

game in which two people in separate locations were asked to withdraw sums of money from a known quantity in an envelope. The assumption was that the person who withdrew less demonstrated a greater concern for cooperation and the common good than his/her counterpart. However, I have trouble conceiving of anything's being an instance of “cooperation” when people are not engaged in some sort of interaction. Rather, I consider the game a hypothetical case of anticipatory self-serving profit optimization, not an indication of commitment to one's community and willingness to cooperate for its benefit.

While, technically, some statistically significant differences were observed (sometimes at levels as liberal as between .1 and .05), the differences were so minor as to be probably of no practical significance. Since the religious men and the secular men and women all withdrew 30 shekels on average and the religious women only 3-plus more, it does not seem appropriate to conclude that there is a religious-secular difference, even if statistically significant.

Intersubjective knowledge accessed through ethnographic research techniques points to cultural reasons that secular and religious Jews might perceive the experiment differently. The *mitzvah* has become a cultural metaphor for a good deed, and religious Jews are known for altruistic concern for persons who experience poverty or misfortune. A person who gives money to a poor person is said to have performed a *mitzvah*. It is possible that the religious Jews might deliberately have left more in the envelope because they thought they were doing a *mitzvah* by leaving a gift to benefit others.

Another interpretive problem is the researchers' failure to present the social form *minyán* from an emic (insider) rather than simply an etic (outsider) point of view. Religious Jews view the *minyán* in a narrow, legalistic fashion. The Jewish Code of Laws stipulates that certain prayers cannot be recited unless specific requirements are met, including the presence of a minimum of ten praying Jewish men. Unlike the *mitzvah*, the *minyán* as a symbolic form has little or no emotive valence. *Minyan* members may be and frequently are total strangers. An etic definition of the *minyán* is not the same as the emic concept, and any solidarity it involves is an “unanticipated effect of action” not intended by the social actors.

If there is conventional wisdom in the social sciences, it is that rituals are symbolic systems of communication (Cohen 1976). Symbolic forms mediate concrete socio-political relationships and cultural systems (values and norms), but rituals are more than just information about society: they are “blueprints or models” of a society (Geertz 1973:92). Rituals are a medium for communicating core symbolic forms and passing traditional cultural information from one generation to the next. Solidarity, ethnic identity, and the solution of practical problems may all be consequences of ritual behavior, but from the actor's perspective rituals are

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powerful systems of communication about the very essence of reality.

Reply

RICHARD SOSIS AND BRADLEY J. RUFFLE¹
Department of Anthropology, U-2176, University of Connecticut, Storrs, CT 06269-2176, U.S.A.
(richard.sosis@uconn.edu) /Department of Economics, Ben-Gurion University, Beer Sheva, Israel 84105
(bradley@bgumail.bgu.ac.il). 5 VI 04

Rozen is needlessly concerned that we are reviving a “structural-functionalist research paradigm of a past generation.” Our paper was not motivated by Durkheimian functionalism, nor did we invoke our results as support for this paradigm. We simply pointed out that Durkheim was among the first in a long line of social scientists, the majority of whom were not structural-functionalists, to have posited a relationship between ritual performance and group solidarity.

Our interest in that relationship arose out of the evolutionary work of Cronk (1994) and Irons (2001), who develop a costly-signaling model of religious behavior. Bliege Bird and Smith (n.d.) outline four necessary conditions for the evolutionary stability of a costly signal in a population: (1) there is within-group variance in some unobservable attribute; (2) observers can benefit from reliable information about this variance; (3) higher-quality signalers can benefit from accurately broadcasting this information, but lower-quality signalers have the potential to achieve benefits at the expense of recipients through deception; and (4) the cost or benefit to the signaler of sending the signal is correlated with the signaler’s quality. Sosis (2003) has argued that religious behaviors meet these conditions: (1) the intensity of religious beliefs varies within communities and this variance is unobservable; (2) individuals benefit from accurate information about this variance because intensity of belief is related to one’s commitment to the group and its goals, committed members being more likely to be cooperators and thus preferred social interactants; (3) religious groups offer various benefits for members that are mutually provided and are at risk of exploitation by those not committed to group goals; and (4) the perceived cost or benefit of ritual performance, which can include payoffs received in an afterlife, is correlated with intensity of belief. Thus, religious behavior can be understood as a costly signal that reliably advertises the unobservable condition of religious belief and group commitment. The time, energetic, material, and opportunity costs of religious activity serve to deter those who lack sufficient belief from displaying the signal.

1. We thank Candace Alcorta, Sharon Feldstein, Sam Martinez, and Eric Smith for valuable comments.

Although these insights initially motivated our research, ultimately our data could not rule out alternative mechanisms that might explain the relationship between ritual performance and cooperation. Therefore we set a more modest goal of documenting the existence of this relationship in a controlled environment. Our results, however, are certainly consistent with costly-signaling interpretations of synagogue attendance as a reliable signal of cooperativeness (see Ruffle and Sosis n.d., Sosis 2004, Sosis and Alcorta 2003, Sosis and Ruffle 2004). We agree with Rozen that rituals are symbolic systems of communication; indeed, Alcorta and Sosis (n.d.) have argued that costly-signaling theory offers a possible mechanism through which symbolic ritual communication evolved, and our results offer some support for this understanding of religious behavior.

It should be noted that evolutionary explanations such as costly signaling are indeed functionalist. In contrast to structural-functionalism, however, evolutionary explanations do not assume that communities or social systems have needs, nor do they lack a causal theory that can explain the feedback loop inherent in functional explanations, a logical problem with structural-functionalist interpretations (see Smith and Winterhalder 1992 and Wilson 2002).

Our findings are not trivial or an aberration as Rozen implies; they parallel patterns observed in other communal societies (see Kanter 1972, Sosis 2000, Sosis and Bressler 2003). Similar to their nineteenth-century American predecessors, on average religious kibbutzim have been more economically successful than their secular counterparts, and this disparity has increased over time (Fishman and Goldschmidt 1990). Explanations for this are undoubtedly multifaceted, but, given that many of the rituals maintained by religious kibbutz members inhibit economic productivity (see also Sosis 2000), this relative economic success may seem surprising. If the costly ritual behavior of religious kibbutz members increases intragroup cooperation, however, there may be an overall net economic gain from adherence to these rites (Sosis 2000).

Rozen is concerned that our experimental design did not allow kibbutz members to interact with one another. If we had permitted communication between participants, our interpretation of their claims would have been confounded with their identities, the relations between them, the content of their conversation, and possibly many other factors unobservable to us. Conducting the game as we did provided experimental control, allowing us to interpret a participant’s claim as a measure of cooperation with an average fellow kibbutz member. Contrary to Rozen’s impression, there are countless contemporary examples of cooperative behavior in which individuals do not interact with others, including donating money anonymously to charities and recycling plastics. Protocol-based interviews revealed that among the many cooperative challenges kibbutz members face, the excess consumption of common-pool resources (e.g., gas, electricity, food, water) is the most salient. This con-

sumption problem, a tragedy of the commons, motivated the design of our experimental game.

Rozen also raises concerns about the statistical significance of some of our results. While the raw numbers point to minor differences in the cooperative behavior of religious and secular kibbutz members, controlled regression analyses reveal that these differences are large and robust. The additional regression results presented in Sosis and Ruffle (2004) and Ruffle and Sosis (n.d.) confirm these results. Rozen inaccurately implies that we were liberal in our acceptance of significant p values. We reported p values between .05 and .1 to inform readers of marginally significant variables; all of our main findings were supported by p values less than .05.

We find Rozen's alternative explanation of our results, namely, that religious kibbutz members believed they were performing a *mitzvah* of *tzedakah* (charity), unlikely and incapable of explaining the differential claims of religious males and females. We are not sure what "intersubjective knowledge" Rozen "accessed through ethnographic research techniques." Our own post-experiment interviews revealed no differences in the explanations offered for claims between religious and secular kibbutz members or between males and females. Most religious kibbutz members would find Rozen's righteous interpretation of their behavior amusing, since they were simply attempting to earn additional spending money. Kibbutz members' experimental partners were not poverty-stricken but anonymous fellow members who enjoyed a standard of living very similar to their own.

Rozen asserts that we did not pay attention to the context in which our experiments were conducted. It is true that in large Jewish communities, particularly in public places such as airports and commercial districts, *minyanim* often do occur among individuals who otherwise have little social interaction. Indeed, the reason that the "anonymous *minyan*," which fails to "lift the individual out of his solitariness" (Heilman 1983: 23), has received any sociological attention is that it contrasts sharply with normal synagogue life. Kibbutz members live, work, and socialize together in a well-defined community; their synagogue life could hardly be more different from the ephemeral anonymous *minyan*.

Rozen concludes that "solidarity, ethnic identity, and the solution of practical problems may all be consequences of ritual," which was exactly the point of our report and what we were able to demonstrate empirically. It is likely that there are diverse social and ecological conditions in which ritual can both assuage psychological anxieties, as he maintains, and promote group cohesion. Contrary to his contention, we never claimed that solidarity was an intended goal of Jews who attend *minyanim*. We agree with him that traditional Jewish law (*halachah*) defines and motivates ritual as well as mundane behavior among religious Jews. His claim that "from the actor's perspective rituals are powerful systems of communication about the very essence of reality" does not challenge or contradict our findings, nor is

it surprising that ritual performers have a different perspective of their behaviors from social scientists. Emic views are critical for understanding variance in human behavior; yet were these views to hold exclusive explanatory rights in academic discourse, the analytical strength of the social sciences would be remarkably limited.

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On Language and Culture: Another Strand

DAVID B. KRONENFELD

Department of Anthropology, University of California, Riverside, CA 92521, U.S.A. (kfeld@citrus.ucr.edu).

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Duranti's review of anthropological linguistics (CA 44: 323–47) has omitted one important strand of anthropological linguistics—the study of word meanings in culture that began with the early ethnoscience work of Lounsbury, Goodenough, Conklin, and Fraake and that has continued in contemporary work. His remarks on p. 38 touch on one aspect of this work but do not speak to its major thrust—basic issues of meaning (relations of opposition and inclusion [and the attributes that structure or reflect these relations]) and of reference (including semantic extension), both of which can involve marking relations. This work is highly relevant to the goals Duranti discusses because no accurate understanding of the social or political use of words can be based on an inadequate or naive view of meaning and reference.

Berlin's discussion of universal processes in the development of folk ethnobiological nomenclatural systems (1992) considers the effects of increasing social and technological complexity on that development; he explores the mechanisms by which such systems develop—including the role played by semantic extension and marking—and cognitive pressures toward simplification and communicative effectiveness. Ellen (1993) looks at the manner in which interaction among culture, cognitive processes, and the material world shape a particular culture's system of animal categories. Medin and Atran (1999) bring together a range of contributions that explore how people perceive, categorize, and reason about living kinds. The collection not only sheds light on human nature but addresses the relationship of ethnobiology to the global economy and to related issues concerning the protection of our environment and of local cultures. These books, as well as other work by these and other researchers, do not simply and narrowly limit themselves to the content of these systems but consider the social and communicative role played by classification, the cognitive purposes served by it, the constraints that limit it, and the ways individuals actually use these systems.

Kempton's rigorous experimental study of words for ceramics in Mexico (1982) provides powerful evidence in support of a prototype approach to referential semantics and shows how word meanings differ along socially significant lines. MacLaury's study of color terminology (1997) demonstrates that categorization is composed by one or more points of view, which, when multiple, are related to one another by near synonymy, coextension, inclusion, or complementation. His approach, vantage

theory, applies generally to other semantic domains (2002). I have proposed a general theory of word semantics based on a joining of semantic relations of contrast and inclusion to referential extension (Kronenfeld 1996). I contrast denotative, connotative, and figurative extension and show how metaphor depends on paradigmatic constraints among prototypic senses. My approach emphasizes the social nature of linguistic constructs and considers the role of various kinds of cognitive constraints on semantic systems; the theory is applied, *inter alia*, to several contested systems of political and social categories.

Adopting a different approach, Romney and coauthors (Moore, Romney, and Hsia 2000, 2002; Moore et al. 1999; Romney et al. 1996) describe the cognitive structure of a variety of semantic domains (including emotion terms, color terms, and kin terms). The approach, described in Romney and Moore (1998), enables a direct measure of the degree of intracultural consensus about the objectively induced structures; where the categories permit, it makes possible the direct measurement of intercultural (or intersubcultural) similarities and differences.

The analysis of kinship terminologies, while often seen as its own esoteric special field, has provided an important laboratory for the exploration of language categories, including the relationship between formal definitions, cognitive interpretations, and patterns of everyday use (see Kronenfeld 2001 for a general overview, with citations of relevant work, 1980 on native-speaker reasoning about category membership, 1991 on observed usage, and 2000 on the relationship of linguistic categories to the categories which structure relevant behavior).

The above studies take a variety of approaches to a variety of problems and produce a variety of kinds of findings and therefore do not make up any tight "paradigm," even though they all have developed out of the common ethnoscience base. The integration of older approaches into newer ones in this tradition is nicely illustrated in Hutchins's (1980) sophisticated formal and detailed cognitive analysis of real-world reasoning about a case of contested land ownership, which makes telling use of traditional ethnoscience classifications of types of land ownership and of gifts. These studies share a commitment to empirical work and the understanding that any full appreciation of the social and political nuances of language use will have to be based on an adequate analysis of the underlying semantic basis of the terms and categories in question.

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