COMMENTARY

REGAINING THE GOLDEN STOOL:
SOCIAL ANTHROPOLOGY AND APPLIED RESEARCH

Introduction

In this paper I wish to consider whether there is a distinctive role for
the social anthropologist in applied research. This is a question which
has preoccupied me since 1978, when I was offered funding by the EOC/SSRC
Joint Panel on Equal Opportunities to carry out a study of women and
training in Plymouth companies.
 The nature of social anthropology as an academic discipline, and its
relationship with non-academic clients, cannot adequately be understood
without reference to historical developments: changes in the subject-
matter available to social anthropologists, in the relationship of their
discipline to other subjects, and in the clients and general audience for
their work. I shall therefore begin with a brief (and necessarily over-
simplified) account of these events as they affected my own consciousness,
and (I would surmise) the consciousness of many social anthropologists
trained in the post-colonial sunset of the classical fieldwork tradition,
who attempt to apply that education in non-traditional fields.
 The main part of this paper will deal with the fieldwork in Plymouth:
not as any kind of model for social anthropologists in applied research,
but to provide examples for illustration of the general theme. I shall
raise some of the problems (and advantages) of working to a policy-oriented
brief; the relationship between social anthropologist, client, and
informants; the effects on method of limitations both of funding and
access; and the question of the presentation and dissemination of findings.
I think it is of more than merely personal interest that I began fieldwork
(and presented myself initially) as a social researcher with a qualification
in social anthropology, not unequivocally as a social anthropologist; the
research proposal with which I applied for funding could have been pre-
sented by an industrial sociologist. However, in the course of the research,
I rediscovered a belief in social anthropology as a distinct (though not
hermetically separate) discipline, with a specific approach to offer
clients. This particular experience leads one back to further questions:
how social anthropology can be defined and presented to non-anthropolo-
gists, and how applied work may feed, and be fed by, the development of
theory and method. Within the paper as a whole, when I speak of social

anthropology, I am referring to the British tradition; this is especially true in the brief account of historical events. This is a deliberate piece of insularity, purely for the sake of keeping the argument within reasonable bounds: it does not preclude the necessity of looking to Europe, the USA, and further afield, for fresh interpretations of anthropological approaches.

Applied anthropology and the Golden Stool

The idea of applied anthropology is not new; it is as old as the subject itself. The reading of any general introductory text on the subject (for example, Godfrey Lienhardt's *Social Anthropology*) makes this clear. One might go so far as to suggest that social anthropology is *by definition* an applied subject, whether designedly so at the time of the research, or by the interpretation of ethnographic data after the event. As Lienhardt comments, 'anthropological advances have often followed on the interests of governments in their own practical and moral problems'. [1] The case he presents as illustration is that of the Golden Stool of the Ashanti. This is a familiar tale: how, in 1900, the Governor of the Gold Coast asked the Ashanti why he was not invited to sit on the Golden Stool - their sacred symbol of spiritual nationhood - to confirm his rule as Queen Victoria's representative. The Ashanti wars ensued; but conflict was curtailed by the secondment of Captain Rattray to make an anthropological study of the Ashanti and their Golden Stool. As Lienhardt puts it, 'There followed a series of books by Rattray on many aspects of Ashanti culture, and a greater understanding between the Government and the people.'

As an anthropologist's myth, this story could hardly be improved upon. It combines, in a satisfying way, the production of rigorous ethnographic research with a practical application of the most striking kind. Indeed, the high academic quality of the research is crucial to its practical effectiveness: Rattray, the ethnographer,[2] could offer advice to the colonial administration which was of more direct value than the blundering approaches of the political or military 'common man'. The Government (miraculously) listened, and peace was made with the Ashanti. The Golden Stool may fairly be said to be a sacred symbol for anthropologists, as well as for the Ashanti. It represents the intellectual self-confidence and human responsibility of a generation of social anthropologists (or ethnologists, as they initially styled themselves) for whom the maintenance of academic quality, and the consideration of social and administrative problems, were not incompatible.

The development of academic social anthropology

The development of academic social anthropology, as a discipline based on University departments, cannot readily be separated from the applied work of Government anthropologists. Fieldworkers were readily interchangeable from one type of post to the other. The ethnographer working from a research institute or University department could expect to have note

[1] Godfrey Lienhardt, *Social Anthropology*, London: Oxford University Press 1964, p. 3.

[2] It might be argued that, strictly speaking, Rattray was not a trained ethnographer. However, he certainly became one (in any reasonable meaning of the term) through the extent of his fieldwork experience.

taken of her work by overseas administrators: the Government anthropol-
ogist could hope for a relatively free hand (within the context of a
regionally limited and possibly problem-solving brief) to produce good
quality academic work with general applicability. This, at any rate, is
how social anthropology from about 1900 to 1955 appeared to a student
in the 1960s.

However, as a result of the post-war expansion of British univer-
sities - hence, also, the expansion of university departments of social
anthropology - there was a development of fieldwork not directly connected
with policy-oriented research. The emphasis on carrying out at least one
extended piece of fieldwork in another culture, as a definition of a
professional social anthropologist, created a body of published work
which might be used for policy applications, but which was not, as a
rule, generated by policy questions. Especially in doctoral work, the
student could, within reason, change topic or achieve a *post hoc* defini-
tion of a subject by what was found particularly interesting in the
field. This was justified intellectually by the holistic approach to
societies, and by the primacy (if one could apprehend it) of the world
view of one's informants.

Though the gap between academic and applied anthropology therefore
widened, British social anthropology in the 1960s appears, in retrospect,
to have a certain unity. The vital importance of fieldwork (usually with
a lone observer, or, at most, a small team) was accepted (is accepted?)
by social anthropologists of all kinds. The subject-matter of social
anthropology was still clearly defined: small-scale societies[3] and 'other
cultures', in Beattie's phrase,[4] usually of the kind called 'exotic'.
The continued presence of known clients for commissioned work contrib-
uted to a secure sense of an audience for academic work of a more general
kind.

Return and renewal

The period which followed - from the late 1960s - was a time of change
and reassessment; change, first of all, in the subject-matter available
to anthropology. Post-colonial political developments in the Third World
led to difficulties in access to many of the traditional fieldwork areas.
The use by European anthropologists of Third World informants was called
into question; by new governments, and by radical social anthropologists.[5]

It may now be argued that 'colonialist anthropology' is another
historical myth: that the critique discounted the intellectual indepen-
dence of anthropologists, and the importance of the large body of work
carried on outside the framework of the colonial administration. Never-
theless, after such open debates, fieldwork in the Third World could
hardly continue in quite the same way without being an anachronism.

One possible direction for social anthropology, it seemed, was as-
similation to a general body of sociological work. Combined departments

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[3] Predominantly, but not exclusively: one may note, for example, the
work on Mass Observation as an important exception to the rule.

[4] J.H.M. Beattie, *Other Cultures: Aims, Methods and Achievements in
Social Anthropology*, London: Cohen and West 1964.

[5] See, for example, Talal Asad, ed., *Anthropology and the Colonial
Encounter*, London: Ithaca Press 1973.

of sociology and social anthropology - teaching joint courses and carrying out joint research in Britain and overseas - demonstrated the fruitful convergence of interests and methods. For a student of social anthropology like myself, returning to work in an industrial society on social policy issues, it appeared that education as a social anthropologist could be regarded as a source of theoretical capital and useful techniques for research, rather than as a passage to distinctive professional status. Through fieldwork experience in Plymouth, however, I came to believe that this view was wrong, and that fieldwork in itself still defines the meaning of social anthropology as a discipline. I shall now turn to an account of this work, and some of the conclusions I drew from it.

Defining the brief: organising the project

The research was carried out from January 1979 to August 1980, with funding from the EOC/SSRC Joint Panel on Equal Opportunities Research. The subject was 'The Effects of Company Training Policies on Women's Employment Opportunities in Plymouth'. Defining the brief is the first task; this, in turn, means identifying the client. The EOC/SSRC Joint Panel was set up to fund and administer a national programme of policy-related research. SSRC administrative procedures and academic requirements applied to the selection and control of funded projects; policy interests apparently stemmed from the EOC. Therefore, despite the creation of a Joint Panel, there was not a strictly unitary client: and it seems likely that, in funded research in general, the anthropologist will be working for diffuse organisations or temporary alliances of interests rather than for one individual or cohesive group. It is therefore helpful to have some statement of the client's aims, and (at a fairly early stage) clear agreement about obligations to report to the client (how, when, and at what length), and rights to publish findings.

The Joint Panel provided a general brief, to aid those applying for funds.[6] The keyword (to use Dr. Kreager's apt term)[7] was 'underachievement'. This apparently begs all kinds of questions: on whose terms are women 'underachieving'? Even within the male-controlled areas specified by the brief - employment, education, and training - are women 'underachieving', or are they (to paraphrase André Gunder Frank) 'being underachieved'?[8] The notion of 'underachieving woman' has echoes of the myth of the 'resistant peasant', which was a recurring theme in development studies of Third World countries, and which has since been justly demolished by the work of Shanin, Hutton and Cohen, and others.[9] However, it is not helpful to quarrel

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[6] Equal Opportunities Commission/ Social Science Research Council Joint Panel on Equal Opportunities Research, *Programme of Research*, 1978.

[7] Dr. Kreager referred to 'keywords' in his paper given at the ASA Panel Meeting on Applied Anthropology, held at Queen Elizabeth House, Oxford, on November 17, 1981.

[8] A.G. Frank, 'The Sociology of Development and the Underdevelopment of Sociology', in *Latin America: Underdevelopment or Revolution?*, Monthly Review Press 1969.

[9] See T. Shanin, ed., *Peasants and Peasant Societies: Selected Writings*, Harmondsworth: Penguin 1971; C. Hutton and R. Cohen, 'African Peasants and Resistance to Change', in Oxaal, Barnett and Booth, eds., *Beyond the Sociology of Development*, London: Routledge and Kegan Paul 1975.

with the apparent premises of the funding body before research has even
started, unless the fieldwork conditions are strictly controlled by them
(which was not the case with this programme). The Joint Panel allowed a
wide choice of specific subject; there were general guidelines, within
the areas of employment, education and training, and different types of
approach were invited. Despite this freedom, the brief was policy-oriented;
it was related to the framework of the existing law (the Equal Pay Act and
the Sex Discrimination Act) and the areas of women's lives which are af-
fected by them. This clearly precluded certain kinds of approach to any-
one who read the brief carefully, with attention to internal logic rather
than verbal detail.

In my research proposal – defining a selected area of the brief – I
concentrated on training, as I had worked in that area before. Access was
taken to be the key factor; the employing company, as the appropriate level
for fieldwork. There was very little empirical material on women on the
shopfloor,[10] so there was scope for an ethnographic approach. Later, the
actual field research shifted away from the original proposal in some
respects, but the basic elements remained constant: company case studies
and the extensive interviewing of women workers.

In submitting a proposal, I found the existence of a general, policy-
oriented brief helpful and stimulating, despite reservations about its
underlying premises. In the field, it led to some problems; it entailed
directing attention away from issues which interested me as an anthropol-
ogist, but which might seem byways to policy-makers. I kept wishing to be
more general in approach, within a very limited fieldwork period, than
either the brief, or my own research proposal, warranted. As I remarked
earlier, the research proposal was presented as social research rather
than as social anthropology; I would have been better able to pursue the
theoretical issues which interested me, had I presented a research pro-
posal unequivocally anthropological in its approach, yet acceptable (in
practical terms) to a policy-oriented funding body.

The research proposal was accepted for funding, in principle, but the
SSRC had to be satisfied that access could be gained to respondent com-
panies before funds would be released. This led to a reversal of the
planned research method, as the case study companies had to be selected
and approached before general enquiries about the local labour market
could be undertaken. The anomalies of my status – applying for funding as
a private individual – also caused delays. It eventually emerged that I
could not receive direct funding, so Plymouth Polytechnic provided a
temporary institutional base. The first three months of the fieldwork,
therefore, were carried out without funds, and were funded retrospectively.

To make contacts with informants – often the most delicate and time-
consuming aspect of fieldwork – I used a suitably anthropological network,
stemming initially from local personnel managers to whom I had taught
sociology on the IPM course at Plymouth Polytechnic. This provided con-
tacts mainly with electrical engineering companies. I decided to limit the
research to engineering: it provided a bounded field, with useful statis-
tical information on the employment of women nationally and locally; a
clear structure of employment, education, and training; and an excellent

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10 See R. Brown's article in Barker and Allen, eds., *Dependence and
Exploitation in Work and Marriage*, London: Longman 1976; L. Mackie and
P. Pattullo, *Women at Work*, London: Tavistock 1977.

and helpful Industrial Training Board, which had already produced a considerable volume of reference material through its own Research Division.

Relationship with informants and client; layers of concern

The relationship with respondent companies, within the fieldwork period, was mostly happy, though I doubt whether their managers would endorse the more radical conclusions of the research. The companies were consciously chosen as 'best practice' companies, since I wished to tap the underlying structures of discrimination against women at work, rather than cataloguing the more obvious horrors. Access to companies of any other kind would, in any case, have been problematical.

The main issue, in dealing with companies in research of this kind, is confidentiality. The companies which gave me access were, often justly, proud of their record in training women workers; but, on occasion, they (or individual managers employed by them) could be in breach of the law. The Sex Discrimination Act is notoriously complicated and difficult to enforce, and the concept of indirect discrimination is particularly hard to interpret. For the protection of informants, a 'confidentiality clause' was written into the contract between Plymouth Polytechnic (as my direct employer) and the funding body; this meant that there could be no legal compulsion on me to reveal the identity of respondent companies to the EOC. In the field, I discovered that companies (and individuals) are more worried about confidentiality than about any other issue, especially where company documents are used or machine-readable data are collected. This requires not only reassurances, but constant vigilance on the part of the anthropologist, to protect the confidences of informants not only from the world at large, but from each other.

In any applied anthropological research there are layers of obligation and concern. There are formal obligations to the funding body or client (even if these are not always as clear-cut as one would wish). There are obligations to informants; these, too, may have conflicting interests. In my own case, I felt under a certain obligation to the respondent companies and their managers (some of whose views diverged from offical company policy). Above all, I had a clear obligation to the women workers: the women who are the consumers of company policy and national legislation. I attempted to ascertain and reproduce their world-view, wherever possible, and to avoid blaming the victim in my own generalizations. However, it is with the most vulnerable informants that the problem of reciprocity becomes most acute: how can one adequately repay them for their time, and their help, when no immediate benefits can be promised as a result of the work? For the companies, this was not such a problem: adequate correspondence, and the provision of copies of the final report, meant that they were informed of the results of the research. However, one could not ensure that this information reached the shopfloor (though I have since had the opportunity to discuss the research findings with Trade Union officials in one company). As Shirley Ardener remarked when this paper was originally presented, anthropologists could do much more to involve informants in discussion of their findings; in modern applied anthropology this may become essential.

Method

The main research tool was the interview. Long, unstructured interviews were held with fifty-five managers across five companies. One hundred and fifty-one women workers were interviewed with a semi-structured question-

naire, which had been tested in a pilot run with female employees of
Plymouth Polytechnic. There were also numerous brief, informal discussions
in offices, training departments, and on the shopfloor of the respondent
companies, which were recorded in field notebooks; but the formal inter-
views form the main data base.

The use of a 'male-type positivist grid' (Edwin Ardener's phrase) to
create a picture of female informants is clearly open to objections. How-
ever, in this project, it was an essential condition of gaining access to
the shopfloor. Managers (and Trade Union representatives in some companies)
wished to examine the questions that would be asked before interviewing
was agreed. There was only one piece of censorship as a result of this:
in one company a question on Trade Union membership was omitted. Manage-
ments were primarily concerned with the question of time; that each inter-
view should not overrun the agreed half-hour, so that production should
not be disrupted.

Providing information to informants was an important part of the
interview method. It was essential to avoid any implied compulsion to
attend for interview, or to give the impression that the research was a
'management survey'. Interviewees were (within reason) freely selected,
and were sent a handout in advance explaining the nature and purpose of
the research, and asking for their help. Some time was spent in interviews
answering their questions and giving more assurances about confidentiality.

The use of a standardized interview was more helpful than I had anti-
cipated. It produced a body of quantifiable data within a limited field-
work period, and still serves as a point of reference for more qualitative
assessments. There were open-ended questions as well as codable ones, and
detailed responses were tape-recorded. There were questions at different
levels of generality; it is interesting that a bland, general question,
such as 'Should an applicant for a job be chosen on merit alone?' tends
to evoke a bland, general response, whereas a more specific query, such
as 'Do you think of your own job as being a "woman's job"? And if so, why?'
elicits answers of a more illuminating kind. Thus, the interviews tapped
different levels of meaning. As a research worker, I gained valuable new
skills, learning to code interview responses for machine-readability, and
to run SPSS on the computer. [11] The method remains the means, and not the
end, when the critical checks of anthropological analysis are still ap-
plied to quantitative data.

Some aspects of the method used in this research were distinctively
anthropological. I would suggest that anthropological method is primarily
concerned with two areas: language, and structural context. Participant
observation - the classic method of approaching these areas within an-
other culture - could not be used in this project, though it surfaced
briefly in shared canteen meals and conversations. Non-participant obser-
vation, on the shop-floor and in company training schools, had to suffice.
Structural information was provided by company documents, and through
interviews with managers as well as workers. These interviews (and the
circumstances in which they were granted) yielded more than the direct
information given in replies. They could be analysed reflexively, and in
combination with each other, showing where and how information was blocked
and released, and which recurring language patterns suggested the trans-
mission of myths and ideologies.

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11 For help in acquiring these skills, I should like to thank Geoff
Payne, Marion Ulas, and the staff of the Computer Centre at Plymouth
Polytechnic.

Nor was the initial step of the classic fieldwork method - becoming a competent user of the informants' language - entirely absent. One cannot assume a shared language (in the fullest sense) with informants in one's own country. As a fieldworker in Plymouth factories, I found it helpful that I had already lived in the area for three years. From casual conversations in the school playground, the baby clinic, in shops, and on buses, I was already well aware of the themes of gender segregation.

Presentation and dissemination of findings

There are two main considerations: fulfilling one's obligations to the client, and making a theoretical contribution as a practising anthropologist. In this project, I hope I accomplished the first (submitting a final report to the Joint Panel in October 1980), and I am beginning to attempt the second. I have experienced some difficulty in disentangling the two; I was uncertain about the Joint Panel's requirements, in length and in nature, for a final report. The report I submitted was factually detailed, generally ethnographic, and far too long for the purpose; I am currently isolating, for the EOC, the policy aspects which are of interest to them.

My advice to an applied anthropologist would be to write, initially, a short report with a strong, clear argument, geared specifically to the policy interests of the client; and to include sufficient hard data only to support the main points and indicate that the work has been done. The general writing-up must be a more long-term operation. It is also important to be positive: 'Band-Aids' in policy, to effect modest improvements, are always possible and are better than nothing. Such suggestions should be presented first in any report. General conclusions (which may well tend to greater pessimism, or call for total social reconstruction) may then follow.

It is essential to maintain quality, even in short reports, without giving non-academic or busy clients material which they do not want, but without underestimating their capacity to appreciate an intellectual argument. There may be a conflict of interests between client and informants, and hence pressure (subtle and not so subtle) on the researcher to toe the line in the presentation of findings. Happily, I did not experience the dilemma in which the anthropologist has to distinguish between compromise and betrayal. However, scrupulous attention to contractual commitments (deadlines for work, and specific requests from clients) should give one more room for manoeuvre and for general honesty in written submissions: it also means that any unpalatable recommendations will carry more weight.

I cannot yet speak with confidence of the second stage of presenting findings to other anthropologists, and (one hopes) to a wider audience. I am still quarrying for meaning in tapes and field notebooks, and exploring anthropological themes in writing-up - boundaries between 'male' and 'female' work, and training as ritual - which were not explicit in the text of the final report. However, it is clear that the quality of this work must be determined by the quality of the fieldwork on which it is based. Whatever the nature of short-term reporting obligations, close attention to language and to non-verbal symbolic systems are still an essential part of anthropological fieldwork, within a holistic, structural approach. The training through 'other cultures' still holds good; any culture is 'other' to the careful observer, and, through close attention, the commonplace becomes exotic.

Regaining the Golden Stool

It is no longer possible to return to the era of the Golden Stool, the confident heyday of applied anthropology, even if we wished to do so.

However, the image still has considerable force: not only that the anthropologist was seen to be the appropriate adviser, but that the government actually listened to him. It was suggested at the ASA Panel Meeting on Applied Anthropology (held at Queen Elizabeth House, Oxford, on 17 November 1981) that the problems of applied anthropologists were specific, practical ones, and that they should not be reified to the point that an all-purpose theoretical answer should be sought for them. I would agree that this is true; but theoretical issues are nevertheless involved in the application of the subject, notably the extent to which social anthropology can (or should) be regarded as a distinct discipline in applied research.

Finally, as the feminist aspect of my own work is self-evident, I have made no comment upon it, assuming that the social anthropology of women can be taken as being as general in its application as that of men. However, I should like to suggest that the theoretical regeneration of social anthropology in the past ten years has come about largely through the impetus of the women's movement outside academic circles and within them, in women's anthropology groups. Feminists in the early 1970s suggested that there were lessons to be learned from social anthropology; some of these suggestions were in themselves naive, harking back to Morgan, Engels, and mythical primitive matriarchies. However, the idea was sown that social anthropology could help to provide a reconstructed image of women in society. In this context, theory and practice seem indivisible: perhaps such an image is our Golden Stool.

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