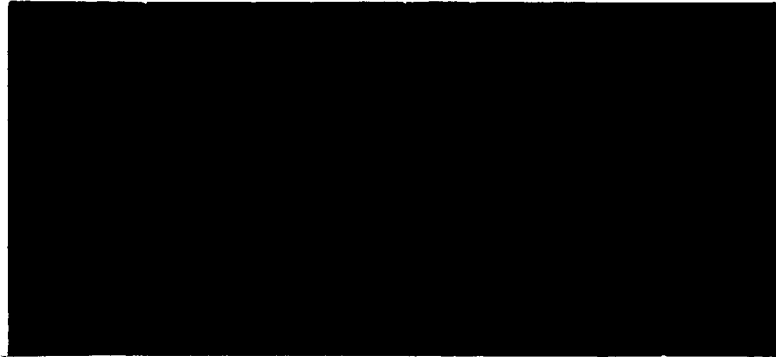




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AUTHOR MEETS CRITICS: REACTIONS TO
"THEORY IN ANTHROPOLOGY SINCE THE SIXTIES"
edited by
SHERRY B. ORTNER

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**Author Meets Critics: Reactions to "Theory in Anthropology since the
Sixties."**

edited by

Sherry B. Ortner

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PREFACE

This Working Paper contains the papers from the 1987 American Ethnological Society invited session, "Author Meets Critics: Sherry Ortner and Theory in Anthropology," held at the American Anthropological Association Meetings in Chicago. The session proposal (written, I believe, by the AES Program Chair, Katherine Verdery) read as follows:

In 1984, Sherry Ortner published a paper entitled "Theory in Anthropology since the 1960's," offering an ambitious overview of the directions recent theorizing has taken and in which she would like to encourage it further.* The paper stimulated immediate comment and has continued to generate discussion, disagreement, reformulations, and refinements. Given her rightly pointing to the theoretical disarray in our field and the potential utility of further pursuing her proposals to reduce it, Ortner's views deserve a wider public discussion in hopes of building momentum for a theoretical refocusing in anthropology - the objective of this session.

The papers have been subjected to only minor editorial revisions. Collier and Yanagisako's paper will be published in Critique of Anthropology. My "Response" has become, with some revisions, part of the Introduction to High Religion (Princeton University Press 1989).

Due to technological limitations, we regret that we have had to omit accent marks throughout the text.

I wish to thank Stephanie Moore and Rachael Cohen for technical support, and particularly Larry Frohman who did the bulk of the word processing and general junk work associated with getting this large Working Paper out.

Sherry B. Ortner

* Sherry B. Ortner, "Theory in Anthropology Since the Sixties." Comparative Studies in Society and History 26:1 (1984), pp 126-66.

"ANTHROPOLOGY SINCE THE SIXTIES" SEEN FROM ACROSS THE ATLANTIC

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"ANTHROPOLOGY SINCE THE SIXTIES" SEEN FROM ACROSS THE ATLANTIC

Ortner's paper of 1984 is a real feat. It gives the best overview of the period available in clear simple language. By and large it is fair and sympathetic towards the authors discussed. It is stimulating and thought provoking. My comments here are, however, largely critical. This is because there is no point in stressing the obvious: the service Ortner has done to the profession in writing the article; but also because her viewpoint is so fundamentally challenging and thought provoking. Any anthropologist who does not want to tell her story her way, after reading Ortner, is not worth her salt.

Ortner tells us that she has chosen to concentrate on the period in anthropology 'since the sixties' because that is when she started as an anthropologist. Indeed there are many other good reasons for doing so. But there are also dangers in identifying oneself too closely with such a historical survey. Ironically she discusses these herself at the end of the paper when she reviews actor centred theories of history. In particular what is emerging in a number of recent anthropological and historical studies is that actors are very poor at evaluating their role and that of their contemporaries in forming events. Either they tend to underestimate their innovativeness, like the maddening informants who explain everything that is done by telling you that this is how the ancestors did it, or they tend to overestimate it, like those other, largely western, informants who explain everything as being simply caused by personal inner desires. These two types of informants are like those cultural and social anthropologists who either believe the system is king, or like the transactionalists who see everything in terms of "actors" and intentionality. I shall return to this point, but

here I would like to note that these different attitudes tend to yield very contrasting images of the past. In the first case it is believed that not much seems to happen ever, in the second everything seems to be changing at a terrifying rate, at the whim and fancy of individualistic subjects.

I feel that Ortner tends to be this second kind of peculiarly western and more specifically American informant. The value of such an approach and the energies it creates are immense, but coming from a somewhat different background I find myself reacting dialectically to such a view. I too began anthropology at much the same time as Ortner and reading her historical account makes me feel retrospectively exhausted at being told that I have been through so much. Indeed this is a bit like the impression I get when I come to the U.S.A. where I believe there is an exaggerated tendency to stress the demise of what was done in the past, (hence the inevitable announcement of a crisis) and the discovery of a new theory, when in fact we are merely dealing with a shift in interest to a new area of enquiry, such as symbolism which became "symbolic anthropology", or more recently an examination of the production of ethnography which became "reflexive anthropology". These new "theories" rapidly gain such reification that advertisements soon appear for posts for these new kinds of anthropologists. Such brittle response to fashion has a lot to do with the professional organisation of American universities and also, dare I say it, with a view that political and moral commitment is more a form of self expression than anything else.

And so as a bewildered foreigner I react against this image of recent history and exaggerate the other way so that I pretend to feel appalled that nothing has advanced. This is what I shall do here. In fact both views are misleading but Ortner gives too much of the first and that leads her to

conclusions which are in part unacceptable and so, to balance her position, I shall take the opposite tack.

As an example let us look at her discussion of the influence of Marxism. For Ortner Marxism is very much "a ship which arrived in the night" in the late nineteen sixties, but this must seem a very strange view to older Americans and Europeans.

This view of Marxism as emerging out of nowhere is especially puzzling when Ortner discusses the stream which she calls the "political economy" school. However defined, the issues which she sees as characterising this school go back to the beginning of this century at least, as the writings of Lenin and Luxemburg bear witness. Subsequently to this pioneering period these problems were then analyzed in the works of a large number of anti-colonial leaders in Asia and Africa. These views concerning the determining influence of world capitalism on the nature of African and Asian societies were principally formulated in the political arena but they were also very influential among professional first world anthropologists in France (Boiteau, Leiris, even Balandier), in Britain (Worsley) and in the U.S.A. (Wolf).

The importance and influence of this approach is shown in part by the reaction and evolution it caused, precisely the kind of reaction which Ortner would like to see. This occurred in the form of an attempt which theoretically took into account the combination of the growing domination of a world economic system and the specificity of endocentric socio-cultural processes. The search for such a theory developed in France in articles which began to appear in the journal Presence Africaine and subsequently developed in the work of such anthropologists as Suret-Canale, Meillassoux and Terray who attempted to develop theories about the articulation of modes of production inspired in part by the work of Althusser and in part by politico-

economic circumstances. Perhaps such models were unnecessarily mechanical and smelled of false realism but the problem is not new and the questions which anthropologists have asked and are asking are inscribed in this much longer term debate.

Admittedly there have been many new developments and political economy has gained, for example, from economic geography and history in the U.S.A. as it has in France and Britain. But, "political economy" did not begin "since the sixties" or in a context limited either to universities or anthropology. I do not think much can be understood about it by pretending that it did. The painful theoretical progress which has been made during this century and which continues, of which the work to which Ortner refers is a little part, is liable to be wasted by anthropologists with such an approach. Furthermore there is no reason to think "political economy" will run out of steam even if totally new fashions turn up in the academic American community. The issues which have shaped the world economy this century have perhaps changed but they have not gone away and anybody who knows the countries in which most anthropologists' work is carried out will know how bizarre is the idea that we live in a "post modern world." In any case new and innovative studies in this tradition have continued coming out since the Ortner article as the work of Carol Smith and Verdery among others show. In spite of wishing they did, academic anthropologists do not live in a closed world where they can change the laws of physics simply because they have got bored with them.

Similar points can be made concerning Ortner's view of structuralism and especially "structural Marxism". The criticism she directs against it, and her call for a "practice orientation" seem also to forget much of the history of the subject where the issue has always been central. If we forget how we got

there we condemn ourselves to just going backwards and forwards on the same spot.

In Marxism the role of the individual, of group practice and of praxis in supra individual processes has always been the central theoretical issue of Marxism since its inception in Hegelianism and all the major rifts in Marxism have concerned precisely this matter. The history of Marxism will show this (Lichtheim <1961> is particularly good here, also see Bloch <1983>).

The following brief account will suggest how these issues are part of an ongoing debate which anthropologists ignore at their peril. Hegelianism was originally a response to the pseudo-individualism of utilitarians and in Marxism this was extended to a criticism of classical economists and their notions of choice and freedom. A response to this position was various forms of existentialism of which the pre-war existentialism of Sartre is an example. This was a response to Marxist theories of history which at that time were lurching towards mechanistic models of society. Sartre's work became the dialectic base for Althusser's and above all for Levi-Strauss's structuralism. When these reactions became developed into a theory of history they were explicitly a challenge to Sartrean voluntarism and cannot be understood outside this context.

I go over this well known history because without it we are likely to misunderstand its products and this is what has happened in the way Levi-Strauss was seen both in Britain and the U.S.A. Ortner, like Harris and Leach before her, seem mystified by Levi-Strauss's claim to be a Marxist, but that is because they forget the historical context out of which his work arises. In many ways Levi-Strauss represents a modified return to the most fundamental notions of Marx, that is that culture exists not as a state but in the process of dialectical transformation. As Gibson points out nothing could be more

different from this than the static and innatist theories of the likes of Needham and Dumont.

In fact Levi-Strauss's work is first and foremost a theory of historical change and is part of a largely Marxist long term debate on the role of individual practice and agency in history. Admittedly Levi-Strauss attributes little determinative historical significance to individual practice, but that is because like Marx he believes that the disjunction between individual desire and social dialectic is a product of a particular phase in our history, not the basis for theory, and in this Levi-Strauss rejoins Althusser.

This Marxist-structuralist debate was inevitably continued by the writers who Ortner labels as "structural Marxist" who refer back to Althusser and Levi-Strauss among others. And so the concern with practice has remained centre stage. For example Terray discussed how the exploitation by elders is experienced by the Abron as a form of interlineal antagonism and how it leads to actions directed by this perception (Terray 1975). But at the same time the Hegelian Marxist conflation of historical and individual practice was itself proving too simple and a number of writers were turning to other earlier participants in this dialogue, among them anthropologists. Godelier discussed the problems in Levi-Strauss's radical dissociation of intention and effect in his theory of myth (Godelier 1971). For my part I gave in 1976 (Bloch 1977) a lecture which was entirely devoted to some of the issues raised by practice in Marxism and anthropology and which was intended to bring into the forum of anthropology some of the discussions on this matter by non-anthropologists, e.g. Lukacs and Thompson. In particular I drew attention in this paper to the dangers of equating culture with ideology because of the implication such an equation would have for the possibility of change. I have gone on at length on this matter elsewhere (e.g. "From Cognition to Ideology"

1985) and Godelier has made the same point elsewhere (1984). I feel therefore a little aggrieved of being accused by Ortner's paper twice of equating culture with ideology in an article which is centrally concerned with arguing the opposite, and of falling into the trap which such an equation would imply, that of making historically significant action impossible, when in fact I was pointing out that indeed this would be the inevitable effect of such a misleading equation.

Now that I have got this off my chest let me return to the main point. That is that Marxist theory has always been centrally concerned with the issue of practice and praxis and that there is nothing to be gained and much to be lost in seeing "practice anthropology" as a new fashion. Of course this is not to say that it is not very useful to remind people of this old and central issue, but pretending that something dramatically new is being born runs the risk that we shall waste all the important work that has already been done in advancing and defining the issues and that we shall entangle ourselves in brambles which have already been cleared.

An example of such brambles is transactionalism. This was a theory which appeared in Britain in the fifties and which was in its dying throes in the first part of the period Ortner discusses. It took a number of forms such as those found in the earlier work of Barth, Bailey, Barnes and others and appeared as "formalism" in economic anthropology. Basically it sought to explain socio-cultural situations as the products of a cumulation of individual maximisation. Soon, however, it was realised that explanations of actions as the product of hypothesised desires were neither interesting, nor psychologically believable; nor did they provide explanations of those desires, or of the conditions in which they were formulated and acted upon. In response transactionalist theories were modified so as to exclude from

explanation ever more factors, which were simply to be considered as "givens" (values in Barth), but then, of course, there was nothing left of interest that was being dealt with.....and so transactionalism faded away.

Transactionalism was a waste of time precisely because its protagonists seem to have been unaware how such theories had already been demonstrated again and again to be misleading, by Keynes in Economics, by Kant, Hegel and many others in philosophy, and most importantly by Marx in his demonstration of the historical specificity of the idea of maximising choice and of its unsoundness when it was separated from historical process. In other words the transactionalist went back to a philosophical starting point somewhere in the middle of the eighteenth century without being aware of their own regression. Now the point of mentioning this tendency is that I find to my horror that it is seen by Ortner as one of the precursors of "practice anthropology." If that is indeed an indication of what this new movement will be, the course of its life can be predicted: it will be short and nasty.

But in fact the fate of being born dead is not as necessary for "practice anthropology" as the association with transactionalism would suggest. This excursion into the long term history of social science can also enable us to point towards a more fruitful direction and I believe this direction is also suggested by Ortner. However, it is not something different but the continuation of a theoretical enterprise which is not limited to anthropology, but to which anthropology has made a significant contribution.

The trouble is that Ortner is very vague, not surprisingly as she is setting herself a formidable task, and this vagueness is reflected in the difficulty she has in finding a proper name for her recipe: practice, praxis, action, study of the everyday, etc... This is not surprising since it is "the study of all forms of human action." Ortner is aware that this may be a

little wide, and so she adds a qualification: it will be human action seen "from a particular-political angle;" but she is also forced to recognise that "almost anything people do has such implications". At this point some of you might be tempted to fear that the real problem is that the emerging practice anthropology is totally continuous with the primeval slime which surrounds it.....but this would be a pity.

The reason why this would be a pity is because she has identified what I feel is a most serious development in anthropology, but like her I find it difficult to put my finger on it. Like her I would attempt to do this by first looking back but I would look further back. Especially I would look back to what is for me the fundamental Marxist theoretical advance: that is the refusal to separate individual motivation from historical process. Ortner's choice of Bourdieu and Sahlins as pointing the way is a good one for this since both these authors are building on the refusal of the dichotomy which post sixteenth century ideology has taught so many of us to accept as natural. The strength of Sahlins' and Bourdieu's approach is largely due to the fact that they do not waste the theoretical dialectic to which I have referred.

Secondly Sahlins and Bourdieu are aware that their academic contribution is only one among much more general theoretical work which is in no way limited to anthropology or universities and that to make a contribution they therefore need to build on whatever it is that anthropologists in particular have to offer. What they have to offer is above all their intimate knowledge of conceptual systems fundamentally different from those from which other social sciences have sprung, and a personal intimacy with the people they discuss which is an abiding strength of the anthropological tradition. This need not always be direct but we may be able to use our aroused sympathy for

understanding analogous situations. Thus Sahlins comes alive when he thinks by means of his imagined representation of the arrival of Cook in Tahiti and similarly so does Bourdieu when talking, for example, of the Kabyle sense of honour.

These writers have outlined new styles (not theories) for such things as handling socialisation in history, in the case of Bourdieu, and action in political history, in the case of Sahlins, which do take advantage of many of the theoretical insights which have been built up gradually but they have added the effort towards intellectual community between observed and observer which can make anthropology transcend its avowed aims.

But their work has also severe limitations and these limitations come in part from the ethnographic information which they use to think their theories with. The cases are too simple in the case of Sahlins, and too poorly documented in the case of Bourdieu. This is perhaps not an accident; the effort at combining theory and personal involvement is so daunting that we try all kinds of subterfuges to run away from it.

The eternal return to the primeval scene on the beach in Hawaii is extremely thought provoking but also severely limiting because of its exceptional character, especially that it is a history with a zero beginning. As a result Sahlins is able to abolish the distinction between event and structure much too easily and as a result to get away with a model of social change which by-passes most of the difficulties which the critics of primitive Marxism have had to face.

Bourdieu chooses to focus on the crucial importance of the creation of the historical person in historical process. But in fact his account is not based on any direct study of these processes. This is not all that surprising

since the project will be very difficult. But the absence of real cases leads to rather similar simplifications to those of Sahlins.

But as Ortner points out the work of Sahlins and Bourdieu is both symptomatic and stimulating of developments in anthropology which, like them, build on the past and perhaps will build on it even more securely.

What Bourdieu's questions require are studies of the formation of persons in real socio political contexts and this requires new combinations of anthropology and psychology which are extremely difficult to carry out in practice, so it is not surprising that as far as I know these have been few and only tentative. In Britain the, as yet unpublished, studies of Boyer and Toren go a little way in this direction and the radical questions asked by Wilson and Sperber may give us a new impetus. In the U.S.A. you might be able to tell me what to hope for.

However it is much more in the new rapprochement between anthropology and history that really exciting things seem to be happening. It is not so much in the adding of history to anthropology that anything new is happening. This is as old as the hills and a lot of history done by anthropologists is embarrassingly amateurish. What is encouraging is the new sophistication in posing questions about the historical implications of anthropological theories, the bringing to bear of much more of the theoretical thought which has developed this century to the questions which we have been asking for a long time.

And we have done this in a particularly good anthropological way. First of all we have considered these questions in the light of examples which we construct from our knowledge of non-western systems. It is not that I have anything against "anthropology at home", it is simply that our theoretical thinking is still desperately Euro-centred (and this is particularly true of

Marxism) and that we (all social theorists) need to continually bring what to us is the exotic as a critique of our theoretical effort. The anthropological tradition is available to do that. Secondly, because of our personal contact we have been able to imagine historical action in a particularly immediate and theoretically testing way. In this we are joining the long march of theory in a way that I find very hopeful.

Actually, I feel things are going on quite nicely and I do not believe in a crisis in social theory of which anthropology would be a part. Admittedly a lot of time is being wasted by stupidities which could have been avoided by a better understanding of what had already been achieved in the field of theory, sometimes quite a long time ago. Some relatively minor but yet important debates are given quite disproportionate attention, but then this is only for a time and so no permanent harm is done, an example of this is the recent discussions on the nature of ethnography. Then there is a lot of work labelled "anthropology" which I feel would be better done within other academic traditions. But all these distractions are not new. What is new is the great amount of first rate work which is also appearing and which seems to me of higher quality than most of what had gone before.

A period of less than five years has produced such works as Pina-Cabral's work on Portugal (1986), Myers' work on Australian Aborigines (1986), Lan's work on Zimbabwe (1985), to give only three examples among many, many other excellent studies both in the press and published, all of which face the significance of action in historical process in the light of the subtle theories which we have all been forging over time. This is an exhilarating period. These are important, scholarly and sensitive works. They have their roots in work done since the sixties and long before.

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THEORY IN ANTHROPOLOGY SINCE FEMINIST PRACTICE*

by

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*This article is now in press in Critique of Anthropology. Permission to reprint has been requested from the journal.

THEORY IN ANTHROPOLOGY SINCE FEMINIST PRACTICE

In a footnote to her paper on "Theory in Anthropology since the Sixties," Sherry Ortner observes that "feminist anthropology is one of the primary contexts in which a practice approach has been developing" (1984:145). In this paper, we expand on this footnote to explain why feminist practice has led many anthropologists to adopt theoretical positions Ortner identifies with a "practice approach" and how practice approaches might benefit from the insights of feminist theory. We agree with Ortner that "practice" is neither a theory nor a method in itself, but rather a key symbol, "in the name of which a variety of theories and methods are being developed (1984:127)." Some of these, we think, offer critical concepts and promising analytical agendas; others, however, lead back to old impasses. We argue that some of the insights that have emerged from feminist practice help us steer clear of what might be called "common sense" readings of practice theory and lead us instead toward "good sense" readings of it (Gramsci 1971).

It is no accident that feminist analysis has been one of the primary contexts in which a practice approach has developed in anthropology. Likewise, it is not surprising that Sherry Ortner, who has been a major contributor to feminist anthropology, should herald this approach as a "new trend that seems to be gathering force and coherence" (1984:144). With the revival of the women's movement in the 1960's, feminists searching for ways to overturn Western patriarchy looked to anthropologists for explanations of sexism, its origins, and for models of liberation. As we were called upon to focus on women's lives and the construction of gender systems, we soon found ourselves asking questions that could not be answered within available theoretical frameworks. Like other social theorists who were tiring of the

sterile formalism of structuralism and the static utilitarianism of structural-functionalism, feminist scholars were dissatisfied with the failure of prevailing theoretical approaches to confront issues of power and inequality. Feminist practice demanded the development of new theory and concepts, and feminist anthropology provided a nurturing environment for nascent ovular, rather than seminal, approaches.

The features of a practice approach identified by Ortner speak directly to the concerns of feminist scholars seeking ways to understand and undermine male dominance.

(1) Ortner observes that a central element of the practice approach is its focus on real people doing real things (1984:144). Feminist practice also fosters a focus on actors--or more accurately, actresses--because it requires that women be treated as social agents. Heeding the call to focus on women, feminist anthropologists soon found that they were largely absent from existing ethnographic accounts or, if present, were portrayed as preoccupied with childcare and housework. As a result, many took as their first task the recovery of women's voices. They searched ethnographies for women's actual words and interviewed women about their activities and their views of social life.

(2) Like practice theorists, many feminist anthropologists combine a focus on real people doing real things with "a view that 'the system' does in fact have [a] very powerful, even 'determining,' effect upon human action and the shape of events" (1984:146). When forced by feminist practice to recognize that women, by their actions, often contribute to women's oppression--through footbinding, female infanticide, infibulation, malicious gossip, or anti-abortion agitation--many feminist anthropologists began to ask why women act against what appear to be their own interests. They soon focused

on "the system" to understand the social construction of women's desires, perceptions, and possibilities (Rosaldo 1980a).

(3) "The system" for these feminist anthropologists, as for practice theorists, is a system of inequality, constraint, and domination--a system that restricts women's possibilities and may lead them to collude in their oppression, even while it enables them to resist and shape the form of male domination. Although many feminist anthropologists search for "egalitarian societies," they usually do so to highlight the inequalities of modern capitalism. Feminist practice is inherently critical.

(4) Because feminist practice calls for changes in gender roles, it highlights the cultural construction of concepts of femininity and masculinity, thus leading many feminist anthropologists to share the assumption of practice theorists that the "system of domination" is a cultural system--that "society and history...are governed by organizational and evaluative schemes...embodied within institutional, symbolic, and material forms" (Ortner 1984:148). Sherry Ortner and Harriet Whitehead have written that "gender, sexuality, and reproduction [are] symbols, invested with meaning by the society in question, as all symbols are. The approach to the problem of sex and gender is thus a matter of symbolic analysis and interpretation, a matter of relating such symbols to other cultural symbols and meanings on the one hand, and to forms of social life and social experience on the other" (1981:1-2).

(5) Feminist practice also leads us, like practice theorists, to question the utility of breaking the system into paired analytical oppositions such as base and superstructure, society and culture, domestic and politico-jural domains, production and reproduction--in which one half of the pair is viewed as determining the other. Whether or not they view the system as a

"relatively seamless whole" (Ortner 1984:148), most feminist anthropologists agree that the seams sewn by traditional Marxists, Durkheimians, and structural functionalists obscure women's participation in politics and men's participation in domestic relations, thus concealing the gendered character of all social relations and the pervasiveness of gender inequality.

(6) Because the feminist political agenda calls for change, feminists have always been interested in distinguishing the processes that promote the perpetuation of systems of gendered inequality from those that facilitate their transformation. Consequently, most feminist anthropologists share the concern of practice theorists to understand "how practice reproduces the system, and how the system may be changed by practice" (Ortner 1984:154).

In summary, feminist anthropologists have been active participants in the development of a practice approach because feminist practice forces us to grapple with the central problem of practice theory: that of trying to understand how the system constructs actresses and actors and how these agents realize and transform the system.

COMMON SENSE AND GOOD SENSE READINGS OF PRACTICE THEORY

In his foreward to the English Edition of Outline of a Theory of Practice (Bourdieu 1977), the translator Richard Nice voices his fears about the misreading to which the text might be subjected. In addition to the possibility that Bourdieu's book might be merged in the reader's mind with the very tendencies it combats in the "structuralism" and "structural-Marxism" dominant in France in the late seventies, Nice feels "there is still reason to fear that the frequent references made to the Anglo-American philosophical tradition--a heaven-sent weapon against the theoreticism which so strongly characterizes French social science, from Durkheim to Levi-Strauss--may, when

returned to their original universe, take on a significance very different from the one they were given in a context in which that tradition is disdained or unknown, and be seen as a sign of allegiance to positivism" (1977:viii). While one might question this overly simplified dichotomization of national philosophical traditions, we agree with Nice that one of the dangers of the attempt to "break out of a scheme of thought as deeply embedded as the opposition between subjectivism and objectivism" is that it may be "perceived through the categories which it seeks to transcend" (Nice 1977:viii).

In particular, we are concerned that an emphasis on agency, strategy, and the interests of individuals in practice approaches can easily lead to an implicit opposition between the "practical" and the "symbolic." Such a scheme overlooks the fact that people's practical concerns and strategies are as culturally constructed as so-called "symbolic" ones and leads us back to old impasses generated by Durkheim's deeply gendered distinction between the sacred and profane.

Common Sense and Good Sense Readings of Strategy

In his analysis of matrimonial strategies and social reproduction in Kabylia, Bourdieu distinguishes official kinship, which the ethnologist often treats as representing the social reality of marriage practices, from practical kinship, "the field of relationships constantly reused and thus reactivated for future use" in which ordinary marriages are contracted (1977:52). Bourdieu opposes official kinship to practical kinship "in terms of the official as opposed to the non-official...., the collective as opposed to the individual; the public...as opposed to the private...; and collective ritual...as opposed to strategy" (1977:35). The collective matrimonial strategy of a lineage is the product of the differing interests of the various

agents in domestic power relations--not all of whom identify their own interests with those of the lineage. In particular, women do not share men's commitment to the "symbolic and political interests" of lineage unity. Instead, economic calculation, which is repressed in men, "finds more overt expression in women, who are structurally predisposed to be less concerned with the symbolic profits accruing from political unity, and to devote themselves more readily to strictly economic practices" (1977:62). Excluded as they are from "representational kinship," women are "thrown back on the...practical uses of kinship, investing more economic realism (in the narrow sense) than the men in the search for a partner for their sons or daughters" (1977:66). Like the poor (1977:213), women are "less sensitive to symbolic profits and freer to pursue material profits" (1977:62).

A common sense reading of Bourdieu's analysis suggests that women operate outside of, and apart from, symbolic systems. The alternative good sense reading would be to view women's commitment to the breaking up of joint ownership and the division of the patrimony as motivated by opposed, but equally symbolic and political, interests shaped by the same system of representational kinship.

A good sense reading may be hard to achieve, however, because the distinction between men's strivings to accumulate symbolic capital and women's economic calculation so easily becomes a symbolic/practical opposition which, in turn, easily becomes Durkheim's gendered sacred/profane opposition in new guise. The problem results from the term "symbol," whose use often conjures up a misleading opposition with the non-symbolic. The dictionary, for example, defines a "symbol" as "something used for or regarded as something else" (The Random House Dictionary of the English Language), thus establishing, by contrast, non-symbols as things which are not used for, or

regarded as, representing something other than themselves. If symbols require interpretation, non-symbols are by this logic, transparent. They are simply what they are.

The conceptual opposition between symbols and non-symbols leads to two related analytic impasses. First, when we define certain actions as symbolic, we risk setting ourselves the task of ferreting out the "true meaning" of these actions--a task which too often results in reducing symbolic action to familiar motivations, such as self-interest or the Oedipus complex. The other side of the coin of this reduction is the presumption that actions not labelled as symbolic have obvious--i.e. pragmatic, and equally familiar--aims. Michelle Rosaldo observed that by "separating the symbolic from the everyday, anthropologists quickly come upon such 'universal' facts as correspond to their assumptions, and fail to see that common discourse as well as the more spectacular feats of poets and religious men requires an interpretive account" (1980b:23). So, many anthropologists continue to view the sexual division of labor in society as an extension of the "biological facts" that set up functional prerequisites rather than as an aspect of symbolically mediated system of social identities.

The concept of "symbolic domination" (Rabinow 1975) or "symbolic power" (Bourdieu 1977:159) can lead to the same impasses as the concept of "symbolic capital." When an anthropologist chooses to call a historical instance of domination "symbolic," she or he risks suggesting, by default, the existence of other forms of domination that do not require cultural interpretation. Just as the term "symbolic capital" can suggest that those who pursue "material well-being" are operating outside of, and apart from, symbolic systems, so the term "symbolic domination" can suggest that those unfortunates

who are subjected to economic or physical coercion experience their subjection outside of, and apart from, systems of cultural meaning.

The conceptual impasse generated by dividing the universe of social practices into the "practical" and the "symbolic" closely parallels the impasse that Asad (1987:605) argues arises out of opposing force (coercion) and consensus (misrepresentation) models of domination. Asad faults Marxist anthropologists such as Bloch and Godelier for assuming that relations of unequal power are essentially dependent either on consensus or on force--or on a combination of the two. He argues that "'force' is not a logical alternative to 'consensus'--that is to the sharing of concepts that define common social conditions. Indeed, we can go a step further and say that the effectiveness of 'force' as a means of domination is itself dependent on a minimal sharing of concepts--as Hobbes long ago pointed out" (Asad 1987:605).

All of these tendencies to differentiate practices into the "symbolic" versus the "practical," the "material," the "economic" or the "coercive" replicate Durkheim's sacred/profane opposition. It is an irony of history--and a testimony to the continuing power of Durkheim's sacred/profane distinction--that practice theory, which deliberately set out to analyze the "practices of ordinary living" rather than the rituals that concerned most of Durkheim's followers, should nevertheless sometimes lead to the labelling of only some actions as symbolic, thus reproducing the sacred/profane opposition.

Feminist practice, however, can help us to recognize, and so steer clear of, the impasses generated by reinventing the sacred/profane dichotomy. Because the dichotomy is deeply gendered, feminist practice reveals its inherent limitations. It is not merely that Durkheim overlooked women's participation in rituals and treated women's tasks as fulfilling biological rather than social needs, but that by defining as sacred those practices that

represented and reproduced certain kinds of collectivities--such as moieties, tribes, and whole nations--Durkheim privileged male domains of action. A central dimension of male dominance in most, if not all societies, is men's authority to define their actions and the social relations they organize as constructing culturally valued collectivities. Durkheim's concern with "official" collective action led him to slight the practices of women and children and, consequently, to relegate them by default to the domain of the non-symbolic. But feminist practice has taught us that women's actions, as well as men's, are formulated and interpreted through symbolic processes.

Common Sense and Good Sense Readings of Individual Interest

Given its focus on the symbolic construction of collectivities, Durkheimian analysis in anthropology has been less concerned with displaying how it is that individuals in different structural positions come to realize the system through their various strategies, much less transform it. In attempting to correct this oversight, practice approaches have sometimes fallen prey to a crude version of interest theory. We agree with Ortner that an "interest theory" reading of practice is fraught with problems, and that it conveys the misleading appearance of filling the need for a theory of motivation in post-Durkheimian social theory.

From the moment feminist anthropologists began to empower female subjects as social agents capable of acting in and upon social systems as well as being constrained by them, we have been vexed by the problem of how to conceptualize the interests of individuals. As we began to question Durkheimian assumptions about the unity of the desires and interests of members of collectivities ranging from domestic groups to social classes, concepts such as "family goals" and "household strategies" became immediately

suspect. Feminist scholars, of course, have not been the only ones to challenge the solidarity of Durkheimian collectivities; so have Marxist scholars and practice theorists. Bourdieu summarizes this challenge in writing that "from the individual family up to the largest political units, the cohesion endlessly exalted by the mythological and genealogical ideology lasts no longer than the power relations capable of holding individual interests together."

Having challenged the mythological ideology holding together collectivities, many anthropologists shifted their gaze to the strategies actors use to pursue their individual interests. The results, however, have often been disappointing, particularly when the actors have been actresses. Too often, as in Bourdieu's analysis of Kabylean matrimonial strategies and in essentialist accounts of women's strategies, the interests of women are assumed rather than explained. This has led some of us to wonder whether we have traded a mythological concept of collectivity for an equally mythological one of individual interest.

A good deal of the problem with the concept of "individual interest" that has risen out of the ruins of Durkheimian collectivities is that a concern with strategic agents making their way in a contentious world can easily lead to assuming that the "interests" of individuals are focused on themselves. Having thrown out a naive notion of altruism, we have too often grasped a naive notion of selfish interests, or at least self-oriented ones. So, for example, the family is construed as the locus of struggle between individuals, each pursuing their egocentric projects.

Such a view, however, blinds us to the collective nature of a good deal of what we call individual interests. We do not mean by this that people are committed to the same collective goals, but rather that the projects

individuals pursue are as often motivated by ideological models of collectivities as by ideological models of self-interest. In any particular social system, men and women may be structurally predisposed to constructing different collectivities. As Bourdieu points out in his analysis of matrimonial strategies, whether these are labelled collective or individual interests is a matter of cultural hegemony. The strategies women sometimes use to destroy the collectivities men pursue and valorize (as, for example, patrilineal extended households and lineages) are often motivated by ideological representations of alternative collectivities.

Moreover, the "selfish" desires and interests that motivate individuals, whether men or women, are no less mythological, ideological constructs than the collective ones. Surely no one knows better than observers of contemporary American society that the interests of individuals in "maintaining one's independence," "getting into shape," and "finding oneself" are as much the ideological constructions of a system of inequality as are the "collective goals" of "family unity." However, because they are attributed to individuals whom we construe as discrete physiological and motivational entities, individual interests have not been as closely subjected to interpretation as collective ones, which are by definition social.

Finally, the interests of individuals change as the possibilities open to them change and they find themselves pursuing projects they had not previously imagined. As Asad (1987:607) writes, "What makes us see and desire new things is the prior reformation of conditions that was only marginally the result of intent." To subject the interests of individuals, whether complementary or opposed, to a systematic analysis that presumes either social reproduction or social transformation is to collapse time--and so to retreat into the very atemporalism that practice approaches seek to transcend. A good

sense reading of practice theory would instead analyze the symbolically-mediated processes through which all interests are constituted by people living in specific historical circumstances and shaped by particular ideological systems.

Common Sense and Good Sense Readings of Reproduction

One of the important attractions practice approaches hold for feminist anthropologists is that they confront a central question of feminist scholarship: how systems of inequality, with all their contradictions and inherent instability, can be reproduced. In viewing cultural systems as simultaneously constraining people and enabling them to resist and shape the system, practice approaches substitute a dynamic instability of struggle and resistance for a static, Durkheimian equilibrium.

Yet some strands of practice theory, like some strands of feminist anthropology, come dangerously close to eroding this analytical advantage by characterizing some spheres of social life as the privileged sites of social reproduction. For example, in her article on "Theory in Anthropology since the Sixties," Sherry Ortner writes that: "...much of systemic reproduction takes place via the routinized activities and intimate interactions of domestic life. To the degree that domestic life is insulated from the wider social sphere...important practices--of gender relations and child socialization--remain relatively untouched, and the transmissions of novel meanings, values, and categorical relations to succeeding generations may be hindered" (1984:157).

This image of an intimate domestic life of routinized gender relations and child socialization, sheltered from the transforming forces of political struggle in the "wider social sphere," is commonplace in Marxist anthropology

and practice theory as well as in structural functionalism. Some Marxist scholars, including many Marxist feminists, for example, assert, without demonstrating, that working class families produce socialized labor for capitalism, thus assuming the socially reproductive nature of domestic relations. Likewise, practice theorists such as Bourdieu argue that the taste and cultural knowledge professional families inculcate in their children enable the latter to reproduce their parent's class status. But because he does not document precisely how this inculcation is achieved, nor demonstrate convincingly that it is a significant determinant of the reproduction of class, rather than part of a mythological model of achievement, this process remains as shrouded in shadows as the mystified process of cultural transmission through child socialization so central to the work of culture and personality theory.

Above all, there is a troubling inconsistency in the way all these models of domestic life treat relations of domination and inequality outside the domestic sphere as opposed to those inside it. While the former are viewed as constituting a site of contention and struggle requiring either forms of coercion or symbolic domination, or both, the latter are viewed as constituting a site for the stable,° unproblematic reproduction of routinized activities and relations, free from social conflict.

This inconsistent characterization of relations of inequality arises from a Marxist privileging of relations of production and from assumptions about the biological basis of relations of production. Although Marx saw production and reproduction as a unified process, there is a strong tendency among some Marxists and Marxist-feminists to follow Engels' distinction between production and reproduction. Once this functional division of social processes becomes conflated with a gendered dichotomization of domains, the

domestic sphere becomes construed as the locus of gendered relations of reproduction while the public sphere is construed as the locus of nongendered relations of production (Yanagisako and Collier 1987).

Commonsense notions about the universal, biological base of reproductive relations then come into play. When cast as logical extensions of the functional requirements of the biology of human reproduction, relations of domination and inequality in the domestic sphere take on a natural, stable, and uncontentious aura. In other words, by eliding a folk model of biological reproduction with an analytical model of social reproduction we end up assuming that domestic relations are inherently conservative.

CONCLUSION

In summary, our argument is that feminist practice has not only provided a primary context for the development of practice approaches in anthropology, as Ortner observes in her footnote, but it can steer us clear of impasses generated by reinventing, in new guises, Durkheim's sacred/profane opposition. Like practice theorists, feminist anthropologists are concerned with understanding the "practical strategies of everyday life," the motives of individuals, and the ways that systems of inequality and domination are reproduced as well as transformed. But feminist anthropologists are perhaps especially alert to the dangers of assimilating new theoretical interests into old conceptual oppositions. Because common sense too easily suggests that "practical strategies" can be opposed to "symbolic" ones, "individual" interests to "collective" rules, and "reproduction" to "production," anthropologists pursuing a practice approach may unwittingly find themselves working within Durkheim's opposition between the profane world governed by biological needs and the sacred world created by man's capacity for symbolic

thought. Feminist anthropologists, through the practice of fighting sexual inequality, have come to recognize that those who use the word "man" too often mean just that. In our continuing struggle against the ideological and analytical privileging of male symbolic action, our interest lies in contending that all human practices are created by people living and acting within historically situated systems of meaning.

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ARE SOCIAL WHOLES SEAMLESS?

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ARE SOCIAL WHOLES SEAMLESS?

In "Theory in Anthropology since the Sixties," Sherry Ortner provides an account "from the actor's point of view" of what other schools of thought look like from a "Geertz-Weberian" perspective (Ortner's phrase, not mine; 130). As she foresaw, I find my favourite school of anthropology, a variant of structural Marxism, "oversimplified, if not outright distorted" (127). She justifies her biased account by claiming that she is interested not in particular approaches themselves, but "in the relations between various theoretical schools." Not surprisingly, however, she is at her best when relating different approaches to her own approach, and at her worst when attempting to relate different approaches to one another. In particular, she misses out the most important interactions between French and British anthropology over the past three decades, and most of this commentary will be taken up with correcting these omissions. I have not been able to resist the temptation, however, of giving an equally biased and oversimplified account of American cultural anthropology from a European perspective in the latter part of the paper. I conclude with a few remarks on what I see as the main theoretical tasks of the coming decade.

CONCEPTUAL SCHEMES AND PRACTICES IN FRENCH STRUCTURAL ANTHROPOLOGY

Ortner's claim that structuralism represents "the denial of the relevance of the intentional subject in the social and cultural process, and the denial of any significant impact of history or 'event' upon structure" (138) is simply wrong, at least as far as Levi-Strauss is concerned. She may well be confusing his thought with that of the Oxford structuralists discussed below.

What structuralism tries to accomplish in the wake of Rousseau, Marx, Durkheim, Saussure and Freud is to reveal to consciousness an object other than itself; and therefore to put it in the same position with regard to human phenomena as that of the natural and physical sciences, and which, as they have demonstrated, alone allows knowledge to develop. Recognition that consciousness is not everything, nor even the most important thing, is not a reason for abandoning it. (Levi-Strauss, [1971]: 629)

Marx, Durkheim, Saussure and their followers start not from the human subject but from the social structures of production, classification and communication which underlie and which make possible all human practice. Ortner is clearly very uncomfortable with any approach to human practices in terms of the unconscious structures they might exhibit, whether these be those of class interest, social classification or grammar. For instance, she notes, with apparent approval, "that there was an early rejection of structural linguistics and a strong move to view language as communication and performance" (144), as if structural linguists would ever have denied this could also be done. Philosophical approaches such as speech act theory or sociolinguistics are not alternatives to structural and generative grammar, they merely use the same data--human speech--for different, and complementary theoretical purposes. More generally, Ortner seems to view structural accounts as incompatible with "actor-centred" and historical accounts. Either language is the generation of meaning by creative human subjects, or it is governed by unconscious rules. Now, as Chomsky among others has endlessly pointed out, the infinite creativity of speech is made possible by an unconscious framework of grammatical rules. She appears to deny the existence of rules which are both collective and unconscious. If she allows a level of psychic reality separate from explicit, conscious meanings it is a privatized Freudian unconscious, full of egoistic drives, desires, anxieties and emotions. The Durkheimian and Marxist traditions stress, on the contrary, in their respective concepts of collective representation and ideology, the fact

that individuals are never fully the authors of what they think and do, but always operate within a field of unexamined assumptions with a social origin, which must often remain unexamined if they are to be able to act at all. The interest of social science for those within this tradition is to analyze the structure of these unexamined assumptions, in the way that a linguist examines the structure of the grammar on which speakers rely.

In fact, if left to themselves, individual consciousnesses are closed to each other; they can communicate only by means of signs which express their internal states...Thus social life, in all its aspects and in every period of its history, is made possible only by a vast symbolism. (Durkheim, 1976 [1915]: 230, 231)

With his shift of focus from individual consciousness to shared symbolism, Durkheim made a decisive break with the old philosophical anthropology of Kant and his successors which was obsessed with the meanings and intentions of the transcendental subject. He pointed out that all meanings must have a material substrate, a signifier, if they are to be publicly accessible, and that the relation of the particular substrate to its meaning, its signified, is essentially arbitrary and established by social convention. Now, what interested Durkheim most about religious symbolism was its practical function, which he took to be its crucial role in the maintenance of social cohesion. It both evoked and communicated internal states of social solidarity. He was less interested in the intrinsic properties of symbolic forms, or what Saussure labelled as the subject matter of semiology. Thus one might say that beginning with Durkheim, French social science viewed conceptual schemes as necessarily both embodied in material symbols and embedded in material and social processes. As we shall see, many of the subsequent developments in French theory occurred around these two axes: of differences in the mode of the embodiment of conceptual schemes and of differences in the mode of their embeddedness.

Drawing on both Durkheim and Saussure, Levi-Strauss attempted to develop a unified theory of social life as systems of communication.

This endeavour is possible on three levels, since the rules of kinship and marriage serve to insure the circulation of women between groups, just as economic rules serve to insure the circulation of goods and services, and linguistics the circulation of messages. ([1958]: 83)

Here and there he notes in passing certain intrinsic differences between these systems, although one wishes he had developed these insights further. For example, he admits that, "words do not speak, while women do; as producers of signs, women can never be reduced to the status of symbols or tokens" (Levi-Strauss, [1951]: 61), the point being that while kinship systems can be approached in certain respects as systems of communication, the analogy breaks down given the different nature of the material embodiment of linguistic and kinship schemes. Kinship schemes may serve to regulate marriage, the transmission of productive resources, succession to office, recruitment to political groups, the circulation of material goods and services and many other material processes requiring the mobilization of concrete psychobiological individuals. The meanings and roles assigned to these individuals may be regarded as largely or even wholly determined by each culture, but the political and economic processes just enumerated require "warm bodies" which cannot be produced out of thin air the way phonemes can be produced.

Systems of marriage and systems of speech have different relations to material reality. Each governs the circulation of objects under a cultural form, but for different ends and under different material constraints. Even when he turned to systems of classification, he recognized differences in their relation to material reality:

Unlike other systems of classification, which are primarily conceived (like myths) or acted (like rites), totemism is always

lived, that is to say, it attaches to concrete groups and concrete individuals because it is an hereditary system of classification. . . In totemism, therefore, function inevitably triumphs over structure. ([1962]: 232)

Now, if Levi-Strauss moved from the investigation of unconscious systems of reciprocity which regulated social processes, such as cross cousin marriage, to classificatory schemes which are lived and so still subject to demographic chance, such as totemism, to purely conceptual systems such as myth, it is because he is ultimately interested precisely in the operations of the human mind in general, and not in particular cultural patterns.

Mythology has no obvious practical function: unlike the phenomena previously studied, it is not directly linked with a different kind of reality, which is endowed with a higher degree of objectivity than its own. (Levi-Strauss, 1964: 10)

Thus the universal axioms and postulates of "objectified thought" uncovered by structural analysis refer to general properties of the human mind, while particular mythical systems are explained as the modification of cultural traditions in response to changes in the infrastructure:

Each version of a myth, then, shows the influence of a two fold determinism: one strand links it to a succession of previous versions or to a set of foreign versions, while the other operates transversally, through the constraints arising from the infrastructure which necessitate the modification of some particular element, with the result that the system undergoes reorganization in order to adapt these differences to necessities of an external kind. (Levi-Strauss, [1971]: 629)

Even in the realm of mythology, Levi-Strauss never imposes a rigid separation between "superstructural" and "infrastructural" phenomena, but argues for their continual interaction. Further, it is a basic postulate of Levi-Strauss's structuralism that structure is discoverable only through the study of transformations as they occur between neighbouring societies or through time.

Despite Levi-Strauss's refusal to separate conceptual schemes from the material signs in which they are embodied or from the material functions and

purposes they serve, his choice of functions (circulating, thinking, communicating, acting, conceiving, classifying) is strikingly one-sided. What he neglects are cultural knowledges and practices that have as their object the appropriation and transformation of the material world, in other words, the "labour" of production and reproduction. These are vaguely allocated to an "infrastructure" always treated as itself unproblematic. In fact he vacillates between viewing this infrastructure as purely material and inert in character, the objective constraints of the natural environment, demography, and so on, and as also being part of culture, as including tools, technologies, or even as "lived-in experience" (1971: 629). Given his explicit preoccupation with anthropology as a form of general psychology, a search for universal properties of the mind through ethnographic comparison, this neglect is understandable, and perhaps even legitimate. This preoccupation has been followed up by Sperber, who has also done a brilliant job of disentangling Levi-Strauss's real discoveries in the realm of symbolism from some of his more misleading statements (Sperber, 1975).

For those concerned with anthropology as comparative sociology, however, this concern with general psychic mechanisms is clearly inadequate. What the former require is precisely a theory of the relation of cultural form to material effect. Another way of stating this is in terms of "structures" and "functions", so long as the material functions are understood as historically variable effects of conceptual structures, and so long as it is recognized that both have their own inner logic and historicity.

One intellectual tradition that has always stressed human transformation of nature rather than its contemplation is, of course, that deriving from Marx, and it was from Marxism that French anthropologists gained a renewed

interest in material production and reproduction. Now Marx is quite clear that labour is always guided by a conceptual scheme.

We presuppose labour in a form that stamps it as exclusively human. A spider conducts operations that resemble those of a weaver, and a bee puts to shame many an architect in the construction of her cells. But what distinguishes the worst architect from the best of bees is this, that the architect raises his structure in imagination before he erects it in reality. (Marx, [1867]: 174)

There is thus no reason to oppose productive labour to symbolic communication as "material" to "mental": both involve the application of conceptual schemes to material substances, the one transforming them, the other not. As Levi-Strauss put it in a famous passage.

Marxism, if not Marx himself, has too commonly reasoned as though practices followed directly from praxis. Without questioning the undoubted primacy of infrastructures, I believe there is always a mediator between praxis and practices, namely the conceptual scheme by the operation of which matter and form, neither with any independent existence, are realized as structures, that is as entities which are both empirical and intelligible. (Levi-Strauss, [1962]: 130)

Beginning with Althusser, French Marxists attempted to shift the dominant metaphor within structuralism from "circulation" to "production", from "thought" to "practice".

We can assert the primacy of practice theoretically by showing that all the levels of social existence are the site of distinct practices: economic practice, political practice, ideological practice, technical practice and scientific (or theoretical) practice. . .We think the relations establishing and articulating these different practices one with another by thinking their degree of independence and their type of 'relative' autonomy, which are themselves fixed by their type of dependence with respect to the practice which is "determinant in the last instance": economic practice...We regard an element of "knowledge", even in its most rudimentary forms and even though it is profoundly steeped in ideology, as always already present in the earliest stages of practice, those that can be observed in the subsistence practices of the most "primitive" societies. (Althusser, [1968]: 58)

Althusser here attempts to overcome the sterile opposition between "theory" and "practice" by showing that every practice implies some form of knowledge,

and all theorizing is a form of practice. What differs from one practice to another are the objects, means and relations of production involved in carrying them out. But he and his followers (such as Balibar) remained wedded to a mechanical notion of determination by the economy, a tendency to reduce the economy to the techno-environmental conditions of production, and an ahistorical view of modes of production. These were all parts of a general "theoreticism" which they later repudiated, and which derived in no small part from their position as philosophy professors at the Ecole Normale Supérieure (seat also, be it noted, of the contemporary guru Derrida).

Other students of Althusser, such as the historian Foucault, or the anthropologist Terray, were more involved in empirical research. The latter, together with other Marxists such as Rey, Meillassoux and Coquery had first hand experience with non-capitalist societies during their fieldwork in West Africa. As with many British anthropologists they worked in functioning colonial or post-colonial societies and were concerned from the beginning with issues of power and wealth. They were among the first in the mid-sixties to introduce historical material in a theoretically rigorous manner and to discuss the "functional articulation" of modes of production in a manner far more sophisticated than the latter "world systems theorists". Even a cursory reading of their work on the "lineage mode of production" in Africa and the effects of the slave trade makes nonsense of Ortner's statement that "structural Marxism was largely nonhistorical" (141: see, for example, Terray, 1974, 1975, Coquery, 1975; Meillassoux, 1971). At the same time, they were actually quite close in some of their concerns and their theoretical concepts to the Cambridge anthropologists, which is hardly surprising given the fact that Fortes and Goody both worked in West Africa as well. A point overlooked in Ortner's paper is that French structural Marxists took at least as much

from British "structural functionalists" in order to upset the structuralist apple cart as the younger generation of British took from the French, just as Mauss had had drawn on Malinowski to revise Durkheim. The rhetoric should not blind us to the fact that "Althusserian" students of Balandier often had more in common with Goody than with Levi-Strauss.¹

The writings of Bourdieu must be read in terms of this tradition. Far from representing an ally of the subjective humanists, he merely insists that Levi-Strauss's cognitive interests distort the ethnographic analysis of certain types of practice, in particular, the practices of reciprocity in gift exchange and marriage, and the perpetuation of political and economic subordination in non-capitalist societies. All of these involve consideration of the temporal dimension of practice which is likely to be ignored in the reversible diagrammatic world of structuralism. He is not advocating a return to what he calls the "naive humanism" that criticizes "scientific objectification in the name of 'lived experience' and the rights of 'subjectivity'"(4). His continuing proximity to the Althusserian tradition can be gauged, despite his call for a break with Marxist theory, in the following quotation from a recent paper:

In reality, the social space [social formation] is a multi-dimensional space, an open set of fields [practices] that are relatively autonomous, i.e., more or less strongly and directly subordinated [in the last instance?], in their functioning and their transformations, to the field of economic production.
(Bourdieu, 1985: 736)

One of the obvious dangers of structural Marxism was that it was liable to collapse back into the same sort of circular arguments which characterized classical structural functionalism. The only real difference then would be the stress on material reproduction rather than on social cohesion or on basic needs as the telos of the system. Everything depends on whether the material effects of cultural forms such as kinship or religion are interpreted as if

they were designed to perpetuate the relations of production. As Bloch has argued, the only way to escape from this is to look at how specific cultural forms have actually functioned in history, whether they have actually been used to advance (culturally defined) individual or group interests, whether they have undergone internal transformations, whether they have always had the same political and economic effects.

STRUCTURE AND FUNCTION IN BRITISH SOCIAL ANTHROPOLOGY

In mainstream British social anthropology, paramount interest has always been in those conceptual schemes which have a prominent role in regulating central material functions, essentially in kinship as a means of organising the economy and polity in stateless societies. As I shall discuss further below when comparing them with American cultural anthropologists, there was a reluctance among those working in still partially autonomous colonial societies to recognize a level of cultural reality separate from the continuing concerns of social and economic life. So long as religious ritual appeared to play little role in this life, it tended to be left on the sidelines, or included only to the extent that it could be shown as a means of attaching individuals to socially valuable sentiments, of moving them from one social status to another, or only insofar as an economic (e.g. surplus generation) or political (e.g. legitimation of ruler) function could be found for it. The major exception to this was the Oxford version of structuralism developed by Evans-Pritchard and his followers in the fifties and sixties. I shall have more to say on this below.

Certain teleological aspects of the mainstream tradition, i.e. its tendency to reduce practices to the role they played in reproducing the social whole, were already being criticized by Leach and Barth in the fifties.

Unfortunately, these criticisms were articulated in the name of the maximizing human subject of formal economics and led many writers off into the dead end of transactionalism.² One of the key problems raised by the criticisms of both Oxford and Cambridge structuralism was precisely the extent to which the systems of ideas and the social functions in any society were both fully integrated and all of one piece, i.e. the extent to which society was "seamless". Different writers qualified the coherence of the system in different ways, Firth introducing a distinction between social structure and social organization, Evans-Pritchard, Fortes and others arguing that some institutions were more vulnerable to outside disruption than others, and so on. As Bloch pointed out in his Malinowski lecture, the assumption, made by authors like Geertz or Needham, that people operated with a single, integrated conceptual system ruled out the possibility of their criticizing or changing the system in which they lived (1977). He raised the question of whether all conceptual schemes are acquired, respond to historical events, and accomplish their tasks in the same way. In a series of papers and in his latest book, he proposes a very general distinction between the acquisition of conceptual schemes in ritual and in everyday contexts, noting the highly restrictive conditions placed on communicative codes and the often deliberate assault on everyday knowledge in rituals (1974, 1985, 1986).³ He has recently shown how one particular cultural form, the circumcision ritual of the Merina, has taken on a whole series of different functions in the past two hundred years.

From the formal point of view, the ritual seems to have altered surprisingly little in its symbolic aspects: the ritual acts, the songs, the objects used. On the other hand, if we take a functionalist theoretical perspective, which stresses transformations in the the ritual's role in the organization of the social and economic system, the ritual seems to have changed fundamentally - passing, for example, from a descent-group ritual to a royal ritual and back again. (Bloch, 1986:157).

The questions raised by Bloch were part of a more widespread interest in the acquisition of conceptual structures, inspired in part by the work of developmental psychologists like Piaget and those putting Chomsky's theories of innate linguistic structures to the test. Barth, for example, devoted his New Guinea ethnography to the examination of how ritual knowledge is actually transmitted in the course of rituals.

I have argued that such a corpus [as Baktaman knowledge] will only persist to the extent that its parts are frequently re-created as messages and thereby transmitted. The immediate determinant of such messages may be described as social praxis. The mutual feedback between thought and action, culture and society, may thus best be approached through social organization. (Barth, 1975: 255, see also Sperber, 1985)

In Britain in the seventies, the structural Marxism made possible by the work of Levi-Strauss and Althusser seemed to offer a means of breaking out of the circularity of some structural functionalist reasoning, of giving a fuller account of the structure and function of indigenous conceptual schemes, without reducing the one to the other, and of escaping the sort of antiquarian Orientalism implicit in the cultural relativism of the Oxford structuralists and Chicago symbolists. Following Bloch's lead, we have been investigating the changing material functions of conceptual schemes over time. David Lan, for example, has shown how the ideology of spirit mediumship played a crucial role in mobilizing support for the nationalist guerillas during Zimbabwe's second war of independence, while I have shown how certain concepts of group solidarity generated in Buid spirit seances have enabled them to organize in defence of their land and autonomy (Lan, 1985; Gibson, 1986; see also the earlier work by Fry)).

The point I have been trying to make in regard to the dialogue between British and French anthropology over the last three decades, is that there has been a progressive movement away from a view of society as a seamless whole.

This movement may be found both within the writings of individual anthropologists and within the discipline as a whole. At the level of "meaning", or conceptual schemes, such unity as the cosmology of a society possesses is always a second or third order attempt to construct an overall system out of conceptual schemes with divergent material embodiments and purposes, and it is always retrospective in nature, a mythological reinterpretation of the historical and "infrastructural" events which continually destabilize the cultural structures on which they impinge. At the level of "functions", or material effects, the degree to which practices actually contribute to the "reproduction" of key social relations, particularly those of power and hierarchy, has come to be viewed as an open question which can only be answered through historical research.

II

Quite at variance with these developments at Cambridge and the London School of Economics was the more "idealist" approach prevalent at Oxford. As early as 1950, Evans-Pritchard wrote:

The thesis I have put before you, that social anthropology is a kind of historiography, and therefore ultimately of philosophy or art, implies that it studies societies as moral systems and not as natural systems, that it is interested in design rather than in process, and that it therefore seeks patterns and not scientific laws, and interprets rather than explains. (Evans-Pritchard, 1962 [1950]: 26)

There is an older tradition than that of the Enlightenment with a different approach to the study of human societies, in which they are seen as systems only because social life must have a pattern of some kind, inasmuch as man, being a reasonable creature, has to live in a world in which his relations with those around him are ordered and intelligible. (*ibid.*: 28)

While Ortner clearly cannot cover everything in her paper, her omission of the autonomous development of the Oxford school of "structuralism" is crucial because it allows her to confuse their concern with intelligible patterns with

Levi-Strauss's concern with structural transformations.⁴ The distinction between the two structuralisms is all the more necessary since a Frenchman, Dumont, is actually closer to Oxford than to French structuralism (cf. Dumont, 1968: 329). While structure for the latter is only discoverable through a comparison of several transformations, and is better described as a process of the second order structuring of primary knowledge, for the former structure is a fixed pattern of values and beliefs inherent to a culture and stable for centuries if not millenia. Thus Dumont on "Hindu hierarchy", and Needham and his students Barnes and Forth on dual classification and circulating connubium in Eastern Indonesia, assume "a general concordance throughout all phases of [the] conceptual order" (Barnes, 1974: 305) in the collective representations of Hindus and Indonesians, based on the abstract concepts of hierarchy and dualism, respectively. It is this sort of British structuralism which provides an analogue for American symbolic anthropology, as Ortner notes (137), but it must be recognized that it had more to do with Evans-Pritchard than with Levi-Strauss. It produced the same kind of "hypercoherent" account of cultures that Geertz came to criticize among his own followers (Ortner, 144)

"ACTION" AND "BEHAVIOUR" IN AMERICAN CULTURAL ANTHROPOLOGY

Sociology is a science which attempts the interpretive understanding of social action in order to arrive at a causal explanation of its course and effects. In "action" is included all human behaviour when and in so far as the individual attaches a subjective meaning to it. (Weber [1922]: 88)

In this section I want to argue that Ortner's use of the term "practice theory" is misleading because it has less to do with the concept of practice as it is used by structural Marxists, and more to do with Weberian "action theory", which she has inherited via Parsons and Geertz. It is greatly to be

doubted that "most modern practice theorists including those who write in Marxist and/or structuralist terms, hold an essentially Geertzian view" of the way "culture" shapes "behaviour" (152). There is an enormous difference between practice as understood by structural Marxists and "action" as understood by hermeneutic Weberians.

A number of initially puzzling emphases in Ortner's paper make sense only when viewed in this light. Some examples are her astonishing inclusion of sociobiology as part of the "general trend" toward an emphasis on "intentional choice on the part of actors to maximize" (146); her view that "the system" constrains "practice" through "essentially cultural and psychological mechanisms" rather than through "constraints of material and political sorts" (153); and, in general, her insistence on the centrality of psychological anthropology to "practice theory" (151). She favours the injection of more emotion and passivity into what she views as the overly rational and energetic picture of human motivation drawn by "interest theory" (151-2).

But there is a reason why "action theorists" tend to derive their "motivation theory" from "interest theory" (151). It is that the maximization of a single value such as power or wealth constitutes intentional action in its purest form. Since the "sciences of social action" are distinguished from the natural sciences by their ability to use teleological explanation, their role in explaining human action increases to the extent that human action becomes oriented toward explicit goals and adapted toward achieving those goals with the greatest efficiency. The coherent world-views of the great religious innovators represent the best examples of action oriented to a coherent set of ultimate ends, while the profit-maximizing behaviour of the capitalist business man represents the best example of efficient achievement

of an explicit end. Weber's concern for rational, goal directed behaviour follows logically from his definition of the sciences of social action, and from his neo-Kantian division of the world into two types of objects and events: those governed by objective causality and those governed by subjective causality.⁵

In the limited time available I can do no better than to quote from the cogent critique of Weber made by Hirst:

Weber never defines the content of the term "subjective meaning". Subjective meaning is pre-social or not necessarily social; it precedes the definition of social action in the logic of the discourse and helps to establish it. Meaning in this sense cannot be the product of language or other signficatory system, for it would then be already social or other-regarding . . . Weber's subjective meaning must therefore be significance or value for a pure consciousness. It is pre-social and pre-linguistic, a property of consciousness. Weber's subject is the pure subject of classical philosophy - a pure pre-social consciousness. (Hirst, 1976: 69-70)

However much Weber's American followers, such as Geertz and Schneider, have attempted to "socialize" symbolic meaning, they too remain fundamentally oriented toward the interpretation of the subjective meanings which individuals attach to particular symbols, and which come to be widely shared through "socialization". Socialization is viewed as a process of imbuing individual subjects with the ideas and values appropriate to the "cultural" and "social" systems. The unit remains the individual subject, rather than the differential reproduction of social practices or transmission of conceptual schemes in specific types of social situation.

Parsons and his followers did not just appropriate Weber's "science of social action", they combined it with a concept of culture developed in America. The point has often been made, but it bears repeating in this context, that the division of labour between anthropology and sociology worked out by Kroeber and Parsons, as that between the study of "culture" and of

"society", seemed plausible because of the peculiar view of culture developed by those who had worked either in museums or with Native North Americans whose societies had often been severely disrupted by demographic decline, forced migration, and the thoroughgoing imposition of alien political and economic forms. What was preserved for analysis tended to be just those aspects of "culture" most easily reduced to "texts", such as language, ritual, myth and art. The whole notion that "cultures" are "texts" demanding humanistic interpretation descends from this kind of field research, and is quite foreign to the European anthropological tradition which dealt with colonized societies whose conceptual schemes were still functioning, to greater or lesser degrees, to organize economic and political practices. It is only in this context that an "emphasis on the practices of ordinary living" can be viewed as part of "the newer practice approaches" (154). A particularly striking example of the fetishization of formalized cultural performances appears in Geertz's 1980 book on "The Theatre State in Nineteenth Century Bali", where he states:

The state drew its force, which was real enough, from its imaginative energies, its semiotic capacity to make inequality enchant. (1980: 123)

This sort of approach, which combines the need to "interpret" subjective meanings with a view of "cultures as texts to be read", naturally leads to a view of anthropology as one of the humanities, to be pursued with humanistic methods for humanistic ends. It is hardly surprising that American anthropology, along with American literary criticism, has thus provided fertile ground for the spread of the least practice-oriented, most apolitical forms of French "post-structuralism". Indeed, these forms are more popular here than in their homeland.

Now, it is true that Ortner appears to endorse the fact that in "practice theory" "the system is not broken up into units like base and

superstructure, or society and culture, but is rather a relatively seamless whole", and "the analytic effort is...to explain the system as an integral whole...by referring it to practice" (148). But as we have seen, this practice is actually the intentional action of a subject, whose motivations are inculcated through socialization, and so on. The seamlessness of the system is due to what Althusser calls "expressive causality".

As Ortner notes, American anthropology has long been riven by a split between those in the Parsonian tradition and those who totally reject its definition of sociology in favour of a "natural science of society" approach. Among these latter are cultural ecologists such as Lee and Rappaport and "cultural materialists" such as Harris. They begin by defining culture as the specifically human means of adaptation to the environment, and go on to analyze human "behaviour", which they admit to be governed by cultural as well as genetic codes, as an ethologist analyzes animal behaviour, i.e. in relation to how it contributes to subsistence and reproduction. They claim further to have given a causal explanation for behaviour by showing that it has beneficial material consequences, while at the same time denying the causal relevance of intended effects.⁶ The circularity of this sort of cultural functionalism, the illegitimacy of its appropriation of concepts such as adaptation and behaviour from biology has been pointed out again and again. In the extreme form of socio-biology, this approach becomes a caricature of scientific argument. Hence the general distaste of the "action theorists" for what they view as the scientistic objectivism of the behaviourists, although it is also possible for an outsider to sympathize with the distaste of the behaviourists for the sometimes frivolous humanism of the more extreme activists. Both camps share, however, a view of "the system" as an integrated whole generated by a single underlying principle, whether it is defined as an

objective cybernetic system of energy flows, or as a subjective system of symbols and meanings.

The opposition between those who stress the material effects of behaviour and those who stress the meaning of symbols derives in part from the dissociation of conceptual schemes from material practices which goes back to the origins of American cultural anthropology. In my view, some of the greatest contributions to theory have come from that current of American anthropology which ignored both these extremes, and combined the study of a culture's relation to its natural environment with the study of cognitive schemes, i.e. the "ethnoscience" school which is completely ignored by Ortner.⁷ Conklin, for example, has concentrated on the practical knowledge people have concerning the world about them, knowledge which they need to be competent swidden farmers or wet rice cultivators (1956, 1980). Others, such as Berlin and Kay, have searched for cognitive universals. The work of these and other "ethnoscience" may be open to criticism in terms of the particular psychological theories one or another has adopted, but their demonstration of the fantastic amount of empirical knowledge present in non-literate societies, and the limits they demonstrated for the cultural relativity of conceptual schemes, were crucial for the development of French structuralism. As I have argued, the "infrastructure" as understood by Lévi-Strauss, Sperber and Bloch really consists in just this kind of "encyclopedic" knowledge of the world, while the "superstructure" of myth and ritual symbolism constitutes a second order elaboration based upon, but irreducible to, this primary knowledge.

CONCLUSION

In my opinion, the theoretical tasks for the future require, first, that we take as our unit of analysis not the individual and his or her conscious or preconscious tactics, strategies or motivations, but the conceptual scheme as both materially embodied and embedded, as transmitted in specific types of social situation to specific categories of person, and as developing unevenly with other conceptual schemes through time due to differences in the nature of their respective embodiments, functions, situations and personal carriers.⁸

Second, we must approach the renewed interest in the "construction of self, person, emotion and motive" with caution, and not confuse it with psychological anthropology (151). Accounts of ethnopsychology should be treated in the same way as accounts of ethnozoology or ethnobotany: the subject matter of the latter is no more that of biological anthropology than that of the former is the subject matter of psychological anthropology. Thus while I would argue that individual human motivations cannot be used to explain social phenomena, I would agree that variations in socially recognized types of motivation, both within and between societies, are a legitimate, even central, object of investigation. Further, variations in the degree to which self-aggrandizing individual strategies and initiatives are recognized as legitimate may often provide us with a sketch of the power structure within a society. Thus the structurally defined degree of legitimate maneuverability will vary not only by social situation but also according to the social class, gender, and ethnicity of the individuals involved. The sort of stratified society with a weak or non-existent state and minimal constant capital described by Bourdieu among the Kabyle, and which is also characteristic of much of island Southeast Asia, for example, demands a great deal of deliberate

manipulation and strategizing by would-be big men. As Bourdieu argues, this is because:

Once a system of mechanisms has been constituted capable of objectively ensuring the reproduction of the established order by its own motion, the dominant class have only to let the system they dominate take its own course in order to exercise their domination; but until such a system exists, they have to work directly, daily, personally, to produce and reproduce conditions of domination which are even then never entirely trustworthy. (Bourdieu, 1977 [1972]:190).

Thus in capitalism, the concentration of the means of production in the hands of the capitalist is reproduced without the capitalist having to personally defend his property, while among the Kabyle or the Swat Pathan (Barth, 1959) every big man must continually struggle to maintain his political position. The capitalist must, on the other hand, continually struggle to expand his market and reduce his costs of production if he is not to lose his economic position. Societies like the those of the !Kung or Buid, on the other hand, may attach a strongly negative value to overt displays of competitiveness or self-advancement by any person in any situation, except, perhaps, the romantic. In other societies like the Nuer or the Iban all adult men may be expected to compete militarily. And so on. Every society, in short, assigns a different place to individual strategizing.

Third, we must develop more sophisticated accounts of the behaviour of conceptual schemes in history. This will demand ever closer cooperation between theoretically minded social historians and social anthropologists. Such cooperation has been developing rapidly in Southeast Asian studies, as hybrid works by authors such as Tambiah (1976), Ilete (1979), Warren (1981) and Bloch (1986) show. Some of the issues raised by these authors are: the way traditional rituals are transformed into ideologies legitimating the growth of states and empires; the way societies retaining a relative autonomy in their conceptual schemes, particularly those governing political and

religious situations, participate in regional or world economic systems integrated only at the level of commodity exchange and military domination;⁹ the use of traditional religious symbolism to mount challenges to the political order; and the genesis of indigenously based multi-ethnic empires on the fringes of and in response to the world economic system.

Fourth, in addition to the historical approach just mentioned, many of these same issues may be profitably approached from a regional, inter-societal perspective, since it is precisely where groups of people who share relatively few conceptual schemes are interacting on the levels of politico-military depredation and commodity exchange that the functional articulation of autonomous conceptual schemes can best be observed. A propos of the second point above, the relative success of certain societies in a region may rest in part on the degree to which individual status within one society is achievable by means such as success in war or long distance trade, providing a socially constituted motivation for foreign trade and conquest.¹⁰

NOTES

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1 Godelier was closer to Levi-Strauss than to Althusser, and through a syncretic tour de force he attempted to integrate the traditional concerns of British structural functionalism, a cybernetic view of structure, and Marxism with Levi-Strauss's insights into the structure of systems of classification:

[It] is not enough for an institution such as kinship to assume several functions for it to be dominant within a society and to integrate all levels of social organisation, all the parts of society, as the functionalists would say. Over and above this, kinship must also function as the system of the relations of production regulating the rights of groups and of individuals in respect to the means of production and their access to the products of their labour. . . Levi-Strauss's structural analysis explains the logic of forms but ignores the logic of functions. (Godelier, 1975: 14, 15)

His student, Friedman, coming from a background in American cultural ecology, put it another way:

It is absolutely necessary not to confuse the levels of functioning of a social formation with the cultural institutions that take on those functions. What appears as "religion" in terms of a number of inherent cultural characteristics might function as

a superstructure in one society and as relations of production in another. . . Money capital has the same internal properties whether it is restricted to children's games or dominates the process of production. (Friedman, 1975: 163)

2 Transactionalism may in fact be regarded as a British analogue of sociobiology, albeit a far more sophisticated one, in that both of them edit culturally relative conceptual schemes out of their analysis, and replace them with the strategies of a culture-free maximizing agent.

3 Ortner, incidentally, is quite mistaken in her representation of the Marxist concept of ideology as a "narrowing of the culture concept" (140), or in her assumption that "culture (= 'ideology')" for Bloch (153). If one had to attempt a translation into Ortner's theoretical language, one would have to say that ideological schemes are a subset of cultural schemes, and have certain properties, such as being transmitted in certain formalised social situations, contradicting common sense knowledge, and stability through time (see also the quotation from Althusser on different types of practices above). Culture also includes non-ideological schemes, which may or may not be compatible with current scientific theory, but which are acquired in a different type of situation and which are formally more flexible or "creative" in response to new situations. These would include the sort of schemes investigated by the ethnoscientists, for example.

4 This omission is all the more surprising considering the importance she attaches in her monograph on Sherpa ritual to Divinity and Experience by Godfrey Lienhardt, a product of this tradition. It must be said that the best work in Britain on religion was produced at Oxford until French structuralism made a real impact in the late sixties.

5 Weber was the first to admit that not all human action is subject to cold, rational calculation, but that much of it is "emotional":

The more readily we ourselves are susceptible to them the more readily can we imaginatively participate in such emotional reactions as anxiety, anger, ambition, envy. . . Even when such emotions are found in a degree of intensity of which the observer himself is completely incapable, he can still have a significant degree of emotional understanding of their meaning and can interpret intellectually their influence on the course of action and the selection of means. (Weber [1975]: 92)

It is always much more satisfactory, however, to build ideal constructs of rational actions, where it is much easier to achieve "adequacy on the level of meaning".

We apply the term "adequacy on the level of meaning" to the subjective interpretation of a coherent course of conduct when and in so far as, according to our habitual modes of thought and feeling, its component parts taken in their mutual relation are recognized to constitute a "typical" complex of meaning. (Weber, 1975: 99)

Internal coherence and adequate subjective grounds for action are much clearer in rational than in irrational action, and it is the search for these that define sociology. Ortner's plea for the consideration of more complex "motivations" such as "need, fear, suffering, desire, and others" (151) was anticipated by Weber, but has met with little effectual response since his day, for the reasons I have given.

6 Animal behaviour can be "explained" in terms of its material consequences because one can safely assume that genetically determined behaviour which produced deleterious effects has been eliminated by natural selection over long periods of time. The cultural materialist claim that culturally determined behaviour can be "explained" in the same way rests on the assumption that conceptual schemes unfavourable to subsistence and reproduction are eliminated by natural selection in just the same way as genes. This assumption is highly dubious, given the relatively fast rate of cultural as compared to biological change, intra-generational learning, and the coexistence of very different cultures in the same natural environment.

7 Other writers who deserve more attention than I can give them here are "political economists" such as Wolf and Mintz, whose early focus on peasant societies caught up in national political movements and the world economy anticipated and indeed stimulated much current writing on these matters.

8 Foucault's call for a theory of strategies without subjects may be understood in this sense: certain practices require those engaged in them to continually expand the sphere of application of those practices. For Foucault, power relations are embodied in the discursive practices of the disciplines and are embedded in the non-discursive practices of institutions. These disciplines and institutions produce both a certain type of known object and a certain type of knowing subject: neither the motivations of the subject nor the structure of the object can serve as the point of departure in historical explanation.

9 It is quite true, as Ortner states, that using "history" as a cover term only hides important distinctions between theoretical approaches (159). Indeed, each approach will generate its own sort of historical methodology. The Oxford school has long adhered to a sort of historical particularism as a means of recovering primordial cultural patterns which have broken down under the impact of modernity, or as a means of demonstrating the continuity of the unique world views through the ages (cf Dumont, 1972 [1966]: 242).

10 A related situation arises "when class differences are also, historically, cultural differences", as a result of colonial conquest (cf Ortner, 155). In this case the "culture" of the masses may have a greater relative autonomy from the "culture" of the rulers than in cases where class differentiation arose more gradually within a society, and may allow for the more violent structural changes if an indigenous class of rulers is able to replace the foreign one. But it may equally be the case that local conceptual schemes are simply irrelevant to the objective functioning of power and succeed in producing only futile millenarian type movements, while dominated classes with a more accurate understanding of the way the dominant system operates are more successful in undermining it.

Anthropology Since the 60's, Theory for the 90's?

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Anthropology Since the 60s, Theory for the 90s?

In "Theory in Anthropology since the Sixties," Sherry Ortner (1984) contrasts the current anthropological interest in "practice"--an orientation focusing on various actor-oriented issues of "praxis, action, interaction, activity, experience, performance" (p. 144) and on historical issues of "time, process, duration, reproduction, development, evolution, transformation" (p. 158)--to earlier perspectives emphasizing essentially ahistoric structures of rules and roles constraining human thought and action. Such structural approaches, she argues, "established the reality of the thinglike nature of society, but . . . failed to ask, in any systematic way, where the thing comes from and how it might change" (p. 159). In general, she sees a transformation since the 60s from an emphasis on "structures and systems to persons and practices" or from "static, synchronic analyses to diachronic, processual ones" as positive and theoretically invigorating (p. 158). She explicitly notes (p. 158) that her historical discussion is far from a disinterested inquiry. Rather, her selective discussion of various schools, approaches, and meta-orientations suggests that the most fruitful line for future anthropological research is the elaboration of a general, unified theory of practice (at present less a coherent theory than an emerging set of concerns) that will explore, in a systematic way, complex questions about "the impact of the system on practice, and the impact of practice on the system" (p. 148).

Much could be said about Ortner's discussions of particular schools and approaches, both within periods of time and across time. Her view of anthropology since the 60s as a field in transition, even fragmentation, is incontestable. Marxists, self-reflective new ethnographers, world systems

proponents, postmodern discourse analysts, and feminist anthropologists often seem to lack a common ground even for fruitful arguments. Here, however, I would like to explore further Ortner's historical interpretation of the causes and meanings of this fragmentation. My own selective comments are themselves far from disinterested. This paper aims to present an alternate historical analysis, with fundamental implications for understanding the current state of anthropological theory and for suggesting fertile areas and directions for future theoretical and ethnographic work. Basically, I will be questioning the possibility and desirability of developing a unified, general theory of practice at all, at least in the sense that Ortner suggests in her paper. I argue that her reflections on general relations between "the system" and "the practice" of human beings are largely framed in terms of one particular culture's construction of the world, one particular dominant mode of bringing together outside and inside, objective world and interested actor. A more powerful theoretical agenda for the 90s would involve rethinking the problematic, but still fundamental anthropological notion of "culture" in order to explore and systematically compare very different modes of making and unmaking humanly constructed, historically developing worlds. Within different sorts of cultural worlds, the relations of "structure, agency and history" take on very strange and convoluted shapes that a general theory of "the system" and its relation to "practice" does more to obscure than to illuminate.

Ortner does not systematically explore the links between global social, economic and political transformations and changes in anthropological theory since the 60s, but she does suggest (p. 138) that radical social movements of the late 60s--for example, the counter-culture, anti-war and women's movements--prompted many anthropologists to ask questions about their own

social world, the ways it was changing, possibilities for conscious intervention in social change, and the degrees to which our theoretical frameworks "embody and carry forward the assumptions of bourgeois Western culture." I suggest that the foundations of a more powerful and systematic analysis of the social/historical context of anthropological theory since the 60s can be framed in terms of general theories of "late capitalism" as a fundamental reorganization of capitalist structure and practice dating from the late 60s and early 70s. (See Harvey 1985a and b, Mandel 1987.) Such theories begin to help us to see why the relations between "structure and history" or "structure and practice" are so deeply problematic to us now and why many anthropologists, concerned with the experiences and perceptions of people "on the ground," should be more concerned today with questions of persons, practices and history than with the delineation of coherent social or cultural systems.

David Harvey (1985 a and b) discusses major shifts in the political economy of "late" capitalism that seem to involve a new mode of capital reproduction and accumulation and new sorts of "structured coherences" of time, space, urban life and social consciousness. After a long postwar boom, with its relatively coherent structures of big business, industrial production and controlled markets and its "standardised mode of capital accumulation," a series of economic and political crises in the early 70s led to a new "regime of flexible accumulation," characterized by a startling diversity and flexibility of labor processes, labor markets, products and patterns of consumption. New "coping mechanisms"--from black and underground economies to systems of home work and a vast array of small entrepreneurial endeavors--introduced new ways of producing goods and especially services. The move to late capitalism involved rapid shifts in traditional patterns of uneven

development--both between sectors of the economy (such as skilled manufacturing and unskilled service sectors) and between geographical regions (cities, regions, states and global sectors)--that were aided by the rapid evolution of new financial systems and markets. Relations among industrial, merchant, property and finance capital shifted drastically, as new and expanded credit hierarchies greatly increased the power of finance capital and contributed to the development of flexible, small-scale, competitive economic ventures on the ground. This new regime of flexible accumulation has been accompanied by political agendas stressing the virtues of free market competition and government deregulation.

A full discussion of the recent transition to late or advanced capitalism (or post-industrial, postmodern or even, in some formulations, post-capitalist society) is obviously far beyond the scope of this paper, but it is clear that such a shift involves a multitude of contradictions in people's everyday experience and practice. However, Harvey and Mandel, among many others, would argue that while the dissolution of previously stable structures and moves to flexible practice are the most immediately perceived aspects of a late capitalist transformation, we can see behind free market competition, deregulation and innovative economic action--or rather, as operating through them--key government interventions and vast waves of mergers, corporate consolidations, and linkages between supposedly rival firms in automobile, electronics and financial firms. These suggest not so much a destructuring move to flexible practice as a world-wide restructuring of an ever more concentrated and centralized capitalist system. Harvey's argument is that late capitalism is a structure ever more tightly organized through dispersal, geographic mobility and flexible responses in labor markets, processes and consumer markets. Capitalism as a system has always been

reorganized through crises, and contemporary instability cannot be taken as a sign of disorganization.

Late capitalist political economic transformations can be linked to new sorts of acting "selves," to new structures of experienced need and libidinous desire. An increasing gap between the very rich and a swelling category of the poor, the homeless and "the needy" renders this period of transformation one that is experienced by many as a time of general insecurity, profound loss and the breakdown of coherent structure. Even favored beneficiaries of recent economic changes, the much-maligned "yuppies," seem to live in uncertain and unstable worlds, motivated to work incessantly, often in jobs they dislike, by the promise of some peak sensual moment of consumption that will make all the rest worthwhile.

The shift from liberal to late capitalism can be seen not only in capitalist political economic structures and in experiences and practices of people on the ground, but also within the realm of general academic "discourses" that cross-cut disciplinary boundaries. Ortner (pp. 144-5) notes a rethinking since the 60s of the field of structural linguistics, as many linguists have moved towards a view of language with greater emphasis on communication and performance. This reorientation resonates with similar moves to more "action-based" analyses in sociology, literature and anthropology. A "deconstructive" move to practice orientations is manifested in many other realms as well--for example, in postmodern or performance art or in a postmodern architecture that is highly critical of traditional international style design and favors instead an eclectic vernacular style more responsive to changing local practices and histories.

There are important, though admittedly tortuously complex, links between such sea changes in intellectual discourses and the late capitalist

transformations mentioned above. It makes sense that many anthropologists, concerned with the experience, consciousness and practice of people in local communities, should formulate their own perceptions of recent historical transformations in terms of a theoretical shift from structure to practice and history. However, in light of theories of late capitalist transformation that see "flexible practices" as the means by which capitalism is being reorganized on an unprecedented global scale as a system characterized by new forms of integration and constraint, we may suspect that various forms of "practice anthropology" tell only part of the story.

Ortner (p. 144) notes that for strategic reasons, she places her discussion of a more structure-oriented political economic approach in the 70s, in order to emphasize the novelty of various practice approaches in the 80s, even though a more historically accurate discussion would see both theoretical lines as developing side by side--often with very little interaction--into the 80s. I am inclined to see Ortner's attempt to characterize an emerging "practice anthropology" as an attempt to mediate between two concurrent, seemingly opposed approaches--one primarily concerned with the analysis of autonomous structures (in which action becomes mainly systematically constrained re-action) and the other primarily concerned with creative practice (sometimes to the extent of denying not only the power, but even the existence of coherent constraining systems).

The first, structure-oriented approach is evident in the works of world systems proponents (for example, Eric Wolf's Europe and the People without History (1982), in which "modes of production" often seem to take on independent existences of their own, and a capitalist mode of production seems to pull whole societies and cultures in its wake, dissolving and restructuring traditional worlds for its own uses). The second, action- and experience-

oriented approach is represented in the works of various postmodern anthropologists (for example, in Clifford and Marcus' collection, Writing Cultures, in which various authors often seem to suggest that any structure we identify in other people's lives involves the illegitimate imposition of "ethnographic authority" onto the complex realities of creative local practices and multivalent discourses).

To support these assertions adequately would lead me in directions far afield from the main line of my discussion of Ortner's paper. My main point here is that these seemingly polar forms of structure- and action-oriented approaches, world systems and postmodern anthropology, may be reconceived as two sides of the same late capitalist coin. As the "thinglike nature" of an earlier structure of capitalism is being broken down, a new "thing," whose dimensions and possible historical developments we are only beginning to glimpse, is being formed. Narrowly structure-oriented political economic theories tend to reify a global system that seems to take on a natural objective existence independent of conscious human practice. Some postmodern anthropologists, on the other hand, simply ignore or dismiss "the system" as an ideological construct, precisely because its global scope and complex forms of integration place its structure as a whole far outside the experience and intellectual grasp of any particular actor. Neither approach allows us to understand new forms of integration, contradiction or incoherence in the newly emerging system, nor new forms of consciousness, interest and motivation as local actors variously encounter, conceptualize, question and respond to changing circumstances of their everyday lives.

In stressing either structure or systematically unrestricted creative action, these approaches forfeit the possibility of envisioning truly different cultural modes of relating world and self, system and practice. For

world systems theorists, the real connections among people are economic and political: "culture" (in the form of religion, ethnicity or other transparently "symbolic" domains) becomes merely a reservoir of symbols to be strategically picked out and used to further the real interests of political economic actors. A notion of culture as a particular society's mode of constructing meaningful actors and objects and of organizing their culturally significant relations in time is dismissed as merely an illegitimate analytical concept that obscures the real material connections of men and nature around the globe. Actors here become the "self-interested, rational, pragmatic" reproducers of structures that Ortner sees as represented in an ethnocentric "interest theory" of motivation. "What actors do, it is assumed, is rationally go after what they want, and what they want is what is materially and politically useful for them within the context of their cultural and historical situations" (Ortner, p. 151).

Though postmodern anthropologists seek, in contrast, to celebrate the incommensurable differences among people's experiences, motivations and singular voices, they cannot offer a radical challenge to structure-oriented views. There is an equal danger here of simply dissolving "the other" into "us," insofar as unwillingness to impose our representational structures on the realities of others' lives shades into an unwillingness to recognize and represent the structures actually made by people in the course of their collective histories.

Ortner's development of a "practice approach," not in opposition to the study of systems or structures, but as its necessary complement (p. 146), is clearly an attempt to go beyond the static opposition of approaches that emphasize either structure or structurally unconstrained and undefined action. She argues (p. 159) "that society is a system, that the system is powerfully

constraining, and yet that the system can be made and unmade through human action and interaction." She maintains (p. 154) that "a unified theory of practice" should ideally be able to account for both historical reproduction and transformation, constraining system and active practice within a single framework.

Her whole discussion of "practice anthropology" is framed in terms of merging, interpenetrating or cross-fertilizing metaphors. To pick out just a few, she notes that a practice approach represents the theoretical merger of Marxist and Weberian, materialist and idealist approaches (p. 147). She wants to see a cross-fertilization between "sociologically oriented practice accounts, with their relatively denatured views of motive," and "more richly textured accounts of emotion and motivation" developed within a tradition of symbolic anthropology that has had problems dealing with questions of political economy, history and institutional practice (p. 151).

Ortner is clearly grappling throughout her paper with the dominant social problem of our world, the existing and possible relations between a powerfully constraining, seemingly monolithic external system and internally motivated, creative local practices. It is little wonder that Giddens (quoted in Ortner, p. 145) has dubbed the relation between "structure and agency" one of the central problems of modern social theory.

My problem with all this is the idea that our theoretical agenda for the 90s--and beyond--should focus on developing a general, unified theory of practice, concerned with relations between "the system" and "the practice" of diverse actors, that is supposed to help us make sense not only of dominant system/practice relations in our own "late capitalist" world, but also, in Ortner's terms, of "the practices and modes of dominated groups that to varying degrees 'escape' or at least 'resist' the 'prevailing hegemony'"

(Ortner, p. 155). It seems to me that insofar as we do not adequately deal with the social/historical contexts of our current theoretical concerns and formulations, we are likely to posit as foundations of a "general, unified practice theory" forms of "system and practice" that are really more specific to our own world, thus blinding us to just those modes of consciousness and action that might pose the greatest challenge to "hegemonic" forms of world creation and destruction.

Ortner is clearly worried about the persistent Western slant of much current "practice anthropology," putting less emphasis on the heroic project by which "man makes himself" than on aspects of practice related to the "hard times of today: pragmatism, maximization of advantage, 'every man for himself'" (p. 160). She argues (p. 148) against assuming a priori divisions of the social world into base and superstructural domains that seem to characterize our own world, and she maintains (p. 157) the importance of looking for "patterns of cooperation, reciprocity, and solidarity" alongside more individually rationalizing and maximizing forms of interested action.

Nevertheless, her own discussion of general practice theory continually reveals the distinctively Western cultural foundations of a preoccupation with how "the system" relates to "the practice" of interested actors. She notes (p. 148) that while the system is a relatively seamless, integral whole, "at the same time all of its parts or dimensions do not have equal analytical significance. At the core of the system, both forming it and deforming it, are the specific realities of asymmetry, inequality, and domination in a given time and place." There is a tendency to see the most important aspects of structure as "constraint, hegemony, symbolic domination" (p. 147), while the most important forms of practice are "those with intentional or unintentional political implications" (p. 149).

Ortner wants to develop practice theory in a direction that goes beyond a mere reflection of Western categories and assumptions, to see "the system" as potentiating as well as constraining and to envision practice as complexly motivated, in culturally variable ways by no means congruent with Western notions of natural, individual interests. Still, it seems to me that any general theory of practice seeking to bring together system and action sets out with a whole world of assumptions about what they are and how they differ, about the boundaries between external structure and inner being, form and content, objects and subjects, that theoretically blinds us to radically different cultural worlds. It is just not enough to hope for a "cross-fertilization" between sociologically and symbolically oriented accounts of self and world or for a "merger" of materialist and idealist approaches. Such hopes seem to me still to be caught within what Ortner (p. 134) recognizes as "pervasive schemes of Western thought: subjective/objective, nature/culture, mind/body," and to forfeit the distinctively anthropological promise to bring these schemes to consciousness as only one culture's mode of being and world-making.

While I have argued that Ortner's general discussion of practice theory can be illuminated by contextualizing it within the problematic dichotomy of system and practice characterizing late capitalism, this seemingly "natural" division can be traced, in various forms, much further back into the early history of western culture. Saint Augustine's City of God explores relations between the "City of God" and the "City of Man," between spirit and flesh, mind and body, already constituted as separate things. Human society, as the "pale shadow" of heavenly order, is already constituted here as a coercive and constraining system, necessary to keep fallen, self-interested individual actors within orderly bounds. The deepest inner thoughts and motivations of

every individual are impenetrable to others, although men are constrained to work together to produce the objective conditions of their physical survival. Each man is a limited being, working alongside others, though not in any real communion with them, in order to wrest physical necessities from a "disenchanted" external nature, a world that God created and then abandoned without a spirit or spirits of its own. The cultural roots of the opposition between "structure and agency" are indeed deep and convoluted.

Our dominant cultural project is production, a project that stresses "natural" divisions between nature and human will, world and self. Our cultural problem is connection, and edifices of connection tend to be seen as constraining "systems." Much of my own work has focused on an interpretation of early accounts of Scandinavian Sami (Lapp) hunters and fishers. Obviously, this is not the place for an extended ethnographic exploration, but I am increasingly convinced that a key to understanding this Sami world is a vision of their dominant "cultural project" as something like "ritual transformation," with material production and political negotiation as subordinate concerns. Divisions between individuals and objects, person and person, even humans and gods, were seen as provisional, temporary constellations, appropriate in certain contexts of action. But underlying all this was a real unity of substance: the blood of human beings was the same blood that coursed through the bodies of animals and indeed the same "blood of the land" that coursed through the body of the world. Their cultural problem, I believe, was the necessity for perpetually making and remaking significant differentiations--between hunters and prey, men and women, Sami and outsiders--in this substantially unified world, and holding these distinctions sufficiently steady in their appropriate contexts for an ordered, predictable

social life to unfold temporally within a world always threatening to collapse time and difference into itself.

Sami hunters developed an elaborate complex of practices centered upon the act of seeing. In certain highly charged ritual contexts, people looked through brass rings in order to control the potentially dangerous connection between seer and things seen that was believed to be created by the visual act itself. Likewise, the production of sound--for example, in shamanic ritual drumming--was a potent means of extending the boundaries of the active self to incorporate other human beings, land and animals formerly seen as outside the drummer. As ritual drumming produced waves of sound that moved out through the earth, various "external structures"--territorial boundaries, divisions between household groups and Sami communities--could be symbolically incorporated within collective Sami actors and could be remade according to consciously transformative intention.

The point here is that because structures in the world were seen as only provisional and to a large extent created by human action, they could be periodically made and unmade with a facility and transparency that seem extraordinary to us. People could be regularly redistributed among territories and hunting areas reallocated among human groups, not simply in the interests of practical material or political advantage, but according to Sami notions of "what constitutes goodness--in people, in relationships, and in conditions of life" (Ortner, p. 152). Within this sort of world, the relations of "structure, agency and history" have temporal, contextual dimensions that, it seems to me, would be difficult to illuminate by any general theory of "the system" (especially one conceived primarily as natural or political constraint) and its relation to "practice" (conceived as pragmatically interested actions). Indeed, it is precisely such theoretical

assumptions that have led to a dominant understanding of Sami hunters (and of other Arctic peoples) as archetypal pragmatists, engaged in a never-ending struggle for survival that leaves them little time for a complex cultural life--a notion that may serve self-affirming purposes for western analysts, but does not go very far in making even superficial sense of the complex ethnographic material.

It seems to me that in order to transcend the persistent western opposition between system and practice, if only partially and conceptually within the realm of anthropological theory and ethnographic understanding, we need theories that emphasize complex ethnographic understandings of the different historical dynamics associated with different modes of world-making and experience. Ortner suggests something of this sort when she states (p. 149) that "practice theory seeks to explain . . . the genesis, reproduction, and change of form and meaning of a given social/cultural whole." No general theory of the relations between system and practice, or between structure and history, could account for such phenomena, which become intelligible only in terms of specific cultural worlds and fields of social action.

It is interesting that much of the attention focused on Sahlins' elaboration of "structure and history" in Oceania arises from a desire for a general "model which derives systemic change from changes in practices" (Ortner, p. 155). The more interesting and far-reaching part of his argument is that history "works" differently in different sorts of societies. Hawaiian "heroic history" works differently from Western capitalist history, because different cultural schemes of significance and desired ends of action constitute qualitatively different sorts of actors with different interests, within worlds in which the practice of various kinds of actors is differentially weighted in its potential historical significance. In "heroic

societies," quarrels or marriages of kings become wars or alliances between kingdoms. Here, the "Great Man" theory of historical explanation actually seems to work, as long as we remember that heroic "Great Men" are not rational, maximizing Western individuals. Rather, they act in terms of a "cultural self" constituted less as a creative individual actor, than as a re-enactor of mythological narratives of gods, with whom divine kings are substantially connected and through whom the bodies of kings become the foundation for a substantial integration of people in society.

In contrast, "natural" divisions in Western culture constitute fixed boundaries between individuals and between God and man. Human society becomes an aggregate of individuals, rather than a substantially integrated entity, and western history "works" according to a very different dynamic. Here, it takes the combined practice of aggregates of individuals, acting at particular moments of structurally produced political/economic crisis, to effect social revolutions. "Great Men" become merely the particular representatives of "external" historical forces. (See Sahlins 1981 and 1983.)

Again, the crucial point here is that a general "practice theory" (inevitably incorporating, in its very formulation, fundamental western assumptions about "the system" and "the practice" of individual actors) could not go very far towards illuminating particular heroic or western worlds, and the ways and reasons they are historically made and unmade, reproduced and transformed. Rather than seeing development of a general, unified practice theory as a theoretical agenda for the 90s (and beyond), I think a more productive direction would be theoretical exploration of different sorts of cultural dynamics--different modes by which actors and objects, selves and worlds are constituted and brought into relation in various contexts of action directed towards particular ends (for example, economic production, political

negotiation or ritual transformation) and different modes by which various cultural contexts and particular ends are themselves related. This would allow us to distinguish, for example, between western culture, with its dominant cultural project of material production and accumulation, and other cultures with fundamentally different "kingdoms of ends" or "images and ideals of what constitutes goodness--in people, in relationships, and in conditions of life" (Ortner, p. 152).

Many anthropologists today are wary of the term "culture," given as many different meanings and functions in social life as there are anthropological schools or approaches. Ortner's article suggests a number of these, ranging from "culture" as a "distinctive flavor, an ethos" that stamps one's "sense of self, of social relations and of conduct" (p. 129), to culture as an internal logic of symbols and meanings, essentially cut off from action, to a set of symbolic operators in various social processes with their own realities, to an ideological reflection of more fundamental political economic realities. She also suggests a notion of culture as a human mode of constituting whole worlds, and this is the sense of "culture"--as a mode in which various collectivities meaningfully constitute subjects and objects, inside and outside, self and world--that I'd like to develop here. It makes sense that in a kind of cultural world such as our own, with its historically developed, systemically dominant opposition between external structure and internal being or mind and body that there would arise theoretical debates about whether "culture" is some sort of "superorganic entity" or something merely "inside" individuals, and if the latter, whether it is really a structured set of cognitive rules and roles or a more subjective, affective "ethos" or stylistic orientation.

A broader, more comparatively powerful notion of "culture" would require exploration of modes of world-making that could not be confined to just the way people think or feel "inside." Culture, in terms of this broader understanding, is embodied in inner feelings, motivations, forms of libidinous desire and physical need, as well as represented within individual cognitive frameworks. But it is also manifested "outside," in constructions of "humanized nature," giving a particular form and magnitude to the structures of "objective circumstance." It is present "outside" in the structures of humanly built environments or in humanly constituted political institutions that seem to gain a kind of life and internal dynamic of their own. The point here is that as soon as we talk about culture in general, we must move to considerations of cultures in particular, because it is only in the context of the latter that we can make sense of particular configurations of external constraint, inner possibility, evolutionary tendencies and historical dynamics.

Ortner (p. 143) notes, in opposition to a capital-centered view of the world, that "the attempt to view other systems from ground level is the basis, perhaps the only basis, of anthropology's distinctive contribution to the human sciences." Such a "ground level" basis for ethnographic fieldwork has often been identified with the analysis of a local community, of a particular constellation of practices, interactions, world views and differentially valued ends of action. It is on the basis of such ground level analyses, I have argued, that many anthropologists, in the period of late capitalism, see traditional structures of self and world as breaking up (or as never having actually existed at all) and struggle to make sense of their ground level visions through theoretical moves from structure to practice. In contrast, I have suggested that what we are seeing in the move to late capitalism are new

sorts and levels of integrated structure, new constraints on and possibilities for local practice, and a relation between seemingly opposed "levels" of structure and practice that is rendered increasingly problematic and opaque to actors within the system. For anthropologists today to understand "local practices on the ground," intensive, local fieldwork is essential, but not sufficient. We also need a comparative theory of broadly cultural "modes of production" that allows us to penetrate the fundamental assumptions of western, capitalist culture and the ways these have developed historically to global proportions. "Culture" here is not a localized, bounded entity, but a particular mode of making (and periodically unmaking and reconstituting) a world.

Despite various dire predictions that the development of global capitalism would inaugurate an era of "monoculture," manifest differences between local groups, regional societies and national cultures seem, if anything, more pronounced than ever before--an observation that should not surprise us, given an understanding of late capitalist concentration and centralization proceeding on the basis of competition between groups, some of whom have become very proficient in the strategic employment of "cultural symbols" and "ethnicity" in pursuit of material and political advantage. Kayapo Indians in Brazil, native Fijian politicians, and Scandinavian Sami reindeer herders speak today in remarkably similar ways about their "cultures," often as if they were external forms or markers that people could simply put on or take off in different contexts. During my own fieldwork in northern Scandinavia, I pondered the motivations of a Sami reindeer owner, with a pocket of business cards printed with his name and the English words, "Reindeer Products, Inc.," who drove to the local airport in his new Mercedes to fly to Australia for a meeting of the World Council of Indigenous Peoples,

where he would don traditional clothes, sing traditional Sami songs, and bemoan the incursions of "Western culture" into his own "indigenous culture, living in harmony with nature." An obvious lesson here is that we should never underestimate the power of western capitalism to penetrate into other worlds, to transform the threat of truly different cultural modes of being into "ethnic" or narrowly "cultural" differences that merely reinforce, rather than fundamentally challenge capitalist cultural hegemony.

Nevertheless, as Ortner (p. 155) observes, at least some of the practices and modes of dominated groups, or of groups only partially or peripherally incorporated into a capitalist "world system," escape or resist the prevailing hegemony. A major problem is, however, that without a theory that allows us to make systematic comparisons among different cultural modes of constituting worlds and making histories, we are unable to judge whether various practices represent escapes from or alternatives to "the prevailing hegemony" or merely local differences that feed into and help to reproduce a late capitalist system. Indeed, we are left unable to illuminate the particular nature of "relationship(s) that obtain between human action, on the one hand, and some global entity which we may call 'the system'" (Ortner, p. 148) in the prevailing hegemony itself.

There are still infinite possibilities for ethnographic research into truly different cultural modes of constituting self, world and history--both within non-western communities profoundly affected, though not totally transformed by "the world system" and within the late capitalist system itself. "A regime of flexible accumulation," no matter how integral to current capital restructuring, also opens up new places for alternative visions and practices and for various forms of "counter-hegemonic resistance" as people "on the ground" variously experience, question, seek to understand

and adapt to or transform the changing circumstances of their everyday lives. Rich possibilities for ethnographic work exist now, for example, in the contemporary women's movement, in connection with new constellations of religion and politics, in newly emerging coalitions of environmental and human rights activists. For the most part, general theories of late capitalism simply assume that the consciousness and practice of people in new circumstances will change in theoretically predictable ways. Insofar as they don't, something we can only know through careful ethnographic study, these general theories themselves will have to be revised and reformulated.

A general "practice orientation" that does not just assume that people act in ways that simply reflect "the system" is enormously useful in doing ethnography, but it is not an adequate foundation for future anthropological theory building. I would argue instead that it is only in terms of a comparative vision of different cultural dynamics that we can begin to locate current forms of "practice anthropology" within the historical development of one particular--and from an anthropologically comparative perspective very peculiar--mode of cultural production. Moreover, it is in terms of such theoretical comparisons that we can make sense of the local practices, persons and histories that we study around the globe as, predominantly, aspects of a capital-centered cultural mode or of truly alternative cultural projects. What would it take for us to reclaim the "late capitalist system" now emerging as an historical product of human intention and meaningful action? What sorts of critical perspectives might be brought to bear upon the dominant western, capitalist mode of constituting and relating "system and practice"? Are there new points of contradiction and incoherence in "the system" that might be critical areas for new sorts of practices, aimed not just at the reproduction of dominant structures, but at their transformation?

These are all questions that Ortner clearly wants to illuminate through her discussion of newly emerging "practice approaches" in anthropology. I hope it is clear from my own far from disinterested comments on Ortner's paper that I have great admiration for what she sets out to do in this paper--to make integrated, historically grounded sense of a seemingly fragmented field in the interests of clarifying potentially significant areas for future research and theoretical development and ultimately, of providing the foundations for a truly critical anthropology. Her willingness to put herself on the intellectual firing line in this session is equally praiseworthy. My criticisms center around how accurately her discussion of an overall move to "practice anthropology" characterizes the recent history of anthropological theory and how positive and theoretically invigorating such a move really is. I would like to see in the coming decades much more attention given to the social/historical contexts of our current theoretical concern with issues of practice and history, and of our skepticism about the usefulness of received anthropological notions of system, structure, organizing principle, cultural order. How might we redefine and rework the elusive, but I believe still fundamentally central, anthropological notions of "culture" and "cultural difference" to meet our present, historically and culturally constituted, theoretical needs?

The point I'd like to end with here is that any workable notion of comparative culture in contemporary anthropological theory would have to take account of criticisms of the "culture as constraining system" concept developed from within current "practice approaches." Culture could no longer be simply identified with a clearly delimited community or society on the ground, with an unproblematically integrated structure of rules and roles that people simply enact, or with a set of beliefs or patterns of behavior that are

unproblematically shared by all actors. The hope would be that while drawing much from current practice approaches, the further development of comparative cultural theory could itself illuminate those approaches, as one culture's attempt to make sense of the dominant social problems of its time--the existing and possible relations among structure, human agency and history. "A lot of work," as Ortner (p. 160) notes, "remains to be done."

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SCIENTIFIC FIELD AND SCIENTIFIC THOUGHT
Marginal Notes on Sherry B. Ortner's article
"Theory in Anthropology since the Sixties"*

by Pierre Bourdieu
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*Translated from the French by Loic J. D. Wacquant. A few comments and cursory observations jotted down rapidly with the liberty of improvisation allowed by the oral presentation that I would like to have been able to deliver in person in order to express the great esteem in which I hold Sherry Ortner's synthesis.

SCIENTIFIC FIELD AND SCIENTIFIC THOUGHT

Sherry Ortner divides the recent history of anthropology in three decades: structuralism in the sixties, a return to Marx and structural Marxism in the seventies, and to "practice" in the eighties. This convenient classification is premised on a Hegelian-Marxist philosophy of the history of science which tends to portray scientific undertakings as being closely dependent upon a sort of intellectual Zeitgeist (others would say a fad) and to consider that the products of such undertakings are directly determined by the most general historical conditions¹ and, being bound to them, are thus doomed to disappear with them. In fact, all the remainder of the article seems to me to belie this philosophy which we tend to accept with too little thought. There is in reality an autonomous history of scientific problems and solutions because there is an autonomous history of scientific fields (conceived as fields of forces and fields of struggles) in which these problems and solutions are produced. (One cannot proceed directly from the expansion of the sixties to structuralism or from the student movement to the demise of this current.) A question essential to the understanding of scientific production is that of the practical limits of these fields which are cut up along disciplinary boundaries and national traditions. (Here, the logic of trend-report tends to bring into coexistence areas of research which have developed in total independence from one another within the limits either of a given disciplinary subfield--anthropology, sociology or philosophy--or

¹ "Like any theory, it is a product of its time. Once practice had the romantic aura of voluntarism: 'man,' as the saying went, 'makes himself.' Now practice has qualities related to the hard times of today: pragmatism, maximization of advantage, 'every man,' as the saying goes, 'for himself.' Such a view seems natural in the context of the sixties and seventies and in the context of a disastrous economy and a heated-up nuclear threat." (Ortner, p. 160).

even within the limits of a national disciplinary subfield, insofar as these disciplinary fields are not fully internationalized.)

Indeed, the oppositions, at once social and intellectual, which arise within each field and which are often incarnated in names of schools and even names of scholars that serve as sign-posts, often form the principles of structuration of the dominant problematic at a given moment in time and consequently the foundation of a consensus which binds together the various cultural producers in a given field.² I shall refer here to the excellent analysis of the conflict between the cultural ecologists and the symbolic anthropologists put forward by Sherry Ortner:

Whereas the cultural ecologists considered the symbolic anthropologists to be fuzzy-headed mentalists, involved in unscientific and unverifiable flights of subjective interpretation, the symbolic anthropologists considered cultural ecology to be involved with mindless and sterile scientism, counting calories and measuring rainfall, and willfully ignoring the one truth that anthropology had presumably established by that time: that culture mediates all human behavior. The manichean struggle between 'materialism' and 'idealism,' 'hard' and 'soft' approaches, interpretive 'emics' and explanatory 'etics', dominated the field for a good part of the decade of the sixties, and in some quarters well into the seventies. (Ortner, *op. cit.*, p. 134)

These social oppositions functioned as principles of vision and division ("most of us thought and wrote in terms of such oppositions," writes Sherry Ortner), as schemes of construction of reality, very similar in this respect to the "primitive forms of classification" dear to Durkheim and Mauss and to the pairs of oppositions of the "savage mind" analyzed by Levi-Strauss. (It is no doubt because it arises within scientific fields which function as

² I am prepared to defend the hypothesis that the distribution of the contenders between the two sides is not randomly generated and that significant relationships (statistically and semantically) could be found between the scientific stances taken up by various researchers and their positions (and trajectories) within the field (university of origin, academic rank, professional positions, etc.) and, by extension, their social origins.

fields of struggles, battlefields, that thought is so often organized around such pairs of antinomic concepts.)³ In the same way as the paired oppositions of dualist thought, they exert a closure effect: just as their proponents--colluding adversaries--support each other in and through their antagonism ("the emic/etic struggle of the sixties had a number of unfortunate effects, not the least of which was the prevention of adequate self-criticism on both sides of the fence. Both schools could luxuriate in the faults of the other and not inspect their own houses for signs of serious weakness," Ortner, *Ibid.*), similarly, antinomic concepts, which often serve as insults, in their antagonism hide the fact that they tend to delimit the space of the thinkable by excluding the very intention to think beyond the divisions they institute.

It is obvious that these oppositions which structure the field of a given discipline and the minds of all those who participate in it are in no way universal. Different oppositions operate within another discipline in the same country and within the same discipline in other countries. For example, among French anthropologists, during the same period, the main oppositions were very different even though the field of anthropology was undoubtedly more unified at that time than that of sociology or philosophy. As a result, many misunderstandings arise in the international circulation of "theories": indeed, as Marx himself pointed out long ago, texts circulate without their context, or, more precisely, they do not carry with them the scientific field within which they were constituted, i.e., the system of oppositions with reference to which they were defined and which varies by discipline, national tradition and intellectual generation.⁴ These texts therefore have every

³ Cf. P. Bourdieu, "The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason," Social Science Information 14:5 (1975) and "The Peculiar History of Scientific Reason" (forthcoming).

⁴ Thus one is often surprised by all the "errors of categorization" that Anglo-American readers are induced to make about the works of French authors

chance of being read by readers who, being integrated into a different field, apprehend them through completely different schemes of perception and problematics. This fateful disjunction is all the more likely when the temporal gap between original publication and translation further muddles synchronic relations.

I was thus a bit taken aback to find myself placed in the "current practice trend" of the 1980's. Indeed, my book Outline of a Theory of Practice,⁵ published in the early seventies but elaborated in the mid-sixties, i.e., at the heyday of structuralism, was explicitly conceived against two theoretic opponents which were just as deeply opposed to each other as were, at that time in the United States, the cultural ecologists and the symbolic anthropologists: on the one hand, structuralism, in its Levi-Straussian version or its structural-Marxist version,⁶ which took the structuralist philosophy of action to its limits by making explicit the theory of the agent as the mere support (Trager) of the structure; on the other hand, Sartrean existentialism, which no doubt stands as the most systematic and the most

and about the authors themselves by virtue of being deprived of all the information on disciplinary affiliations, generations, academic origins, etc., which come with indigenous familiarity, or because they cling to erroneous or superficial reference points given by "travelers" and "jet ethnographers" who believe themselves to be well informed simply because they have spent a couple of months in Paris listening, in their own language, to talks destined for foreign visitors, and whose accounts are then repeated again and again without verification (for example, the idea that Michel Foucault was once a "student" of Althusser...).

⁵ Geneva: Droz, 1972, and Cambridge: Cambridge University Press, 1977.

⁶ "Structuralist Marxism" was not born, as might be inferred from Sherry Ortner's periodization, from a reaction against the structuralism of Levi-Strauss. Rather, it issued from the application of the structuralist mode of thinking to the reading of Marx and, through the influence of Althusser, to anthropology (where it wreaked havoc) and represents one of the manifestations (Foucault's oeuvre being another) of the domination that anthropology has exerted, through Levi-Strauss, on the totality of the intellectual field and on philosophy which had, until then, been dominant. (cf. P. Bourdieu, "Preface" to the English edition of Homo Academicus (Cambridge: Polity Press, 1988).]

radical expression ever given to the philosophy of the intentional subject. This is why I cannot without hesitation agree to be placed in the "practice trend" of the eighties (however flattering it might be to be thus situated at the end, albeit provisional, of the "intellectual dialectic"...). In effect, and this is what I would now like to argue briefly, one cannot confound under the same concept, and a vague one at that, of "practice" the theory of habitus that I put forward and the very subjectivist theories of action (such as interactionism, ethnomethodology or Rational Action Theory) against which it was conceived, no less than against the objectivist theories of the structuralist or structuralist-Marxist type.

If it is true that the various fields are organized according to different oppositions which vary by discipline, national tradition and historical period, it nonetheless remains that there is a limited number of fundamental oppositions which, being inscribed in the nature of things, that is, in the very peculiar form assumed by the relation between the scientist and his or her object in the case of the social sciences, are found to underlie the various states of the intellectual field in a variety of forms. Such is the case with the opposition between objectivism and subjectivism: these two antagonistic points of view, like the oppositions between materialism and spiritualism or between physicalism and semiologism, in a way artificially mutilate the intrinsically double reality of human existence as a thing of the world for which there are things. (It is this fundamental anthropological reality which is well captured by Pascal when he says: "Le monde me comprend et m'aneantis comme un point, mais je le comprends"--the world encompasses me but I understand it.) And the logic of the fields of cultural production, to which the scientific field is no exception, the logic of orthodoxy and heresy, which incites newcomers to break with the dominant

discourse, promotes false revolutions which are nothing more than switches from pro to anti, cyclical returns to a subjectivist phase after an objectivist phase and conversely. Thus, in sociology today, the reaction against the short-lived domination of an approach of the structuralist-objectivist type (to which some of my work in the sociology of education, Reproduction in Education, Culture and Society in particular, is mistakenly assimilated) inspires a return to a subjectivist conception of action against which the structuralist tendency had formed itself. I have in mind these trends which scholarly taxonomies sometimes gather under the label of "constructivism," and which range from the more or less updated variants of symbolic interactionism or ethnomethodology that emphasize the contribution of agents to the construction of social realities, structures, social groups and so on, right up to forms of discourse analysis which forget to take into account the position of the locutors within the space of production and consumption of discourses. Likewise, in anthropology, the reaction against the hardest forms of structuralism and structural Marxism incline some to embrace a form of subjectivist nihilism which, on the basis of a falsely radical critique of fieldwork, reduces discourse on the social world to a rhetoric wavering between the suspect charms of poetics and the underhand dealings of politics.

In short, in the face of these pendular swings that have never stopped since the emergence of a science of the social world pretending to autonomy, one has the impression that history is repeating itself: thus the triumph, during the sixties in France, of the "philosophy without a subject" which asserts itself, at least among philosophers (Althusser and Foucault in particular), in reaction against the philosophy of the subject, of free conscience, of project, which, with Sartre, had dominated the entire French

intellectual field in the fifties,⁷ seems like a come-back and revenge of Durkheim against whom, at least in part, the French philosophers of the generation of Sartre, Aron and Nizan, had defined their positions, by drawing on the phenomenology of Husserl and Heidegger.⁸ This being said, for reasons that have to do with the quality of the protagonists and also with the progress of the intellectual experience accumulated within the field, the opposition between Sartre, who took subjectivism to its breaking point,⁹ and Levi-Strauss, who affirmed in the most provocative manner the philosophy of action inscribed in Durkheimian theory and extended by Saussure (with the notion of the unconscious), no doubt represents--or is this an illusion of familiarity?--the most accomplished expression of the opposition between objectivism and subjectivism.

It is this opposition that I have sought to transcend.¹⁰ Against the objectivism of action without an agent and history as a "process without a subject," and against the subjectivism for which action is the product of a conscious intention, the free project of a conscience positing its own ends or the rational calculation of a homo economicus guided by the search for the

⁷ Cf. A. Boschetti, Sartre et "Les Temps Modernes": une entreprise intellectuelle. (Paris: Editions de Minuit, 1985). English translation by Northwestern University Press, 1988.

⁸ See P. Bourdieu and J.-C. Passeron, "Sociology and Philosophy in France Since 1945--Death and Resurrection of a Philosophy without a Subject" Social Research 34:1 (Spring 1967) for a fuller discussion.

⁹ So much so that anyone who has in mind Sartre's analyses of bad faith or of oaths will recognize the contortions of a Jon Elster, particularly in Ulysses and the Sirens (Cambridge: Cambridge University Press, 1979, rev. ed. 1984), as the mediocre remake of a familiar show.

¹⁰ I have tried to explicate all the implications of this position, particularly with regard to temporality, in my book Le Sens pratique (Paris: Editions de Minuit, 1980, English translation forthcoming), in which I reexamine more systematically and in greater depth the analyses presented in Outline of a Theory of Practice.

maximization of profit or, more largely, by the pursuit of his interest, I wished to put forth a theory of practice as the product of a "sens pratique," of a "sense of the game," or, in a word, of habitus. By habitus, I meant a system of dispositions or, if you prefer, of schemes of perception, appreciation and action which are the product of the incorporation of objective structures and which, as long as the social games to which they are confronted are not radically different from the games in which they were constituted, allow one to anticipate the necessity immanent in the game, the tendencies inscribed in its very logic, in the manner of a wide receiver who finds himself right where the ball lands on a broken pass play.

Within this framework, actions have as their principle agents (which does not mean, as Sherry Ortner suggests in the enumeration given on page 144, subjects, persons or actors--so many words which imply a philosophy of conscience, of intention) who do not need to posit their goals as such, as part of a conscious project or a rational plan, in order to produce actions that are reasonable--and this does not mean rational. Actions engendered by habitus can have an objective intention without being the product of an intention; the strategies suggested by habitus, like those of the accomplished player who anticipates the anticipations of his opponents and thereby beats them, are not the product of a strategic intention. Neither, for this matter, are they automatic reflexes, mechanical reactions or the mere execution of a programme inscribed in the structure. Being active, inventive, in the manner of the improvisations of a story-teller or an inspired orator, they are not the product of the conscious and calculated decisions of a creative invention: their principle is not explicit rules or procedures constituted into a method, but rather an art, "pure practice without theory," as Durkheim put it, a practical modus operandi which reveals itself only in the opus operatum and

which allows all kinds of inventions, but within the limits of a style, inscribed in the schemes of habitus. It would be necessary here again to specify the notion of interest: I hold that interest emerges in the relation between a definite habitus and the social field whose structure and dynamics it embodies.¹¹ This implies that there are as many forms of interest as there are fields: what, for a "well-socialized" Kabyle, is a matter of life and death, a crucial stake, might leave indifferent an agent lacking the principles of differentiation which enable him to make the difference and to be taken in by the games of honor. But this implies also and above all that the pursuit of interest does not presuppose, as one might believe by following the utilitarian philosophy associated with certain states of the economic field, the conscious positing of rationally-sought self-interested ends. (Indeed, one could show that there are many social universes where disinterest, in the ordinary sense of the word, is a particularly effective way of satisfying one's "interests.")¹²

In order to capture the gist of human action, one must thus get rid of all the paired concepts (such as subject/object, thought/thing, conscious/unconscious, etc.) which block our thought and forbid us to grasp and adequately render the ontological complicity between the social agent (who is neither a subject nor a consciousness) and the social world (which is never a mere "thing"): social reality exists, so to speak, twice, in things and in minds. And, as the relationship between the sense of the game and the

¹¹ An analysis of how this "fit" between habitus and field generates interest in the case of the literary field can be found in P. Bourdieu, "Flaubert's Point of View," Critical Inquiry 14 (Spring 1988).

¹² For an elaboration on this, cf. P. Bourdieu, "On Interest and the Relative Autonomy of Symbolic Power," Working Papers and Proceedings of the Center for Psychosocial Studies 20 (Chicago: Center for Psychosocial Studies, 1988) and "The Field of Cultural Production, or the Economic World Reversed," Poetics 12 (1983).

becoming of the game clearly demonstrates, when habitus is confronted by a social world of which it is the product, it is in a certain way this reality which communicates with itself, below the level of discourse and consciousness, in a sort of "body-to-body" struggle (corps a corps).

THEORY IN ANTHROPOLOGY THREE YEARS LATER:

A Response to Papers

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Let me start off by saying that, to preserve my sanity, I had to start drafting these remarks about two weeks ago, when I had only one of the papers in hand (Tom Gibson's). (I can't complain too much since I have done the same thing to my discussants, when I was a panelist in other times and places.) This meant that I had to come up with a strategy for responding coherently to critiques of whose shape I had only the most vague and general idea. The strategy I settled on was this: After calling most of the panelists, I decided to focus on five terms that are key to the issues discussed in "Theory in Anthropology since the Sixties," terms so general that - I fervently hoped - they could not fail to correspond in some way to at least some of the critiques put forth by the panelists. The terms are: practice, structure, actor, reflexivity, and history. After seeing the papers I am reassured that these will cover a reasonable part of the territory, although there will necessarily be a number of important points that get missed. Hopefully these will be picked up later in the floor discussion.

Before getting to the five terms, however, let me make a few general comments.

First, I want to say that I feel very firmly situated by the panelists in my various contexts - global and local, epochal and historical. I cheerfully admit that my thinking operates within a society and a historical moment that carries all of the following baggage: Durkheim's distinction between the sacred and the profane; Freud's emphasis on the complexity of human emotional and sexual life; the Victorian emphasis on the opposition between domestic and public; the American cultural obsession with the individual and Americans' near inability to conceive of structural rather than psychological

constraints; the American university system with its emphasis on the constant production of new and improved products; and late capitalist society which masks corporate and governmental consolidation behind an ideology of flexible opportunity and who-says-you-can't-have-it-all yuppieism. To this list I would add two situating moves derived from some earlier critiques of the theory paper: that my thinking must be situated in a context in which the colonial heritage of anthropology has become increasingly clear, and in which more and more anthropologists are working in so-called complex societies. And finally I add one that I emphasized myself in the paper: that we are all inheritors of the Sixties - of Vietnam, of Woodstock and the Days of Rage - the impact of which is still unclear. To all this I would say that, while I come from a time and place in which all these things are in some extended sense part of my world, I do not think I carry all of them as part of my personal intellectual baggage, and certainly not all in the same way. If I did I would be as exhausted as Maurice Bloch feels when he contemplates the feverish trendiness of American academic life.

Pursuing this point about trendiness, I want to agree with Bloch that one of the general problems with the theory paper is that it portrays the various earlier forms of anthropology as more dead or out of fashion than, for better or for worse, they really are. In my own defense, the point of lining things up in linear fashion was to show the ways in which new developments represented responses to past configurations of the theoretical landscape. Further, I think it is the case that the relative dominance, if not the presence or absence, of certain schools has shifted in more or less the ways the paper describes, at least in the United States. And finally, I did try to show in later sections of the paper the ways in which older schools of thought

were being transformed rather than jettisoned. But this last point was probably insufficiently stressed, and I would like to give it more emphasis today. I think everything I talked about in the paper (as well as several things that I didn't) are still alive and part of the current intellectual tool-kit, and I neither wish to abolish them, nor did I ever imagine that my words had the power to do so.

Moving now to the list of keywords, I will begin with what is obviously the central term, "practice," and will reaffirm the claims made in the paper to the effect that "practice" both is and deserves to be a major symbol of current theoretical direction.

So what is "practice?" I agree with Bloch that it is the most poorly defined term in the paper. While I do not wish to use up my response time giving a lecture on practice theory, it seems critical for further discussion to be as clear as I can in a short time. In the paper I said that any form of human action or interaction would be an instance of practice insofar as the analyst recognized it as reverberating with features of asymmetry, inequality, domination, etc. in its particular historical and cultural setting. The emphasis on the centrality of asymmetry and/or domination is one of the primary elements distinguishing current practice theories from older theories of social action, interaction, and transaction. Thus human activity regarded as taking place in a world of politically neutral relations is not "practice." To this minimal definition I would add the following: Practice is action considered in relation to structure; that is, in contrast to symbolic interactionism, say, structure is not bracketed analytically, but is central to the analysis of action or practice itself. Practice emerges from

structure, it reproduces structure, and it has the capacity to transform structure. Human action considered apart from its structural contexts and its structural implications is not "practice." (My emphasis on this point is not granted by Maurice Bloch, Tom Gibson or Pierre Bourdieu and I will come back to it later.) And finally I would add an optional third dimension: history. History is optional in the sense that Bourdieu's Outline of a Theory of Practice is certainly an instance of practice theorizing (one could say he wrote the book on the subject), yet it is not historical. But I think it is only in historical context that one can see the relationship between practice and structure fully played out, and most current anthropological work utilizing practice theory is in fact historical.

One other general point about "practice theories." A practice approach can be used to analyze quite a wide range of problems. In terms of published examples, we know it can be used to analyze statistical conformity and non-conformity to cultural rules, as when Bourdieu (1977) used it to explain the range of variation of conformity to marriage rules in Kabyle society. It can be used to analyze historical events, as when Sahlins (1981) used it to explain the occurrence and shape of certain very dramatic incidents in Hawaiian history. It can be used to analyze an existing configuration of a cultural system, as when I used it to explain the pattern of gender beliefs in traditional Polynesian society (1981). And it can be used to analyze structural transformation, as again in Sahlins' Hawaiian case, or in my own current work (1989) on the foundings of celibate monasteries among the Sherpas of Nepal in the early 20th century.

In all cases the general line of analytic attack is the same: to try to understand something the people did or do or believe, by trying to locate the point of reference in social practice from which the beliefs or actions emerge. This is not just a question of locating the actor's point of view, although that is a part of it. It is a question of seeking the configuration of cultural forms, social relations, and historical processes that moved people to act in ways that produced the effects in question.

In order to be more specific, and also to return more directly to the panelists' criticisms, let me move on to the next keyword, "structure." In addition to referring to the panelists' comments, I will also refer briefly to points made in three papers published in 1986 in Comparative Studies in Society and History: by Aram Yengoyan, Arjun Appadurai, and Ulf Hannerz, each responding at least nominally to "Theory in Anthropology since the Sixties."

"Structure" as a symbol appears to be the most hotly contested term on the list. Further, my discussion of structure seem to be the most prone to misreading of any of the discussions in the paper. It is almost as if the term practice could exist only in mutually exclusive relation to structure, such that if I talked about the importance of practice, I could not possibly have any appreciation of the presence, and the constraining force, of structure. Thus Tom Gibson, Maurice Bloch and Pierre Bourdieu fault me for not giving structure (in the sense of unconscious, collective ordering principles) its due; Ulf Hannerz suggested that I am insufficiently appreciative of social organization (in the more empirical sense); and my colleague Aram Yengoyan seemed to think I was recommending abandoning the concept of culture. In each case this reading of my paper seems to stem

directly from a reaction to my expressed interest in actors and human agency; thus in the context of worrying that I pay too much attention to practice, Yengoyan accused me of promoting what he insisted on calling "behaviorism," Gibson accuses me of being a crypto-Freudian, and Bloch and (implicity) Bourdieu accuse me of turning back to transactionalism.

I have already indicated rather firmly that my notion of practice is inextricably tied to a notion of structure. But in order to defend my original text for a moment, let me quote a brief section of it:

The newer practice theorists...share a view that 'the system,' (in a variety of senses to be discussed below) does in fact have very powerful, even 'determining,' effects upon human action and the shape of events. Their interest in the study of action and interaction is thus not a matter of denying or minimizing this point, but expresses rather an urgent need to understand where 'the system' comes from - how it is produced and reproduced, and how it may have changed in the past or be changed in the future. As Giddens argues in his important recent book (Central Problems...), the study of practice is not an antagonistic alternative to the study of systems or structures, but a necessary complement to it. (pp. 146-147).

Somehow these assertions are not being heard. Let me then discuss briefly the way in which notions of "structure" operate in the context of a practice perspective. In general my point is that any of the standard notions of structure can be used in conjunction with a practice approach, but they will tend to undergo certain changes. Specifically, I would say that the image of

structure, of what structure would look like if you could see it, changes in the context of a practice perspective. Where in earlier representations structure looked like a building or a machine or an organism, or like one of those geometric spaces in La Pensee Sauvage, now it appears in forms that themselves contain an active assumption. Probably the clearest example of this point is Bourdieu's notion of habitus. Habitus is at one level structure in the Levi-Straussian sense, as is clear from those diagrams in Outline depicting the relations between wet and dry, up and down, inside and outside, male and female. Yet at the same time the image of structure in habitus is profoundly transformed by its theoretical linkage with practice. Thus it is structure that is doubly practiced: it is both lived in, in the sense of being a public world of ordered forms, and embodied, in the sense of being an enduring framework of dispositions that are stamped in and on actors' beings. I made a similar point in the paper when I contrasted Foucault's notion of discourse (which assumes a context of multiple unequal interactions) with established notions of culture, which assume an actor's point of view but do not assume the actor to be involved in any particular kind of interaction. Discourse is culture in motion as it were, both communicationally (within a certain kind of social/political field) and also historically, in the sense that discourses are portrayed as intrinsically more transformable than what we think of as culture.

And finally, my own recent work on Sherpa social and religious history utilizes a notion of cultural schemas, recurring stories that depict structures as posing problems, to which actors must and do find solutions. Here again structure (or culture) exists in and through its varying relations with various kinds of actors. Further, structure comes here as part of a

package of emotional and moral configurations, and not just abstract ordering principles.

The point in all these examples is that - contrary to the assertions of Bloch, Gibson, and the others - practice approaches have very robust notions of structure, and of structural dynamics, forces, constraints, and outcomes. But the way in which structure is imaged, represented, and conceptualized is itself changing, as a result of its being conjoined with an equally robust notion of practice. If anything one can imagine the criticism coming from the opposite direction: that too much weight is still given to structural forces, and that the poor actor still has very little creativity in the historical process. Since none of the panelists raised this particular charge, I will not respond to it here, but it brings me to my third keyword, "actor."

I will repeat first that the focus on actors in the context of contemporary practice theory is not a new form of either voluntarism or transactionalism. The actor is not viewed as a free agent, engaged in unconstrained creativity on the one hand or manipulation on the other. Rather the actor is recognized as being heavily constrained by both internalized cultural parameters and external material and social limits. Thus the central problem for practice theory is, as all its practitioners seem to agree, precisely the question of how actors who are so much products of their own social and cultural context can ever come to transform the conditions of their own existence, except by accident.

Now in the paper I complained that much of practice theory today, including some of my own work, tends to fall back on an interest theory view of actors:

actors are rational strategizers, seeking to maximize or at least optimize their own advantage. I said then, and I would still say now, that while such rational calculation is always a part of actors' intentions, it is never exhaustive of those intentions, and in many cases it is not even the dominant part. I said that we needed a more complex view of actors.

Although at one level Tom Gibson's and Sharon Stephens' criticisms of my paper come from radically different perspectives, at another level both arrive at the point that, instead of trying to theorize the actor from our own point of view, we must attend more to the ways in which actors are culturally constructed in different times and places. I agree very strongly that the historical and comparative study of the cultural construction of persons, and of the stuff (like motive, will, interest, intention) that move persons to act, is an enterprise of major importance. Indeed I just finished an entire book organized around the question of how various individuals in Sherpa society arrived in various ways at a certain configuration of felt need and active will at a certain moment in history. That is, I framed my history of the founding of the Sherpa monasteries as a question of the social, cultural, and historical construction of "interest".

Yet at the same time I think we must recognize that an emphasis on the person as entirely a cultural product poses problems which are merely the inverse of the overly westernized actor. It evades the problem of adequately theorizing the actor, and leaves the scene to reductionist theories in which people are either overly rationally calculating or overly propelled by biological and/or psychological drives. It also has the potential for falling into what might

be called the Talcott Parsons effect, in which the only actors capable of changing the system are either deviants or geniuses.

The terms practice, structure, and actor, which I have scanned at an absurd speed, exhaust the central terms of practice theory as such. However, before moving to the other terms I intend to tackle today (reflexivity, history), I must attend to the two more general critiques made by the panelists concerning practice theory as a whole. On the one hand I have Bloch saying that practice theory is ok but that, in addition to the fact that Ortner doesn't do a very good job of laying it out, she fails to recognize that it has been around for a very long time and thus falls into old traps and beats old dead horses. On the other hand I have Sharon Stephens saying that practice theory is already outmoded, embedded in old categories and modes of thought which must be transcended. I can only respond very briefly to each of these positions.

First, I agree with Bloch that an interest in the relationship between human action and social transformation can be traced back quite a long way. It can be traced back strongly to both Marx and Weber though I will not review their positions here. But I disagree that there has been a serious attempt in modern social science, until this current body of so-called practice theory, to re-raise the issue. Instead we have had, as Bourdieu emphasizes in his comments, oscillations between overly structural and overly actor-focusing frameworks. The arguments between Levi-Strauss and Sartre are of course paradigmatic here, and I must say parenthetically that I find Bloch's and Gibson's attempt to cast Levi-Strauss as a thinker deeply concerned with the role of actors in history rather hard to wrap my mind around. In any event while I think one could construct a syllabus on the problem of action and

structure, I do not think one could say that people have been consistently and self-consciously working on a synthesis in which, as in the present case, the two terms are given equal power. The problem is that even the attempted syntheses in the current situation get heard as one or another pole of the opposition. Mention the actor, and get heard as another form of transactionalism. Mention the importance of the cultural construction of anything at all, and get heard as another form of "culturology" or "subjectivism." Mention the importance of theorizing anything at all and get heard as another form of objectivism. Perhaps Bloch is right after all, and I am misreading other people's syntheses, locating them on one side or the other, as he is misreading the one that I have been trying to represent. I will come back to this point later, particularly with reference to the subjectivism/objectivism dichotomy.

Coming from the other direction, Sharon Stephens questions "the possibility and desirability of developing a unified, general theory of practice at all." Instead she proposes that anthropologists rethink the concept of culture, "in order to explore and systematically compare very different modes of making and unmaking humanly constructed, historically developing worlds." Now it may be mischievous of me but it seems to me that this formulation, with its emphasis on "making and unmaking", is already paying some dues to a practice perspective. But more generally, as I indicated a moment ago in the discussion about actors, I would resist what I hear as a call, albeit a very sophisticated and eloquently argued call, for a new form of cultural particularism. I agree that different cultures construct actors, structure, and history very differently from our own, and that a large part of our project is to understand this. But I disagree with the suggestion that "a

general theory of the system and its relation to practice [will] obscure rather than illuminate" these relations. On the contrary it seems to me that the examination of cultural constructions of persons, of social life, and of history, on the one hand, and of theories of what we call agents and systems on the other, take place most fruitfully in dialogue with one another. Indeed each alone tends to be a dead end.

Turning now to the remaining two terms for my discussion, I will first take up what is usually referred to as reflexivity. Reflexive anthropology argues that both our categories of analysis and our styles of writing our work are warped by our own history and by the structures of capitalism and/or colonialism. Its practitioners urge us to focus on our own modes of representing other cultures, and to attempt to develop alternative modes that would somehow break through these distortive screens of thinking and writing.

There are many things to say about this position, but for today I wish to make only one set of interrelated points, using the paper by Jane Collier and Sylvia Yanagisako as a way of focusing the issues. Specifically, I want to argue that reflexive anthropology has excluded feminist anthropology from its self-defined domain at some real cost to itself; that there is no good reason for this, and several bad ones; and that at least certain forms of feminist anthropology (as exemplified by Collier and Yanagisako among others) actually offers a more desirable and powerful model for incorporating the reflexivist insight into anthropological work.

The exclusion of feminism from the key text collections of contemporary reflexive essays is rather extraordinary. There is a long passage in James

Clifford's introduction to Writing Culture in which he wrings his hands and says that he just can't figure out how feminism got left out of the book but somehow it just happened.

Of course it is the case that there is a great deal of conventional social science work done from a feminist perspective. It is also the case that the feminist work that does challenge received categories of analysis does not do so primarily through experimentation with new forms of prose, discourse, and presentation. Nonetheless, there is a large body of feminist anthropology (including here works by Collier, Yanagisako, Michelle Rosaldo, Harriet Whitehead, and with all due humility Ortner) which could reasonably be classified as reflexive, and which has been operating in that mode for a good 15 or so years. Collier and Yanagisako exemplify the point nicely, prying open from a feminist perspective our assumptions about the relationship between the practical and the symbolic, production and reproduction, collective and individual interest, and even that old sacred cow, the sacred and the profane. Other arenas in which feminists have argued that we have fundamentally misread and mis-written the natives, because we have been trapped in our own categories, include the debate over the universality of male dominance, and the debate over the meaning of equality in so-called egalitarian societies.

Up to this point, feminists (or at least the not insignificant subset that have concerned themselves with these issues) and reflexivists have been going down the same track, arguing that the discoveries of ethnographic research must be allowed to return to subvert our analytic categories and to transform the lenses through which we look at our own and other societies. Thus both are analogously suspicious of attempts to translate native categories directly

into theoretical terms. But they handle this suspicion differently, and this is where they very decisively part ways. The reflexivists' diffidence toward the native categories leads them to seek new and experimental modes of representation, which would allow the native categories to be heard through and around the distortions of social science discourse. The analytic emphasis comes to rest heavily on these representational modes, and on the history and practices of our own tradition that have generated these modes. In the most problematic extreme, ethnography loses all intrinsic justification, becoming merely a moment in the Western intellectuals' project of self-understanding.

The feminists' diffidence toward the native categories leads them in quite the opposite direction. They criticize unselfconscious analytic work as a way of illuminating our own ideologized categories much as the reflexivists do. But they make this a moment in a fundamentally ethnographic project, in which both other cultures and our own are equally subject to critical analysis. This may be done in a variety of ways, although again one of the dominant styles of analysis is currently a variety of practice analysis, in which native categories are illuminated by setting them in local contexts of social practice and the production of meaning. Such work thus sustains (one could also say it anticipated) the central critical insight of reflexive anthropology. But it puts this insight back into the effort of analysis and interpretation of cultures (including our own), and does not privilege either our representational styles, or the history and ethnography of our own society, as the primary objects of anthropological attention.

I turn finally to my last major category for today, "history." Maurice Bloch faults me for neglecting it in the theory paper, and says that "it is much

more in the new rapprochement between anthropology and history that the really exciting things seem to be happening." I would agree, and would note again that I have just finished a work of historical anthropology myself. But the general area of historical anthropology contains at the moment one of the sharpest oppositions in the field: that between the so-called political economy approach (as exemplified most recently in the work of Eric Wolf, Sidney Mintz, Richard Fox, and others), and what I think some people are starting to call structural history but what I will call the histographic (i.e., historical-ethnographic) approach (this would include works by many of the practice theorists discussed earlier, but also works like Geertz's Negara, Bloch's own recent book on Malagasy history and ritual, Kelly's Nuer Conquest and so forth). In the political economy approach, as I characterized it, the analytic emphasis is on impingement of external forces on the society in question. For the histographers, on the other hand, the emphasis is on internal developmental dynamics of the society. Three years ago, in the theory paper, I called attention to the opposition between these two contemporary schools, and in some quarters the opposition has been getting sharper since then.

There are certain historical reasons for the current antagonistic relations between the two schools. As I indicated in the paper, the relationship is in many ways a continuation of that between symbolic anthropology and cultural ecology in the sixties, even including continuity of many key players. But there are also many more terms of shared perspective between the two schools than there were 20 years ago, and this needs to be noted first today. In particular, there is a wing of the histography camp that takes a good deal of its inspiration from Marx, as do the political economists, and here the

similarities between the two schools are at least as striking as the differences. Their parallel commitments to a historical perspective is of course one point of commonality. Further, the historiography side is much more interpretively and analytically critical than it used to be, whether in a strict Marxist sense, or simply in the sense of attention to more broadly defined structures of domination. And finally, it seems to me that the historians have largely accepted the necessity for considering the impact of external political/economic forces on a society's history and culture. This is a real shift from the '60's, when the external forces in question were those of the natural environment, which most of the people now doing historiography wouldn't touch with a ten foot pole.

The historiographic anthropologists, however, still seek much more extensively to show the way in which the impact of external forces is internally mediated, not only by social structural arrangements (acknowledged as important by the political economists as well) but also by cultural patterns and structures of various kinds. This strong emphasis by the historians, both Marxist and non-Marxist, on the importance of cultural mediation, reinterpretation, and transformation of outside forces is probably the main point of difference between the two schools at this time, and it brings us back to the old subjectivist/objectivist controversies of the 60's. Here we are hearing the old familiar name calling. The political economists (e.g., R. Fox, Lions of the Punjab) accuse the historians of "culturology," of a form of idealism or mentalism that does not recognize the real world. The historians accuse the political economists of ethnocentrically projecting their notions of agency and social action into other times and places, calling it the real world when it is only their own unrecognized image of it. I find this all

very depressing, particularly since I thought we had beaten that particular horse to death some time ago.

Perhaps I should have taken my own comments in my '84 paper more seriously, when I said that this opposition, and others that are linked to it, may be too deeply rooted in the practices of our trade to be got rid of. Bourdieu reiterates the point today.

As I have pondered the problems in writing these remarks, I have come to the conclusion that we will never mediate this opposition, because we have inappropriate notions of mediation. In the worst case, mediation appears as merging or synthesis, which everyone opposes, since all would lose their identities. In the intermediate case, mediation appears as a dual perspective, in which both sides accept the fact that the world is both subjectively and objectively constituted. I believe this view is correct, but I do not believe it can be sustained in practice, since the underlying opposition is posed precisely as an opposition, an either/or relation.

Instead of trying to mediate, then, I urge acceptance of this opposition, but within a controlling theoretical framework. And here I return to practice theory, which is in itself a theory of translation between an objective world and a subjective one, between a world constituted by logics beyond actors' perceptions, and a world constituted by logics spun by thinking and acting agents. Practice theory always has two moments, one largely objectivist and one largely subjectivist. In the first, the world appears as system and structure, constituting actors, or confronting them, or both, and here we bring to bear all our objectivist methodologies. But in the second, the world

appears as culture, as symbolic frames derived from actors' attempts to constitute that world in their own terms by investing it with order, meaning, and value.

Practice theory in fully developed form attends seriously to both of these moments. But its special contributions lie in the ways in which it plays on the margins between them, examining those processes by which the one side is converted into the other. Thus we watch actors in real circumstances using their cultural frames to interpret and meaningfully act upon the world, converting it from a stubborn object to a knowable and manageable life-place. At the same time we watch the other edge of this process, as actors' modes of engaging the world generate more stubborn objects (either the same or new ones) which escape their frames and, as it were, re-enter ours. Here subjective and objective are placed in a powerful and dynamic relationship, in which each side has equal, if temporary, reality, and in which it is precisely the relationship between the two that generates the interesting questions.

At one level, then, the friction between historiography and political economy represents perhaps the most problematic relationship in contemporary anthropology. At another level, however, it is perhaps the most hopeful area, in that it may force us finally to rethink and possibly resolve our most tenacious opposition, that between subjective and objective, emic and etic. And here I think practice theory offers real promise, since it embraces this opposition within itself, and theorizes it as a productive rather than a destructive relationship.

On this optimistic note, then, I will end my remarks. I would repeat here the point with which I started the theory paper: The worst enemy of our field is fragmentation and disengagement. The most important thing is that we keep talking to each other; preferably in civil terms, but I would still take name calling if the only choice were between name calling and silence. If I have made some small contribution to re-engaging the dialogue, then, I am delighted to have done so.

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