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2 The limits of auto-anthropology

In her account of fieldwork with Travellers in the home counties, Okely records how she 'had to learn another language in the words of my mother tongue' (1984: 5). But this distance did not do away with the Travellers' location in her own social universe. 'Any latent tendency to treat people as objects or distant curios has to be confronted, not left repressed' (1984: 6). Shamsul appeals to Malay anthropologists not to assume that because they are Malay they can bypass the long periods of familiarization in the field that non-Malays have to undergo in studying rural Malay society; he emphasizes the inevitable social distance between scholar and villager. Unless they are prepared to approach their own society in a spirit of honest difference, scholars simply become 'academic mercenaries' (1982: 29). Obviously neither writer means to imply that situations might exist in other places where one did not have to guard against such tendencies. Yet there is more to these two comments than simply the point that moral problems take a particular form when anthropologists turn to 'their own society'. They raise the preliminary question of how one *knows* when one is at home.

For if in adjusting their double vision, as Okely calls it, there is in the end more in common between her and Shamsul than between either of them and their field areas, in what sense can they be said to be working at home? The grounds of familiarity and distance are shifting ones. Home can recede infinitely: would a Traveller studying the Travellers be at home? Or would it have to be a Traveller from this region as opposed to that region? The answer I propose is highly specific, and does not preclude other ways in which one might be 'at home'. But it does point to an aspect of anthropological practice that cannot be ignored. I consider one way, then, of rescuing the concept of home from impossible measurements of degrees of familiarity. The continuum obscures a conceptual break. What one must also know is whether or not investigator/investigated are equally at home, as it were, with the kinds of premises about social life which inform anthropological enquiry. One

suspects that while Travellers and Malay villagers are not so at home, in their talk about 'community', 'socialization', or 'class', for example, Elmdoners are.¹ Auto-anthropology, that is anthropology carried out in the social context which produced it, in fact has a limited distribution. The personal credentials of the anthropologist do not tell us whether he/she is at home in this sense. But what he/she in the end writes, does: whether there is cultural continuity between the products of his/her labours and what people in the society being studied produce by way of accounts of themselves.

Starting with two assumptions

Two sets of commonly made assumptions are:²

1. That, as ethnographers, anthropologists on familiar terrain will achieve a greater understanding than elsewhere, because they do not have to surmount linguistic and cultural barriers. Greater understanding may appear as immeasurably enriching, or as immeasurably trivializing, but in either case the amount of information to be gained by an insider augments what people know about themselves, or what can be learned about the total society in aggregate.
2. That the systematizing anthropological enterprise will be exposed for the contrivance it is everywhere. It makes the commonplace complex, its systematizations not revealing anything more than everyone knew anyway and amounting to a set of unnecessary mystifications.

Contradictory as these two assumptions are, both stem from what is regarded as a general implication of anthropology at home, greater reflexivity. The assumption is that we become more aware, both of ourselves when turned into objects of study, in thus learning about our own society, and at the same time, of ourselves as doing the study, in becoming sensitive to methods and tools of analysis. The prospect of anthropology at home thus suggests a contribution to the increasing reflexivity which is urged on the subject from numerous directions. Marcus and Cushman, for instance, conclude their analysis of ethnographies as texts with an admonishment to ethnographers to develop a critical sense for the form as well as the content of ethnographic discourse (1982: 65–6); Fabian's (1983) book on the construction of the Other in traditional anthropology explores the premise that the construct is constituted by our knowledge of ourselves (cf. Burridge 1979: 12). The goal is enhanced critical awareness. In the same way as anthropologists attune themselves as registers of alien cultures, they are invited to register the grounds of their own practice (Scholte 1974). Not surprisingly, as Marcus and Cushman indicate, it is difficult to do both at once.

There is a tendency to equate reflexivity with heightened self-

consciousness, and thus to regard it like a personal virtue, which this or that sensitive person displays in their writings. It might seem that anthropologists are destined only to increase an ever more exquisite self-consciousness. However, a conceptual reflexivity exists outside the sensitivities of individual practitioners, in the extent to which the anthropological account *qua* anthropological account does or does not render people's conception of themselves back to themselves – a point which applies equally to ethnography and to anthropological analysis. Where it does, in either case one can speak of auto-anthropology. Yet I do not mean rendering back information in the form in which it was given: rather, where the anthropological processing of 'knowledge' draws on concepts which also belong to the society and culture under study.

On the face of it, it looks absurd to make such a claim for (say) an account of an Essex village. The Elmdon project might have begun in a milieu in which it could be assumed that the villagers broadly participated in the world view also held by the anthropologist. Yet what started as continuity ended as disjunction. The ethnographic text was hardly continuous with indigenous narrative form; one was not rendering back to the residents of the village an account immediately contiguous with those they had given, as social history or as biography might be regarded. It is clear that simply being a 'member' of the overarching culture or society in question does not mean that the anthropologist will adopt appropriate local cultural genres. On the contrary he/she may well produce something quite unrecognizable. Commonsense descriptions are set aside. Indigenous reflection is incorporated as part of the data to be explained, and cannot itself be taken as the framing of it, so that there is always a discontinuity between indigenous understandings and the analytical concepts which frame the ethnography itself. These derive from a specific theoretical focus which may make intelligible the anthropologist's behaviour (as an 'academic') but not necessarily what he/she writes. Attempts to make such accounts more accessible rest either on educating the audience anthropologically, or on abandoning the traditional ethnographic genre in favour of a popular one – a history or report. The manner in which anthropologists set aside indigenous framings would seem to make their activities in Essex, then, not so dissimilar from their activities (say) in Melanesia.

This is one source of much recent self-scrutiny of form, that is, the form of anthropological representation itself; hence the experimentation with ethnographic texts, of which Clifford (1983) gives a compelling analysis. At issue is the manner in which ethnographic authority is constructed in reference to the voices of those supplying information, and the part they are given in the resultant texts. Favret-Saada refers to as fantastical the construction of anthropological accounts in which the speaker is denied subjectivity (the informant can never occupy the position of 'I') and the authorial subject (the anthropologist) has no name (1980: 28). The going assumption seems to be that by imaginative effort in the act of representation the ethnographer can

play with subject-object relations so as to bring back into his/her texts the distinctive voices of his/her interlocutors. The new genre may display itself as dialogical or polyphonic (Clifford's terms for a construction preserving dialogue and producing discourse rather than text), and taken as standing for shared authorship. I would regard such 'joint products' with suspicion.³ And I think we must do more than worry about 'voices' and 'speakers', or complicity with informants so-called. Quite critical is not simply the extent to which actors are allowed to speak, the openness with which the original dialogues are reproduced, or the restoration of their subjectivity through narrative device, but what kinds of authors they themselves are. We need to have some sense of the productive activity which lies behind what people say, and thus their own relationship to what has been said. Without knowing how they 'own' their own words, we cannot know what we have done in appropriating them.

This is relevant to the domestic dilemma (knowing more about ourselves as objects and knowing more about ourselves as subjects).⁴ The question is the form in which our own productive activity becomes the basis for such relationships as might exist between 'ourselves' as anthropologists and the selves under study. The quality of the social relationship established here is not simply a matter of personal management. It depends on the nature of the society in question. In the same way, anthropological self-knowledge is not simply a function of personal characteristics such as how much is shared with the people being studied (closeness and distance) or degree of sensitivity to one's own scholarly constitution (self-consciousness). Such self-knowledge is also to be located in the social techniques of ethnographic/anthropological production. Gudeman and Penn (1982: 99) refer to this as 'systemic reflexivity'. Fabian's conclusion, 'that our theories of their societies are our praxis – the way in which we produce and reproduce knowledge of the Other for our societies' (1983: 165, emphasis removed), suggests that if we are to be attuned to anything it should be to the nature of the productive activity.

The two assumptions about reflexivity – that it leads to both greater understanding and to unnecessary mystification – are specifically artefacts of auto-anthropology. To demonstrate this, I open up certain differences between the Essex village and a Melanesian one. A mutual context is provided by a criticism which relates directly to productive activity. (This applies especially to 'ethnographies' in so far as they are perceived to be about specific people at specific places and times; but most books which contain ethnography comprise a mixed genre, including attempts at anthropological theory, a state of affair which contributes to the criticism noted here. I intend reference to this mixed genre when I refer to 'ethnographic' or 'anthropological' accounts.) Criticism is made in both places of the relationship which members of the community in question perceive between themselves and the investigator in reference to what the investigator is producing. They suspect that they are being exploited.⁵

Types of exploitation⁶

In the late 1970s the student body of the University of Papua New Guinea became concerned with the issue of exploitation by academics, and singled out anthropologists for attack. They were seen as appropriating information which belonged properly to Melanesians, deploying it for personal gain. No commensurate return could be made to the true owners. On the surface this echoes the distaste with which anthropological accounts on home ground might be greeted: that the anthropologists had used other people's lives and experiences for ends of their own. Not only do they transform experiences into objects of contemplation, they produce analyses informed by terms which apparently belong only to themselves.

Academics may be seen to create an exclusive domain within which their accounts have value. Theoretical models circulate endlessly between practitioners with different analytical intentions, but the origins of these models are attributed to academic discourse itself. Their origins in other lives, other cultures, become overshadowed. Ultimately the use anthropologists make of their data is for ends also of their own making. In this sense anthropology domesticates an exogenous world, making new uses for materials originating under quite different circumstances, and thereby encompassing the different uses which people have for the way they live their lives. Such encompassment is experienced as exploitation when people perceive that others have the power to turn data into materials whose value cannot be shared or yielded back to them in return. Anthropologists are thus seen to convert lived experience into items (units, constructs, concepts) whose usefulness, as elements for their own models, they alone control.⁷ Yet when Melanesian students or Elmdon villagers feel they are being used by anthropologists, it is certainly not because they wish they were anthropologists too. They have no desire to enter the domain where the anthropologists' data are valued. So why does it matter to them? One answer lies in the way in which they conceptualize productive action.

Elmdon villagers see the academic as doing something to their private property, which if put to use should be for their own benefit. The academic enterprise has raised the question of its utility. Melanesian students may well espouse private property notions; or they may instead recall indigenous conversion processes typical of male political behaviour – unequal relations arise when one person's 'work' contributes to another person's 'name'. Let me consider not the student, then, whose sensibilities are shaped in part by those of expatriate academics, but the Melanesian villager.

From time to time I have been made to feel that I was exploiting those who assisted my researches in Mt Hagen, in much the same way as all European employers of wage labour were felt to be using people. My relationship with them was going to further my prestige to the exclusion of theirs. In traditional Hagen society prestige is gained through transactions between equals; the

gifts they exchange are mutually enhancing. In this sense partners assist one another's reputation. The problem with the itinerant associate is her imminent departure. This constructs the supposition that she will derive prestige from another sphere of interaction, in respect of which previous relationships become reclassified as 'service'. For on departure the fieldworker clearly turns to investing in relationships elsewhere. There is a Hagen term for persons in non-reciprocal service of this kind.

The appropriation of information is not, I think, at issue. Information is normally produced in the context of an exchange in which both parties retain their autonomy. A person knowingly gives or withholds information; under normal circumstances it cannot be extracted. Moreover, information is to be evaluated by its source, and necessarily always has reference to its social point of production; it cannot in this sense be alienated. If one were to think of exploitation at all in the Hagen context, it would be in the way one set of social relations was evaluated in respect of others. Value conversion in this situation, then, results not in one party extracting something from another and using it for ends of his/her own. Rather it consists of one party re-evaluating the relationship between them on the analogy of reducing a partnership to a service.

The reactions (as I construe them) of the Hagen villager as opposed to those of his counterparts (the Elmdon villager/Hagen student), incorporate indigenous models of productive activity, and of the relationship between producer and product. Sahlins (1976) implies that the kinds of dualisms by which 'we' create our exercises – individual and society; symbol and function – inhere in Western bourgeois culture, providing us with both problems and practice, how to understand the cultural productions of other societies and how to embody cultural creativity ourselves in decoding alien systems. There is a coda he fails to add: that this creativity we award ourselves requires that we exploit the creativity of others. In endlessly producing 'products' Western bourgeois culture is constructed as endlessly creative, a model that does not simply involve the permutations of products but the notion that production is also control, including control over the values given to things. What irritates the English villagers is not a wish to gain what the anthropologist has but the way they see the arrogation of authorship. That is, the anthropologist is making himself the author of an account in which their authoring of events, acts, feelings is displaced. Their authorship becomes encompassed as part of the anthropological data.

This I think is the Western sense in which all value-conversions can be seen as potentially exploitative, that is, the value which people acquire for themselves must be at the expense of others. Extraction is envisaged because it is supposed that what lies at the basis of value-conversions is something being done by other agents to the work people do or to the products which embody their agency. The eighteenth-century idea that persons are the natural owners of both themselves and their labour is still with us; this notion of singular

ownership/authorship also sets up the conceptual possibility of one author supplanting or displacing another. The anthropologist as Western academic is sensitive to charges of exploitation that derive from such possibilities of encompassment and displacement. One is turning events or situations to ends of one's own, as though extracting 'raw materials' for 'social' use. Not to share with someone is to put oneself into a separate class in relation to such conversions; hence the movement in ethnographic writing towards representing accounts as somehow the product of shared experience. Whoever the Other are, whether at home or abroad, they should be given voice. Yet this model of exploitation as displaced authorship is a particular home-based one.

For the Hagen reactions rest on a quite different political-economic base to people's expectations of one another. The question is not one of extraction, but of who has the power to convert a relationship into personal prestige. Not to share means to devalue an ongoing relationship in respect of relationships elsewhere. If I were thought to be using people in Mt Hagen, it was because my relationships with them were going to further my prestige and not also theirs. Of course people supplied a material dimension where none existed, assuming that one would make vast sums of money in one's own world. Although they were losing little by telling me things, the point is that the profit would be realized in a sphere of activity to which they had no access; a situation, in other words, in which nothing is extracted, yet a relationship exists which one person turns to unilateral advantage. This is akin to what happens between Hagen husbands and wives. Husbands and wives labour together, producing things which the husband then takes off to use in male political exchanges. Indeed, men may refer to women in general as their 'servants'. The analogy is worth pursuing briefly.

There is no doubt that a conversion of value has taken place. In converting pigs from food to be eaten into gifts to be exchanged the husband reclassifies their social origin: the pigs are now seen to be the result of other previous gift-transactions (cf. Josephides 1985). Men thus eclipse their own productive activities, as well as women's. There is a special feature of this value-conversion. It is not the case that converting pigs into gifts reproduces them or reauthors them in terms of production.⁸ Men are not claiming the labour belongs to them, and the work does not become disguised as something else because in turn agents are not hierarchized in respect of their ability to turn the less useful products ('raw materials') of one person into more useful ones for another. Gifts do not make useful objects out of useless ones. Whatever work went into the production of the pigs, exchange partners do not appropriate it as 'work' – they acquire the gift as a debt to be repaid (cf. Damon 1980). Moreover, between husband and wife work is evidence of a differentiated commitment to the relationship between them: the wife is no 'owner' who can transfer that ownership, or have it wrested from her control, because there is no one-to-one relationship between her working capacity and

the products of her work. When men take off the pigs they are not supplanting women's authorship – or their own for that matter – as producers. For products of labour are conceived as originating from socially heterogeneous sources. One source cannot render the other anonymous. It follows that whatever exploitation exists is not to be grasped through Western notions of single authorship, of a one-to-one relationship between a person and their products.

The intention of this example is to draw attention to the Hagen constitution of 'joint products' when the persons involved – such as husbands and wives – are differentiated by social interest. They sustain their heterogeneity, as do the authors of words. The English concept of a person naturally owning himself, on the other hand, leads to the possibility of appropriating other things for the self. In this view, all knowledge can be turned into self-knowledge: the more one learns about others, the more one learns about oneself. By the same token, in their appropriative acts, selves supplant one another. Above all, if knowledge of others becomes a vehicle for knowledge of the self, then it is turned into the constitution of the self. In respect of those others, authorship has been displaced.

Displaced authorship is not a problem in Hagen. The problem is that of superseded relations. Knowledge is only ever brought into the open as an instrument in the negotiation of relationships; self-knowledge is contained within the inscrutability of people's minds, or evinced in their bodily health. Knowledge of others has a profound effect on the relationship in question, but there is no split between information and the author of it.⁹ One author cannot supplant another, a point which Goldman (1983) demonstrates linguistically for the nearby Huli.¹⁰ Accounts may be juxtaposed. But what someone says cannot be recast in a different version by another, for it remains only what he/she said. Such practice, it may be added, does not lead to summary or systematization in an organizational mode. Where does this put the productive activity of the ethnographer? If Hageners are concerned about the amount of money one's books will earn, it is for blatant evidence of superseded or eclipsed social relationships. A mutual activity has become an instrument to exclusive prestige, which is why the magnitude of the gains is important. But there is no way in which the ethnographer can substitute his/her own account for people's own: he/she neither authenticates nor displaces them. From their point of view such accounts are only relevant to the active management ('exploitation') of relations with outsiders, including the ethnographer as outsider.

The conclusion to be drawn is that the kind of author which the ethnographer becomes in his/her writing of texts is not determined by an act of will. What 'our' representations of others will mean must depend in part on what 'their' representations mean to them. And this in turn will depend on whether or not the anthropologist is indeed at home. For it is a question not simply of authorial choice but of cultural and social practice.

Holy and Stuchlik (1983) conclude their enquiry into anthropological understanding and explanation with the observation: 'When actors' meanings are replaced in the course of analysis and explanation, the anthropologist is not explaining social reality as it exists in the only meaningfully possible sense, but through his explanation creating it' (1983: 121). How that replacement is accomplished will depend on the status of the indigenous meanings. Do we know that they function as 'explanations', for instance?¹¹ It must matter whether the anthropological framings are exogenous in intention, or whether one type of explanation is replacing another, indigenous one. In the second circumstance, the anthropologist has, as it were, substituted his/her authorship of the events under question from that of those who gave them meaning in the first place (his/her explanation for their explanation). I have suggested that this might be a significant element in any irritation expressed by Elmdon villagers: that my version of events supplanted theirs – in not reproducing their descriptions in their own genre I had, as it were, displaced their authorship of the narrative. It is important to note, of course, that this displacement could not occur if the ensuing account were not in some way regarded as a *version* of their own accounts (e.g. that both 'explained' something). This view further implies that authors somehow own their words, in the same way as persons own themselves, and thus have a natural control over their actions and intentions. The usurpation of natural ownership sets one of the conditions for exploitation.

Exploitation was raised for the light it might throw on how to construe the ownership of ethnographic accounts. I used the term 'author' in a metaphorical sense. The single term is not, however, adequate for the differences in nuance which arise from comparing the producer of ethnographies at home and abroad. Comprehending the character of the domestic dilemma (see p. 19) is helped considerably by the distinction which Rabinow (1984) brings into the open between authors and writers.

Rabinow addresses himself to the recent upsurge of self-consciousness in the creation of ethnographic form. Following self-scrutiny in other disciplines, notably literary criticism, the urgent issue has become just where the narrator is to be placed, in respect of his text and in respect of his readership. Rabinow applies to a number of ethnographic productions Barthes' distinction between the writer, on the one hand, who absents himself from the text, treating language as a transparent tool for the ends of explanation and instruction, and the author, on the other hand, whose texts embody his relationship with the world, where language is its own end, supremely self-reflexive. Rabinow is concerned with the differing intentions which anthropologists display towards ethnographic production, and the ethical significance of form. For the anthropologist is necessarily caught in a triad of relations, not only with a vernacular readership but with so-called inform-

ants,¹² and cannot speak equally to both as audience (Webster 1982: 108–09). In the difference between being an author and being a writer, the anthropologist–ethnographer holds these sets of relationships discrete.

For Melanesia, at least, it does not make sense to see the ethnographer as supplanting an original account in such a way as to make it a new version for the people concerned. Other people's authorship cannot be displaced.¹³ The ethnographer for his/her part is put into the position of laying out the relationship of his/her representations alongside 'theirs'. Here he/she is acting as a *writer*. Writing is used as a vehicle for explanation via comparison, above all the comparison of ideas from different social sources whose origins can be juxtaposed. The analytical job thus includes accounting for people's ideas, which become part of the data, and he/she reveals the relationship between their ideas and those of analytical discourse (actors' models and analytical models, the rationale for Holy and Stuchlik's 1983 enquiry). This explicit theoretical exercise displays the juxtaposition of indigenous and exogenous concepts, a preoccupation of much anthropological writing.

If the ethnographer is also *author*, it is in relation to the readership at home. Presentation through the filtering consciousness of the ethnographer-who-was-present (cf. Kuper 1980: 20; Clifford 1983) is all important. With respect to those at home who will be reading the account of Papua New Guinea, the ethnographer is in total control. On the one hand, his/her readers have no other access to the ideas which are being put forward. Actors' models are rendered through the analytical models. From this is derived the rhetoric of ethnography as translation. On the other hand, its authorship supplants or substitutes for other versions of the same material which the readership at home might also have to hand. It modifies how they think about Papua New Guinea.

Rabinow repeats Barthes' lament, if it is that, that in the self-conscious twentieth century narrators can be neither writers nor authors. Yet the traditional anthropological exercise, in its representation of an exogenous other, allowed these two roles to be separated out, writer and author being oriented to distinct social fields. For the home readership the ethnographer is author, being an authoritative source through which his/her readership have access to the other. Towards those being studied, the ethnographer is writer, creating an explicit relationship between their ideas and his/her framings. In this way the ethnographer traditionally negotiates the 'fundamental contradiction' of 'ethnographic research involving personal, prolonged interaction with the Other' becoming 'a discourse which construes the Other in terms of distance, spatial and temporal' (Fabian 1983: xi). The first allows the experience of immediacy in one set of relations (in the field) to validate authorship apropos another (home readership). The second allows a theoretically constructed distance (fabricated at home) to inform the job of writer *vis-à-vis* the narratives and texts provided by informants (in the field). I have suggested, of course, that this latter position is not simply engineered by

the anthropologist but also by the social realities of the other's construction of authorship.

The challenge of anthropology at home is that it sustains a different structure of distinctions. The ethnographer becomes *author* in relation to those being studied. The proposition rests on there being continuity between their cultural constructs and his/hers. For they too analyse and explain their behaviour much as he/she does. At base they are agreed that 'society' or 'culture' can be conceptualized as an object of study. They are familiar with the vocabulary of 'relationships', 'roles', 'community'. What the anthropologist seems to be doing is simply using these ideas in specialist ways.¹⁴ The specialized analysis thus appears to give a further view which encompasses and overrides the original explanations, supplanting them in effect with further versions. Versions can always be challenged, of course. The possibility of authors supplanting one another comes from conceptualizations of productive activity as a process by which useful things are made out of materials thereby relativized as useless. People may object to the value put on what they supply.

If the ethnographer at home remains a *writer* it is not so much for those he/she studies, who may well challenge his/her versions, but for colleagues, the main readership. For the ethnography is always to be compared and brought into relationship with a body of shared knowledge, and the contrivances of method and theory. Now, in reference to other cultures, the contrived nature of anthropological constructs is trivial. The social disjunction between the anthropologist and members of the society under study turns the contrivance into a deliberate bridging. Of course the outsider uses his/her frames of reference, whether it is a definition of 'marriage' or a decision about 'patrilineal descent'. It does not have to be demonstrated that Hageners have arrived at their notions of patriliney by a very different route from the observer. Since it is, after all, a postulate of the enlightenment that objects of study are created by subjective self-awareness, the exotic society is authored through the filter of the observer's consciousness. The contrived appearance of anthropological reportage in its own society, on the other hand, creates writers not authors. It is as writer that the anthropologist must distinguish his study from other professionals in the business of representation (Barnett and Silverman 1979: 17), and make evident the domain that has been captured (Thornton 1983).

But I speak of home in a limited sense here. These contrasts give insight into the initial assumptions which really only applied to auto-anthropology. One was the charge of mystification. For the informants, anthropologists' accounts of the home society may be regarded as partial, obvious, repeating what is known, but also as idiosyncratic and trivial; he/she has merely authored another version. For fellow scholars, on the other hand, the conventional basis of the analytical framework is made transparent; writing is revealed to be a device. The other assumption was in terms of greater

understanding. As author the anthropologist may cast people's experiences into a different light in an illuminating way: people will know more about themselves. And in the manner in which, for his/her colleagues, the analysis is given meaning in relation to other analyses, the anthropologist as writer offers reflection on how the bases for those analyses are established. Knowing ourselves better as both objects of study and as the subjects doing the study: the two are fused in the cultural premise that all knowledge is a species of self-knowledge (see n. 4).

Knowledge and self-knowledge

We can now add a second characterization for auto-anthropology. The first was the proposition that this kind of anthropology at home is recognizable by its rendering back, to the culture or society from which it comes, the culture's central constructs, such as 'relationship', 'role', or more particularly the concept of 'culture' itself. This is, so to speak, an outsider's view. The second characterization comes from an insider's view, from the folk model that anthropology contributes to self-knowledge. And this is self-knowledge both for those under study (as author he/she presents a new version for them), and for the anthropologist as scholar (as writer he/she uncovers the premises of scholarship). This must be elucidated by reference to Western cosmology, specifically to ideas about 'the relationship' between individual and society.

Self-knowledge is bottomless. If authors supplant, they can also be supplanted. As authors among authors, auto-anthropologists merely offer 'another view', 'an alternative perspective', or whatever. They add complexity to the understanding of what they are constantly telling themselves is a 'complex' society. To put oneself in the position of an author is to witness the world through different eyes; but what is seen must be provisional by the very act of consciousness in taking someone else's point of view. The charge of triviality speaks to the fact that any number of points of view may be set alongside one another, and measured with one's own seem to offer little which was not already apprehended in other ways. If the viewpoint is not reauthored – absorbed as an enriching process to the ends of self-knowledge – it may be regarded as little improvement. It does not yield anything not already known, even if it is known in other ways.

The effectiveness of authors' versions is supposedly registered in the impact they have on individual consciousnesses. Where it is self-knowledge about ourselves that 'we' are after, society may be perceived as a singular person. 'It' comes to know more and more about 'itself'. But knowledge can also be regarded as collectively constructed, the product of many minds working together, and laying out the relationships between them. A discipline does not merely give a point of view, but also organizes itself in relation to other disciplines; as writers, anthropologists place themselves in respect of the

theoretical premises of others.¹⁵ From the inside, this may feel like contrivance; from the outside, scholars talking exclusively to one another.

Anthropologists at home are thus also fulfilling the conditions of auto-anthropology when they produce as writers. They turn findings into artefacts of a particular kind, of which the most notable is the concept of 'culture' or 'society' itself. Understanding comes to be contingent on the elucidation of 'society', say, as a social system, or set of interrelated parts, or whatever. Thus the investigation of *x* or *y* is justified in terms of its contribution to the general understanding of English/European/Western society. At the least, there is refuge in the postulate that self-knowledge is possible. The individual (particularly the individual mind) as a microcosm of society is the effective register of self-knowledge, and the improvement of individual knowledge is taken to contribute to collective knowledge. The very idea of society is significant here – that as a 'society', 'we' can improve knowledge of 'ourselves' (i.e. of 'our society').

Anthropology first developed among a people who thought of themselves as forming 'a society'. They were conscious both of their distance from and proximity to others. This self-description is based on a quasi-ethnic model (the world is made up of numerous societies all dealing with similar problems in different ways), which sets the scene for the traditional monograph, bringing an exotic place to a home audience. The contrivance of the anthropological ideas, the hypothetical nature of the constructs ('convention' in Wagner's phrasing), is transparent in the mere disjunction of cultural content (cf. Marcus and Cushman 1982: 48). The contrivance itself is regarded as a necessary means of access to the unfamiliar. The monograph thus emerges as the implicit comparison of two cultures or societies – ours and theirs. This leads to a routine reflexivity, the constant discovery that analytical concepts are context-dependent.

When the anthropologist turns to home, contrivance, I have suggested, must take a different place. The auto-anthropologist cannot use language simply as a device for comparing (say) two cultures or societies: he/she is talking, more or less, about his/her own. He/she locates him/herself as a professional within that culture over all (an acceptable genre) but must show the way the culture contrives. What comes over is an account of contrivance. Thus people's commonsense understandings of the roles they play and their place in society are shown themselves to be contrived. Their interrelationships are displayed through a form of knowledge exhibited as the interconnection of units within a system (Barnett and Silverman 1979). Now 'models', 'structures', 'systems' are apprehended not simply as objectifications (Asad 1973: 17) but as a mode of organizing data (Anderson and Sharrock 1982).¹⁶ In so far, of course, as anthropologists take for granted how they know (as a matter of overview, functional interrelation, organization, shared experience), they evince their own society's or culture's knowledge of itself. Yet the notion that events can be summarized and

systematized, individuals organized, social rules and cultural maps elucidated (cf. Salmond 1982) begs the question, to whom is the knowledge directed. For as a cultural construct self-knowledge is not to be confused with self-expression. Knowledge has an instrumental element; at a minimum it is 'for' the self.¹⁷ The self (individually or collectively) thus benefits from its knowledge. In this context, to become conscious of convention and contrivance is to produce knowledge of anthropologists as contrivers: first, as participating in a social life which rests on contrivance; and second, as active contrivers in constructing knowledge about that social life.

Auto-anthropology has its own advantages and pitfalls, then, but one might reasonably ask whether the same circumstances do not arise whenever an anthropologist turns to study 'home'. Are not the conditions for auto-anthropology met when, say, a Hagen trained as an anthropologist embarks on a study of Hagen? Neither of the conditions can, in fact, be satisfied. The indigenous anthropologist¹⁸ in this kind of situation is not contributing to self-knowledge in any straightforward sense. He/she is not in a position to reauthor events, and thus set his/her version alongside other proprietary narratives; nor is he/she as a writer utilizing the conceptual resources of that society as the foundations of description.

Consider again Malinowski's claims to 'create' the Trobrianders.¹⁹ He meant that he alone would bring them to life for his readership, that he would be their author in that sense; at the same time, in juxtaposing the world as seen through Trobriand eyes with European prejudice about primitive society, he had also to be writer: to make his narrative transparent enough to be an authentic description of the Trobriands which required technical elucidation. As author, he was displacing no Trobriand accounts as far as they were concerned, because he could not possibly have been an author in respect of knowledge of local social interest. Nor did his organization of material, his functionalist models and principles of social organization, participate in Trobriand modes of self-knowledge. Their self-scrutiny was managed through different techniques. When, many years later, Kasapwalova (1975) planned for Trobriand 'cultural development', it was through hopes of founding an art school based on the inspirational balance between meaning and form whose elucidation till then had been in the hands of master pro-board- and house-carvers.

It would be absurd, however, to imply that Malinowski's relationship with the Trobrianders was contentless. It did matter what he wrote, in so far as he mediated between themselves and the colonial world in the information conveyed. This is the significance of subsequent accusations that he got things wrong, or gave an unrealistic view of Trobriand society to the outside world. The point to stress is very simple. Anthropological accounts of exogenous societies such as the Trobriands can never be self-knowledge in the way that a parallel account of the social world of Elmdoners (say) would be. It does not matter where the anthropologist comes from: it cannot be self-knowledge in a

reflexive sense because it does not draw on the specific techniques by which people know themselves.

Conclusion: where home is matters

Shamsul was right when he said that a Malay anthropologist should deliberately familiarize himself with Malay society. We cannot conclude that non-Western anthropologists will stand in the same relationship to their own society or culture as a Western anthropologist does to his/hers. This is a projection of a specific (Western) modelling that supposes societies are a series of homologues, that if other societies throw up anthropologists then these would all be anthropologists at home, in an analogous relationship to one another as far as their relations with the home society were concerned.

Many of our constructs conspire in that modelling. 'Culture', for instance, is construed as a repository of information, explicit in the techniques of ethnomethodologists whose entry into another culture is through acquiring the tools of 'knowing how' to operate within its categories. The goal is to uncover ground rules, templates, codes, structures as information-bearing devices. The concept of culture thus demarcates the distinctiveness of the kind of information needed to be a member of a particular group, enclave, institution. To elucidate the culture of this or that congeries of persons is to elucidate such information. In this view all cultures, like all societies, are homologues of one another. They all do the same job, informing the members of each society what they should do and how they should do it. All societies thus 'have' culture, and the 'how to' rules and practices by which people conduct their lives afford an unwitting reservoir of information for the outsider. It is because we thus think that all societies have cultures, we can play one off against another, engage in comparison, and ultimately use one's own culture as a foil for understanding others (cf. Kuper 1980: 18). This gives rise to that further source of routine reflexivity, that one learns more about one's own culture while studying others, as one learns more about any culture by placing it alongside any other. Moreover, any social distinction, among sets, groups, clusters of people, will know itself through internal cultural practice, and thus be open to anthropological scrutiny. Here there is an infinite regression, as many 'cultures' as there are systems of self-knowledge. For the supposition that all societies 'have' cultures means that, in reference to these other cultures, the anthropologist can turn their self-knowledge into information for him/herself about them. This model of cultures as systems of self-knowledge suggests that auto-anthropology could be done everywhere. My argument is to the contrary, of course. It is all very well for Giddens to state blandly that 'all social actors . . . are social theorists' (1984: 335), but the phrase is an empty one if techniques of theorizing have little common ground. I have laid stress on the production and writing of ethnographic texts in order to stress the specificity of techniques, as far as 'knowledge' is concerned.

As writers, auto-anthropologists participate in the varieties of self-knowledge which are gained through systematic enquiry and scrutiny. It is thus part of a genre of knowing: that knowledge is organization. This genre of knowing is not necessarily analogous to self-knowledge gained through divination, myth, the gathering of ritual congregations, or the infliction of injury. The question is not simply one of consciousness. People set up a variety of avenues to self-knowledge in a conscious manner. They may test their own system, so to speak, through the interpretation of events, health and sickness, and so on, and implement ('probe') their interpretations in social action. It is a short step for Wagner (1975) to argue that in their encounters with Western cultures, the reverse anthropology contrived by Melanesians should take the form of cargo cults, as an active engagement in a new social field. Outsiders tend to explain cargo cults as greater self-knowledge on the Melanesians' part – they were making cognitive sense of a new situation, using old tools for new problems. Jarvie (1984: 126–27) still argues from this position, and it is intriguing to note that homologously the only positive role he awards anthropology itself is self-knowledge. Those Melanesians who embarked on cargo cults of course told us otherwise (and cf. Harrison 1985); they insisted that their break with the past was radical, that they had accomplished ways of knowing and created relationships previously inconceivable.²⁰

On occasion it is misleading to take too far the methodological premise that all societies 'have' cultures. A notion of 'culture' does not form part of the techniques of knowledge all peoples have of themselves, any more than is true of the concept of 'society'.²¹ If elucidating culture or society is not part of the way in which they organize their experiences, then it cannot hold the same place in anthropological accounts of them as it does for the auto-anthropologist. Were techniques of self-knowledge to constitute a universal class, then we could argue the case for acknowledging anthropology at home wherever anthropologists turned to their own societies. But we should not mystify ourselves here. It is anthropologists themselves who constitute a universal class, they who share precepts and concerns and who as writers manage data in specific ways.²² Such 'self-knowledge' in turn can only be expressed in circular form: the auto-anthropologist comes from a culture/society that 'has' a concept of culture/society. Whether anthropologists are at home *qua* anthropologists, is not to be decided by whether they call themselves Malay, belong to the Travellers or have been born in Essex; it is decided by the relationship between their techniques of organizing knowledge and how people organize knowledge about themselves.²³

The students who carried out the Elmdon study were really too coy, though none of us had any problems with imagining that Elmdoners would concur with our stated interest in 'history'. Pressed to the point, I think they would also have agreed that society is a proper object of study. I wish now we had pressed the point – it would have helped us to feel more at home.

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Notes

- 1 Elmdon is a village in Essex, the subject of a 'survey' by social anthropology students from Cambridge in the 1960s/early 1970s. Wright (1984) gives an interesting account of her official brief for a study of decision-making in rural areas: it was packed full of assumptions about 'communities' and 'communication' which had to be unravelled in terms of both bureaucratic and local theory. Everyone was participating in promoting the idea of community as an explanatory concept in the description of rural society. I might add that the idea that 'rural communities' in Britain are somehow peripheral ones effectively blocks our understanding of the way their self-acknowledged differences draw on common British ideas about difference. One thinks of Ennew's work among Hebrideans who turned to the bookshelf for answers (for a summary see Condry 1983), or the essays in Cohen (1982).
- 2 The examination was stimulated by Anthony Cohen's observation (personal communication) that we imagine anthropology at home either takes too much for granted or else mystifies everything. The first assumption is stated clearly by Bradley and Lowe (1984: 8); apropos the second, Giddens (1984: 334) proposes that sociology might be construed as a critique of lay knowledge.
- 3 Elsewhere (Strathern 1985) I offer a brief critique of the postulate of jointly authored products. Rabinow (1977: 153; 1983: 204) emphasizes their hybrid nature; Crapanzano (1979: xv) cautions against dialogical interchange being taken as 'the cultural reality' of the other.
- 4 It should be clear that I am simply taking such a subject-object dichotomy as one culturally appropriate ('positivist') formula for the contemplation of our own activities; I go part of the way towards elucidating its contrived nature (see Webster 1982). This makes my account ironic (aware of its own context), especially in the passages on self-knowledge developed below.
- 5 The issue of exploitation is germane for the wider equation Asad notes (1973: 16-17) between unequal power relations and anthropological understanding as 'overwhelmingly objectified in European languages'. I acknowledge the criticism made (by Raymond Firth and Lydia Sciana) when this paper was delivered, that I appear to rely on an 'economic' metaphor. Apart from the fact that 'exploitation' was a term used by the Papua New Guinea students to whom I refer, it is intended as a comment on the anthropological aspiration to enlarge the 'moral community', in Hymes' words (1974: 53 - 'building a world culture that is a moral community'). What I expound in a property idiom apropos anthropological products could equally well be put in terms of subjective relations: the extent to which the investigator properly regards others as versions of him or herself.
- 6 There is some evidence - mainly from men, not women - that I was 'using' the people I knew in Hagen. Many were quite cynical about what my relationship with them would do for me as opposed to themselves. That I myself put a different construction on that relationship meant I was always a little upset, or at least

- surprised, by such reactions. This was a felt ('real') disjunction. In the Elmdon case on the other hand, my account here is largely fiction. That is, I have almost no evidence that Elmdon villagers really did think I was using them in any way. I just thought they would think that. Most of the evidence is to the contrary - in so far as people knew about the book, it was in terms of mild interest. However, I sustain the fiction because my projection of such reactions on to Elmdoners - my version of what I imagine they must be thinking - profoundly shaped my attitude towards writing the ethnography and my own feelings about the village. That they might feel exploited is a social fact in the context of what I thought I was doing as far as they were concerned. This paper, then, adopts two approaches to ethnographic verity, treating the Hagen 'facts' and the Elmdon 'facts' in different ways, corresponding to the distinction pursued below between being a writer (apropos Hagen) and being an author (apropos Elmdon) in respect of one's subjects of study.
- 7 Yet, in seeing that enterprise as transformative, anthropologists also have to preserve the discrete origins of the subjects under study. These subjects cannot be collapsed into replicas of the observers. It is important that their exogenous status is retained, for in making differences intelligible, anthropological 'work' is seen to have been done. Some of the following remarks are taken from Strathern (1984).
 - 8 Apropos the production of pigs, neither the woman's nor the man's labour encompasses the labour of the other partner; as an embodiment of labour the pig in question cannot stand for one and not the other: it constitutes the product of the relationship. The one partner does not convert or transform the work of the other into use for her or his own self, but consumes the products of a specific other. Things produced for the use of others are thus not subordinated to some overriding purpose which redefines the ends for which they are conceived. But when Westerners imagine that everything can be turned to use, what emerges are hierarchies of use, fashioned by agents who turn the artefacts of others into useful things for their own ends. In this latter model, both the provisioning of raw materials and the productive transformation is 'work', so that one can be measured against the other, not as separate domains but as stages in a process. One 'uses' the work of another in one's 'own' work.
 - 9 Compare Favret-Saada's (1980: 26) complaint that traditional ethnography accomplishes a split between the stating subject (the author of the account) and the statements (the text). Such facility in splitting is a culturally appropriate approach to a world conceived as beyond the subject.
 - 10 Goldman demonstrates that this is both a matter of syntax (one cannot appropriate another's words through indirect speech; they are reported as direct speech) and the structure of discourse, which in the disputes he examines proceeds by example and counter-example (there are no summations). He relates these linguistic features in turn to the management of Huli talk oriented to agreement though consensus rather than judgement.
 - 11 As Cohen (1978) suggests, this is like the introspection of Whalsay islanders who find themselves in the position of having to give a cultural accounting of their past. This seems to me an appropriate extrapolation, whereas for instance Jarvie's (1984) account of cargo cult activity as 'explanation' does not. Here Southwold's (1983) critique of the notion 'belief' affords a parallel comment. In spite of its propositional form, a credo which manifests a state of body and spirit is not to be confused with the credo of a detached consciousness which imagines truth in the specific form of an intellectual proposition. 'Belief' in the first sense cannot be taken as a variety of 'belief' in the second sense.
 - 12 Favret-Saada attempts to generalize this triad as at once a social and a syntactical proposition: 'only a human being who names himself "I" can refer to another human being as "he"; and he can only do so by addressing a "you"' (1980: 27). It

- should be clear in what follows that I do not regard the triad as universally determined by such subject-object relations (cf. Fabian 1983: 85–6). If one *must* go to the syntax of representations (as an ethnography is a representation) then one should recall the syntactical possibilities of the New Guinea constructions already referred to (see n. 10). The juxtaposition of ‘reported’ speeches allows a simultaneity of single authorship (the speaking agent) and dual authorship (what another said remains what ‘he’/‘you’ said); similarly, the Hagen dual pronoun can pair distinct social agents on an alliance rather than an incorporative model (cf. Strathern and Lancy 1981), and is not to be understood simply as an inclusive ‘we’. The dual in effect elides ‘he’ and ‘I’ (Favret-Saada 1980: 28).
- 13 As far as domestic consumption is concerned. I am not dealing here with versions which might be useful in transactions with the ‘outside world’. In cases where people might go to the anthropological account for a record of what was done ‘in the past’, I think the account would be regarded as a transparent rendering of what significant social others said (even if their identity is no longer recoverable). I intend a technical observation here about ownership and production.
 - 14 Although in order not to burden the account with too much abstraction I have presented the case as though it were a matter of what people understand, the argument does not require evidence that people share a common vocabulary with the anthropologist. The point is that they and he/she belong to the culture which, so to speak, produced ideas such as that of ‘society’, or of ‘culture’ itself.
 - 15 Disciplines may thus be seen to respond to particular social interests. Here is not ‘ourselves’ as a mass whose self-knowledge will be augmented, but ourselves as divided and split between numerous different interests, including academics as an interest group.
 - 16 The metaphor of ‘observation’ (Fabian 1983: 106 ff.) displaces the construction of ‘organization’. We imagine our organizing metaphor in the visual one (cognitive maps etc.), but that particular self-reflection hides the role of organization itself in our images. Sight is a world-wide metaphor for knowledge; it is the ‘systemic reach’ into other cultures which is ‘unique to the European or Western intellectual tradition’ (Burridge 1979: 9). One would need to specify the kind of systematics, however. For one account of non-Western systematizations of knowledge, see Salmond’s work on Maori oratory with its claim to be non-universalistic (1982: 83; 1983).
 - 17 Haraway (1983: 333), observing that ‘projects aiming at self-knowledge’ are ‘basic to the history of Western thought’, raises a question against assuming too much about the identity of the selves so referenced.
 - 18 For anthropology students from the University of Papua New Guinea, investigations at ‘home’ may well constitute a form of personal self-knowledge, but their project is not auto-anthropology. It is likely to have much more the character of Malinowski’s, of deliberate mediation between different worlds, with different interests to hold in mind.
 - 19 The claims are made in various forms throughout Malinowski’s writings. They have remained a topic of anthropological contemplation, cf. Leach’s (1957) early exposition; Sahlins (1976); Kuper (1980); Clifford (1983).
 - 20 Southwold’s comments on Buddhism (that it is not ‘concerned to reflect and endorse social life as it is, but rather to transform it’, 1983: 20) mean that one cannot interpret such religious practices as reflexive sociology. It also follows that one cannot make naïve assumptions about the reflexive status of ‘representations’ (1983: 78, 86).
 - 21 As anthropologists ‘we’ treat ‘other’ cultures as though they had a culture as we do, and other societies as though they modelled themselves as societies. The idea is the essential frame for the organization of data – to reveal the ‘society’ and ‘culture’ supposedly latent in people’s conceptualizations. It also follows that if we were actually to consider the operational analogues of ‘culture’ one might have to confront very different techniques of knowledge – for instance the concept of ‘cargo’, as Wagner argues. In respect of Western society, however, what the Western anthropologist reveals is different: he/she offers a ‘holistic’ or ‘systemic’ perspective, the interconnections between parts. That is, people’s own organization of knowledge, their explanations, are capped with a superorganized account. Here the difference is one of degree rather than of kind (cf. p. 17: the more organized our knowledge is, the greater the ‘understanding’ possible).
 - 22 Shamsul makes an analogous point in terms of the responsibilities anthropologists (and sociologists) have for the training of others in their disciplines (1982: 29).
 - 23 And thus where ‘the process of inquiry has been accepted as part of the ambient pluralist author’ (Barnes 1979: 186).

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