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ABSTRACT

Anthropology research should be relevant to public policy formation. If anthropologists continue to produce research which reflects a "detached observer" perspective, their studies will not enjoy widespread credibility. The use of policy-relevant anthropology (applied anthropology) will depend in large part on the efforts of anthropologists toward making their value biases and research documentation public. One area in which social relevance is particularly clear is ecological anthropology. This relevance is exemplified by widespread public concern over wilderness, recreation areas, rights of native peoples, and depletion of natural resources. Anthropologists can contribute to policy formation by critically approaching an issue, objectively evaluating the data, and providing information to policy makers. A case in point is a 1973 controversy between an Inuit Indian community and an oil company consortium over exploration rights on Bathurst Island, Canada. In this situation, anthropologists re-evaluated evidence relating to ecological damage and provided documentation to policy makers. The conclusion is that anthropologists share the responsibility of all scientists to create an informed public opinion and to undertake research in accordance with this objective. Conversely, anthropologists should avoid endorsement of policy-relevant issues prior to adequate scientific study of the problem. (Author/DB)

ANTHROPOLOGISTS AND POLICY-RELEVANT RESEARCH

THE CASE FOR ACCOUNTABILITY

by

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INTRODUCTION

In 1971 I was a member of a CSAA panel enquiring into the future of sociology and anthropology in Canada. At that time many of today's anthropology departments in our universities were still joined to generally larger and more established sociology departments. Thus it was probably too early then to be making a universal appeal for applied anthropology to be added to the curriculum, for it appeared a nebulous subdiscipline lacking the scholarly and professional standing among academic social scientists necessary to advance the developing anthropological cause of that day. In my report to the CSAA I referred to the need, as I saw it, for anthropology in Canada to establish credibility among the general public and institutional decision makers, if it were to continue to develop as a contributing social science. I suggested, on the basis of responses to a questionnaire survey administered to nearly 500 university-based anthropologists, sociologists and biologists in Canada, that anthropologists (1) had a more negative image of themselves in comparison to members of the other two disciplines, and (2) they collectively did less to counter their perceived irrelevance (Freeman 1971a).

In this present paper I wish to take the issue one stage further, and suggest that concern to establish widespread credibility will in turn depend on anthropologists facing the requirements of public accountability, for credibility is not readily or uncritically bestowed on academics by publics outside of academe. I would suggest that the nature of research we do, as 'applied anthropologists' (and I will return to that term later) does not have the most direct bearing on our establishing public credibility: the *utilization* of our research findings is the far more important factor, and it raises a host of related questions, as to, for example, the role of scientist

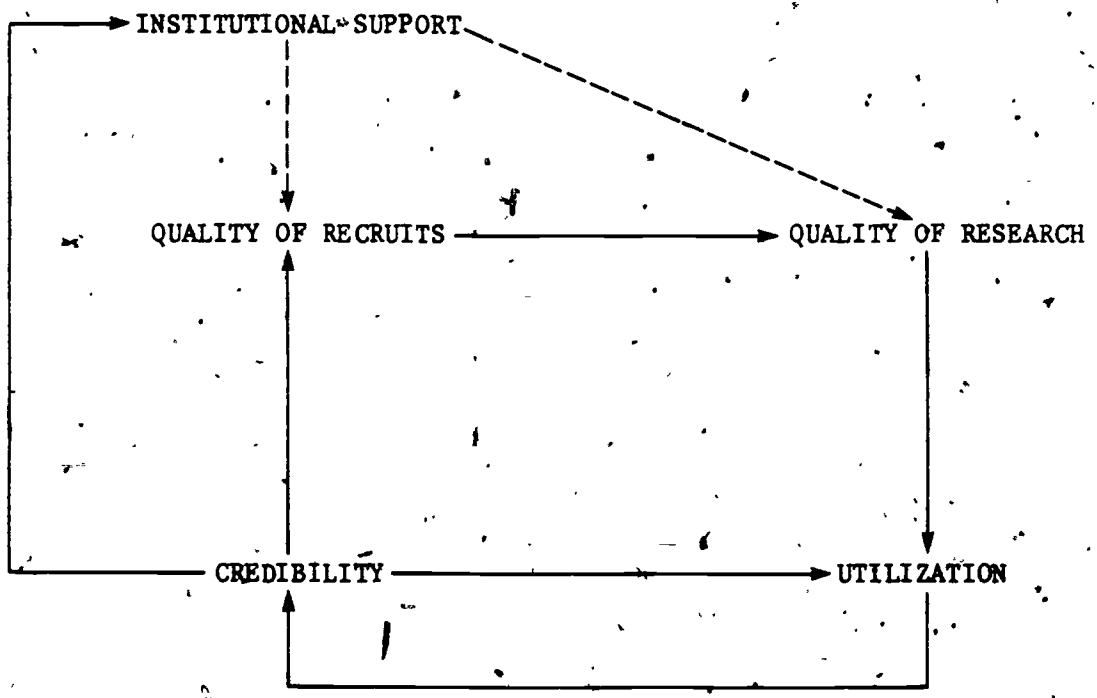

FIGURE 1. RESEARCH/REALITY RELATIONSHIPS IN APPLIED ANTHROPOLOGY

as advocate. The important relationships between research funding, recruitment into the profession, utilization of research results and credibility are summarized in Figure 1. I do not wish to speak further on these relationships, other than to emphasize two important aspects of the paradigm: (1) the important feedback relationship between utilization and credibility and (2) the indirect and secondary role that institutional support plays in influencing quality of research.[1]

CHOOSING A LABEL

The anthropological literature contains several labels and descriptions of suggested roles for anthropologists involved, on the ground, with the process of social/cultural change. The alternatives range from the most detached and least intercessionist to the most societally involved and activist in nature.

Starting from the neutralist position, we identify the anthropologist serving as an honest information-broker, one who doesn't take sides in local issues but who aims to facilitate the acquisition and free-flow of relevant information to all parties involved in the matter (*e.g.*, Salisbury 1973; L. Thompson 1976). The concern to discover knowledge but not apply it had earlier gone under the name of 'action anthropology' (Tax 1958:18-19) though more recently that term has come to imply a far more activist goal-oriented commitment to helping a target population resist change sought by an agency external to that population (Jacobs 1974:209).

The active intercessionist stance is guided by practical as well as ideological considerations for it has been suggested that non-involvement will impede the acquisition of important data[2], whose very collection is the purpose of the anthropological endeavour (Jacobs 1974).

This interactionist stance has also been called 'clinical anthropology'

(Jacobs 1974) and 'advocate anthropology' (Petersen 1974) and is held to differ importantly from the established 'applied anthropology' (Foster 1969) in that in the latter case the anthropologist works to achieve practical ends set by an agency external to the community itself, ends often set prior to the arrival of the anthropologist on the scene. In 'clinical', 'action' and 'advocate' anthropology the anthropologist utilizes anthropological knowledge in the service of the community, either in it's efforts to resist change objectives set from without, or to achieve change goals decided from within, the community.

With respect to these alternatives a recent commentator has cautioned that anthropologists should "set no (societal) goals, engineer no solutions, (and) activate no policies" because it is probably harmful to the long range health and viability of the client society for any outsider to interfere with that society's decision making and action prerogatives (L. Thompson 1976; see also Castile 1975). This view appears not to be shared by the action school, who seem to assert that either as professionally trained scientists (Petersen 1974) or as concerned human beings (Jacobs 1974) anthropologists must assert themselves actively and purposively in the client's cause. On the other hand, there is more usually agreement that social policy is rarely influenced by the results of research, for it appears that a definition of the problem and the solution to the problem has normally been arrived at before the social scientist enters the picture (*e.g.* Jones 1976; Matthews 1975).

In summary then, we can observe that divergent opinions currently exist with regard to the appropriate practise of the applied role. However the underlying motives may be in closer accord, for few would likely disagree with the opinion of Tax that what he called action anthropology is a process directed primarily and unalterably to truth seeking, alternative providing, restriction removing and mutual learning (1958:18-19). I would suggest here that provided

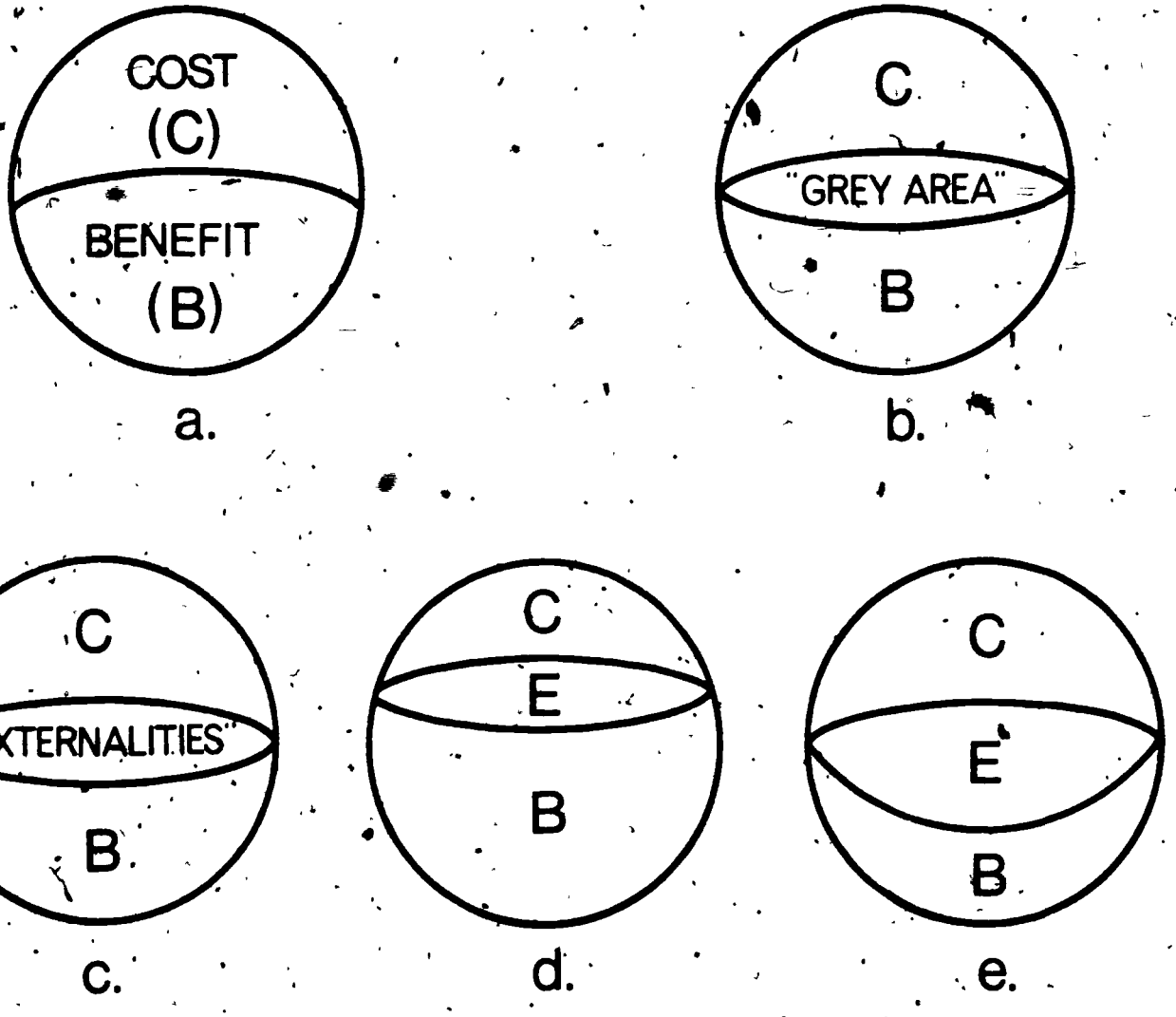

FIGURE 2

VARIABLE PERCEPTIONS
IN COST-BENEFIT ANALYSIS

the anthropologist obtains the necessary assurance that such goal-values are to be paramount, then it matters not who the patron of the research is, whether the local community or some agency external to it.

ECOLOGICAL ANTHROPOLOGY AND POLICY ISSUES

I wish to illustrate the part that anthropologists can play in policy-oriented (applied) professional work by reference to a particular field of cultural anthropology, namely ecological anthropology. Given that (according to Barry Commoner) the first law of ecology is that "Everything is related to everything else" it is not hard to imagine the social relevance of research in this field of endeavour. It is certainly true that a search of the literature points to the sad neglect, by ecological anthropologists, of research into socially relevant areas of concern. However, a series of recent reviews of ecological anthropology (E.N. Anderson 1969; J.N. Anderson 1974; Bennett 1976; Montgomery *et al* 1973; Vayda & McCay 1975) having made explicit reference to these omissions of the past may now serve to direct some anthropological interest into the policy-relevant environmental research field.

I now wish to illustrate my discussion of this topic by reference to environmental decision-making in Canada, and the necessity that public and institutional decision-makers have access to relevant data, some of which can be provided by anthropologists. The reason why anthropologists may have a part to play can be seen by reference to Figure 2 where the limitations of one important tool of environmental decision-making--the cost-benefit analysis--is illustrated. In Figure 2a the grossest error is being perpetrated by the decision-maker for he assumes that all dimensions of the question being decided can be quantified into essentially economic costs or benefits. Thus such externalities as, e.g., social costs, are ignored (or transmuted, which is no better a solution). In Figure 2b the existence of certain non-economic

exigencies are recognized, but given little weight in the decision-making process; these show up as the 'grey area' which is effectively ignored, downplayed, or otherwise of little consequence where politico-economic decisions are being made. Figure 2c represents a redefinition of the situation with the 'grey area' intruded into the public policy awareness arena, perhaps as a result of a public interest group making a well-documented and publicized issue out of the matter under consideration. Such considerations increasingly demand attention in environmental impact statements as for example, the intrinsic value of wilderness and recreation areas, the rights of native peoples to a preferred lifestyle, and the amenity rights of others in general when future options are being lost due to a particular environmental development.

Thus in Figure 2c a recognition of varying perceptions of the problem is allowed; this now obfuscates the original cost-benefit analysis even further, for the perceived costs and benefits may be assigned very different weights according to the analysts' perceptions of the desireability of the planned change. Thus 2d may represent the institutional decision-maker's analysis, whereas 2e may represent an opposing public interest group's evaluation of the case. But cannot this bind be resolved by asking a scientist to undertake research, and to come up with a statement that is impartial, truthful and a wholely objective assessment of the matter? Fortunately natural scientists have little illusion about their limited ability to clarify complex policy issues, though their concern to find satisfactory answers to ethical and humanistic problems of moment seems far more articulated and self-critical than currently manifest among social scientists, whose values and behaviour are rarely the subject of critical study (*cf.* Matthews 1975).

THE ANTHROPOLOGICAL ENDEAVOUR AND THE QUESTION OF OBJECTIVITY

Objectivity, as I accept the term, does not refer to some mental or psychological state of mind entirely in the head of the researcher. It refers to a critical approach to evaluating both data and inferences, that meet the usual non-personal canons of science. In this sense objectivity is contrasted to a subjectivity which relies for validation largely or wholely on an appeal to the authority of a particular ideology or person. As Kaplan (1974:828) points out, to stand outside of objectivity is to place oneself in a world bounded in everyman's case by the particularities of his own individual state of mind, which hardly provides a sound basis for discriminating among many alternative choices in the rational pursuit of 'truth'. Furthermore, value-free enquiry does not require that the anthropologist holds no personal values, for it is obvious we all do (with differing degrees of fervour) on a variety of topics. What value-free science is, therefore, is less a reflection of the ideological stance of the investigator (whose values may every properly influence his decision as to what problem to research) and more a statement of the normative mode of evaluating the data stemming from the research. In other words, the purported value-free position of the scientist is in respect to the stance he adopts in evaluating a piece of work according to whether it is good science, irrespective of whether the conclusions reached accord with his own personal (non-scientist) view of the world.

It seems reasonable to accept therefore that anthropologists can potentially meet the scholarly canons of science without demeaning themselves as human beings, because the individual can be scientist and hold strong, and seemingly antithetical moral ascientific positions (witness, for example, the Christian beliefs of such eminent evolutionary scientists as the late David Lack F.R.S [1957] and Alister Hardy F.R.S [1976]). The next question however,

is how scientific is anthropology -- how near to the understanding of 'truth' and 'reality' do we come in our work? If anthropologists claim, as most do I think, that all human populations govern their behaviour by cultural means and that all cultures are "particular, local, and evanescent" (Harris 1971:6) there certainly is a problem in claiming that a high degree of scientism attaches to the practice of anthropology.[3]

SCIENCE AS TRUTH

Earlier I suggested that science might be in a position to assist 'objective' decision-making by ultimately providing 'truthful' evaluations of researchable problems. However, the public felt-need for scientific 'facts' (truths) presumably results from some widely held perception of the inherent bias of non-scientists (which category includes the lay public and the institutional decision-makers). This viewpoint implies that scientists are somehow free of bias, that is, hold wholely objective, value-free opinions in relation to researchable problems.

Recent studies and exposées have suggested that science is anything but bias-free. Bias may result from either intentional error or unintentional error. There is also plain bad research, which may contain either of these classes of errors and is, in addition, poorly or inappropriately conceived and executed.

Unintentional errors also result in bad science, but may be forgiven as they are often human error or result from current limitations of technology or science. An example would be Shank's calculation of the value of *Pi* to 707 decimal places; only after the advent of computers was his figure checked, whereupon the last 200 figures were found in error. Clearly no harm was done through that particular error and there is no reason to suppose wilful intention to err on the scientist's part. A case of alleged intentional error

TEST	MINAMATA CLINIC	YAMAGUCHI UNIVERSITY	ENVIRONMENT AGENCY
SENSORY DISTURBANCE:			
PERIPHERAL (EXTREMETIES)	+	+	-
PERIORAL	+	+	-
LEFT HEMILATERAL	+	+	+
CONSTRICTIONS OF VISUAL FIELD:			
CENTRIPETAL	+	+	+
TUBULAR	Not tested	Not tested	Psychogenic +
AUDITORY DISTURBANCE	+	+	-
ATAXIA	+	+	-
DYSARTHRIA	+	+	-
OTHERS:	+	±	-
TREMOR (FINGERS)	Not tested	Deformity +	Not tested
X - P CERVICAL SPINE			

FIGURE 3. COMPARISON AMONG THREE INDEPENDENT EXAMINERS: TOKUYAMA BAY FISHERMAN, AGED 69 YEARS (After FUJINO 1976)

was reported recently, when a senior scientist-administrator in a U.S. federal agency was accused of distorting research findings to 'prove' that sulphur-bearing fuels had an adverse effect on health. However, it was hard to obtain evidence, other than hearsay, against the scientist due to other scientists' unwillingness to be publicly identified as critical of an influential colleague (Roffey 1976a:352). Similar charges of intentional bias against scientists are not hard to find in the literature, especially as relating to contentious policy-relevant issues (*e.g.*, the effects of lead on health, *q.v.* Gillette 1971; Robbins & Johnston 1976:355ff). Figure 3, for example, shows the results of a series of diagnostic tests conducted on the same mercury pollution victims by different medical authorities. The results do vary and the issue is a value-laden one with tremendous social, economic, political and medical consequences --but is the observed discrepancy the result of a conscious desire to influence an important outcome? It could be, but then again it could also be the result of the clinicians' varying level of skill or a number of other reasons.

KEEPING SCIENCE HONEST

If it is easy for scientists to be less than truthful in dealing with other scientists (and a recent survey of the subject suggests it is, see St. James-Roberts 1976) then it should be even easier for scientists to perpetrate fraud on non-specialists.[4] It is relevant to observe that those matters most usually involving the interaction of science and society takes place in a public, not scientific, arena. It seems likely that scientists entering such debate are not subject to the sanctions that normally pertain in peer group discussion, and lower standards of proof are asked for and offered. The result of such a situation is very often utterance, by the scientist, of half-truth or suppositions, or what has been termed trans-science (Weinberg 1976:341). There seems little doubt that many policy-relevant issues involve

opinions being expressed before an adequate scientific study of the problem has been undertaken. The absence of pertinent data need not debar a scientist from entering the debate *as a scientist*, but it does require that the scientist be very honest in delineating the extent of scientific knowledge and the beginning of a trans-science opinion. There is a further requirement placed on the scientist, and that is to make very clear that his views are not value free, for as Howard Becker has observed (1966) value commitments (in social scientists as in anyone else) comprise inescapable facts of life. Some go further than suggesting values are merely there, and state that a value commitment is a necessary condition of objectivity. Gouldner has compared the social scientist to a judge. The judge can be impartial even when he decides in favour of one party in a dispute; his decision is not impartial and objective *Because* it is value free, but because it is made in terms of clearly defined and explicit values (Gouldner 1968). The values which a scientist holds as a person as well as a scientist should therefore be made explicit, though it is quite possible that an individual scientist enters the debate for reasons of scientific curiosity, to find out the facts, rather than to advance a particular view-point dependent upon the facts so discovered. In my view this is a legitimate role, though I recognize that other of my colleagues will feel this role is not enough and may be, in their view, morally reprehensible. I recognize that in earlier times I too would have advocated advocacy, but now I feel less happy about that role. Perhaps several years of association with a developing nation in this country have caused me to rethink just *what* was being achieved, in the name of development, or perhaps experience taught me that indeed as Tax cautioned (1958:18-19) it *is* a short step from believing one knows a lot about a people to believing one knows what is best for them.

THE LIMITS OF INTEGRITY

I suggest the practice of applied anthropology by definition requires that the scientist actively relate his research endeavours to some issue having policy implications, though I believe a case can be made against an extra-professional (*ie*. political) advocacy position being adopted. Are not these positions contradictory? Under certain extreme circumstances certainly they may be in contradiction, but my position is less extreme and acknowledges the need for the scientist (1) to engage, as appropriate to his professional specialization, in policy-related research, (2) to ensure that his relevant findings become widely enough known to influence an ultimately well-informed decision hopefully in the public interest, and (3) but almost as a by-product, to contribute, by such activities, to the ultimate enhanced public standing of his profession. These three points are predicated on the assumption that anthropologists do, indeed, have useful skills and should engage them for incremental human betterment.

What I now wish to argue, is that public involvement--at that dangerous intersection of science with society--requires meeting the requirements for full public accountability, for as argued earlier, some of the safeguards appear to be less in evidence or even removed when scientists enter the public arena. It might be useful to consider what institutional arrangements scientists might invoke to arrive at 'truth' when active at or near the science/trans-science interface. What happens, for example, when two experts disagree?

Natural scientists called as experts are constantly faced with conflicting evidence from colleagues when involved with urgent policy-related issues. A U.S. Presidential Advisory Group recently advanced the idea of a Science Court, which, despite its own best proselytizing efforts, has nevertheless attracted little support. It is instructive to enquire why this is so.

THE SCIENCE COURT

The court has limited objectives: to provide best available judgments about matters of scientific fact. It expects "to describe the current state of technical knowledge, which will provide defensible, credible technical bases for urgent policy decisions" (*Task Force Report* 1976:653). Furthermore it acknowledges that it holds no illusions that this procedure will arrive at the 'truth', which it sees as "elusive and (tending) to change from year to year" *(ibid)*

The Science Court is not seen as the final court, but rather, as a provider of best information which would have input into more broadly based institutions where societal values would be applied in the development of public policies. It would ensure that scientists provide the policy-maker with good science and not trans-science[5].

There have been several objections to the idea of a Science Court (*e.g.*, Lipson 1976:890; Cobb 1976:674; Boffey 1976b:129). It is interesting to note that in the 'hardest' of the 'hard sciences', namely physics,--where one might suppose the most incontrovertible scientific truths are to be found--there is concern at the unworkability, for the common interest, of the Science Court (Callen 1976:950). Criticism of the idea centres around the assumption that value-free questions of fact can be separated from political questions of policy. If the Science Court is merely to restrict itself to questions of fact--then it is germane to ask "What questions?" And the sobering answer is that the selection of questions to be asked is itself based on politfcal judgment. Moreover, the answers to the questions being asked are often unknown, and therefore can only be estimated. If the correct decision depends on sequential argument (to arrive at answers), and each unknown is assigned probabilistic weightings of less than unity (certainty), then even slightly different weightings given

each of say six steps in an argument can cause widely divergent conclusions to be drawn.[6] The problem here is that such questions of judgment are not realized by laymen, who will come to think that science is capable of measuring incontrovertible truth in the laboratory, or at least functioning without partiality, bias, or excessive dependence on subjective judgment.

HOW TO EFFECT PUBLIC ACCOUNTABILITY

Clearly then, if public decision-making is too crucial to be left to scientists' judgments what role do scientists play? I find myself agreeing with Jacob Bronowski who observed that it is the scientists' responsibility to create an informed public opinion. This requires effective communication outside of the narrow confines of professional and highly specialized groups, with greater utilization of the mass media, and the production and publication of less esoteric writings. Accountability implies full disclosure of the data having a bearing on conclusions reached and it implies public recognition that many conclusions that scientists arrive at are judgmental in nature. Accountability, therefore, is both educative and value-shaping, and yet will not necessarily or automatically produce positive results.

In a democratic society, where the public *is* the ultimate arbiter on public policy, technically correct decisions will gain little unless acceptable to a significant number of people. This is important, because in a public debate, where the issue involves 'trade-offs', *i.e.* benefits to some concomitant with costs to others, the decision can never be wholely 'correct'. The political trade-off is often between a decision with more 'correctness' and less acceptance by the public on the one hand, and one with less "correctness" but more acceptance on the other. Given the political nature of public decision-making the less 'correct' decision is likely to be the one that is made (A. Thompson 1976:12). Thus the role of the educator is especially

important, for turning the decision toward a greater degree of 'correctness' is dependent upon a full airing of the facts relevant to the issue.

As outlined in Fig. 2b, the 'grey area' of public decision-making is often that very area where anthropologists can provide data of especial relevance. The relativistic point of view, able to stand opposed to the urban middle-class mind set of the senior corporate or public servant may bring a perspective to the debate clearly needed and enriching by means of its diversifying influence.

In conclusion, I wish to illustrate, by reference to a recent research endeavour, the contribution and limits such an applied position may make to a policy-relevant discussion (but not necessarily to a resolution of the conflict surrounding the issue in question).

THE BATHURST ISLAND CASE

In 1973 Panarctic Oils Ltd., a consortium composed of several oil companies and the federal government, began seismic surveying for hydrocarbon deposits in an area of the Canadian High Arctic over which it held exploration rights.

The area in question was the primary caribou hunting territory of an adjacent Inuit community of approximately 220 people. The federal department ultimately responsible for Inuit peoples' welfare *and* the administration of oil exploration was one and the same, namely the Department of Indian Affairs and Northern Development. If exploration were taking place on unused or unoccupied lands, their decision-making in respect to these two divergent roles might be relatively uncomplicated, but in the case of Bathurst Island both the oil companies and the local residents had an intense interest in exploiting the resources of Bathurst Island, specifically caribou and hydrocarbon reserves respectively. Unfortunately the search for hydrocarbons involved moving heavy

equipment across the landscape, setting off detonations, constructing airstrips and staging areas and the coming and going of aircraft and vehicular traffic in areas that appeared to include habitat critical to the wellbeing of the caribou population.

The outcome of this conflict situation was a request by the Inuit community for a meeting with the Minister of Indian and Northern Affairs, which meeting took place in Ottawa during February 1974. Delegates from the community pointed out that they understood very well the normal behaviour of the caribou in that region and had detected significant changes in caribou behaviour since oil exploration started on Bathurst Island five years before. Caribou, it was claimed, were now leaving their preferred range on the Island, moving eastward to other parts of the island and indeed many caribou were leaving the island altogether each fall once the sea froze. This out-migration the community blamed on the increased interference resulting from continued exploration activity on the island. The Minister promised to give careful consideration to their views, and indicated that no final decision for the 1974 seismic season would be made until March when a second meeting with the community delegates would be held in Ottawa.

However, evidently there was a misunderstanding of the Minister's intention, for though a second meeting did take place, in Ottawa on March 21st, the decision to allow the seismic exploration to proceed was made several weeks prior to that second meeting. It was unfortunate that the community believed the second meeting was for the purpose of further discussing the relevant information possessed by all parties, so as to better enable an informed decision to be made, whereas the Minister evidently believed the information on hand in February was sufficient to allow a decision to be made without additional input from the hunters.

I had been present at the time of these meetings as an adviser to the Inuit group, and my assessment of the situation resulted in the following conclusions:

(1) The Inuit had a great deal of relevant information on normative caribou behaviour, both on Bathurst Island and elsewhere in the region.

(2) The Inuit alleged that the caribou had, over the past few seasons, began to behave differently.

(3) There was concern that observed changes in caribou behaviour (*i.e.* out-migration) was not in the best interest of the caribou population.

(4) The Minister asserted that no change in caribou behaviour was taking place and that the health of the herds remained good.

(5) The Inuit asserted that exploration was affecting the caribou adversely while the Minister said that exploration activity had no effect on caribou.

At this point no further discussion seemed useful as the parties held, momentarily at least, unalterably opposing viewpoints based, presumably, on evaluation of different sorts of data.

The role of the scientist involved in such a situation is initially, in my view, to critically re-evaluate the evidence on hand. The scientific evidence upon which the Minister's position was based was made available to the Inuit, and when evaluated was seen to be totally inadequate as a basis for informed decision-making. In summary it included a variety of environmental reports, mostly of a totally irrelevant nature, which the Minister's advisers either in ignorance or otherwise falsely asserted would allow an informed basis for decision-making in this particular case. Furthermore it appeared that several relevant reports had been ignored in preparing advice for the Minister. These findings were communicated on March 25, 1974 to the Inuit (Freeman 1974a).

The conclusion that the government's basis for decision making was

without scientific foundation was only half of the task for a scientific adviser; after all, the counter arguments advanced by the Inuit might be equally groundless. Whereas to not subject them to equal scrutiny might be within the traditions of 'applied anthropology' as defined by some, such an action would not accord with needed professional standards of accountability. In my view, without I hope, seeming pretentious, there is more at issue than merely proving one side wrong, for the exercise of accountability necessarily involves performing an educative, value-forming rolé, for we may ask, is not the point of scientific research to clarify a contentious issue? How does showing the falsity of one side of an argument help achieve that, if at the same time the question as to the status of the counter-argument remains open?

It was therefore necessary that I visit the community to gather data[7] in order to evaluate the evidence the Inuit wished to present in support of their counter-position.

This research certainly fell under the rubric of 'urgent anthropology', for time was of the essence if the imminent start-up of exploration activity was to be further influenced.[8] The data obtained from interviewing were analyzed immediately following return from the field early in April and a copy sent back to the community on April 9, 1974 (Freeman 1974b).

Shortly thereafter the community prepared a press release attaching this 'expert testimony' in support of their position.

At this point it is reasonable to ask whether the Inuit position was supported by the scientific investigation. The answer is both 'yes' and 'no'. The hunters were correct, as judged by the evidence, in claiming that the caribou were beginning to change their behaviour (as manifest by migration away from traditional grazing areas). However there was clearly no more than circumstantial evidence to support their assertion that the *cause* of this changed behaviour was the seismic exploration activity. Data analysis

indicated a high degree of correlation between the onset of exploration and the beginning of movement from these distutbed areas. However, cause is not proven by correlation, and clearly expert opinion as to the origins of particular phases of caribou behaviour--especially unanticipated behaviour--is, in my opinion likely in the realm of trans-science at this moment in time.

DISCUSSION & CONCLUSION

This case study is offered to illustrate my contention that the type of anthropology we do, under the label of policy-relevant or applied anthropology requires that the scientist be fully accountable. In this case full documentation was prepared and made available to all parties with an interest in the dispute. As many of the parties were 'lay persons', the results of this research were also prepared for widespread peer group (i.e., specialist) review (Freeman & Hackman 1975; Hackman & Freeman 1975; Freeman 1975) and field data of particular relevance to the conclusion that the Inuit were right and the Minister wrong (in respect to whether or not caribou were changing their behaviour) was provided to federal scientists for their critical re-analysis (Miller & Russell 1975). Assistance was also provided for media coverage of the dispute (*e.g. News of the Month*, June 26, 1974; interview April 3, 1974 CBC radio).

Was my involvement as an anthropologist value-free? Clearly it was not nor could it be; I chose to be involved because my past experience as a professional ecologist leads me to believe that the extensive empirical database possessed by Inuit hunters is a vital source of knowledge for better understanding of the enigmatic workings of northern ecological systems; some of these informed data have been communicated to specialist audiences in the past (*e.g.*, Freeman 1968a; 1968b; 1971b; 1973), not to prove an ideological point, but rather, to disseminate useful knowledge. My values, in respect to

my holding that these native observations are useful, are tentative as are all my personal values, informed by my current critical evaluation of the evidence on hand. I do not assert that all native observations of this kind are useful in similar fashion and it is my judgment, as a scientist, which decides whether and in what way the information should be made available as a contribution to knowledge.[9]

NOTES

1. I don't wish to deny that more money would likely result in more work being done. However, I do wish to emphasize that funds by themselves will not ensure improved quality of work accomplished.

2. However, others offer cautionary comments concerning the need of anthropologists remaining at a distance from the sponsors of the research (*e.g.*, Jones 1976; Schensul 1974) and this presumably holds true irrespective of who sponsors the research.

3. The position of any particular discipline along a graded continuum from the most predictive 'hard science' to the least predictive 'soft science' depends primarily on the nature of the phenomena the discipline studies. When the discipline deals with assemblages of data that behave according to discoverable invariable norms, then we are dealing with a 'hard science' whereas the extent to which data are increasingly unique and appear statistically scattered and stochastic in behaviour reflects the 'softness' of the science. Indeed, in many of the human and social sciences, the intrinsically refractory statistical nature of the phenomena is more a cause of the dubious claim to scientism, irrespective of the extent to which methodological rigor may be lacking in the research or subsequent analysis of data. The result is that propositions advanced by practitioners of such 'soft science' disciplines may be either real or fictive -- but due to scientific opacity there may be too little evidence for judging which (Stent 1976:35). However, as Stent (*op.cit*) points out this is not to say such activity is without value, for there are obviously many different means of obtaining insights into complex behaviour, and the intuitive approach may be no less useful than the rigorously deductive in the search for understanding of human behaviour.

4. Clearly not all disagreement among scientists results from wilfull or blatant dishonesty. Robbins and Johnston (1976:351*ff.*) cogently argue that scientists tend to disagree among themselves, in respect to 'expert advice' offered to outsiders because of occupational differentiation. They point out (*ibid.*:354) that academic, government, and industrial scientists operate in quite different institutional settings, each of which imposes a different set of 'professional' expectations upon the scientist. It is this differentiation of the scientific community (along cognitive and occupational lines) which, they claim, creates the basis of conflict among scientists in dealing with issues having import in the real world.

5. It would presumably guard against the situation reported by Wolins, which is worth repeating here because of the particular secretiveness and proprietary view anthropologists hold in respect to their data. Wolins wrote to 37 scientists asking for the raw data upon which recent articles were based. Of the 32 who responded, 21 (66%) claimed the data were "lost", "inadvertantly destroyed" or "misplaced". He did obtain seven sets of data in all, but found that three contained errors sufficient to invalidate the authors' conclusions (Wolins 1962:657).

6. Callen (1976) gives a simple linear illustration where at each step of a six step argument the spread of opinion (expressed as a probability of certitude) varies from 75% to 95%. The outcome if this slight divergence of opinion were maintained through each step of the argument could be either a conclusion with a two-to-three chance of being 'correct' or a conclusion with a ten-to-one chance against being 'correct'.

7. At the time of my visit a three-man Canadian Wildlife Service team was also visiting the area to examine the caribou so that I did not attempt to gather any biological data showing, for example, that caribou leaving the traditional grazing lands were living under nutritionally more stressful conditions than those who remained in preferred areas.

8. In view of the urgency of the issues involved a few days only could be allowed for data gathering, and for those who question the worth of a one-week field trip, I should say it was in a community where I knew many, and was known to the majority of, hunters after a series of visits there over a nine year period. With two local people acting as field assistants/interpreters, nearly all hunters were interviewed to provide detailed information on each of their caribou hunts over the past three years. Hunters were also asked other questions relating to hunting, including their personal evaluation (and the basis for these views) of the impact of seismic and other activities on wildlife.

9. I have much in my field notebooks that may one day be written up as a contribution to ethnozoology, which category I hasten to add, is no less worthy of communication than is 'scientific' ecology, but which does represent different data than those whose objective reality I can evaluate and vouch for in terms of my own critical experience. Being less sure of such data require that they be further validated before being disseminated as 'anthropological fact'. An example here would be an on-going ethnozoological study of the polar bear, whose generic categorization is *nanuq*. Thus whereas I now understand what sort of polar bear is being referred to by such categorical terms as *atirttalaaq, atirttaviniq, arnarlu, atirttaluk, atirttalaalik, nukau, nukaugajjuak, angujjuak,* and *ningiqaaluk* (terms used by hunters in eastern Baffin Island), I am not yet sure of the definitive status of those 'polar bears' that are *not* generically *nanuq*, but are variously called *tullaajuituq, nanuttaugaijuttuarluk,* and *tiriganak*.

REFERENCES CITED

ANDERSON, E.N., Jr.
1969 The life and culture of Ecotopia. *In* Dell Hymes (ed.) *Reinventing Anthropology*. New York: Pantheon.

ANDERSON, James N.
1974 Ecological anthropology and anthropological ecology. *In* John J. Honigmann (ed.) *Handbook of Social and Cultural Anthropology*. Chicago: Rand McNally.

BECKER, Howard
1966 Whose side are we on ? *Social Problems* 14(3):239-247.

BENNETT, John W.
1976 *The Ecological Transition: cultural anthropology and human adaptation*. Toronto & New York: Pergamon.

BOFFEY, Philip M.
1976a Sulfur pollution: charges that EPA distorted the data are examined. *Science* 192:352-354.

1976b Experiment planned to test feasibility of a "Science Court". *Science* 193:129

CALLEN, Earl
1976 The Science Court. *Science* 193:950-951.

CASTILE, George P.
1975 An unethical ethic: self-determination and the anthropological conscience. *Human Organization* 34(1):35-40

COBB, John C.
1976 Public involvement in scientific decision-making. *Science* 194:674

FOSTER, G.M.
1969 *Applied Anthropology*. Boston: Little, Brown.

FREEMAN, Milton M.R.
1968a Ethnozoological interpretation of the brow-tine in arctic caribou. *Arctic Circular* 18:45-46

1968b Winter observations on beluga *(Delphinapterus leucas)* in Jones Sound, N.W.T. *Canadian Field-Naturalist* 82(4):276-286.

1971a The Credibility Gap: anthropologists and social responsibility in Canada. Paper read at the 1971 Annual Meeting, Canadian Sociology and Anthropology Association, St. John's, Newfoundland. (Shortened version subsequently published in *Human Organization* 33(4):391-393).

1971b Population characteristics of muskox in the Jones Sound region of the Northwest Territories. *Journal of Wildlife Management* 35(1):105-110

FREEMAN, Milton M.R. (Cont'd)
1974 Polar bear predation on beluga in the Canadian arctic. *Arctic* 26(2):162-163

1974a Analysis of DIAND evaluation of Bathurst Island environmental and land use data and preliminary report on the ecological significance of Bathurst Lsland. Multilith Report, 9 pages, Hamilton.

1974b Environmental Report, Bathurst Island N.W.T. 1974: Part 1, Caribou. Multilith Report, 26 pages, Hamilton.

1975 Assessing movement in an arctic caribou population. *Journal of Environmental Management* 3(3):251-257

FREEMAN, Milton M.R. and L.M. HACKMAN
1975 Bathurst Island N.W.T.: a test case of Canada's northern policy. *Canadian Public Policy* 1(3):402-414

FUJINO, Tadashi
1976 Mercury pollution in Tokuyama. *In, Science for Better Environment,* (Proceedings of the International Congress on the Human Environment, Kyoto 1975). Tokyo: Science Council of Japan.

GILLETTE, Robert
1971 Lead in the air: industry weight on the Academy panel challenged. *Science* 174:800-802

GOULDNER, Alvin
1968 The sociologist as partisan: sociology and the welfare state. *American Sociologist* 3(2):103-116

HACKMAN, Linda M. and Milton M.R. FREEMAN
1975 A land use conflict on Bathurst Island N.W.T. Paper read at the 34th Annual Meeting of the Society for Applied Anthropology, Amsterdam, The Netherlands.

HARDY, Sir Alister
1976 *The Biology of God.* London: Cape.

HARRIS, Marvin
1976 *Culture, Man, and Nature.* New York: Crowell.

JACOBS, Sue-Ellen
1974 Action and advocacy anthropology. *Human Organization* 33(2):209-215

JONES, D.L.
1971 Social responsibility and the belief in basic research: an example from Thailand. *Current Anthropology* 12(3):347-349

JONES, Delmos J.
1976 Applied Anthropology and the application of anthropological knowledge. *Human Organization* 35(3):221-229.

KAPLAN, David
1974 The anthropology of authenticity: everyman his own anthropologist. *American Anthropologist* 76(4):824-839

LACK, David L.
1957 *Evolutionary theory and Christian belief, the unresolved conflict.* London: Methuen.

LIPSON, Leon
1976 Technical issues and the adversary process. *Science* 194:890

MATTHEWS, Ralph
1975 Ethical issues in policy research: the investigation of community resettlement in Newfoundland. *Canadian Public Policy* 1(2):204-216

MILLER, Frank L. and Richard H. RUSSELL
1975 Aerial surveys of Peary caribou and muskoxen on Bathurst Island Northwest Territories. *Canadian Wildlife Service Progress Notes* 44.

MONTGOMERY, Edward, John W. BENNETT and Thayer SCUDDER
1973 The impact of human activities on the physical and social environments: new directions in anthropological ecology. *Annual Review of Anthropology* 2:27-62

NAYLOR, L.L.
1973 Applied anthropology: approaches to the using of anthropology. *Human Organization* 32(4):363-370

PETERSON, John H., Jr.
1974 The anthropologist as advocate. *Human organization* 33(3):311-318

ROBBINS, David and Ron JOHNSTON
1976 The role of cognitive and occupational differentiation in scientific controversies. *Social Studies in Science* 6(4):349-368

SALISBURY, Richard F.
1973 The anthropologist as societal ombudsman. Paper read at the IXth International Congress of Anthropological and Ethnological Sciences, Chicago.

SCHENSUL, Stephen L.
1974 Skill needed in applied anthropology: lessons from El Centro de la Causa. *Human Organization* 33(2):203-209

STENT, Gunther S.
1976 The poverty of scientism and the promise of structuralist ethics. *Hastings Center Report* 6(6):32-40

ST. JAMES-ROBERTS, Ian
1976 Cheating in science. *New Scientist* 72(1028):466-469

TASK FORCE REPORT
1976 The Science Court Experiment: an interim report. *Science* 193:653-56

TAX, Sol
1958 The Fox Project. *Human Organization* 17(1):17-19

THOMPSON, Andrew R.
1976 In defence of accountability. *In, Seminars on Northern Development: invited papers and group reports -- Ottawa and Calgary.* Science Council of Canada.

THOMPSON, Laura
1976 An appropriate role for post-colonial applied anthropologists. *Human Organization* 35(1):1-7

VAYDA, Andrew P. and Bonnie J. McCAY
1975 New directions in ecology and ecological anthropology. *Annual Review of Anthropology* 4:293-306

WEINBERG, Alvin M.
1976 Science in the public forum: keeping it honest. *Science* 191:341

WOLINS, Leroy
1962 Responsibility for raw data. *American Psychologist* 17(9):657-658.