LETTERS

Academy Energy Study

In connection with Philip M. Boffey's article of 28 January (News and Comment, p. 380), we would like to clear up a possible misunderstanding concerning the nature of the interim report of the NAS–NRC Committee on Nuclear and Alternative Energy Systems (CONAES) and the role of the 250 or more "participants" in this study. The study is being conducted on three levels: the 16-member CONAES committee; four panels; and some 30 "resource groups," 27 of which report to one or more of the panels, with the other three reporting to the main committee.

The interim report is solely the responsibility of the parent committee, and no participant aside from the 16 members of the committee should be inferred to be responsible for its conclusions or for the conclusions that will appear in the final report to be issued this summer.

It is intended that reports from each of the four panels will be published by the Academy at approximately the same time as the report of the parent committee, and the panel reports may include contributions from many of the resource groups. The panel reports will be the responsibility of the relevant panel members, and will not necessarily agree with the views arrived at by the parent committee after its review and analysis of the panel and resource group inputs.

It is regrettable that, whereas Boffey's article carefully stated the limited conclusions of the interim report, several stories in the general press contained speculation concerning the agreed-upon conclusions of CONAES that went well beyond any statements made in the interim report.

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"Scientific" Social Anthropology

Gina Bari Kolata's article "Social anthropologists learn to be scientific" (Research News, 25 Feb., p. 770) ascribes certain policies of judgment and action to the Program Director for Anthropology of the National Science Foundation (NSF). I am writing because of a real concern over the policies that are evidently being pursued by this office of NSF.

The burden of the article is that, in an apparent attempt to improve the service of her office, the Program Director for Anthropology has been advising applicants that they should design their studies so as to be "more scientific." Unfortunately, Kolata is not explicit as to what is meant by "scientific" in this practice, but given the history and nature of anthropology, and some recent individual instances with which I am familiar of grant applications to NSF, I gather that it means the formulation of research proposals in the form of the hypothetical-deductive method. The arbitrary imposition of any specific paradigm of methodology on all research projects is dubious; on some social anthropological projects, the requirement is positively destructive.

There are at least two major areas of work in social anthropology to which this approach is prejudicial. First is that known as general ethnography, and second is the so-called "interpretive" approach. The first is scientific exploration that falls into the "context of discovery." Ethnography in its most general sense is the description of the ways of life of peoples of the world. It has been one of the major sources of basic anthropological information. It has ranged in kind from its earlier history of reports of missionaries, explorers, colonial officers, and travelers to contemporary detailed explorations of specific aspects of life in which a systematic series of approaches are used that have become known generally as "ethnoscience." The approach, while historically more concerned with the "exotic societies," has long since become a major methodology for the study of all societies, our own complex and urban way of life included.

The implication in Kolata's article that it is unscientific "to study a particular group simply because it is disappearing or because no one had ever described it before" is simply disastrous. Human society is marked, to a degree uncommon among life forms, with constantly emerging new cultural forms, answering to new conditions and as yet little understood mechanisms. Our best evidence for study is these forms. Of particular importance are those representing adaptations to earlier and now disappearing life conditions on the planet. There are many strong scientific reasons for promoting the investigation of groups that are disappearing or have never been described before. (Not entirely independent of their real scientific importance, there is an additional humanitarian concern, since these groups are often living under increasingly difficult material and environmental circumstances.) For NSF to openly pursue a policy that deters such investigation probably reflects the behavioristic fad that has so marked much of social science in the past quarter-century. It has had some beneficial consequences.

In this instance, however, its effect is to discourage exploratory scientific work of the greatest importance to our understanding of man.

The second area of concern is a newer anthropological development, referred to as an "interpretive" approach, whose leading exponent is Clifford Geertz of the Institute for Advanced Study at Princeton. It rests on the premise that human activity fundamentally pursues a stochastic path; that much of it is therefore "new," innovative, and not previously acted out. As such, there is much in it that is predictable only in a limited, trivial, or banal sense; many of the most important things to be studied are those very aspects that have not yet occurred with any frequency, are not members of any identified category of phenomena, and therefore cannot be subjected to much of the methodology that has emerged from the positivistic approaches that have dominated the natural sciences during the past century. It is very unlikely that Geertz, certainly among the most distinguished anthropologists of his generation, could propose work that would meet the "scientific" criteria that are evidently now being applied by the NSF Anthropology Program.

The arguments, however, are even greater than recent developments in anthropology would suggest. At the same time as theory within anthropology has been placing greater emphasis on the open-ended nature of human behavior and evolution, the natural sciences have been moving in a parallel direction. One need not go to the extreme position of Jacques Monod to assert this. The work of individuals such as the late Conrad Waddington in biology and Ilya Prigogine in statistical mechanics and physical chemistry has clarified the nonlinear quality of much natural activity and represents areas of scientific investigation that could never have emerged if a strict hypothetical-deductive method had been required for them.

Since NSF is central to much anthropological research, I would urge its officials to seriously reconsider their policies and institute an orientation and series of criteria for evaluating proposals that will encourage broader and more exploratory anthropological investigation as well as the work of those who choose a more traditional positivistic path. We have recently been accused by a foreign colleague of "showing signs of a hardness of the arteries"; the policy of the NSF Anthropology Program seems to support that assertion.

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In Kolata's article of 25 February, Nancie Gonzalez is described as making an effort to teach social anthropologists how "to think like scientists," touring 60 universities in order to propagate her views of what the field should be. Gonzalez' approach to the NSF Anthropology Program has long been a source of deep concern and profound misgivings to some anthropologists, since her statements suggest a misconception of what both science and anthropology may be.

Kolata remarks that "in order to get more money from NSF, social anthropologists will have to think more like scientists." No one disputes that proposals in all fields should be clearly written, logically argued, and based on thorough scholarship. Yet social science is neither biology nor physics. The study of society involves factors of value and history radically different from research in those sciences apparently endorsed by Gonzalez as models we must emulate. Are we to assume that NSF has an official policy subscribing to a simplistic and rigid view of social science harking 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?

Gonzalez' pronouncements and tours of campuses underscore something we have suspected for many months, that the NSF program in anthropology may be in the process of becoming unduly influenced by a narrow and ill-informed view of what an entire scholarly field should be. This is not our conception of the task of an NSF program director.

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Adams is tilting at windmills, largely because he seems to have read into Kolata's article things that were not there and not intended. Neither I nor previous Anthropology Program directors at NSF have ever controlled policy in the ways he suggests. Anyone who has ever been on one of our advisory panels will attest to the fact that the Program responds to the field and to the unsolicited proposals received, which compete one against the other in the review process. The Program Director largely follows the advice of this panel, and over the years it has been the *panel* which has tended to reject proposals that do not provide adequate rationale for doing the research in the first place.

It might be instructive to quote the very simple statement concerning research which we send to would-be applicants who get in touch with us before submitting a proposal.

> A good research proposal is focused on specific phenomena and on a specific scientific question or questions that the research proposes to answer. The proposal explains the relationship and importance of its particular scientific question to other scientific questions of interest; it gives a brief account of attempts by other scientists to answer the same or related questions and says what the proposer thinks are the reasons for success or failure of these other attempts. The proposal describes the kinds of things one should know in order to answer the question(s) posed, and how the investigator plans to go about gathering these data. It also mentions possible areas of application *if* they can be foreseen. Finally it describes the amount of money needed and how it will be spent in order to achieve the expected results. It ends by stating the available sources of support and the specific request for remaining funds from NSF.

The statement does not mention hypotheses, nor does it preclude any of the kinds of research Adams defends. I quite agree with him that rigorous adherence to a single theoretical or methodological framework would be disastrous for any science. A review of the actual research projects funded by the Anthropology Program over the past few years might relieve his fears on that score.

In my interview with Kolata, I opined several other reasons why the Anthropology Program has never funded social anthropology–ethnography to the same extent as it has archeology. They have to do with the nature of traditional research in these fields, which has usually demanded extraordinary rearrangements in one's personal life, as well as with the fact that much respectable social anthropological research addresses humanistic, rather than scientific, questions. In addition, there have in the past been many sources of funds other than NSF for social anthropologists. As the number of professionals increases, the competition for limited funds increases. Under such conditions, each agency or foundation becomes even more concerned that funds be awarded only to the most excellent efforts. The proposal is the only fair basis upon which to make decisions concerning allocations. It had better be well done. It was to this issue that I directed my remarks.

Concerning the letter from Beidelman *et al.*, it is indeed disheartening that my eminent colleagues have chosen to criticize me on the basis of an article I did not write, and without firsthand knowledge of the situation. None of the signers has ever been on the NSF Anthropology Advisory Panel. Therefore it is unlikely that any of them can know how many proposals of each type are received, or what their quality and condition actually has been. Nor can they be expected to appreciate the basic continuity in both problems and efforts within the Program over time. The panel has recommended and the Program has funded a variety of studies, including some of the sort carried out by the signers themselves, for many years. The 1976 grant list will soon be published in the newsletter of the American Anthropological Association. I invite readers to peruse that and draw their own conclusions.

I fear my critics forget that not all social anthropologists have had the benefit of the kind of training they provide and the collegial support they enjoy at the elite institutions they represent. In contrast to their uninformed remarks, I have received some 50 approving letters and phone calls asking for copies of the previously quoted statement concerning research—drafted by me, but built upon earlier statements made by previous program directors. Many of these requests have come from respected, senior persons in the field. Clearly, many of my critics' peers believe my efforts are worth something.

Since I became Program Director the total proposal load has increased by 25 to 30 percent, and as of 1 March 1977, we have a set of proposals in which the social anthropological outnumber the archeological—for the first time, we believe. On the basis of the increase in volume and apparent improvement in quality of proposals received, the Program in Anthropology has been slated for a 25 percent budget increase in 1978. This has been accomplished through much patience and hard work, and with the advice, understanding, and cooperation of dozens of anthropologists across the country. I have always welcomed dialog concerning the program. This is one of the purposes of my "touring" campuses.

The position of Program Director is a rotating one, and my successor will soon be named. However, it would be both unwise and unfair to assume that this alone will bring about major improvements for social anthropology. These improvements must come out of the field itself. Hopefully, this heated exchange of words may awaken the profession sufficiently so that more of its members will chose to work with the Program Director in what should be a united effort to enlarge the funding opportunities in our field.

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Krypton-85 and Atmospheric Conductivity

In his article "Meteorological consequences of atmospheric krypton-85" (16 July 1976, p. 195) William L. Boeck arrives at the conclusion that ionization due to the release of ^{85}Kr from the nuclear power industry could decrease the total electrical resistance between the earth and the ionosphere by about 10 percent within the next 50 years. He speculates that this could lead to a form of inadvertent weather modification, the effects of which—be they beneficial or harmful—cannot be determined on the basis of our present understanding of atmospheric processes.

This question is of much significance for differential cost-benefit considerations in connection with the retention of ^{85}Kr by nuclear fuel reprocessing plants, and it could also be a controversial issue in the public debate on the acceptability of nuclear power. It is therefore important to make a realistic estimate of the extent to which ^{85}Kr will be retained and to put the effect of release of ^{85}Kr in proper perspective by comparing it with other factors influencing the electrical conductivity of the atmosphere. As it appears from Boeck's article (equation 4), one such factor is particulate pollution of the atmosphere.

Boeck assumes a ^{85}Kr concentration of 3 nanocuries per cubic meter in the early part of the next century, basing this value on information given in (*1*). This is in fair agreement with the information given in (*2*). In calculating this concentration it is presupposed, however, that no ^{85}Kr is retained by fuel reprocessing plants. According to proposed standards (*3*) it is intended that the release of ^{85}Kr shall be limited to 50,000 curies per giga-