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Article

Seeing with the Strong Programme

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Abstract: Using the Strong Programme developed in Edinburgh in the 1970s clarifies how to do sociology of science freed from Enlightenment paradigms of testing for Truth. This paper uses, as an example, the case of Lewis Binford and his wife (in the 1960s) Sally Rosen, revealing Rosen's work to make Lewis's writing clear and persuasive. Rosen's work was the efficient cause of Lewis Binford's success with the New Archaeology.

Keywords: Lewis Binford; Sally Rosen Binford; Strong Programme of sociology of science

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1. Introduction

Archaeology looks simple: we uncover evidence of human activities in the past. What activities took place are deduced from comparisons with fully observed activities in the present or historically recorded past, that is, by analogy. Stratigraphy ordered the levels of occupations into culture histories. Our sister sciences are paleontology, similarly dependent upon recovery of evidence of past living beings and their activities, and geology, investigating processes that produced inanimate features. Data of these historical sciences are literally "givens", what survives from the past; these data cannot be manipulated and tested in laboratories, they exist in themselves, vestiges [1].

Up to 1968, there was little dispute over such a basic description of archaeology. It was a science, along with paleontology and geology, because it was limited to actual data encountered through fieldwork, meticulously recorded for virtual witnessing [2]. Its greatest challenge was dating, usually a judgment or estimate based on depth, geological context, or whether artifacts and features seemed primitive or closer to civilizations. Most archaeologists working in North America felt frustrated by lack of ruins comparable to those of Eurasia (e.g., [3]).

Then Lewis Binford burst upon us. Publishing *Archaeology as Anthropology* in 1962, leading sessions of his and his students' work in 1963 at Society for American Archaeology and 1966 at American Anthropological Association, finally publishing in 1968 the volume drawn from the 1966 session, *New Perspectives in Archaeology*, Binford proclaimed "The New Archaeology". Its aims were "the solution of problems dealing with cultural evolution or systemic change" [4] p. 31. Not only was the accepted goal of archaeology and culture histories rejected, "the solution of problems" rather than collection of data was the raison d'être. Carl Hempel's 1966 *Philosophy of Natural Sciences* guided Binford to insist on Hempel's hypothetic-deductive method, positing a hypothesis and then seeking data that would validate (or not) the hypothesis. Whence came the hypothesis? This was not discussed.

Binford and his students dominated American archaeology for a generation, with their modus operandus continuing to be accepted, although less often stipulated, in the present century. Followers packed the rooms when they presented sessions at national meetings. As I observed during the years 1966–1990, Binford led a bruderbund of young men in khakis, hair and beard uncut, plus a young woman with hair in a pixie cut. Systems theory and structure being then the fashion in the sciences, presentations included slides of diagrams with boxes linked by lines and arrows. They could have been engineering layouts. Publications were heavy on statistics, in tables and graphs, in line with Binford's graduate-school professor Albert Spaulding's teaching that statistics reveal true meanings obscured when using words.

Both the machismo exhibited by Binford and his bruderbund and the formulaic quality of the science he taught, disturbed me. To begin with a straightforward hypothesis, then *select* data that looked relevant, seemed to me to be tautological, putting the cart before the horse. Privileging statistics over observations seemed gullible: did Binford not know *How to Lie with Statistics* [5]? Even selecting "cultural evolution" and "systemic change" as our topics for research, discomfited me: in what sense did "culture" evolve? why focus on "systemic" change when Boasian anthropology emphasized historical particularism, contra to "systems"? I began reading history/philosophy of science. Kuhn, Popper, Lakatos; Norbert Hanson, Larry Laudan; Hilary Putnam, Rom Harré, Richard Rorty ... Joseph Needham's *Science and Civilization in China.* William Dray and George Gaylord Simpson on why Carl Hempel's recipe for science does not work for the historical sciences (see Kehoe [6] for full discussion and citations of these sources, too extensive for this paper). None of these leading thinkers were cited by Binford or his followers. Instead, Binford had a consulting philosopher of science, Merilee Salmon, and her husband Wesley Salmon. The Salmons analyzed physics, a laboratory science. The scientific procedures they discussed had no application to archaeology.

2. The Strong Programme

Then I saw *Natural Order* [7], a volume describing a radically different approach to understanding science. Its title referred to the assumption that there is a natural order of entities in the world, to be discovered through observation of phenomena, noting similarities and differences until natural taxa are discerned. What was new and startling in the book was the standpoint that natural order is a *construct* particular to a time, place and community. What we term "science" was not discovered by Bacon or Newton or Linné, it was *asserted* to be true. Scientific "facts" are scrutinized by communities of practice in the sciences, "invisible colleges" as Robert Merton termed them (Merton [8] for an overview).

A very anthropological approach was formulated in the 1970s, looking at the social class, time, ideologies and politics surrounding scientists, on the one hand, and doing ethnographic participant observation in laboratories to document actual behavior and activities. Pierre Bourdieu and Bruno Latour went further, beginning as anthropologists in Africa and then using ethnographic observation to analyze their own, French, society and contemporary science practices (refs [9,10]. A spate of ethnomethodological studies of scientists' daily work and conversations seemed balanced by the archival work of the Edinburgh group on historical circumstances of scientific research. David Bloor, one of this group, termed their approach the Strong Programme in sociology of science. From a woman's position, this was too obviously macho, and it expectedly spurred harsh critiques. Let us overlook masculinist tone, to focus on the character of the Edinburgh School's approach that appealed so strongly to me.

The Strong Programme has four basic tenets: "causality, impartiality, symmetry and reflexivity" [11] p. 7. Bloor spelled these out:

1. "It would be ... concerned with the conditions which bring about belief or states of knowledge ... other types of knowledge apart from social ones ... will cooperate in bringing about belief;
2. It would be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation;
3. It would be symmetrical in its style of explanation. [That is,] The same types of cause would explain, say, true and false beliefs;
4. It would be reflexive ... applicable to sociology itself ... [in its] need to seek for general explanations" [11] p. 7.

David Bloor emphasized that "the Strong Program is relativist. Relativism simply means the rejection of any claim to absolute knowledge and derives from an awareness of the natural origins of all processes of cognition ... Scientific knowledge of the material world is indeed about an independent reality, but the knowledge is always shared and collectively constructed" [12] p. 962. The thrust of the Strong Programme is that "At the root of all these phenomena are self-reinforcing, self-referring, sets of assumptions and calculations about what everybody else is assuming and calculating" [13] p. 925. "Knowledge, then, is better equated with Culture than Experience" [11] p. 16.

3. Seeing with the Strong Programme

Now I could understand what was going on in American archaeology. I was well equipped to investigate it, for I was trained to analyze cultures, and I could evaluate cultural premises against years of fieldwork experience. Furthermore, my reading in philosophy of science had introduced me to C. S. Peirce's work, particularly his extension of logics to include abduction in addition to deduction and induction (For a sophisticated and thorough presentation of abduction in practice in the social sciences, see [14]). Abduction begins with "a surprising fact"; it is the process of reasoning from the datum to an explanation:

> "The surprising fact, C, is observed. But if A were true, C would be a matter of course. Hence, there is reason to suspect that A is true" (quoted by [15] p. 9, citing original).

What is the surprising fact? it is Binford's insistence that Hempel's hypothetic–deductive reasoning is the only actually scientific mode of reasoning. Wesley Salmon taught that:

(1) the explanation must be a valid deductive argument;
(2) the explanans [premises] must contain at least one general law;
(3) the explanans must have empirical content [16] p. 12.

It is the second requirement that jolts a Boasian anthropologist. Franz Boas was quite clear that we have too-limited samples of *Homo sapiens sapiens* to justify postulating general laws drawn from what Western scientists have studied [17] p. 4. How could any scientist suppose an archaeologist could derive general laws of human behavior—-other than the most trivial—-from the paltry degraded residue in archaeological sites? From the absurdly small sample of human behavior represented in the tiny number of sites studied by archaeologists? Gad, the hubris!

Between Peirce and Salmon there is another three-fold model. (Three is the magic number for Indo-European speakers—-a cultural pattern.) The master paleontologist George Gaylord Simpson wrote, contra Hempel, that a *historical* scientist follows this procedure:

(1) obtaining and studying the historical data . . . ;
(2) determination of present processes . . . ;
(3) confrontation of (1) and (2) with a view to ordering, filling in, and explaining history [18] (pp. 84–85).

"Historical data" here means all observations, i.e., data, of remains from the past, organisms in paleontology as well as evidence of human behavior. Adrian Currie states that "there is nothing different, as a matter of epistemic principle, between the biologist, paleoanthropologist, nor archaeologist when drawing such analogies" [19]; also [20] for extended discussion of the historical sciences contrasted to physical sciences). This recognition of our common ground (see also [1] distances Hempel, Salmon, and Binford from the scientific practices founded on the primacy of historical field data.

Why would Lewis Binford revolt against the practices of the historical sciences? Why would his revolt so strongly attract followers? To answer, we should, as Bloor stated, "be . . . with the conditions which bring about belief or states of knowledge". Funding would be an obvious condition, where projects that get funded are more likely to advance both data and their interpretation. Bias toward tall blond self-confident men like Binford could be another condition, in Binford's case. Another condition would be his terming his "New Archaeology" to be "processual": the term was adopted in the 1960s almost throughout academia, from theology to ecology, and linked to "systems" [15] (pp. 105–112). Gordon

Willey and co-author Philip Phillips, Harvard professors, in their much-cited *Method and Theory in American Archaeology* [21], stated "Processual interpretation might conceivably cover any explanatory principle that might be invoked" [21] p. 5. How a series of changes or steps, that is, a process, would be an *interpretation*, not merely descriptive, was not elucidated.

4. Follow the Money

Binford's rock-star personality and his overweening self-confidence dazzled students, and even a few senior archaeologists, for example Paul Martin [22]. Sober archaeologists noticed also that Binford garnered financial support for his projects, including from the prestigious National Science Foundation. Many anthropologists did not, prompting in 1977 an article in *Science* by *New York Times* science writer Gina Kolata. NSF anthropology program officer Nancie Gonzalez was quoted saying that archaeologists did better there because their proposals were more specifically focused, and their significance more clearly stated, in what were considered "scientific terms". Gonzalez was then attacked, in *Science* by nine prominent cultural anthropologists, eight writing a joint letter, the ninth writing independently. In the jointly written letter, the anthropologists demanded:

> Are we to assume that NSF has an official policy subscribing to a simplistic and rigid view of social science harkening back to 19th century positivism or, even more disturbing, to an authoritarian insistence that those dispensing funds may dictate to scientists what science is or is not? (quoted in [23] p. 284.)

The answer to these questions was Yes.

Historian of science Mark Solovey extensively researched the National Science Foundation and its relations with the social sciences. He characterizes NSF as "scientistic", elevating the natural sciences, particularly physics, as the model for "science" and accepting the premise that "science" is a unitary practice [11] (pp. 6–7), [5,24,25]. That is, NSF followed the Enlightenment view that all material phenomena can be analyzed and classified, toward discovering their origins and rules ("laws") underlying their developments. It sought to fund proposals "that satisfied (allegedly) universal and rigorous scientific criteria, including objectivity, verifiability, and generalizability" (Solovey *ibid*.). By 1967, it "concentrated on 'the sorts of things that can be studied in a quantitative, objective way—in the same sense that one can do in the natural sciences.' The agency also stood out as virtually the sole source of federal funding for certain fields, including archaeology" [23] p. 129, quoting a report by NSF Director Leland Haworth). By aligning with NSF's scientistic, "quantitative" methodology and framing his goals as objective, verifiable, and generalizable findings, Binford garnered funding from the agency and sources such as universities that followed its guidance. Funding translates into "success".

We can explain Binford's influence, at least in part, by his ability to support fieldwork and analyses, his own and students', following a physics model of science heavy on statistics, deductive reasoning, and a goal of explicating general "laws". Behind what we can see is an agenda far greater than scientific research: the U. S. National Science Foundation was developed as a weapon in the Cold War between the United States and the Soviet Union [26] p. 58, [23,27]. America won World War II by dropping nuclear bombs created by an elite group of physicists, chemists, mathematicians, and engineers working in Los Alamos, New Mexico. With the demolition of Nagasaki, these men's mission successfully ended, though not their camaraderie. The Cold War between the United States and Soviet Russia, developing about 1947, seemed to call for renewed government funding for the sort of research that had provided superior weaponry during the World War. It need not be concentrated in a remote facility, as it could be a funding agency working through research proposals by scientists in universities and independent laboratories. Nor need, or should, it be openly an instrument for military dominance. It should aim for preeminence in science, whence would flow weaponry, psychological techniques for influencing populations, development of global extractive industries, enhanced agricultural production, peacetime uses for inventions such as plastics; even, briefly, better teaching of science in American schools. The National Science Foundation, a central agency for dispersing unprecedented funds for "science", was created. Its governing board, of course, included men from the Los Alamos project whose concept of science mirrored the physics, chemistry, mathematics and engineering they had utilized in the Manhattan Project.

Against this background, we can see Lewis Binford typecast for an American Scientist. Working-class, WASP, schooled in a rural polytechnic, he went to a physicist for advice on proceeding as a scientist. His mentor favored Carl Hempel's hypothetical–deductive method, straightforward statement of the problem to be investigated, straightforward seeking of data that should be relevant, straightforward conclusion on validity, supported by statistics. Like building a chicken coop. Binford told an interviewer,

> Any time you decide beforehand what your problem is and then say "all right, I'm going to exploit this for solving my problem," you're in the position of generating accommodating arguments. You are accommodating what you see in the archaeological record [to] what you believe about your problem (quoted in [28] p. 47.)

Does that fit archaeology? it would if archaeology were to be conducted as a physical science. Any other approach, such as deriving culture histories through stratigraphy and seriation, would not qualify as science and would not be favored by NSF.

5. Extending the Frame

The Strong Programme's "causality, impartiality, symmetry and reflexivity" does not weigh the question of Truth, it seeks empirical causal factors. In principle, it does not limit these to what has been observed in a laboratory or debates between persons characterized as scientists, although a feminist notices preponderance of men as analysts and analysands. In the case of Lewis Binford's New Archaeology, we should *cherchez la femme*, Sally Rosen Binford. She sat beside Lewis on platforms presenting his ideas and arguments and she co-edited the landmark *New Perspectives in Archaeology.* Divorcing Lewis and, it seems, archaeology in 1969, she moved to San Francisco where she became active in sex education and feminist and lesbian groups [29].

Sally Binford earned a Ph.D. in Anthropology in 1962 at the University of Chicago, with a dissertation on archaeology of the Sahara. Lewis Binford came to Chicago at that time, on tenure track; they had adjacent offices and enjoyed talking archaeology. In 1963, Sally went on Lew's summer project in Illinois, contributing to the publication of the work, and when they returned to Chicago she agreed to marry him to help him publish enough to gain tenure. It was a third marriage for each, and she was seven years older than he. In spite of Sally's efforts as stimulating fellow archaeologist and amanuensis, Lew did not get tenure at Chicago, and the couple went to UCLA where Lew was tenure-track and Sally, adjunct faculty. Being a woman, a challenging person, and Jewish penalized Sally in the job market and in opportunities for research; at home, she did the housework and tried to continue her Paleolithic archaeology, while Lew focused on his career. Abandoning archaeology in 1969 was painful for Sally. The profession did not notice the woman's disappearance.

Extending the frame of a sociology-of-science analysis of the impact of 1960s New Archaeology brings in not only the success of the charismatic WASP man who rigorously followed the NSF model of science, but also the ugly misogyny he lived in. The Strong Programme's first concern is to seek causality. From its broadened perspective, Aristotle's four aspects of "cause" are helpful:

- The material cause: "that out of which";
- The formal cause: "the form", "the account of what-it-is-to-be" or is;
- The efficient cause: "the primary source of the change or rest";
- The final cause: "the end, that for the sake of which a thing is done" [24].

To understand the success of the New Archaeology, we analyze:

- the material cause: contestation within prehistoric archaeology over whose procedures are correct;
- the formal cause, an interpretive procedure;
- the efficient cause, *Sally Rosen's years of working with Lewis Binford to develop and refine his ideas and render his writings clear, scholarly, and persuasive;*
- the final cause, a coherent, workable procedure easily taught to students.

As the Strong Programme insists, this sociological approach is not concerned with whether the New Archaeology was good or bad, true science or scientistic. Basically, it asks *how did it happen?*

6. Conclusions

The Strong Programme for sociology of science has faded as its 1970s proponents pursued each his own particular interests. Its insistence on searching out the social conditions in which scientists labor is now acceptable, along with its abjuration of questions of truth or falsity in scientists' claims of success. Social justice issues underlay much of the Strong Programme's work, supported by its Scottish milieu [6] (pp. 22–24). Two generations later, postcolonialist diversity and the influx of women into the professional workforce have powerfully illuminated social forces molding scientific practices and what is accepted as knowledge.

Sociology of archaeology is essential to reveal foundations of knowledge derived by archaeologists. To begin with, we need epistemic humility [30], [31] (pp. 14–18). White Patriarchal Supremacy excluded women and members of colonized nations from recognition for scientific practice and knowledge. A sociology-of-science approach discovers significant contributions by archaeologists' wives, field laborers, local residents near sites, and lab workers. It is more than a matter of giving them their due, it leads us to critique a great deal of published archaeology that by excluding subalterns' knowledge, compromised our understanding of the past.

Let me give but one example, to make my point. After Sally Rosen left, Lewis Binford introduced the word "forager" to replace "hunter-gatherer". "Forage" derives from the same root as "fodder" and refers to the food and grazing of herbivores; it can apply to humans only in regard to livestock managers obtaining food for their animals [25]. Customarily, it was derogatory to call a human a forager. Cultural geographers use the phrase "seasonal shifting harvesters" to refer to communities that anthropologists have termed "hunter-gatherers", and this phrase proves insightful in comprehending the millennia when North Americans cultivated native plants and managed food animals (e.g., Eastern Agricultural Complex, Pacific Coast mollusk and root gardens). Binford's training and encouragement to use the hypothetic–deductive mode of framing research, and the Cold War support for narrow scientism rather than epistemic humility, led to a generation of American archaeologists blindly perpetuating Western imperialist pictures of America's past. Whence came their hypotheses? from colonialist "conjectural history" [6] p. 32 and John Locke's treatise to support his patron's takeover of First Nations' lands in North America [32]. Seeing with a strong program of sociology of science tears the veils off imperialist science, its goal of revealing "general laws". With humility and respect for empirical data, we can go back to discovering the histories of human communities.

In this short essay, I choose Sally Rosen Binford to illustrate the kind of vital data that the Strong Programme approach can discover and present. Focusing on Sally illustrates also, that the familiar mythical theme Hero's Quest tends to understate the role of women in a hero's journey. Historians endeavor to be factual, but facts are many, editors demand a structured focus, and custom embeds storylines in our thinking. The Strong Programme took an ax to the edifices Novick described in *That Noble Dream: The "Objectivity Question" and the American Historical Profession* [33]. Working consciously with direct empirical data, open to what traditional strictures and foci tend to overlook, we can better understand the factors that produced what we see when looking at the past.

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