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LET'S BE REALLY CLEAR ABOUT THIS.
Reply to Goldsmith

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The response in this journal by Goldsmith ("Why are there still social metaphysics?", 1981:1-7) to our article ("Why are there no social physics?", 1979:33-58), draws the line between the scientific (or positivistic), and the humanistic traditions in our discipline. In what follows, we hope to draw the line even more clearly.

The most striking thing, to us, about Goldsmith's remarks is that they are so unconstructive. We do not object to others taking non-science approaches in order to understand phenomena. There are, after all, many roads to insight. We do object, however, to those like Goldsmith who seek to discredit the very possibility of doing science on human phenomena. Let's be really clear about this. The positivistic and the humanistic perspectives on the study of human behavior and human internal states (values, for example) really are quite different. It's time to draw the lines unambiguously and to get on with the job of doing two different kinds of anthropology, scientific and humanistic. (We dislike the use of 'humanistic' as an antonym for 'positivistic' because it appears to connote a lesser concern for humanity by positivists than by humanists. Nonetheless, having said this, we will distinguish hereafter between positivists and humanists, since this is such a common distinction in our field.)

Now, the question must be asked: why is it that the humanist anthropologists are sometimes so antiscientific?. What on earth for? Two things come quickly to mind: 1) competition for material resources, such as university posts, grant funds, journal space, graduate research assistants, consulting income;

and 2) honest conflict over the potential abuse of the powerful knowledge that positivistic science produces. Still, what have humanists achieved by railing against positivism? They have done well in pointing out the limitations of the scientific method, but they have done so on faith and they have failed to provide a program of study that would lead to useful results ... specifically, results that can be predictably applied to the solution of human problems (disease and poverty for example).

Now it is clear that knowledge which permits prediction of phenomena also permits their control, and this, of course, is where the legitimate fear of abuse comes in. It is also clear that there is no way to guarantee the beneficent use of scientific knowledge; science has played a part, at many points in history, in producing unspeakable human suffering as well as in alleviating it. On the other hand, efforts to discredit positivistic science as an effective way to achieve understanding (and, by the nature of understanding, control) of human phenomena, are ill-conceived. Worse, it seems to us that those efforts must play into the hands of those whose values are eschewed by all humane scholars, humanists and positivists alike. Humanists rarely attain the political power that is needed to keep the abuse of science in check. Scientists often do.

Honesty compels the admission that the scientific method is also based on faith ... faith that it will permit understanding, prediction and, ultimately, control of phenomena. Today, several hundred years into the Western-science cultural era, that faith seems, for the time being, justified. Wherever the scientific method has been systematically turned on a phenomenon, the results have been spectacular in terms of prediction and control. And, as we have said, that is at once the lure of science and the source of many moral dilemmas.

Perhaps as a result of those dilemmas, some scholars of humanity prefer not to participate in the development of a science of humanity. We respect those motives. And there are those scholars whose interests in humanity are beyond the scope of the scientific method. Theologians, poets, and some philosophers who are concerned with the "meaning of life," for example, are in this latter group. We respect those pursuits and we suggest that humanist anthropologists would do better to identify with that group of scholars than with social science at all.

We would find it amusing to see so many anthropologists trying to straddle science and nonscience, trying to be taken seriously as scientists in the scientific world and flailing against science at the same time, were it not for the fact that their ambivalence really does impede science, and impedes the efforts of positivists to become useful servants of humanity. Goldsmith is free to be a nonscientist; but what business does he have being an antiscientist? He produces no evidence that our program for the development of a science of humanity would fail to lead to greater prediction and control, and to the alleviation of human problems. Goldsmith produces no evidence that human beings are inherently unsusceptible to scientific

investigation, or that their behavior is not subject to the kind of discoverable rules that permit prediction. The fact is that we already know a great deal about prediction and control on the individual level from the efforts of psychologists. We are beginning to learn a fair amount about social groups as well. One may question whether the control of masses of people through scientific marketing, for example, is a salutary outcome of social and behavioral science. But is such a legitimate concern an adequate basis for claiming that a positivistic science of humanity is not possible? This is no time for the fainthearted. It is surely time for all of us to think very hard about the ethical responsibilities that such knowledge produces. But is no time to confuse such issues with that of the development of a science of humanity.

Now for some specifics. Goldsmith says that we "could claim ... that the question (Why Are There No Social Physics?) is not addressed" to him because he does not share our presuppositions (p. 1). Precisely. We have absolutely nothing against the presuppositions that Goldsmith does hold about metaphysics (presumably just as absolute!); but since the scientific approach rejects metaphysics entirely, we would find no need for further discussion of the matter ... except for the fact that Goldsmith (and others in anthropology) keep raising the issue. From our perspective, it is the utterly futile attempt to find a middle ground between metaphysics and physics that guarantees the failure of social science to develop the power of prediction and usefulness that is the hallmark of the physical sciences.

Goldsmith notes that "for many anthropologists, including myself, 'social physics' is a contradiction in terms" (p. 1). For us, the a priori rejection of a method (science) for understanding humanity, when that method has worked for so many other things (rocks, stars, genes, oceans) seems silly. Still, it is anyone's privilege to reject positivism ... so long as they recognize the privilege of others to pursue this line of inquiry. Good poets usually recognize the value of science, just as good scientists recognize the value of poetry.

Goldsmith says that our commitment is to a method rather than to a subject. Precisely. We are, indeed, committed to the scientific method; this is because we have evidence that it works. We want to know what the results will be if we intervene in a particular social problem in a particular way. We also want to know what will happen if we <u>don't</u> intervene. We want to be able to do this so that we can have the <u>choice</u>, based on something more than just a hunch, of whether to intervene or not; and how to intervene; and for how long. The fear that positivistic, scientific knowledge of human behavior and values will lead to engineering of those things is well justified. But from our perspective, engineering is part of the scientific process. It is the <u>test</u> of knowledge in all areas of science. From our perspective, applied anthropology is not merely justified on the grounds that we cannot escape our responsibility to make the world a better place. That's an easy argument to reject, because it is just as easy to claim that no such responsibility exists, in the first place, and that, anyway, we don't know enough to intervene in people's lives. We

believe instead that applied anthropology (if taken seriously) is the foundation of a science of humanity, based on the study of natural experiments, the goal of which is prediction and usefulness. By 'natural experiment' we mean the tests, going on all around us, of rules governing human behavior. We cannot choose which rural village gets an irrigation system and which village does not; we cannot provide clinics to some villages and not to others; we cannot randomly assign some corporate offices to become automated and others not to be; but every time others make those decisions, tests of rules governing human behavior are set in motion. A naturalistic, positivistic discipline seeks to discover and to test those rules. Anthropologists who are not interested in the development of social engineering should not, therefore, align themselves with the camp in our discipline that likes to consider itself scientific.

Goldsmith criticizes our research on the small world problem. We reported a few of the essential results of the research in our 1979 article in this journal, and we did so in order to illustrate some points. We did not report fully on our research because it was already in print (Killworth and Bernard, 1978). After criticizing our work, Goldsmith says 'perhaps my criticism could be countered by a fuller exposition of the research. In its abbreviated form it fails to convince me that it represents a major advance in understanding human behavior' (p. 4). Well, Goldsmith could just go and read the 'fuller exposition' if he likes. He will find all the objectionable assumptions there, discussed in detail, that he rails against. And he will remain unconvinced that our research is a major advance in understanding human behavior. On the other hand, we are not convinced that it is a major advance, either, and we never claimed it to be. Goldsmith's accusation that we think our research is a major advance is a misreading. We are, to be sure, committed to the scientific method. But we are committed neither to the techniques we use in any given experiment, nor to the results from any given experiment. In science, single experiments do not usually establish much. We have since repeated our experiment twice (Bernard, Killworth and McCarty, 1982; Killworth, Bernard and McCarty, 1984) and we are in the process of repeating it three more times in three different cultures.

Goldsmith also misunderstands our statements on small-world research. We stated (1979:41) that the "small world technique tells us nothing about the rules which govern the structure." However, Goldsmith (1981:4) decides to define a result (incidentally, misquoted by a factor of 100, almost certainly a typographical error) of some excellent research (by Milgram) as a rule or law. Perhaps we just differ on our definition of laws; Goldsmith's operant definition would include "water freezes at zero celsius," a 'rule' which tells us nothing whatever about the properties of heat, water or temperature. Laws tell us why water freezes at zero celsius.

But without a doubt, Goldsmith's pet hate was our back-of-the-envelope model of human group evolution. It seems to us that his reaction operated at two different levels. The first level is the (understandable) fear that if the entire

course and process of human group evolution could ever be reduced to a small model which ran on an HP-25 pocket calculator, then a lot of anthropologists would be out of a job. This also says, in a disguised way, that "I believe human group evolution to be a very complex thing, and therefore anybody - even for the purposes of making a point in a paper - who purports to be able to model such a thing is obviously wrong." Well, we too made no bones about the complexity - but that's not what modelling is all about. One extracts some of the salient features of a situation (based on prior scientific, tested research, or failing the existence of these, based on a good hunch) and sees what happens when these features are simulated mathematically. If we don't like the output, for whatever reason, then back to the drawing board. If there are some interesting features in the output, then we explore those more closely and see if we can understand why they came about.

Modellers in the physical sciences do this all the time; it's a standard technique for turning up interesting ideas that can be tested in the field, i.e., in the laboratory or in natural situations. One of us spends much of his time running models of oceans in square boxes. Oceans aren't square. But using a square box in a model cuts the complexity down and (hopefully) lets us see some of the wood between the trees. Oceans aren't simple. But concentrating on just some features (e.g. ignoring temperature in one model, or wind and waves in another) lets us see what's going on. People aren't square or simple either. But does that make them fundamentally less susceptible of being modelled, a priori, than oceans? Sure, they may turn out to be; but we would be foolish to assume so from the beginning. Occam knew what he was doing.

The second fundamental objection is also based on worry, but is more subtle. Purely for didactic purposes, our little model contained a great deal of random number generation. Many people, Goldsmith among them, would probably rail at randomization being considered as one of the fundamental driving mechanisms of human group evolution - at least in the deliberately straightforward, uncomplicated, manner that we introduced it. After all, if things are really that random, why study them?

We confess that this was furthest from our minds when we wrote our paper. But recently, Mayhew (1984) has also argued that in order to study any phenomenon, we should convince ourselves first that the phenomenon yields events which differ significantly from those which might occur by chance alone in a random universe. He is, of course, quite right. At sufficiently small scales, most phenomena look pretty random. Fluids behave themselves (i.e., obey laws) until one gets to tiny length scales where Brownian motion comes in; and still smaller, when quasi-random molecular motion occurs. Perhaps individual people are like molecules; they certainly seem pretty random to us, though they obviously also seem to satisfy laws most of the time. En masse, though, people may well be more intelligible. We believe that most anthropologists share this belief, but not necessarily our way of looking at it. And they surely don't have to share our quantitative method of studying

people. There is nothing inherently nonpositivistic about qualitative data. Physical science relies desperately on fuzzy, descriptive data as much as it does on precise measurements. Both yield insight, but quantitative data also yield replicability ... and that, of course, is why all scientists move from qualitative data to quantitative data as quickly as they can.

So, can there be a science of humanity? Obviously, we think there can be. In our earlier article in this journal, we pointed out some of the reasons for the failure of anthropology to contribute to the development of such a science so far. We can, summarize those reasons here, in one phrase: because it's never been tried in a systematic way by a critical mass of practitioners at the same time. In order for a science to develop, there must be a large group of practitioners who are unambivalent about the enterprise in the first place. They cannot be rendered impotent by concerns about whether the enterprise is possible.

It seems to us that that is just what goes on in anthropology. More time is spent in existential crisis, worrying about whether or not science is possible than in conducting science as if it were possible. It could turn out that another method will be found that is better for studying humanity than is positivistic science. For the moment, though, no such method has been found; so statements about metaphysics and the limitations of positivism will have to be based on something other than faith if the antipositivists hope to win this battle.

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